Wealth Shocks and Health Outcomes: Evidence from Stock Market Fluctuations

Hannes Schwandt*

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Abstract

Do wealth shocks affect the health of the elderly in developed countries? I exploit the booms and busts in the US stock market as a natural experiment that generated considerable gains and losses in the wealth of stock-holding retirees. Using data from the 1998-2011 Health and Retiremesnt Study I construct wealth shocks as the interaction of stock holdings with stock market changes. These wealth shocks are highly predictive of changes in reported wealth. And they strongly affect health outcomes. A 10% wealth loss leads to an impairment of 2-3% of a standard deviation in physical health, mental health and survival rates. Effects are heterogeneous across physical health conditions, with most pronounced effects for the incidence of high blood pressure, smaller effects for heart problems, and no effects for arthritis, diabetes, and lung diseases.

JEL Classification: G10; I10; J14

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*Affiliations: Department of Economics and Jacobs Center for Productive Youth Development, University of Zurich; CEP, London School of Economics; IZA. Mailing address: Schoenberggasse 1, 8001, Zurich, CH. Email: hannes.schwandt@uzh.ch. I am grateful to Steve Pischke for his continuous advice and guidance. I am also thankful to two anonymous referees, the editor, Antonio Ciccone, Dora Costa, Janet Currie, Gabrielle Fack, Ada Ferrer-i-Carbonell, Robin Hogarth, Michael Hurd, Richard Layard, Adriana Lleras-Muney, Susann Rohwedder, Yona Rubinstein, James Smith, Thijs van Rens, Joachim Voth, Till von Wachter, Amelie Wuppermann and seminar participants at Princeton, LSE, UPF, CREI, RAND, UCLA, University of Bonn, Mannheim, LMU Munich, and UDEP Lima, IZA ESSLE 2011, EEA 2011 and VfS 2011, SAEE 2011, ASHEcon 2016 for helpful comments. Financial support from AGAUR grants FI 08-5208 and BE 09-10019 and the Economics department at Universitat Pompeu Fabra is gratefully acknowledged. This research benefited from visiting RAND and I thank Arie Kapteyn and Mike Hurd for making this visit possible. Previous versions of this paper circulated under the title “Is wealth causing health? Evidence from stock market induced wealth shocks,” June 2011, and as IZA Discussion Paper No. 8298, June 2014.

1 Introduction

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

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

I construct wealth shocks as the interaction of stock holdings with stock market changes. These constructed wealth shocks are highly predictive of changes in reported wealth. And they strongly affect the health of elderly retirees who are of average age 75 in the HRS. A 10% change in lifetime wealth over a two-year period is associated with a change of 2-3% of a standard deviation in four different health measures: a physical health index, self-reported health, mental health, and the probability of surviving to the next interview two years ahead. This means that among 100 retirees losing 10% of their remaining lifetime wealth, 2.5 will develop an additional health condition and one additional retiree will not survive the next two years (at a baseline 2-year mortality rate of 12%). The analysis of individual health conditions reveals a plausible pattern underlying the effect on physical health. Effects are strongest for hypertension, which we would expect to be most responsive in the short run.

[1] For reviews of the literature see Smith (1999); Deaton (2003); Cutler et al. (2006, 2011).

I find smaller effects for heart diseases, which are typically caused by high blood pressure. There are no effects on arthritis, diabetes, and lung disease, which in general take more than two years to be affected by external factors (Braunwald et al., 2001). Compared to the cross-sectional relationship of wealth and health, the estimated effects are large in magnitude.

For a causal interpretation of these estimates, constructed wealth shocks must be 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 educated, wealthier, and more risk-loving individuals typically hold larger fractions of their wealth in stocks. 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.

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

Despite a broad existing literature, effects of wealth shocks on elderly health have been documented so far only for poor retirees in Russia (Jensen and Richter, 2004) and South Africa (Case, 2004). To my knowledge, this paper is the first to document health impacts of wealth shocks on elderly in the developed world, to show effects on mortality, and to suggest psychological stress as a central mechanism. As Cutler et al. (2011) summarize the existing literature, "[A] preponderance of evidence suggests that in developed countries today, income does not have a large causal effect on adult health". The most prominent papers providing this evidence set forth three main approaches.

A first set of papers uses approaches related to Granger-causality (Smith 2005; Adams et al. 2003; Michaud and van Soest 2008). These papers use HRS data to show that wealth changes and lagged wealth conditional on socio-economic controls do not predict health changes in the working-age population. I show that zero results, such as in Smith (2005), could be driven by measurement error in self-reported wealth and that using my constructed wealth shocks as an instrument solves that issue.

Another set of papers analyzes aggregate time series of income and health at the state or cohort level (Ruhm 2000; Deaton and Paxson 2001; Deaton and Paxson 2004; Snyder and Evans 2006; Adda et al. 2009). None of these papers find evidence of a positive relationship between income changes and health changes at the macro-level. But, as the authors of these papers note, aggregate income changes might be correlated with macro shocks that also have non-income effects on health.[2] A third set of papers exploits lottery winnings as a source of exogenous variation in wealth (Lindahl 2005; Gardner and Oswald 2007; Apouey and Clark 2015; Cesarini et al. (2016)). These papers find positive effects on mental health, while results are less conclusive for physical health. A general challenge of lottery studies is that only positive wealth shocks are observed and that the number of significant winnings is usually limited.

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

How plausible are the effects that I find? Should we expect the positive physical health effects found for poor retirees in poor countries to carry over to wealthy retirees in the US? Health inputs like medical treatment, medication, or mere calorie intake might be affected by wealth shocks for poor retirees in Russia or South Africa. But this is probably less of an issue for stock-holding US pensioners, who have enough money left to afford basic pills and food even after a considerable wealth loss. Further, Medicare covers the entire 65+ population in the US, so wealth shocks do not affect basic health insurance coverage as they do 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.

