Wealth Shocks and Health Outcomes: Evidence from Stock Market Fluctuations

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JOB MARKET PAPER

November 8, 2011

Abstract

Do wealth shocks affect the health of the elderly in developed countries? The economic literature is skeptical about such effects which have so far only been found for poor retirees in poor countries. In this paper I provide evidence 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 over the past two decades 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 2-4% of a standard deviation in physical health, mental health and survival rates. Effects are heterogeneous across health conditions, with most pronounced effects for the incidence of high blood pressure, smaller effects for heart problems and strokes and no effects for arthritis, diabetes, lung diseases and cancer. Further, the impact on physical health and mortality increases with age and negative wealth shocks tend to have stronger effects than positive ones. The comparison with benchmark regressions suggests that effects are larger than the average causal effect of wealth on health in the sample.

JEL Classification: G10; I10; J14

Keywords: health; mortality; pensioners; stock market

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1 Introduction

Richer people are healthier, happier and live longer. Little is known, however, about the causal effects underlying this relationship. 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. A broad literature has investigated to which extend this relationship is driven by effects of wealth or income shocks on health outcomes. So far such effects have been documented only for poor retirees in Russia and South Africa. For adult health in developed countries existing studies find little evidence of any income or wealth effects and tend to emphasize reverse causality.[1] Contrary to this literature the findings in this paper suggest that wealth shocks affect physical health, mental health and mortality of wealthy retirees in the US.

To tackle the endogeneity of wealth changes, this paper exploits stock market fluctuation in the wealth of US retirees. 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 two decades generated dramatic unexpected gains and losses in the wealth of stock holding retirees. I analyze this natural experiment using 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.

These data allow me to 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 health outcomes. I find that a 10% change in life-time wealth over a two year period is associated with a change of 2-4% of a standard deviation in four different health measures: an index of health conditions, self-reported health, mental health and the probability to survive to the next interview two years ahead. The analysis of individual health conditions reveals a strong effect on hypertension, smaller effects on heart diseases, strokes and psychiatric problems but no effect on arthritis, diabetes, lung disease and cancer. Further there is evidence of effect assymetry and heterogeneity across age. Effects on physical health and survival rates seem to increase with age. And negative shocks tend to have a stronger effect than positive ones. The comparison with benchmark OLS regressions suggests that estimated effects are larger than the average causal effect of wealth on health.

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

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

ucated, 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 positive - stock market changes. As a consequence constructed wealth shocks are likely to be correlated with unobserved 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. 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 constructed wealth shocks or the macroeconomic environment that do not run through stock wealth. For example, stock market changes might be correlated with changes in other wealth components such as house prices. I show that the correlation of the stock market with the overall wealth of households without stocks is essentially zero. This suggests that there is not much of an effect of the stock market on non-stock wealth. But there could be non-wealth effects of the stock market or macro shocks on retiree health. I argue that retirees without stocks are at least equally strongly affected by such direct effects as those with stocks. I include time effects to absorb any macroeconomic shocks common to both groups.

The evidence of strong positive effects of wealth shocks on health presented in this paper is in contrast with much of the existing literature. As Cutler, Lleras-Muney and Vogl (2011) state 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 from this literature typically use either approaches related to Granger causality or aggregation to deal with the endogeneity of income and wealth. Looking at the HRS data Adams et al. (2003), Smith (2005) and Michaud and Van Soest (2008) show that lagged wealth conditional on socio-economic status or changes in wealth do not correspond with changes in health. I replicate Smith's (2005) findings and discuss Adams et al. (2003) in detail below. Measurement error in stock 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 uses aggregate income or wealth variation. Ruhm (2000) analyzes mortality-unemployment state-level time series finding that higher unemployment is associated with lower mortality. Deaton and Paxson (2001) and Deaton and Paxson (2004) look at mortality-income cohort times series for the US and the UK, respectively. Their results indicate that periods of greatest mortality declines do not match with periods of greatest income increases. Adda et al. (2009) follow a similar approach analyzing income and health outcomes of English cohorts with a particular focus on income shocks. They find little effects for a broad set of health outcomes but a negative association with mortality. Snyder and Evans (2006) compare cohort mortality around the so called 'Social Security notch' finding that the notch generation which received lower Social Security had slightly lower mortality.[2] Endogeneity of wealth and measurement errors are less of a problem in aggregate data. 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). Group averages are independent of individual-specific endogeneity and cancel out random measurement error. However, aggregate income might be correlated with macro shocks that also have non-income effects on health. This invalidation of the exclusion restriction, as the authors of these papers note, makes it difficult to infer causal effects of wealth or wealth shocks on health from these findings.[3]

So far only a few papers identify income shocks at the micro level. Jensen and Richter (2003) look at the pension crisis in Russia during which many retirees were not paid their pensions for an extended period of time. They find that for affected pensioners, a 24% decrease in income leads to a decrease in nutrition and health care use, while the likelihood to die over the next two years increases by 5%. Case (2004) analyzes the effect of a pension reform in South Africa that lead to a large income increase for coloured pensioners. She finds positive health effects, which run through improved nutrition, living conditions and psychological factors such as a reduction in stress. Lindahl (2005), Gardner and Oswald (2007), and Apouey and Clark (2009) analyze lottery winnings. All of them find positive effects of winnings on mental health, while results are less conclusive for physical health. Finally, Sullivan and von Wachter (2008) find that exogenous job displacements increase the mortality hazard of male US workers by 50-100% during the years following the job loss, while long-run earnings decrease by 15-20%.

Should we, however, expect positive physical health effects found for poor retirees in poor countries or for displaced workers in the US to carry over to wealthy, stock holding US retirees? Health inputs like medical treatment, medication or mere calorie intake might be affected by wealth shocks for poor retirees in Russia or South Africa. But this is probably 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.

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

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

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 a positive effect on health (for a review see Chida and Steptoe 2008). This paper finds strong wealth shock effects on high blood pressure and mental health and smaller effects on heart problems and strokes. This is exactly the kind of health response the bio-medical literature would predict if wealth shocks have an effect on psychological stress.[4] However, this does not rule out that health is also directly affected through health inputs or consumption.

The focus of this study on 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, the elderly are closer to the margin of severe health conditions than younger adults so that wealth effects on health are more likely to become manifest in measurable health outcomes.

However, caution must be exercised when extrapolating from my estimates of wealth shock effects on retiree health 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. 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 benchmark regressions indeed shows that the pattern of estimated effects is different from the overall relationship of wealth and health in the sample. While wealth shocks affect only particular health conditions, wealth levels have a positive association of similar magnitude with all of them. And for the affected conditions the impact of wealth shocks is 1.5 to 3 times as large as the overall relationship. However, given aging societies in the Western world and the ongoing shift from defined-benefit to defined-contribution pension schemes the effect of stock market induced wealth shocks on retiree health is not only of scientific interest but also of increasing political importance.

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.

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

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 gains and losses for retirees owning stocks. This natural experiment comes quite close to the ideal setting. As stock market changes are unpredictable for retirees without insider information buying 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 demographics:

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

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.

2.1 Are constructed wealth shocks causal?

Stock market changes are unpredictable without insider information (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 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.[5] 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.

One concern might be that retirees with the same fractions 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. To rule out that results are driven by such potential

[5]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.

correlation of the stock market cycle with the type of investor I present two stage least squares regressions in which I instrument actual stock holdings with the individual's average stock holdings over the entire observation period or initial stock holdings in the first period. Both average and initial stock holdings are constant over time for a given individual. Hence they are uncorrelated with were we are in the stock market cycle. Although all results carry over with the instrumental variable estimate, the more efficient ordinary least squares estimates are chosen as baseline specification.

Another way to check whether estimated effects are driven by changes in investor types is to include predetermined demographic controls ($X_{i,t}$). 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, region, etc. should change the coefficient on wealth shocks. However, adding a wide range of demographic controls to the baseline specification hardly changes any of the estimates. Following a similar logic, it is insightful to look at simple OLS benchmark regressions of health on wealth and other socio-economic variables. Assume that constructed wealth shocks merely pick up a relationship of health with the investors' socio-economic background. Then the OLS benchmark regressions should reveal a similar pattern across different health measures as found for constructed wealth shocks. However, the pattern emerging from OLS regressions is clearly different.