[2]See also Miller et al. (2009) and Handwerker (2011), respectively.

But two years might not be enough time for consumption to affect health outcomes as dramatically as observed.

Other plausible channels include 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 intended as an inheritance for the grandchildren. Extensive literature in medicine, psychology, and biology has documented effects of psychological stress on coronary artery diseases, clinical depression, and mortality (Strike and Steptoe, 2004). Positive emotions, on the other hand, were found to have positive effects on these health outcomes (for a review see Chida and Steptoe (2008)). In the HRS data, I find strong wealth shock effects on high blood pressure and mental health and smaller effects on heart problems. This is exactly the kind of health response the bio-medical literature would predict if wealth shocks have an effect on psychological stress.[3]

Importantly, a wave of recent papers shows that stock market fluctuations are correlated with mental health assessments, hospital admissions for psychological conditions, and antidepressant use among U.S. adults (Engelberg and Parsons, 2016; Liu, 2017; Cotti et al., 2015; McInerney et al., 2013). These effects tend to be stronger for individuals more likely to be exposed to the stock market. These findings strongly support the role of stress and mental health for the physical health effects that I find.[4]

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

However, caution must be exercised when extrapolating from my estimates to other settings.

[3]The responsiveness of elderly mental health to income-related shocks has also been documented by Grip et al. (2012).

[4]Another set of recent papers analyzes fluctuations in housing wealth, finding evidence of impacts on stress-related mental health outcomes as well as health behavior (Heiss et al., 2016; Fichera and Gathergood, 2016; Yilmazer et al., 2015).

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 for a given wealth shock.[5] Last, my estimates represent the effects of wealth shocks. They might not be representative of the long-run effects of gradually accumulating wealth differences. A comparison with the cross-sectional relationship of wealth and health indeed suggests that the long-run wealth elasticity of health is smaller and more homogeneous across health conditions than the estimated impact of wealth shocks.

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

2 Identification

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

The ideal experiment to solve the endogeneity problem would be a lottery that randomly assigns wealth losses and gains to people and measures their health before and some time after the assignment. This paper exploits the booms and busts of the US stock market over the past two decades as a natural experiment that generated considerable wealth gains and losses for retirees owning stocks.[6] This natural experiment comes quite close to the ideal set-

[5] Sullivan and von Wachter (2009) provide related evidence for younger adults. They show that exogenous job displacements dramatically increase the mortality hazard of male US workers during the years following the job loss. The authors interpret their findings to be consistent with job loss "causing acute stress, which may substantially raise the mortality hazard in the short term."

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

ting. Given that stock market changes are largely unpredictable for retirees without insider information, holding stocks is equivalent to buying lottery tickets.

I construct stock market-induced wealth shocks (hereafter *constructed wealth shocks*) as the interaction of the lagged fraction of lifetime 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 lifetime wealth (see below) and $\frac{\Delta SP_t}{SP_{t-1}}$ the percentage change in the Standard & Poor's 500 stock market index (S&P500) between two waves. For example, an individual with 20% lifetime wealth held in stocks in the past wave and a 50% stock market increase between the past and the current wave is assigned a 10% positive wealth shock.

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

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

where $H_{i,t}$ are different health measures, $\frac{s_{i,t-1}}{W_{i,t-1}}\frac{\Delta SP_t}{SP_{t-1}}$ are constructed wealth shocks, ϑ_t are time 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 reduce the number of observations since the construction of wealth shocks already requires a lag).

For the interpretation of β as the effect of wealth shocks on health two conditions must be satisfied. Constructed wealth shocks must be independent of any unobserved heterogeneity in health changes. And 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 largely unpredictable (for a review of the finance literature on market efficiency see Malkiel (2003)) and therefore random for the individual retiree, but stock holdings are not. The richer, the more educated, and the more risk-loving typically hold larger fractions of their wealth in stocks. Similarly, individuals facing lower medical risk have been shown to make more risky portfolio choices (Hugonnier et al., 2012; Goldman and Maestas, 2013). Given the finite number of booms and busts in my data, these factors may result in a correlation of constructed wealth shocks with unobservable, endogenous determinants of stock holdings. Regressing health measures in first differences cancels out unobserved heterogeneity that is constant over time. But determinants of stock holdings might not only correlate with health levels but also with health profiles over time so that first differences alone do not rule out potential endogeneity.[7] Therefore it is important to control separately for the lagged fraction of wealth held in stocks ($\frac{s_{i,t-1}}{W_{i,t-1}}$).

This means I compare health changes for individuals with the same amount of stocks at different points in the stock market cycle. Or in terms of the lottery analogy, I measure the health response to lottery winnings and losses conditional on the amount of lottery tickets bought. One potential caveat is that part of the identifying variation is driven by the timing of interviews within waves, which could be confounded by main effects of the survey timing. For that reason, I include year x month dummies (ϑ_t) in all regressions. Moreover, I show regressions based on average wave-to-wave stock market changes, eliminating any within-wave variation.

A further potential caveat is that investor types may change over time so that retirees with the same amount of stocks during a boom and during a bust might not be comparable. To address this issue, I show 2SLS regressions using initial stock holdings (which are fixed over time) as an instrument for actual stock holdings, I explore the role of covariates, and I provide balancing tests that directly test for selection. I discuss these approaches in more detail in the Findings section when the main results are more easily at hand.

[7]For example, individuals who anticipate a health risk might want to reduce financial risks and redistribute their portfolio from stocks to safer assets. Or people with less education have more declining health profiles due to worse health behavior and at the same time hold less stocks due to less financial literacy. A similar argument can be made at the intensive margin, with risk-seeking individuals picking more risky stocks in their portfolio, or healthier, more attentive investors updating their stock portfolio more regularly (see section 2.3).

2.2 Are effects running exclusively through stock wealth?

Stock market changes might not only determine stock values but also correlate with prices of other wealth holdings. 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 ups and downs in the S&P500.[8] But for retirees without stocks, wealth changes are positive in all waves and seem uncorrelated with the stock market, suggesting there is not much of an effect of the stock market on non-stock wealth (detailed regressions presented in the Findings section).

Still, the stock market or more broadly the macroeconomic environment might also affect health through non-wealth channels. 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 retirees, effects running through the individual's employment status are limited. But retirees might be troubled about their children becoming unemployed or their grandchildren not finding a job after graduation. They may also rely on the provision of public goods, which could depend on the macroeconomic environment. However, it seems reasonable to assume that these direct effects are at least as strong for retirees who do not hold stocks (who are poorer and less educated) as for those with stocks. If anything, economically less advantaged retirees depend more on public goods and their children are the first to get fired in a recession (Hoynes et al., 2012). This suggests that the inclusion of time fixed effects is a conservative way to control for such potential direct effects.

It could also be argued that on the contrary, wealthier and more educated retirees are more exposed to such direct effects, e.g. many may follow business cycle news more closely as they are inherently more interested in the macroeconomic development. To test this hypothesis, I construct placebo shocks, interacting stock market changes with bond holdings (another proxy for retiree wealth). Finally, stockholders might just be special types who care more about the economic development than any other group of retirees. I therefore also construct placebo shocks that interact stock holdings with the unemployment rate, which is a better

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

business-cycle indicator than stock markets.

2.3 Measurement error

A further identification threat could be measurement error. Changes in reported wealth are not only endogenous but also notorious for attenuation bias due to measurement error (for a discussion of attenuation biases in first difference models see Griliches and Hausman (1986)). 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 that are not observed in the data. But the resulting measurement error in constructed wealth shocks is negatively correlated with actual returns but uncorrelated with constructed wealth shocks. Importantly, this kind of measurement error (also called Berkson error, following Berkson (1950)) implies less precise estimates but no attenuation towards zero, even though the error occurs in the explanatory variable.[9]

One potential issue could be that changes in the S&P500 might not represent the average return to stocks held by retirees in my sample. If the average portfolio in my sample is more risky than the S&P500, constructed wealth shocks will have a smaller variance than the true shocks. This would imply an upward bias in my estimates. But for a sample of elderly retirees it is more likely to expect the opposite, i.e. portfolios that are less risky than the average market. In this case, my estimates provide a lower bound of the true effect.[10]

Another potential issue could be that individuals in poor health might be less likely to update their stock portfolio, as they pay less attention, ending up with less-diversified portfolios. Changes in the S&P500, an index that is regularly reviewed, would in this case be less representative of individuals in poor health. This would imply estimates are attenuated, as long as the sign of the wealth shock effect is the same for healthy and unhealthy investors.[11]

[9] For a discussion of the measurement error induced by retirees' expectations about stock market returns, see Appendix section A.1.

[10] Note that risk-seeking individuals with more risky portfolios might also engage in more risky health behaviors. This would imply a correlation of the variance in returns and the variance in health outcomes. However, it does not imply a bias as long as returns and health outcomes are not correlated themselves.

[11] Assume the share of healthy investors is h, and the true effect of wealth shocks for healthy and unhealthy investors is β_h and β_{uh}, respectively. The estimated average treatment effect, using constructed wealth shocks as a proxy for true wealths shocks, is the weighted average $\hat{\beta} = h\hat{\beta}_h + (1-h)\hat{\beta}_{uh}$. Now assume the returns of

3 Data

The data used in this study come from the waves 4 to 10 of the Health and Retirement Survey (HRS), covering the years 1998 to 2011.[12] The HRS is a biannual panel that started in 1992 with 12,654 individuals representing US adults of age 51 and older. In 1998 and 2004 new cohorts were added to keep the sample representative resulting in an extended sample of about 22,000 individuals. Moreover, in 1998, the fraction of individual retirement accounts invested in stocks, a variable that is central for my analysis, was introduced. One so-called financial respondent is interviewed about the family's financials per household. Other questionnaire items such as health measures are reported by all household members. The sample of this study is restricted to financial respondents, and their spouses if existent, who report wealth and non-zero retirement income in the previous wave summing to a lifetime wealth of at least $10,000. Further, I restrict the sample to singles and couples who were retired in the previous wave, i.e. either (i) both financial respondent and spouse were neither working for pay (i.e. neither working, full or part-time working, nor partly retired) nor unemployed, or (ii) both considered themselves completely retired. The final regression sample consists of about 40,000 person-year observations, of which 20,000 refer to singles. The average age is 75.43 years (10% of the sample is below age 65, and only 3% below age 60), 63% of the sample are women, and 82% are white (see Table A.1 for further summary statistics).

The interview month is known, so that the HRS data can be matched to monthly stock market data from the S&P500 stock market index.[13] Using a "total return" version of the S&P500, which accounts for dividend payments and assumes that these are fully reinvested, leads to very similar results (see below). Constructed wealth shocks are generated for financial respondents and matched to spouses. Interviews that start in one month and end in a later month are dropped, as are spouse interviews that are conducted in a different month from the financial respondent.

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the S&P500 are entirely unrepresentative of the stock market returns experienced by unhealthy investors. This implies $\hat{\beta}_{uh} = 0$ and an attenuation of $\hat{\beta}$.

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

[13] 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. Using the Dow Jones Industrial Average, which represents only 30 companies, delivers similar results.

3.1 Wealth data

Financial information in the HRS is reported in exact amounts and unfolding response brackets are offered if exact amounts are unknown. This study uses cleaned and partly imputed wealth data from the RAND HRS file. Current household wealth ($A_{i,t}$) consists of net housing wealth, real estate wealth, vehicles, business wealth, individual retirement accounts (IRAs), stocks and mutual funds, checking and savings accounts, CDs, savings bonds and treasury bills, bonds, other savings, and debts. Pension plans such as 401(k)s are not reported for retirees in the HRS because these plans are usually cashed out or rolled over into an IRA upon retirement.