Still one might worry 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 during the observation period in Figure 1 suggests that this is unlikely to be the case. Even though there is an overall positive trend between 1992 and 2009, negative and positive changes follow each other towards the end of the sample. It is hard to imagine that health profiles of stock holders just happen to follow these ups and downs by chance. However, health profiles might be slightly correlated with the overall trend in the stock market leading to a spurious relationship of health changes with constructed wealth shocks. If this were the case we should find a similar pattern in OLS benchmark regressions of health changes on socio-economic background. But as for health levels, the pattern emerging from benchmark regressions of health changes on lagged wealth is very different from the effects of constructed wealth shocks. Finally, I present regressions where I include placebo shocks in addition to the real wealth shocks. I interact the lagged stock fraction with changes in the unemployment rate instead of the stock market. And I interact the stock market change with the lagged wealth fraction held in bonds instead of stocks. Both of these two measures are strongly correlated with constructed wealth shocks. Still, their inclusion does not significantly change my estimates (results reported in the Web Appendix[6]).

To sum up, it seems unlikely that a correlation of the stock market cycle with investor types or with investors' health profiles is driving the results. This suggests that constructed wealth shocks are indeed causing the observed changes in health. To interpret

[6]The Web Appendix is available at http://www.econ.upf.edu/jobmarket/schwandt.html

this causal effect as the effect of wealth shocks on health it is necessary to control for stock market effects on health that might not run through stock wealth.

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, especially after wave 4 when lagged stock holdings are measured more precisely (see Data section). 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 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. 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

Constructed wealth shocks under- or overestimate actual wealth shocks if retirees' expectations of stock market returns systematically differ from zero. Luckily the last four HRS waves include 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 see 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.

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 implies less precise estimates but no attenuation towards zero.

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 (see Web Appendix).

In the empirical specification in (2) I regress changes in health directly on constructed wealth shocks. An alternative specification would be the two-stage least squares regression with constructed wealth shocks as an instrument for changes in reported wealth. Such a specification would provide 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. Regressions of changes in reported wealth on constructed wealth shocks indeed provide evidence of such consumption smoothing (see Findings section). From a policy perspective, however, 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 the direct regression of health changes on constructed wealth shocks is chosen as the baseline regression. Results from two-stage least squares regressions are reported in the Appendix (Table A.9).

3 Data

The data used in this study come from the first 9 waves of the Health and Retirement Survey (HRS), covering the years 1992 to 2009.[7] The HRS is a biannual panel starting in 1992 with 12,654 individuals representing US adults aged 51 - 61 in 1992 (born during the years 1931 - 1941). In 1998 and 2004 new cohorts were added to keep the sample representative for those of age 51 and older. 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. both financial respondent and spouse were neither working for pay (i.e. neither working, nor part-time working, nor partly retired) nor unemployed or both considered themselves completely retired. The final regression sample consists of about 39,500 person-year observations, of which 19,000 refer to singles. 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). 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

[7]The data is drawn from the RAND HRS file. Variables that are not included in the RAND file were added from the HRS raw data. The AHEAD waves are not included as they have been found to suffer from systematic underreporting of stock wealth (Rohwedder et al. 2006)

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.

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 simply take past year's annual income from pensions, annuities, old age social security and veteran benefits as the expectation for future income.[8] 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.

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

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 households with at least one spouse 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 (see Web

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

Appendix).

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 known the percentage of each IRA invested in stocks.

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

For the three initial HRS waves, 1992 - 1996, no information is available on IRAs invested in stocks. In order not to lose these entire waves, IRAs in these years are assigned the average IRA stock investment rate of the year 1998 (52%). This adds a considerable amount of noise and results are tested against the exclusion of these waves.

Table 1 summarizes sample characteristics and main wealth measures per HRS wave (for further wealth summary statistics see Table A.1 in the Appendix). In 1998 older than average cohorts are added and younger cohorts in 2004, leading to discontinuous jumps in these measures. Retiree rates increase with age, but even at age 70 for 30% of the households at least one spouse is still in the labor force. The fourth and fifth row show the information available on the fraction of IRAs invested in stocks and the respective imputed values. The regression sample includes all households 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. Due to the assumption that any IRA is invested 52% in stocks the fraction of households with stocks is inflated in the first three waves. For the same reason stock holdings in these waves are artificially low because many poor households with small IRA accounts that in reality do not own any stocks are included in the group of stock holders.

3.2 Stock market data and constructed wealth shocks

Interview months are reported in the HRS. This allows to match the data with the S&P500 on a monthly basis.[9] 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 and range from -6% to +8%.

Figure 2 plots constructed wealth shocks and the S&P500 over time. Each circle represent one household (i.e. financial respondent) and is placed at the month of the current 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, however, that these dramatic wealth shocks are just constructed and might not correspond to real changes in wealth. Their predictive power is assessed in the Findings section.

3.3 Health data

I use different health measures from the HRS as dependent variables: An index of health conditions, 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 of these measures which are reported on different scales and represent health circumstances of different severity, all measures are transformed the following way. First, 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. To make effect sizes comparable across measures, I follow an approach of van Praag and Ferrer-i-Carbonell (2008) and assign to the categories of each measure the expected value of a standard normal variable conditional on being between the category's lower and upper 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 refer to this as 'probit-adapted OLS'). Summary statistics of original and transformed health measures are reported in the Appendix, Tables A.3 and A.4.

The index of health conditions equals the sum of conditions which have *ever* been di-

[9] 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.

agnosed by a doctor according to the respondent. The HRS questionnaire includes eight conditions: high blood pressure, heart disease, stroke, arthritis, cancer, diabetes, lung disease and psychiatric problems. 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.[10] 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 health conditions index, the mental health index is inverted for regressions so that higher values indicate better mental health. Due to coding differences I do not include the mental health index in the first two waves.

Deaths of survey participants are documented since the third wave (1996). In so-called exit surveys a proxy respondent (usually a surviving family member) is interviewed about time and circumstances of the death. Thus deaths are well documented and not just one possible reason for an observed panel attrition. '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 8 can be included.

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

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: age, age^2, age^3, years of education and 1 dummy for sex, 2 for race, 4 for region of residence, 4 for degree, and 7 for lagged marital status.

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. Including a full set of year x month dummies leads to very similar results (see Web Appendix). Health outcomes and demographics vary at the individual level, wealth at the household level and the stock market at the monthly level. As explained in the Data section all health measures are transformed such that changes are interpreted in terms of standard deviations and positive changes always refer to a health improvement. Alternative transformations such as OLS with standardized measures or ordered probit regressions with original measures lead to very similar results.[11] Standard errors are clustered by households. Clustering at the level of individuals, interview dates or stock market changes result in very similar standard errors.

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. Summary statistics of demographic controls are reported in the Appendix, Table A.2.

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

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 0.74. Excluding the first waves for which the information on stocks in IRAs is noisy results in a slight increase to 0.83. 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.[12] 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 1.6% 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 slightly negative in the overall sample and not distinguishable from zero for the years with exact data on stock holdings. 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 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, the stock market change and year fixed effects. In column (2) a broad set of demographics is added and in column (3) the first four waves with noisy information on stocks in IRAs are excluded. 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 terms of standard deviations.

The regressions in the first column indicate a positive effect of constructed wealth shocks on the index of health conditions, self-reported change in health, the mental health index and survival. Effects are of similar size, ranging from 0.2 to 0.35. This means that a 10%

[12] 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. However, 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.

wealth shock is associated with a change of 2-3.5% of a standard deviation in the different health measures. Only for changes in self-reported health is the coefficient small and not significantly different from zero. Including a broad set of demographic controls 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. Excluding the first four waves slightly increases most of the estimates but the overall pattern does not change. The increase in the coefficients could be driven by the greater precision in stock holding data but also by the changing age composition of the sample over time or by qualitative changes in the nature of stock market shocks.