3.1.1 Construction of lifetime wealth

I construct a measure of lifetime 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 used as a proxy for a retiree's expectations about future income as it can be expected to stay constant (in real terms) if the retiree remains in retirement.[14] 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.

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

Further details about the construction of lifetime wealth are provided in the Appendix section A.2.

[14] Maestas (2010) shows in HRS data that at least 26% of retirees unretire at a later point in time, which may affect later retiree income. In my sample, however, less than 8% of retirees ever unretire, perhaps because I restrict the sample to retiree couples (i.e. the spouse, if existent, is also retired) which results in an older sample. The average age of respondents entering my sample is 72.2, which is considerably larger than the average retirement entry age of about 60 in Maestas (2010).

3.1.2 Measurement of stock holdings

A central ingredient for constructing wealth shocks is the amount of stock holdings. Direct stock holdings are well documented in each wave, but they do not include stocks held in IRAs. Retirees often hold considerable fractions of their wealth in (often various) IRAs. To calculate the total amount of stock holdings it is therefore important to know the percentage of each IRA invested in stocks.

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

3.1.3 Advantages of rescaling shocks by lifetime wealth

For the construction of wealth shocks, the predicted changes in stock wealth ($s_{i,t-1}\frac{\Delta SP_t}{SP_{t-1}}$) are divided, or rescaled, by lifetime wealth. 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 poor. 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 relatively constant pension income and a limited time horizon of remaining years to live. An additional advantage of rescaling by lifetime wealth instead of current wealth is that lifetime wealth has fewer zeros or negative values, which have to be excluded from the analysis. Results, however, are not driven by the inclusion of lifetime wealth. The overall effect pattern remains the same when rescaling wealth shocks by current wealth instead of lifetime wealth.

3.1.4 Summary statistics

Table 1 summarizes sample characteristics and main wealth measures per HRS wave (for further wealth summary statistics see Table A.2 in the Appendix). In 2004, younger than average cohorts are added, leading to discontinuous jumps in these measures. Retiree rates increase with age, but even at age 70 for 30% of the households at least one spouse is still in the labor force. The fourth and fifth rows 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 lifetime 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 lifetime wealth in stocks.

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

Figure 2 plots constructed wealth shocks and the S&P500 over time. Each circle represent one household and is placed at the month of the interview. Wealth shocks roughly range from -30% to +40%. These are dramatic changes. For a retiree who has about 10 years remaining to live, a 10% loss in lifetime 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 constructed. Their correspondence to actual changes in reported wealth is assessed in the Findings section.

3.2 Health data

I use different health measures from the HRS as dependent variables: a physical health index, individual health conditions, self-reported health, self-reported change in health, a mental health index, and survival to the next interview. For better comparability, measures of bad health are inverted such that higher values of a measure always refer to better health. This means that a positive coefficient on wealth shocks always refers to an improvement in the respective health measure. For comparability of effect sizes across measures, which are reported on different scales and represent health circumstances of different severity, I also show results for 'probit-adapted' health measures following an approach by van Praag and Ferrer-i Carbonell (2004). This approach yields effects in terms of standard deviations that additionally account for potential measure-specific non-linear scaling.[15] Summary statistics of original and transformed health measures are reported in the Appendix, Table A.3.

The physical health index equals the sum of conditions which have *ever* been diagnosed by a doctor according to the respondent. The HRS questionnaire includes seven physical health conditions: high blood pressure, heart disease, stroke, arthritis, cancer, diabetes, and lung disease. These health conditions are also analyzed in separate regressions. In theory, the wording of the question only allows for new ever-diagnosed conditions to appear but never to disappear. In the data, however, a significant number of people report a condition in one wave but neglect 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 noise but contain information about actual or perceived changes in the respondent's health.[16] I therefore include such reversals in the baseline regressions, but I also estimate survival models with health conditions that are turned on if the respondent has ever answered "yes."

For self-reported health, respondents are asked to rate their current health as poor, fair, good, very good, or excellent. An additional question, self-reported changes in health, asks whether compared to the previous interview, health is worse, the same, or better. Self-reported changes in health are regressed directly in levels and not in first differences as the question already implies a health change.

The mental health index sums a subset of eight questions from the 20 question CES-D de-

[15] I assign to the categories of each measure the expected value of a standard normal variable conditional on being between the category's lower and upper cut-off points implied by an ordered probit fitted on the raw sample fraction. Changes in these transformed health measures are then regressed via OLS on constructed wealth shocks and controls. van Praag and Ferrer-i Carbonell (2004) refer to this as 'probit-adapted OLS'.

[16] Individuals might understand the question wrongly (overlooking the 'ever') or repress the memory of a cured disease. This implies that at least for a fraction of respondents these questions only indicate the current existence of a condition.

pression score, which has been developed to diagnose clinical depression.[17] Like the physical health index, the mental health index is inverted for regressions so that higher values indicate better mental health.

Deaths of survey participants are documented in so-called exit surveys in which a proxy respondent (usually a surviving family member) is interviewed about time and circumstances of the death. Thus deaths are well documented and not just one possible reason for an observed panel attrition. 'Survival', used as the dependent variable in the baseline regressions, indicates whether the respondent survives until the next interview. This means that survival from t to t+1 is regressed on wealth shocks from t-1 to t.

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 lifetime wealth
ϑ_t: Year x month dummies

[17]Six questions indicate whether the respondent felt the following way all or most of the time during the past week: felt depressed, everything is an effort, sleep was restless, felt alone, felt sad, and could not get going. Two questions, which are subtracted from the index, indicate whether the respondent felt happy and enjoyed life, all or most of the time during the past week.