In Table 4 I repeat these regressions separately for the eight health conditions from the health conditions 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 get the respective health condition) in terms of standard deviations. A problem of the analysis of various health conditions is that the chance of wrongly rejecting the null increases with every additional regression.[13] In the present setup, however, significant estimates would be in line with causal effects of constructed wealth shocks for some health conditions while they would not for others. Health changes are regressed on wealth shocks over a period of on average two years. If estimated effects on health measures are causal they 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, smaller effects on heart disease, strokes and psychiatric problems and no significant effect on arthritis, cancer, diabetes and lung disease. As in the regressions for health measures the inclusion of demographic controls hardly changes estimates. When the first waves are excluded in column (3) the effect on high blood pressure remains large and highly significant. The coefficients for heart disease, strokes and psychiatric problems, however, decrease slightly while standard errors increase rendering the effects insignificant. However, any combination of the wealth shock coefficients from the heart disease, stroke or psychiatric problems regressions is still jointly significant. For arthritis, cancer, diabetes and lung disease, on the other hand, neither pairs nor groups of three or four yield joint significance.

These heterogeneous effects across different health conditions make sense (for a medical text book describing these conditions see Fauci et al. 1998). High blood pressure is the most plausible driver for a short to medium term effect of wealth shocks on the health of wealthy retirees that are covered by health insurance. High blood pressure arises from both psychological stress as well as unhealthy nutrition and behavior. Moreover, high blood pressure is a cause for heart problems and strokes. Therefore a positive effect on heart problems and strokes is what one should expect given the strong effect on high blood pressure.

[13] 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 (see Anderson 2008).

Effects on arthritis, diabetes, lung diseases or cancer would be less plausible. Arthritis is determined by genetic disposition. Diabetes is driven by genes 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).

The effect on psychiatric problems seems rather small compared to the large effect on the mental health index in Table 3. However, it is likely that not every depression that might reveal itself in the mental health index has been diagnosed by a doctor. And people might be more likely to underreport the diagnosis of psychiatric problems than individual symptoms of depression, such as restless sleep or feeling alone, as the latter carry less stigma.

Looking at individual depression symptoms from the mental health index does not reveal a single driver such as hypertension for the health conditions index (results reported in the Appendix, Table A.5). This is what we should expect. 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.

5.3 Interaction with age, gender and the sign of shocks

Table 5 investigates the heterogeneity of effects across age and gender and the effect symmetry between negative and positive wealth shocks. 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 index of health conditions 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 (not reported). 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 stronger affected for women which would be in line with the literature on gender differences in mental health. However, the estimated differentials are imprecise and not even significantly different from zero in joint tests. These results do not imply that effects are the same for males and females but it seems that estimates are not driven by gender.

The effect asymmetry one typically has in mind when thinking of wealth shocks is that negative shocks outweigh positive ones. For survival this seems to be the case. A 10% wealth loss decreases the likelihood to survive almost six times as much as a 10% wealth gain would increase it. For the other health measures (except for the mental health index) the effect asymmetry goes in the same direction but differences are rather small and not significantly different from zero. Notice, however, that in the survival regression the last wave of mostly negative shocks is not included (since the survival coding requires knowledge of the vital status in the following period, see Data section). This means that the effect of negative shocks is estimated only for shocks in 2002/3, while regressions for the other health measures include negative shocks in 2002/3 and in 2008/9. This could explain why the asymmetry is stronger for survival if negative shocks had stronger effects in 2002/3 than in 2008/9.

Repeating regressions for the other health measures excluding the last wave indeed strongly increases the effects of negative wealth shocks (reported in the Appendix, Table A.6). This suggests that negative shocks in 2002/3 had a significantly stronger effect than positive shocks, while this cannot be said for negative shocks in 2008/9. There are two plausible reasons for why the negative shocks in 2008/9 might appear to have a relatively weak impact on health. First, in 2008/9 the stock market had just started to collapse in the months when the interviews were conducted while in 2002/3 the market had been going down already for about two years. This means that in the last wave there was less time for the effects of negative wealth shocks to become manifest in health outcomes. Second, in contrast to the burst of the New Economy stock market bubble in 2002/3, the 2008/9 stock market crisis came along with a collapse of the housing market and the overall economy. As argued in the Identification section the direct health effects of an economic crisis might be stronger for retirees without stocks. In this case the inclusion of time fixed effects attenuates the effect of negative shocks in 2008/9 but not in 2002/3.

5.4 Comparison with estimates from the literature

As mentioned above the economic literature is skeptical about any causal effects of income or wealth on adult health in the developed world. The two studies closest to mine, Smith (2005) and Adams et al. (2003), do not find evidence in the HRS data of causal effects of wealth changes on health. It is therefore important to clarify why my estimates are so different from the existing literature.

Using the first five waves of the HRS, Smith (2005) regresses health changes on reported changes in stock wealth while controlling for proxies of socio-economic status. He finds that this 'best measure of an exogenous wealth change - the wealth increase from the stock market - is only statistically significant in one instance (arthritis), and there it has the incorrect sign...' (p.230). These findings of zero effects are remarkable because we would expect potential endogeneity left in stock wealth changes to bias the estimate up and not towards zero. However, a more severe problem than potential endogeneity 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. In column (2) of Table 6 I substitute constructed wealth shocks by reported changes in stock wealth, replicating Smith's measure of wealth shocks. The resulting coefficients are essentially zero. In column (3) changes in reported wealth are divided by life-time wealth. This scales coefficients more reasonably but estimates remain small and largely insignificant. These results suggest that changes in reported stock wealth might be too noisy to uncover an existing relationship of wealth changes and health outcomes.

Adams, Hurd, McFadden, Merrill and Ribeiro (2003) 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. In their approach wealth is regressed in levels while I am analyzing changes in wealth. But much of the variation left in wealth levels after controlling for a broad set of socio-economic variables is likely to represent recent changes in wealth. Therefore my estimates of strongly significant causal effects of wealth shocks stand in contrast to the findings of Adams et al. (2003). 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. When including these additional waves and cohorts the results for most of the 40 analyzed health measures become inconclusive. Only for cancer, for female lung disease and for male hypertension Granger-causality can still be rejected. A plausible explanation for the different hypertension result could be contemporaneous wealth shock effects. The approach of Adams et al. (2003) tests for a causal effect of lagged wealth on health changes. But 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.

5.5 Comparison with benchmark regressions

How large are the estimated effects? A good way to assess the effect size is the comparison with simple OLS benchmark regressions. Such benchmark regressions do 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. Therefore benchmark regressions provide an upper bound of the (average) causal effect of wealth on health in the sample, in particular if few additional controls are included. The OLS equivalent to the baseline specification would be the regression of health changes on wealth changes. The attenuated coefficients on stock wealth changes in Table 6, however, suggest that such regressions might not be very informative. Indeed, using changes in overall wealth as a regressor results in similarly attenuated estimates (reported in the Web Appendix). Attenuation bias due to measurement error is less of a problem when regressing wealth in levels instead of changes. The levels equivalent to my baseline specification is the regression of health on the logarithm of life-time wealth.

As reported in Table 7 the coefficient on wealth is highly significant in all benchmark regressions.[14] However, the estimated effects of wealth shocks are about 25% above this benchmark for the index of health conditions and the mental health index. In other words, a 10% negative wealth shock leads to a slightly larger health decline than the health gap that is associated with a 10% wealth difference in the data. Lose 10% of your life-time wealth in the stock market and you end up with slightly worse health than your neighbor who has been 10% poorer before.

Benchmark regressions for individual health conditions in Table 8 indicate that this is still not the whole story. While wealth shocks affect only particular conditions the benchmark wealth gradient is strongly significant and of similar size for all health conditions, except for cancer.[15] And for hypertension, heart disease and strokes 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 benchmark 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 and healthier consumption than somebody owning \$300k which over time accumulates to a

[14]No estimates are reported for self-reported change in health and survival as these variables are only defined in first differences and there are no level-equivalents. For benchmark regressions of health in first differences on wealth levels see the Appendix, Tables A.7 and A.8. Further, benchmark regressions only include age, age squared and gender as demographic controls such that life-time wealth proxies for the overall socio-economic status. Including further demographic controls decreases the estimated coefficients on life-time wealth.

[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.

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.

The comparison of baseline and benchmark regressions also provide confidence that the causal 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. Tables A.8 and A.7 in the Appendix report benchmark regressions of health changes on wealth levels. Again the pattern in these regressions is different from the causal estimates.

5.6 Robustness checks

The results in Table 9 provide further evidence that results are not driven by a coincidental correlation of the stock market with investor types. In 2SLS regressions the interaction of the stock market changes with the individual's average stock fraction or the individual's initial stock fraction are used as instruments for constructed wealth shocks. Both the average and the initial stock fraction are time constant and therefore uncorrelated with the up's and down's of the stock market. The first two columns of Table 9 repeat the baseline results. In column (3) constructed wealth shocks are instrumented by the interaction of the stock market with the individual's average stock fraction over the whole observation period, while in column (4) the average for the post-1997 waves is taken. In column (5) the initial 1998 stock fraction is taken instead. Despite the significant loss of information that is implied by this strategy, most estimates in columns 3-5 remain significant. More importantly they do not go to zero but -if anything- tend to increase.

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 about sixty percent, 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 column (5) all households without stocks in the previous period are excluded so that the sample is restricted to the 'treated'. Despite the decrease in the sample size by two-thirds estimates do not change much and the overall effect pattern remains the same. In the final column the bottom quartile from the life-time wealth distribution is excluded, which again changes estimates only slightly.

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 2-4% 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 strokes and no effect on arthritis, diabetes, lung disease and cancer. The analysis of interaction terms reveals that effects on physical health and mortality are significantly stronger for the elderly. Further, negative shocks tend to have stronger effects than positive shocks. The comparison with benchmark regressions indicates that the effect pattern across health outcomes is different from the overall relationship of health and wealth in the data. While wealth shocks affect only particular health conditions, wealth levels have a positive association of similar magnitude with all of them. And for the affected conditions the impact of wealth shocks is 1.5 to 3 times as large as the overall relationship.

The economic literature on the wealth-health relationship has been skeptical about causal effects of wealth on elderly health in the developed world. So far 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. Further it has been shown that the measure of constructed wealth shocks is crucial to uncover this relationship. Constructed wealth shocks do not only allow for a causal interpretation of estimates due to the exogeneity of stock market changes. They are also less prone to measurement errors which are likely to dominate measures of reported wealth in first differences.

Little can be inferred from my results about the precise channels through which wealth shocks affect health outcomes. The pattern of effects suggests that psychological factors might play an important role. But whether these are psychological reactions to the mere arrival of news about future consumption or reactions to actual changes in consumption remains unclear. Further, health conditions like high blood pressure or heart diseases might also be directly affected by changes in consumption or in health inputs. I have looked at a range of potential channels in the HRS, including health behavior, health care utilization, insurance status, household structure and mobility as well as monetary transfers from and to children and expected bequests. Results are rather noisy and provide suggestive evidence at best. In some cases the HRS seems not to be the optimal data source, in others it is not clear in which direction the expected effect should go. Finding ways to examine the channels through which effects operate is a promising path for future research.

Another open question of importance is the timing of effects. It seems likely that the speed at which wealth shocks affect health outcomes varies across health measures as well as the persistence of effects. In the present study the rather small sample size, the noisy measure of wealth shocks and the limited number of stock market changes does not allow for a precise analysis of lags or of the exact path of the stock market between interviews. Comparable data from other countries and extended times series will be needed to advance on this frontier.

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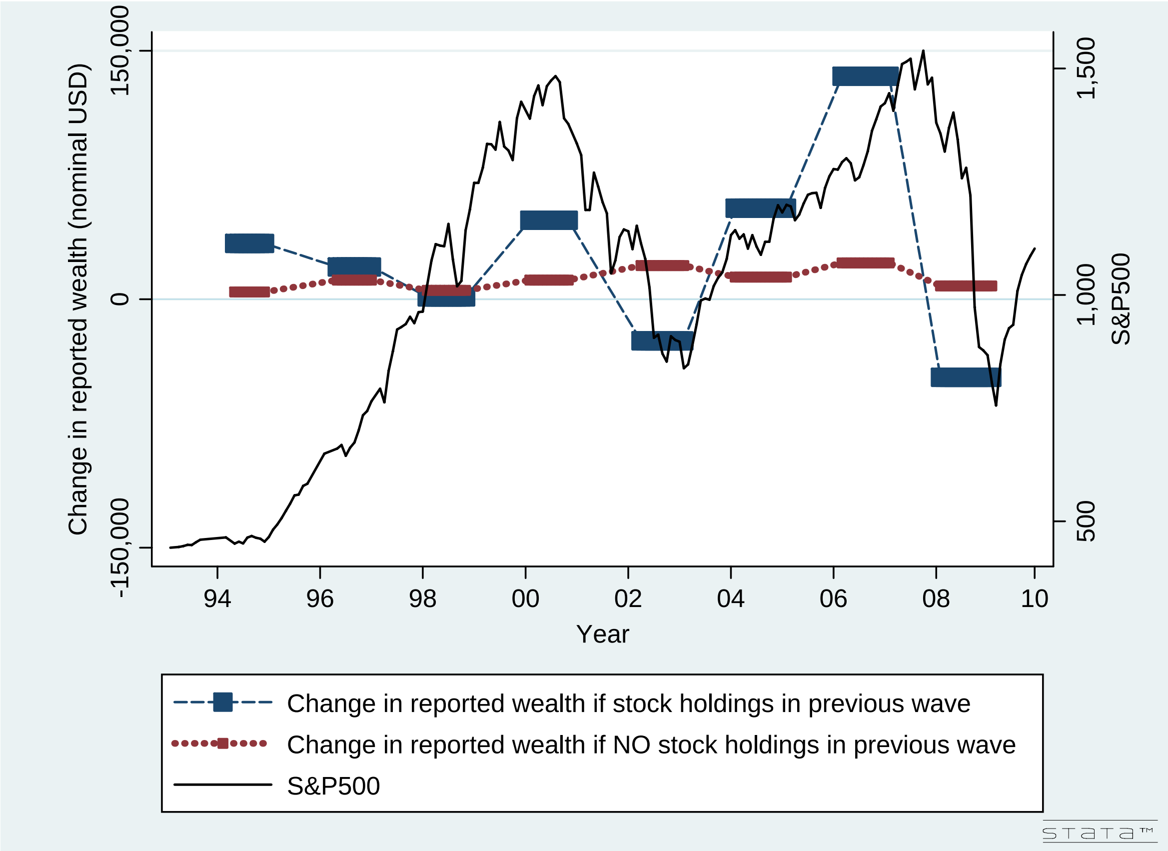
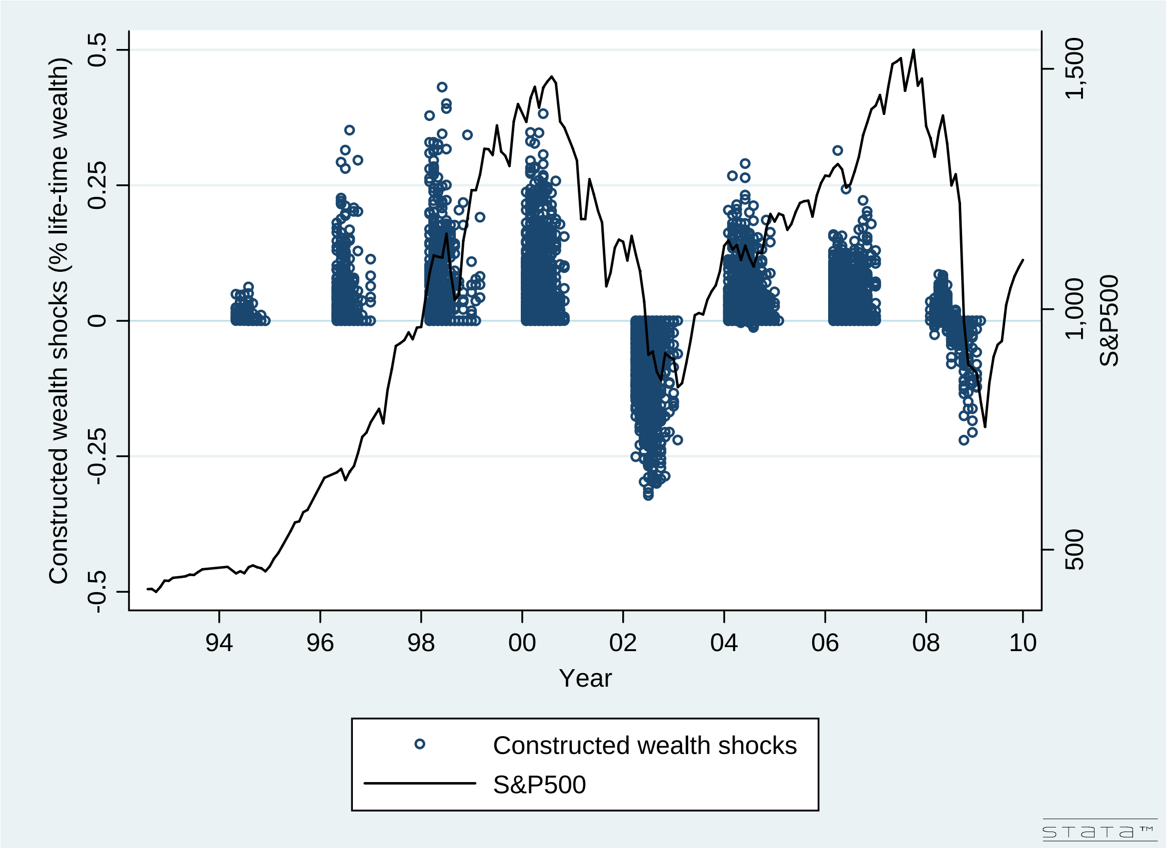
8 Tables and Figures