$X_{i,t}$: Demographic controls: Dummies for gender (1), age group (12), cohort (10), race (2), degree (4), lagged region (4), and lagged marital status (7).

Changes in different health measures are regressed via OLS on the interaction of stock market changes with the lagged fraction of lifetime wealth held in stocks (constructed wealth shocks) while controlling separately for the 'main effects', i.e. the lagged stock fraction and year-month dummies. I additionally include the exact stock market change and a dummy for no stock holdings to control for the main effects in a more flexible way. The first difference specification absorbs time-invariant health differences that exist across individuals. This specification also has an efficiency advantage over an alternative fixed effects specification (Wooldridge, 2010) if health follows a random walk rather than a white noise process (French and Jones (2004) show that within individuals health shocks are highly persistent).

Health outcomes and demographics vary at the individual level, wealth at the household level, and the stock market at the monthly level. Standard errors are multi-level clustered by households and interview month (Cameron et al., 2011). Predetermined demographic controls such as age, gender, race or lagged martial status may be included to decrease the variance of the regression residual and thereby increase the precision of the estimates.

5 Findings

5.1 Predictive power of constructed wealth shocks

Constructed wealth shocks are highly predictive of changes in reported wealth. As shown in column (1) of Table 2, the regression of percentage changes in reported wealth on constructed wealth shocks and controls yields a highly significant coefficient of about 0.82. Including a large number of demographic controls hardly affects the estimate, resulting in a coefficient of 0.8. This means that a constructed wealth shock of 10% corresponds to a change in reported wealth by about 8%. Retirees might adapt their consumption to wealth shocks, which could explain why the coefficient is below unity. Another explanation is attenuation due to measurement error in the lagged stock fraction. In columns (3) and (4) of Table 2, the exact stock fraction is substituted by a dummy for stock holdings. A 10% change in the stock market leads to a 2.1% change in the wealth of stock holders.

The coefficient on the 'stock market change' main effect is small and insignificantly in all four columns, suggesting that there is not much of a stock market effect on the wealth of retirees without stocks. The R^2 is extremely low despite the inclusion of a broad set of demographic controls. This indicates that reported wealth in first differences is a noisy measure. Despite this noise, constructed wealth shocks do a good job in picking up actual changes in reported wealth. Let us now turn to the effects of these wealth shocks on health outcomes.

5.2 Effects of wealth shocks on health outcomes

Table 3 reports the baseline regressions of five health measures (rows) on constructed wealth shocks. Regressions in column (1) control only for main effects, i.e. the lagged fraction of wealth held in stocks, a dummy for lagged stock ownership, the stock market change and year-month fixed effects. In column (2) a broad set of demographics is added. In columns (3) and (4), dependent variables are standardized using Probit-adapted OLS so that estimates are in terms of standard deviations and thus comparable across health measures (see Data section). All estimates displayed in this table refer to the coefficient on constructed wealth shocks. A positive coefficient refers to a health improvement in the respective measure.

The regressions in the first column indicate a positive effect of constructed wealth shocks on all five health measures, ranging from 0.08 to 0.265. The effect is significantly different from zero for all measures except for the self-reported change in health. Including a broad set of demographic controls in column (2) hardly changes any of the coefficients. The estimated effect on the physical health index indicates that a negative 10% wealth shock is associated with a deterioration of the index by about 0.026 units. In other words, among 40 retirees losing 10% of their lifetime wealth, one will develop an additional physical health condition. The effect on survival suggests that among 100 retirees suffering a 10% wealth shock, there will be one additional death within the following two years. The estimates in columns (3) and (4) show that in terms of standard deviations, effect sizes are quite similar across health measures, ranging from 0.15 to 0.3.

In Table 4, I repeat these regressions separately for the seven health conditions contained in the physical health index. For these outcomes, negative coefficients indicate a health improvement (i.e. a lower chance of developing the respective health condition). A problem with the analysis of various health conditions is that the chance of wrongly rejecting the null increases with every additional regression.[18] In the present setup however, significant estimates would be more plausible for some health conditions than for others. Health changes are regressed on wealth shocks over a period of two years on average. Therefore, estimated health shocks must be driven by diseases that are responsive to environmental factors and that do not take a lot of time to develop. The regressions in column (1) of Table 4 reveal a strongly positive effect of wealth shocks on high blood pressure, a smaller effect on heart disease, and no significant effects on other health conditions. For arthritis, cancer, diabetes, and lung disease, there is also no joined significance in SUR models, neither for pairs nor for groups of three or four conditions. As in the regressions for health measures, the inclusion of demographic controls hardly changes estimates. Only for cancer, a slight increase in the coefficent renders the estimate marginally significant (p-value=0.096). Standardized effects in columns (3) and (4) further show that this overall pattern is not driven by differences in the baseline rate of these different health conditions. Effects are strongest for hypertension and heart disease also in terms of standard deviations.[19]

These heterogeneous effects across different physical health conditions are plausible. High blood pressure is the most responsive health problem in the short run (Braunwald et al., 2001) and arises from both psychological stress as well as unhealthy nutrition and behavior. Moreover, high blood pressure is a cause of heart problems, so that a significant effect on heart problems is what one should expect given the strong effect on high blood pressure. Similarly, one might expect an effect on strokes, a condition that is caused by high blood pressure too. But strokes are often fatal so that respondents may die before they could report this condition. In line with this reasoning the summary statistics in Table A.3 show that strokes are the least observed condition even though strokes are among the leading causes of death (Braunwald et al., 2001).