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



Average changes in reported 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. Information on stock ownership is noisy in the first three waves. 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, i.e. the interaction of the previous fraction of life-time wealth held in time with the stock market change between interviews ($\frac{s_{i,t-1}}{W_{i,t-1}} \frac{\Delta S\&P_t}{S\&P_{t-1}}$), 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	1	2	3	4	5	6	7	8	9
Year	1992-1993	1994-1995	1996-1997	1998-1999	2000-2001	2002-2003	2004-2005	2006-2007	2008-2009
Full HRS sample									
N	12,583	11,354	11,142	22,420	20,775	19,542	21,353	19,687	18,446
Age	55	57	59	67	68	69	67	69	70
% retiree households	0.22	0.30	0.38	0.63	0.66	0.70	0.64	0.68	0.70
Information '% of IRA in stocks'	none	none	none	3 categories	3 categories	3 categories	3 categories	exact %	exact %
Imputed % of IRA in stocks	52%	52%	52%	0, 50, 100%	0, 50, 100%	0, 50, 100%	0, 50, 100%	exact %	exact %
Regression sample									
N	749	1,739	2,422	7,719	9,026	9,152	9,270	8,990	6,745
Current wealth (nominal USD)	154,207	215,799	236,489	244,332	289,050	318,799	355,095	442,771	431,243
Life-time wealth (nominal USD)	330,419	472,135	481,758	406,933	442,261	480,235	529,501	610,719	747,729
Fraction owning stocks (excl IRAs)	0.27	0.32	0.30	0.29	0.30	0.29	0.29	0.26	0.25
Fraction owning stocks (incl IRAs)	0.33	0.42	0.43	0.32	0.32	0.31	0.31	0.29	0.27
...those owning stocks (incl IRAs)									
N	166	487	699	1,821	2,069	2,057	2,049	1,892	1,328
Life-time wealth (nominal USD)	607,805	771,061	776,806	763,855	825,181	899,127	1,036,519	1,226,753	1,240,089
% life-time wealth held in stocks	0.11	0.11	0.11	0.19	0.20	0.19	0.20	0.21	0.23
S&P500 change since past interview	-	0.09	0.46	0.71	0.32	-0.32	0.14	0.16	0.02
Constructed wealth shock	-	0.01	0.05	0.08	0.06	-0.06	0.03	0.03	0.003

New cohorts were added to the HRS sample in 1998 and 2004. 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). In the first three waves: due to the uniform imputation of the fraction of IRA invested in stocks the fraction of stock owners (incl. IRAs) is inflated and the fraction of life-time wealth held in stocks is deflated. Average constructed wealth shocks (last row) roughly resemble the product of the previous wave's fraction of life-time wealth in stocks and the S&P500 change since past interview. Further wealth summary statistics are reported in the Appendix, Table A.1.

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

Dependent Variable: Wealth Change	Full sample (1)	Year>1999 (2)	Full sample (3)	Year>1999 (4)
Constructed wealth shock	0.739***	0.827***		
	(0.154)	(0.194)		
Lagged stock fraction	-0.305***	-0.307***		
	(0.066)	(0.069)		
Lagged stock ownership x			0.145***	0.222***
Stock market change			(0.039)	(0.053)
Lagged stock ownership			-0.057**	-0.055**
			(0.024)	(0.025)
Stock market change	-0.056**	-0.032	-0.058*	-0.048
	(0.028)	(0.033)	(0.032)	(0.038)
Demographics	✓	✓	✓	✓
n	29,904	26,971	29,904	26,971
R^2	0.003	0.003	0.002	0.002

The dependent variable is the change in reported household wealth divided by lagged life-time wealth ($\Delta A_{i,t}/W_{i,t-1}$). Constructed wealth shocks are the interaction of the lagged stock fraction and the stock market change ($\frac{s_{i,t-1}}{W_{i,t-1}}\frac{\Delta SP_t}{SP_{t-1}}$). 'Lagged stock ownership' is a dummy indicating stock ownership in the previous period. Regressions include only one observation per sample household and year. For details on wealth measures and sample restrictions see the Data section. Standard errors, in parenthesis, are clustered by household.

Table 3: Baseline Regressions of Health Measures on Wealth Shocks

Dependent Variable	(1)	(2)	(3)
Δ Index of Health Conditions	0.220***	0.225***	0.249***
	(0.063)	(0.063)	(0.070)
n	32,079	32,079	30,048
Δ Self-reported Health	0.067	0.096	0.212
	(0.117)	(0.117)	(0.136)
n	39,292	39,292	35,045
Self-reported Change in Health	0.340***	0.332***	0.235*
	(0.116)	(0.114)	(0.128)
n	39,315	39,315	35,066
Δ Mental Health Index	0.291**	0.313**	0.408**
	(0.139)	(0.139)	(0.160)
n	32,880	32,880	31,043
Survival	0.161*	0.188**	0.233**
	(0.098)	(0.091)	(0.107)
n	31,333	31,333	27,301
Controls			
Main effects	✓	✓	✓
Demographics		✓	✓
Restricted to year>1999			✓

The coefficient on constructed wealth shocks ($\frac{s_{i,t-1}}{W_{i,t-1}} \frac{\Delta SP_t}{SP_{t-1}}$) is displayed. A positive coefficient refers to a health *improvement* in the respective dependent variable in terms of standard deviations (see Data section). '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. In column (3) the first three waves with no data on stocks in IRAs are excluded. The estimation method used is OLS. Standard errors, in parenthesis, are clustered by households.

Table 4: Baseline Regressions of Health Conditions on Wealth Shocks

Dependent Variable	(1)	(2)	(3)
Δ High blood pressure	0.166***	0.167***	0.172***
	(0.057)	(0.057)	(0.064)
n	37,064	37,064	33,990
Δ Heart disease	0.118*	0.123*	0.113
	(0.067)	(0.067)	(0.079)
n	38,479	38,479	34,327
Δ Stroke	0.092*	0.089*	0.082
	(0.050)	(0.050)	(0.060)
n	39,010	39,010	34,896
Δ Diabetes	0.019	0.010	-0.007
	(0.036)	(0.036)	(0.039)
n	38,493	38,493	34,638
Δ Cancer	0.050	0.058	0.043
	(0.047)	(0.048)	(0.055)
n	38,870	38,870	34,792
Δ Arthritis	0.000	0.005	0.021
	(0.066)	(0.066)	(0.072)
n	36,843	36,843	33,829
Δ Lung disease	0.047	0.049	0.043
	(0.034)	(0.034)	(0.038)
n	38,301	38,301	34,428
Δ Psychiatric problems	0.072*	0.073*	0.059
	(0.040)	(0.040)	(0.046)
n	37,819	37,819	34,037
Controls			
Main effects	✓	✓	✓
Demographics		✓	✓
Restricted to year>1999			✓

The coefficient on constructed wealth shocks ($\frac{s_{i,t-1}}{W_{i,t-1}} \frac{\Delta SP_t}{SP_{t-1}}$) is displayed. A positive coefficient refers to a health *improvement* in the respective dependent variable in terms of standard deviations (see Data section). '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. In column (3) the first three waves with no data on stocks in IRAs are excluded. The estimation method used is OLS. Standard errors, in parenthesis, are clustered by households.