Effects on arthritis, diabetes, or lung diseases would be less plausible. Arthritis is a chronic condition that takes more than a few years to develop and is unlikely to respond to psycho-

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

[19] As explained in the data section, the survey question for these outcomes implies a survival process, asking whether a condition has ever been diagnosed. Even though this is not how respondents answer the question in practice, show in Appendix section A.3 and Table A.5, Cox proportional hazard models for measures of physical health conditions that are turned on if the respondent has ever answered yes. Despite the loss of information implied by these models (see discussion in the data section), the negative effect on high blood pressure remains significant at the 5% level. The estimates for heart disease, on the other hand, are not statistically significant (though point estimates are in line with a negative effect).

logical stress. Diabetes is driven by genetic disposition as well as by obesity. One could think of a response in body weight to stress, but such an indirect effect might take more than 1-2 years. And I do not find an effect of wealth shocks on body weight (Appendix section A.4). 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 and estimates are marginally significant in some of the specifications, but such effects remain highly controversial (Chida et al., 2008).

Looking at individual depression symptoms from the mental health index does not reveal a single driver, such as hypertension, for the physical health index (results reported in the Appendix, Table A.4). This makes sense. 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; what makes it a mental health problem is having many of the symptoms at the same time.

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

5.3 Timing of wealth shock effects

One important question is whether health outcomes are affected by leads and lags of wealth shocks. Future wealth shocks should not have any effects if they are truly unanticipated. Past wealth shocks, on the other hand, might have an accumulative effect, or a constant effect, or they may fade over time. Figure 3 shows the coefficients from regressions for the most affected outcomes that include different leads and lags of wealth shocks (regression results for these and further outcomes are reported in Tables A.6-A.8).[20]

[20] In order to utilize the maximum sample size, each regression only includes lead or lag terms up to the one that is plotted. For example, the t+2 coefficient is estimated in a regression that also includes the wealth shocks in t+1 and t, but not any past wealth shocks. The regression for the plotted t-1 coefficient includes the wealth shock in t, but no other lags or leads. I restrict the plots to the range [t-1, t+2] because for these lead and lag regressions, the effect of the wealth shock in time t on reported wealth remains strong and significant, i.e. there

The solid square in Figure 3 shows the highly significant effects of the contemporaneous wealth shock on wealth and health outcomes, corresponding to the baseline results reported in Tables 2-4. None of the coefficients on leads or lags, on the other hand, in any of the figures is significant – point estimates are smaller in magnitude than the contemporaneous shock effects and confidence intervals include zero. This pattern suggests that neither leads nor lags of wealth shocks impact changes in reported wealth or health outcomes.[21] Given the first differences specification, these results imply that stock market-induced wealth shocks have a *persistent* effect on these outcomes. No estimates are plotted for survival, as it is not possible to estimate lead effects (anyone observed with a future wealth shock has survived). However, the (t-1) lag effect is small and not significantly different from zero (Table A.7).[22]

5.4 Linearity, symmetry, and heterogeneity by gender and age

Table 5 explores the linearity of the effects. Instead of interacting stock market changes with the exact fraction of wealth held in stocks, I include interaction terms with dummies indicating 1-10% and >10% wealth in stocks, respectively. If stock market effects increase with wealth held in stocks (i.e. stock market exposure), effects should be stronger for the latter interaction term. This is what the results in Table 5 indicate. Stock market changes affect retirees with more than 10% wealth in stocks two to nine times as much as retirees with 1-10% in stocks. Estimated effects for the latter group are small and therefore not significantly different from zero in most cases, but point estimates are positive for all health measures. Importantly, there is no effect of the stock market on retirees without stocks, as indicated by the coefficient on the stock market change main effect.

Table 6 investigates the heterogeneity of effects across the direction of the shock, across gender, and age. Shown are the main effect of wealth shocks and the interaction with a

remains a 'first stage' (Table A.6).

[21] Including leads and lags reduces the sample size, contributing to the large confidence intervals around the lead and lag coefficients. However, the contemporaneous wealth shock, which is included with any intermediate lead or lag, remains significant in many cases suggesting that these regressions are informative (see Tables A.6-A.8).

[22] If the 2-year wealth shocks that I identify have persistent effects on health, then wealth shocks constructed over longer periods of time should result in similar estimates. As shown in Appendix Table A.9, constructing wealth shocks over three waves, on average 4 years, considerably reduces the sample size and strains the power of the analysis. However, the coefficient on the 4-year wealth shock has the right sign in all regressions and is in the same ballpark as the effect of the 2-year wealth shock. For hypertension the 4-year effect is significant at the 5% level. Including both shocks jointly in column (4) of Table A.9, I cannot reject that the 2-year and 4-year coefficients are the same in any of the regressions.

dummy indicating positive shocks, female gender, and age above 79, respectively.

Surprisingly, there are also no significant differences with respect to the sign of the shock. One might expect negative shocks to have a stronger impact on health due to loss aversion. In line with this idea, the point estimate of the interaction effect for physical health conditions suggests at face value that effects of negative wealth shocks might be twice as large as those for positive shocks. However, this point estimate is not significantly different from zero, and neither are any of the other interaction effects.

In Appendix Table A.10, I report a less parametric test for effect symmetry similar to the test for linearity in Table 5. I interact the lagged fraction of wealth held in stocks with dummies indicating a 10% stock market increase and decrease, respectively. These interaction terms show the effect of holding stocks in a bull and bear market compared to individuals holding stocks across periods with less than 10% stock market change. As expected, point estimates are positive for the interaction with a strongly increasing stock market and negative for the interaction with strong market losses. Moreover, in the physical health regression the negative interaction is significant at the 5% level and almost 10 times larger in magnitude than the positive interaction, which is close to zero and insignificant. This result suggests that the effect of negative shocks is indeed larger than that for positive shocks in the case of physical health conditions. At the same time, none of the estimates in the other health outcomes regressions is significant and the confidence intervals of the effect magnitude of positive and negative shocks overlap broadly in all cases. Overall, these results suggest that both positive and negative wealth shocks matter and that there is not enough power to detect consistent effect asymmetries across all outcomes.[23]

There are no significant gender differences (Table 6, columns (3) and (4)). Mental and self-reported health seem to be more affected for women which would be in line with the literature on gender differences in mental health but the estimated differentials are imprecise. These results do not imply that effects are the same for males and females, but it seems that estimates are not driven by one gender.