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

	Interaction category								
	Age			Gender			Sign of shocks		
	<=75	>75	Δ (p-value)	Male	Female	Δ(p-value)	Shocks≤ 0	Shocks≥ 0	Δ (p-value)
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ Index of Health Conditions	0.140*	0.299***	0.214	0.186*	0.241***	0.671	0.354*	0.235**	0.629
	(0.080)	(0.099)		(0.103)	(0.080)		(0.212)	(0.116)	
Δ Self-reported Health	0.093	0.099	0.982	-0.032	0.168	0.371	0.140	-0.116	0.549
	(0.145)	(0.186)		(0.175)	(0.150)		(0.382)	(0.206)	
Self-reported Change in Health	0.316**	0.166	0.509	0.138	0.470***	0.145	0.204	0.136	0.889
	(0.152)	(0.170)		(0.178)	(0.145)		(0.425)	(0.224)	
Δ Mental Health Index	0.334*	0.305	0.918	0.148	0.436**	0.283	0.267	0.350	0.871
	(0.187)	(0.208)		(0.204)	(0.183)		(0.474)	(0.223)	
Survival	0.066	0.369**	0.105	0.221	0.147	0.696	1.189***	0.180	0.029
	(0.081)	(0.169)		(0.156)	(0.111)		(0.428)	(0.171)	
Controls (interacted)									
Main effects		✓			✓			✓	
Demographics		✓			✓			✓	

The coefficients on constructed wealth shocks ($\frac{s_{i,t-1}}{W_{i,t-1}} \frac{\Delta SP_t}{SP_{t-1}}$) 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 in terms of standard deviations (see Data section). All controls are interacted (except time effects in the 'sign of shocks' regressions) and all waves are included. '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. Numbers of observations are the same as in the baseline regressions, Table 3, column (2). The estimation method used is OLS. Standard errors, in parenthesis, are clustered by households.

Table 6: 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 Index of Health Conditions	0.225***	0.00007	0.002
	(0.063)	(0.00005)	(0.009)
n	32,079	32,079	32,079
Δ Self-reported Health	0.096	0.00007	0.032*
	(0.117)	(0.00008)	(0.017)
n	39,292	39,292	39,292
Self-reported Change in Health	0.332***	0.00006	0.011
	(0.114)	(0.00006)	(0.017)
n	39,315	39,315	39,315
Δ Mental Health Index	0.313**	0.00004	-0.010
	(0.139)	(0.00011)	(0.020)
n	32,880	32,880	32,880
Survival	0.188**	-0.00004	-0.012
	(0.091)	(0.00008)	(0.019)
n	31,333	31,333	31,333
Controls			
Main effects	✓	✓	✓
Demographics	✓	✓	✓

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 clustered by households.

Table 7: Benchmark Regressions of Health Measures on ln of Life-time Wealth

	Regression specification	
	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 $lnW_{i,t}$
Dependent Variable ($H_{i,t}$)	(1)	(2)
Health Conditions Index	0.225***	0.175***
	(0.063)	(0.009)
Self-reported Health	0.096	0.291***
	(0.117)	(0.007)
Mental Health Index	0.313**	0.232***
	(0.139)	(0.008)
Controls		
Main effects	✓	
Demographics	✓	
Age, age^2, male		✓

In column (1) the coefficient on constructed wealth shocks is displayed; for further comments on the baseline regressions see Table 3. In column (2) the coefficient from OLS regressions of health levels on ln life-time wealth is displayed. Only age, age^2 and male are included as controls such that life-time wealth proxies for the overall socio-economic status. The inclusion of further controls decreases the coefficients on ln life-time wealth. There are no level equivalents for 'self-reported change in health' and for 'survival'. For benchmark regressions of health changes on ln life-time wealth, see Appendix Tables A.7 and A.8.

Table 8: Benchmark Regressions of Health Conditions on ln of Life-time Wealth

	Regression specification	
	Baseline	Benchmark
	$\Delta H_{i,t}$ on $\frac{\Delta S\&P_t}{S\&P_{t-1}} W_{i,t-1}$	$H_{i,t}$ on lnW_t
Dependent Variable ($H_{i,t}$)	(1)	(2)
High blood pressure	0.167***	0.077***
	(0.057)	(0.007)
Heart disease	0.123*	0.053***
	(0.067)	(0.007)
Stroke	0.089*	0.060***
	(0.050)	(0.005
Arthritis	0.005	0.054***
	(0.066)	(0.006)
Cancer	0.058	-0.045***
	(0.048)	(0.006)
Diabetes	0.010	0.093***
	(0.036)	(0.006)
Lung disease	0.049	0.063***
	(0.034)	(0.005)
Psychiatric problems	0.073*	0.093***
	(0.040)	(0.006)
Controls		
Main effects	✓	
Demographics	✓	
Age, age^2, male		✓

In column (1) the coefficient on constructed wealth shocks is displayed; for further comments on the baseline regressions see Table 3. In column (2) the coefficient from OLS regressions of health levels on ln life-time wealth is displayed. Only age, age^2 and male are included as controls such that life-time wealth proxies for the overall socio-economic status. The inclusion of further controls decreases the coefficients on ln life-time wealth. There are no level equivalents for 'self-reported change in health' and for 'survival'. For benchmark regressions of health changes on ln life-time wealth, see Appendix Tables A.7 and A.8.

Table 9: 2SLS Regressions with Average or Initial Stock Holdings as Instrument for Actual Stock Holdings

	Regression specification				
	Baseline		2SLS		
			IV for constructed wealth shocks		
			$\frac{\Delta S\&P_t}{S\&P_{t-1}}[\frac{s_i}{W_i}]^{average}$		$\frac{\Delta S\&P_t}{S\&P_{t-1}}[\frac{s_i}{W_i}]^{1998}$
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Δ Index of Health Conditions	0.225***	0.249***	0.293***	0.318***	0.316***
	(0.063)	(0.070)	(0.074)	(0.082)	(0.102)
n	32,079	30,048	32,079	30,048	19,441
Δ Self-reported Health	0.096	0.212	0.016	0.129	0.023
	(0.117)	(0.136)	(0.134)	(0.157)	(0.207)
n	39,292	35,045	39,292	35,045	22,847
Self-reported Change in Health	0.332***	0.235*	0.357***	0.223	0.170
	(0.114)	(0.128)	(0.130)	(0.144)	(0.173)
n	39,315	35,066	39,315	35,066	22,863
Δ Mental Health Index	0.313**	0.408**	0.390**	0.502***	0.513**
	(0.139)	(0.160)	(0.163)	(0.188)	(0.233)
n	32,880	31,043	32,880	31,043	20,047
Survival	0.188**	0.233**	0.190*	0.252**	0.076
	(0.091)	(0.107)	(0.108)	(0.126)	(0.146)
n	31,333	27,301	31,333	27,301	19,237
Controls					
Year dummies	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓
Lagged stock fraction	✓	✓			
Average stock fraction 92-06			✓		
Average stock fraction 98-06				✓	
1998 stock fraction					✓
Restricted to year>1999		✓		✓	✓

The coefficient on constructed wealth shocks ($\frac{s_{i,t-1}}{W_{i,t-1}}\frac{\Delta SP_t}{SP_{t-1}}$) is displayed. The estimation method used is OLS in columns (1) and (2), and 2SLS in columns (3)-(5). The instrument for constructed wealth shocks in (3) is $\frac{\Delta S\&P_t}{S\&P_{t-1}}[\frac{s_i}{W_i}]^{average}_{92-06}$, in (4) is $\frac{\Delta S\&P_t}{S\&P_{t-1}}[\frac{s_i}{W_i}]^{average}_{98-06}$, and in (5) is $\frac{\Delta S\&P_t}{S\&P_{t-1}}[\frac{s}{W}]^{1998}$. First stage t-statistics for these instruments are about 134, 132 and 63, respectively. Further comments as in Table 3.