The interactions of wealth shocks with age 80+ are positive across all measures and significantly different from zero in the physical health and the survival regression. Wealth

[23]Note that symmetric effects would imply a limited role of volatility. If positive and negative effects are equal, an increase in volatility would not impact health as gains would compensate for losses.

shocks affect the physical health index more than twice as strongly for the elderly compared to those below age 80. The effect on survival is entirely driven by for the elderly. This age heterogeneity makes sense. 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.

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

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

One worry might be that the stock market correlates coincidentally with health profiles of those retirees who tend to hold a lot of stocks. Looking at the stock market development over the observations period, this seems unlikely. Positive and negative stock market changes follow each other and it is hard to imagine that health profiles of stock holders just happen to follow these ups and downs by chance. Still, retirees with the same fraction of wealth held in stocks at different points in the stock market cycle might not be 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 a finite number of stock market changes so that there could be a spurious correlation of stock market changes with broad trends in which kind of people hold stocks. Also, individuals do not rebalance portfolios continuously. So a retiree with 20% in stocks who does not rebalance her portfolio will end up with 33% in stocks when the stock market doubles. One way to rule out such correlation of the stock market cycle with the type of investor as a potential driver is to instrument actual stock holdings with individuals' initial stock holdings in the first period. Initial stock holdings are constant over time for a given individual. Therefore, they are uncorrelated with where we are in the stock market cycle. Table A.11 shows results from such 2SLS regressions. Point estimates and significance levels vary slightly compared to the baseline specification, but despite the loss of precision implied by this IV strategy, the overall effect pattern carries over to this IV specification.

An alternative way to check whether estimated effects are driven by changes in investor types is the inclusion of predetermined demographic controls (Altonji et al., 2005). If effects are driven by changes in the type of investors then the inclusion of controls like gender, age, education, and region of residence should diminish this selection bias. As shown in Table 3, adding a wide range of demographic controls to the baseline specification hardly changes any of the estimates (in fact, several of the point estimates increase slightly). But the included demographic controls might just be poorly measured proxies of the actual confounders. As Pei et al. (2017) show, a more sensitive test to detect selection in the presence of measurement error is the inclusion of individual controls as dependent variable on the left-hand side of the regression equation. Table A.12 shows that none of these balancing regressions for various socio-economic controls yields significant wealth shock effects.

These results make it unlikely that effects are driven by selection. Still, effects might not be running through (stock) wealth. For example, estimates could be driven by an impact of the overall business cycle on stock market investors (stock-owning retirees might follow the news more closely and worry more about the country's economic future). To test this, I replace constructed wealth shocks with the interaction of stock holdings with the overall unemployment rate, which reflects the business cycle better than the stock market in column (2) of Table 7. In column (3), I test whether the stock market might for some reason affect wealthy retirees more than poorer retirees, regardless of actual stock holdings. I use the interaction of the stock market change with the wealth fraction held in bonds as an alternative placebo shock (wealthy retirees tend to hold larger fractions of their wealth not only in stocks but also in bonds). Despite the strong collinearity with the constructed wealth shock, the placebo shocks do not consistently affect health outcomes when included separately. And the original wealth shock effect is robust and remains largely unchanged when I include all three shocks in horse race regressions in the fourth column of Table 7.

To sum up, it seems unlikely that a correlation of the stock market cycle with investor types or investors' health profiles is driving the results. Notice that there is also no direct effect of the stock market on retirees without stocks, neither on wealth (Table 2) nor on health outcomes (Table 5). This suggests that constructed wealth shocks are indeed driving the observed changes in health and that effects are mainly running through stock wealth.

5.6 Effect size

5.6.1 Effect size compared to benchmark regressions

How large are the estimated effects? An insightful benchmark is the cross-sectional relationship of wealth and health. Regressing health on wealth in levels does not allow for a causal interpretation due to reverse causality and omitted third factors. But one would typically expect such endogeneity to bias the coefficient upwards, implying that benchmark regressions provide an upper bound for the average causal effect of wealth on health in the sample.

Tables 8 and 9 compare the baseline effects with the cross-sectional relationship of health and wealth. The estimates in the first two columns in Table 8 suggest that the wealth shock effect is in a similar range as the cross-sectional relationship for the physical health index, about 40% larger for mental health and 3 times larger for survival. With respect to the physical health index this means that a 10% negative wealth shock leads to a similar health decline as the health gap that is associated with a 10% wealth difference in the data.

Benchmark regressions for individual health conditions in Table 9 indicate that this is not yet the whole story. While wealth shocks affect only particular conditions the cross-sectional wealth gradient is strongly significant and of similar size for all health conditions, except for cancer.[24] For hypertension and heart disease, the wealth shock effect is about twice as large as the benchmark gradient. This means that after a stock market-induced wealth loss you will suffer more from hypertension and related diseases than your ex-ante poorer neighbor. But your neighbor is still more likely to have arthritis, diabetes, and lung disease.

The differences between the baseline and cross-sectional estimates suggest that the effects of wealth shocks are different from the average causal effects of wealth on health in the sample. This seems plausible. Someone owning \$500k can afford better health care and healthier consumption than somebody owning \$300k, which over time accumulates to a better health stock. But this is a different effect from losing \$200k in a stock market crash, which may involve high blood pressure and psychological factors such as stress and depression rather than just a slight change in health inputs.[25] Equally for positive shocks, a unexpected windfall might have a more positive psychological effect preventing the onset of depression and

[24] 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.