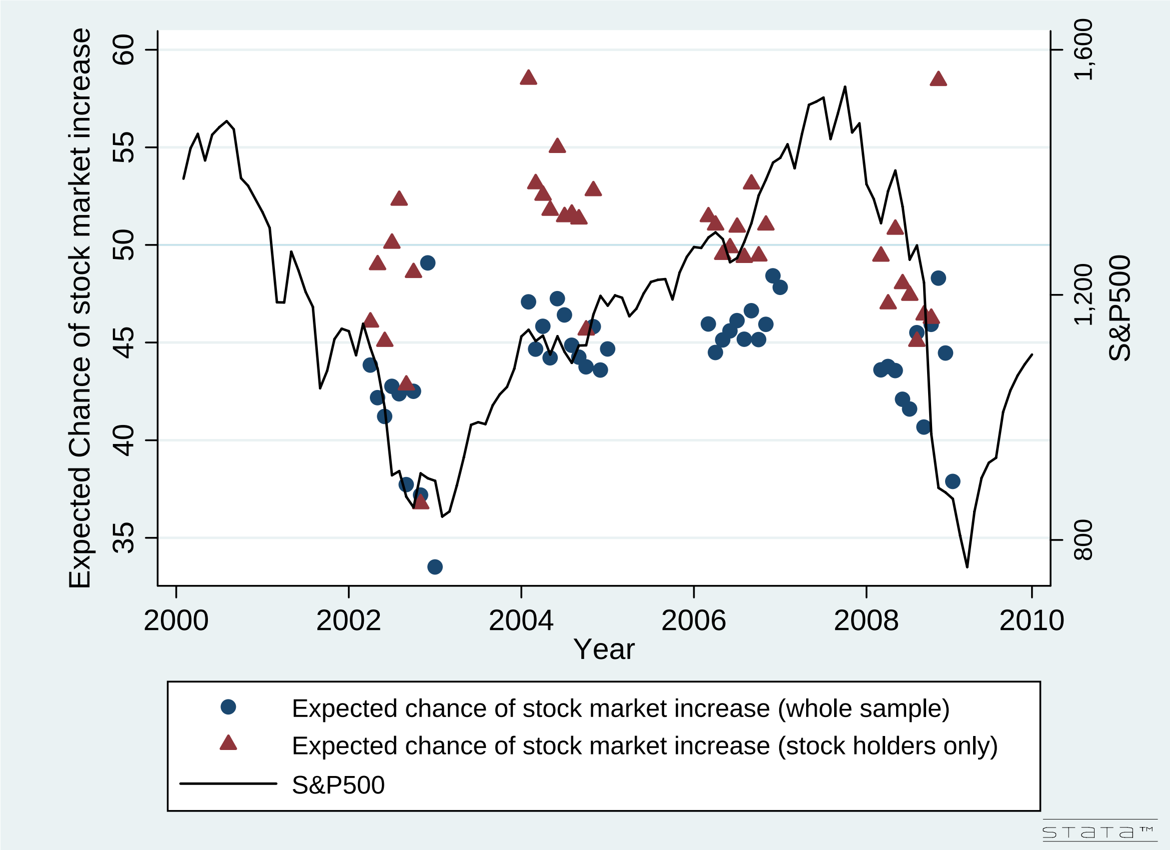
Table 10: Alternative sample specifications

	Sample specification					
	Baseline	Incl. non-retirees	Age>64	Singles only	$s_{i,t-1} > 0$	Excl. poorest quartile
Dependent Variable	(1)	(2)	(3)	(4)	(5)	
Δ Index of Health Conditions	0.225***	0.190***	0.293***	0.415***	0.142*	0.219***
	(0.063)	(0.050)	(0.079)	(0.108)	(0.075)	(0.065)
n	32,079	50,477	23,826	15,185	12,051	23,718
Δ Self-reported Health	0.096	0.137	0.233	0.252	0.144	0.066
	(0.117)	(0.085)	(0.153)	(0.201)	(0.141)	(0.124)
n	39,292	62,617	27,889	18,637	14,505	28,475
Self-reported Change in Health	0.332***	0.327***	0.182	0.353*	0.253*	0.184
	(0.114)	(0.090)	(0.141)	(0.184)	(0.140)	(0.120)
n	39,315	62,648	27,913	18,658	14,507	28,486
Δ Mental Health Index	0.313**	0.211**	0.390**	0.469*	0.260	0.332**
	(0.139)	(0.106)	(0.175)	(0.241)	(0.169)	(0.145)
n	32,880	51,104	24,443	15,695	12,658	24,842
Survival	0.188**	0.154**	0.195	0.238	0.206*	0.204**
	(0.091)	(0.063)	(0.125)	(0.175)	(0.111)	(0.095)
n	31,333	50,503	21,423	14,613	11,827	22,483
Controls						
Main effects	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓

Column (2): non-retired individuals are included (as long as some kind of retirement income is reported for HH). (3): HH are excluded if financial respondent or spouse is below age 65. (4): Only single retiree households are included. (5): HH without stocks in (t-1) are excluded. (6): HH from quartile of the life-time wealth distribution are excluded. Further comments as in 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 Wealth Measures

Wealth measure	Symbol (1)	Mean (2)	Std. dev. (3)
Reported household wealth (nominal USD)	$A_{i,t}$	334,285	1,002,583
Change in reported household wealth (nominal USD)	$\Delta A_{i,t}$	24,519	1,039,176
Household life-time wealth (nominal USD)	$W_{i,t}$	523,615	3,403,015
Relative change in reported household wealth	$\frac{\Delta A_{i,t}}{W_{i,t-1}}$	0.122	1.419
Fraction of life-time wealth held in stocks	$\frac{s_{i,t}}{W_t}$	0.061	0.141
Percentage change in the S&P500	$\frac{S\&P_t}{S\&P_{t-1}}$	0.160	0.302
Constructed wealth shocks	$\frac{s_{i,t-1}}{W_{i,t-1}} \frac{\Delta S\&P_t}{S\&P_{t-1}}$	0.006	0.039

Comments as in Table 1.

Table A.2: Summary Statistics Demographic Controls

Variable	Mean	Std. dev.	Variable	Mean	Std. dev.
Sex			*Education*		
Female	0.621		Years of education	11.59	3.41
			Less than high school	0.316	
Age			GED diploma	0.045	
Age	72.78	9.63	High-school graduate	0.324	
Age^2	5,389	1,418	Some college	0.177	
Age>75	0.388		College and above	0.138	
Race			*Marital status (lagged)*		
White	0.820		Married	0.518	
Black	0.150		Married, spouse absent	0.009	
Other	0.030		Partnered	0.016	
			Separated	0.013	
Region			Divorced	0.079	
Northeast	0.164		Separated/divorced	0.005	
Midwest	0.246		Widowed	0.329	
South	0.411		Never married	0.032	
West	0.178				
Other	0.001				

Standard deviations are omitted for binary variables.

Table A.3: Summary Statistics of Health Measures.

	Transformation						
	Original				Standardized		
	Levels		First difference			Levels	First difference
Health measure	Range (1)	Mean (Std. dev.) (2)	Range (3)	Mean (Std. dev.) (4)	Inversed (5)	Mean (Std. dev.) (6)	Mean (Std. dev.) (7)
Index of health conditions	[0;...;8]	2.350	[-5;...;5]	0.257	yes	0.000	-0.175
		(1.445)		(0.615)		(0.975)	(0.417)
Self-reported health	[0;...;4]	1.904	[-4;...;4]	-0.081	no	0.000	-0.069
		(1.145)		(0.938)		(0.959)	(0.790)
Self-reported change in health	[-1;0;1]	-0.225	-	-	no	0.000	-
		(0.598)				(0.871)	
Mental health index	[0;...;8]	1.694	[-8;...;8]	0.038	yes	0.000	-0.018
		(2.020)		(1.831)		(0.934)	(0.861)
Survival	[0;1]	0.930	-	-	no	0.000	-
		(0.255)				(0.527)	

For comments see the Data section.

Table A.4: Summary Statistics of Health Conditions.

	Transformation						
	Original				Standardized		
	Levels		First difference			Levels	First difference
		Mean		Mean		Mean	Mean
	Range	(Std. dev.)	Range	(Std. dev.)	Inversed	(Std. dev.)	(Std. dev.)
Health condition	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High blood pressure	[0;1]	0.601	[-1;0;1]	0.049	yes	0.000	-0.079
		(0.490)		(0.240)		(0.788)	(0.386)
Heart disease	[0;1]	0.307	[-1;0;1]	0.044	yes	0.000	-0.073
		(0.461)		(0.256)		(0.761)	(0.422)
Stroke	[0;1]	0.103	[-1;0;1]	0.020	yes	0.000	-0.038
		(0.304)		(0.184)		(0.590)	(0.358)
Arthritis	[0;1]	0.201	[-1;0;1]	0.030	yes	0.000	-0.052
		(0.401)		(0.177)		(0.701)	(0.309)
Cancer	[0;1]	0.157	[-1;0;1]	0.026	yes	0.000	-0.048
		(0.364)		(0.172)		(0.660)	(0.313)
Diabetes	[0;1]	0.121	[-1;0;1]	0.022	yes	0.000	-0.041
		(0.326)		(0.160)		(0.617)	(0.303)
Lung disease	[0;1]	0.674	[-1;0;1]	0.045	yes	0.000	-0.073
		(0.469)		(0.241)		(0.769)	(0.395)
Psychiatric problems	[0;1]	0.175	[-1;0;1]	0.025	yes	0.000	-0.045
		(0.380)		(0.185)		(0.679)	(0.330)

For comments see the Data section.

Table A.5: Regressions of Mental Health Index Items on Wealth Shocks

Dependent Variable	(1)	(2)	(3)
Δ Felt depressed	0.258**	0.251**	0.260*
	(0.119)	(0.119)	(0.138)
Δ Everything is an effort	0.129	0.143	0.128
	(0.124)	(0.124)	(0.143)
Δ Sleep is restless	0.146	0.155	0.223
	(0.138)	(0.139)	(0.161)
Δ Felt alone	0.207*	0.212*	0.251*
	(0.121)	(0.122)	(0.141)
Δ Felt sad	0.220*	0.223*	0.258*
	(0.128)	(0.128)	(0.146)
Δ Could not get going	-0.002	0.019	0.113
	(0.134)	(0.134)	(0.152)
Δ Felt happy	0.057	0.055	0.086
	(0.112)	(0.112)	(0.128)
Δ Enjoyed life	0.074	0.079	0.123
	(0.099)	(0.100)	(0.113)
Controls			
Main effects	✓	✓	✓
Demographics		✓	✓
Restricted to year>1999			✓

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*, in terms of standard deviations. Further comments as in Table 3.

Table A.6: Baseline and Interacted Regressions of Health Measures, Excluding Wave 9

	Full sample	Wave 9 excluded			
	Baseline	Baseline	Interacted (sign of shock)		
			Shocks≤ 0	Shocks≥ 0	Δ (p-value)
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Δ Index of Health Conditions	0.225***	0.212***	0.547*	0.249*	0.360
	(0.063)	(0.064)	(0.293)	(0.133)	
Δ Self-reported Health	0.096	0.079	0.064	-0.194	0.676
	(0.117)	(0.120)	(0.583)	(0.217)	
Self-reported Change in Health	0.332***	0.263**	1.378**	0.059	0.054
	(0.114)	(0.116)	(0.616)	(0.252)	
Δ Mental Health Index	0.313**	0.328**	0.436	0.276	0.825
	(0.139)	(0.142)	(0.704)	(0.253)	
Survival	0.188**	0.188**	1.189***	0.180	0.029
	(0.091)	(0.091)	(0.428)	(0.171)	
Controls					
Main effects	✓	✓	✓		
Demographics	✓	✓	✓		

The coefficient on constructed wealth shocks, in columns (3) and (4) interacted with the sign of shocks, is displayed. Column (5) reports the significance level of the difference between the interacted coefficients. The coefficients for survival in columns (1) and (2) are identical as survival is not observed in wave 9. Further comments as in Table 5.

Table A.7: Benchmark Regressions of Changes in Health Measures on ln of Life-time Wealth

	Regression specification	
	Baseline	Benchmark
	$\Delta H_{i,t}$ on $\frac{\Delta S\&P500}{S\&P500}\frac{s_{i,t-1}}{W_{i,t-1}}$	$\Delta H_{i,t}$ on lnW_t
Dependent Variable	(1)	(2)
Δ Health Conditions Index	0.225***	0.000
	(0.063)	(0.002)
Δ Self-reported Health	0.096	-0.011***
	(0.117)	(0.003)
Self-reported Change in Health	0.332***	0.069***
	(0.114)	(0.006)
Δ Mental Health Index	0.313**	-0.014***
	(0.139)	(0.004)
Survival	0.188**	0.056***
	(0.091)	(0.004)
Controls		
Main effects	✓	
Demographics	✓	
Age, age^2, male		✓

In column (1) the coefficient on constructed wealth shocks is displayed. In column (2) the coefficient on the ln life-time wealth from OLS regressions of health changes on ln life-time wealth is displayed. Only age, age^2 and male are included as controls such that life-time wealth proxies for the overall socio-economic status. For further comments on the baseline and benchmark regressions see Table 3 and 7, respectively.

Table A.8: Benchmark Regressions of Changes in Health Conditions on ln of Life-time Wealth

	Regression specification	
	Baseline	Benchmark
	$\Delta H_{i,t}$ on $\frac{\Delta S\&P500}{S\&P500}\frac{s_{i,t-1}}{W_{i,t-1}}$	$\Delta H_{i,t}$ on lnW_t
Dependent Variable	(1)	(2)
Δ High blood pressure	0.167***	-0.003
	(0.057)	(0.002)
Δ Heart disease	0.123*	-0.001
	(0.067)	(0.002)
Δ Stroke	0.089*	0.009***
	(0.050)	(0.002
Δ Arthritis	0.005	-0.010***
	(0.066)	(0.002)
Δ Cancer	0.058	-0.003*
	(0.048)	(0.001)
Δ Diabetes	0.010	0.008***
	(0.036)	(0.002)
Δ Lung disease	0.049	0.004***
	(0.034)	(0.002)
Δ Psychiatric problems	0.073*	0.005***
	(0.040)	(0.002)
Controls		
Main effects	✓	
Demographics	✓	
Age, age^2, male		✓

In column (1) the coefficient on constructed wealth shocks is displayed. In column (1) the coefficient on constructed wealth shocks is displayed. In column (2) the coefficient on the ln life-time wealth from OLS regressions of health changes on ln life-time wealth is displayed. Only age, age^2 and male are included as controls such that life-time wealth proxies for the overall socio-economic status. For further comments on the baseline and benchmark regressions see Table 3 and 7, respectively.

Table A.9: Constructed Wealth Shocks as Instrument for Changes in Reported Wealth

	Regression specification			
	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)	(3)	(4)
Δ Index of Health Conditions	0.225***	0.249***	0.303***	0.304***
	(0.063)	(0.070)	(0.107)	(0.111)
First stage F-statistic	-	-	22.94	19.27
Δ Self-reported Health	0.096	0.212	0.129	0.253
	(0.117)	(0.136)	(0.158)	(0.168)
First stage F-statistic	-	-	29.03	22.85
Self-reported Change in Health	0.332***	0.235*	0.444**	0.299*
	(0.114)	(0.128)	(0.174)	(0.163)
First stage F-statistic	-	-	29.22	22.90
Δ Mental Health Index	0.313**	0.408**	0.400**	0.471**
	(0.139)	(0.160)	(0.194)	(0.210)
First stage F-statistic	-	-	26.30	21.71
Survival	0.188**	0.233**	0.255*	0.282**
	(0.091)	(0.107)	(0.131)	(0.140)
First stage F-statistic	-	-	29.32	22.01
Controls				
Main effects	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Restricted to year>1999		✓		✓

Columns (1) and (2) display the coefficients on constructed wealth shocks in the baseline regressions. Columns (3) and (4) display the coefficient on changes in reported wealth ($\Delta A_{i,t}/W_{i,t-1}$) in 2SLS regressions with constructed wealth shocks as instrument. First-stage regressions are reported in Table 2. First-stage F-statistics vary across health measures due to differences in the number of observations. For further comments see Table 3.