[25] I do not find effects on health inputs or nutrition (see section A.4).

high blood pressure than a difference in wealth an individual has adapted to over an entire lifetime.[26]

Notice that the comparison with the cross-section also provides further confidence that my estimates are not driven by a coincidental correlation of the stock market with the socio-economic status of stock market investors. If this were the case, we should observe a similar pattern of effects across health conditions as in the benchmark regressions. But the pattern is clearly different.

Column (3) in Tables 8 and 9 shows OLS benchmark regressions of health changes on percentage wealth changes. These regressions account for individual fixed effects but changes in wealth might still be endogenous, e.g. if a negative health shock results in large out-of-pocket expenditures beyond what is covered by Medicaid. Across all health measures, the resulting estimates are very small and in most cases not significantly different from zero. These findings of zero effects are interesting because we would expect potential endogeneity left in wealth changes to bias the estimate up and not towards zero. A more severe problem than potential endogeneity — in particular for elderly retirees — might be measurement error in reported wealth that becomes amplified in first differences (as shown in Table 2 changes in wealth are quite noisy). Classical measurement can be addressed with instrumental variables, and a natural candidate for an instrument are the constructed wealth shocks.

Column (4) in Tables 8 and 9 reports such 2SLS regressions using the constructed wealth shocks as instruments for the changes in reported wealth. The resulting pattern of effects closely resembles the estimates from the baseline regressions. Coefficients are highly significant and about 20-30% larger than the baseline estimates. Note that in the IV framework the baseline specification is the "reduced form" while the regression of wealth changes on wealth shocks is the "first stage." Since the IV estimate is approximately equal to the reduced form divided by the first stage and the first stage coefficient in Table 2 is smaller one,

[26]The particularly large difference between baseline and benchmark effects for survival might in part be driven by a competing risk mechanism. To formalize this point, assume that among elderly who enter retirement, 1/3 have a low health stock. Among the poor, this group has a high mortality rate of 30%, while it is zero among the wealthy (it is also zero for those with a high health stock regardless of wealth). Among the poor, this implies a mortality rate of 9.9% in the first period, but already after four periods this rate decreases to 3.17% as poor retirees with a low health stock "die out." The rich group, on the other hand, will face the initial mortality rate of 9.9% in any period in which they experience a wealth shock that makes them poor. However, this mechanism should in particular increase the effect of negative shocks and I lack the power to detect asymmetric effects on mortality.

we should expect a 2SLS regression to inflate the baseline estimates accordingly.[27]

These findings re-emphasize that simple OLS regressions of health on wealth, both in levels and in first differences, should be interpreted with caution. Levels regressions are subject to omitted variable bias (though they might provide an upper bound of the causal effect if we assume a positive bias), while associations in first differences might be dominated by measurement error.

5.6.2 Effect size compared to existing literature

Another important effect size comparison are estimates from the existing literature. In an influential study, Smith (2005) uses a sample of employed individuals from the HRS and shows that changes in stock wealth conditional on socio-demographic controls do not correlate with changes in health. Because my sample consists of retirees with an average age 75, these findings for employed individuals do not contradict my results. However, I obtain similar zero results as Smith (2005) if I regress changes in health on changes in reported stock wealth (Table A.13). As in the benchmark regressions reported above, measurement error could be driving these zero results and using constructed wealth shocks as IV would address this issue. Indeed, the 2SLS reported in column (4) of Table A.13 are highly significant and resemble the baseline estimates.[28, 29]

[27] Which estimate is more relevant, the 2SLS or the "reduced form" baseline? The 2SLS specification provides us with estimates that are scaled in terms of the average change in reported wealth associated with a given constructed wealth shock. But reported wealth is net of consumption. And as people tend to adapt their consumption to wealth shocks, changes in reported wealth tend to be systematically smaller than the original wealth shock. Therefore, changes in reported wealth are the residual change after smoothing, while constructed wealth shocks are a direct proxy for the actual wealth shock. The reduced form presents estimates in terms of the actual wealth shock rather than in terms of the wealth change that remains after people have adapted their consumption, which is why I choose it as the baseline regression.

[28] In terms of effect size, the IV estimates are 60-80% larger than the baseline estimates (compared to 20-30% in Table 8). These inflated effect sizes are explained by a somewhat smaller "first stage" for stock wealth than for overall wealth.

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

5.7 Robustness

5.7.1 Alternative sample specifications

Regressions in Table 10 show that results are robust against various changes in the sample specification. In column (2) all financial respondents and their spouses regardless of their employment status are included as long as some kind of retirement income is reported for the household. This increases the sample size by more than 50%, but coefficients remain largely the same. In column (3) only households are included in which both spouses are above age 64. This rules out the possibility that results are driven by the group of pre-retirement age pensioners who are typically selected into the sample through bad health. The sample in column (5) only includes the financial respondents, a group that might be particularly aware of (and suffering from) the wealth shocks. Point estimates for physical health and survival are indeed 20-40% larger for that group, but confidence intervals also increase due to the reduction in the sample size.

The financial crisis of the late 2000s, considered the largest economic downturn since the Great Depression, is covered by the sample period (lasting from 12/2007 to 6/2009). One important question is to what extent effects might be driven by this dramatic episode. In the last column of Table 10, I exclude the financial crisis, yielding estimates that are remarkably close to the baseline estimates in the overall sample. This result is less surprising when one takes a look at the distribution of stock market-induced wealth shocks over time in Figure 2. During the financial crisis, the stock market fell to a similar extent as in the early 2000s and most interviews in the 2008 wave were conducted early in the crisis before the stock market had bottomed out. As a result, wealth shocks in that "financial-crisis wave" are not as exceptional as one might expect, neither in quantity nor in the magnitude of negative shocks to stock wealth.

In Table A.14, the sample is divided into quartiles of the respondents' lagged wealth. Overall, coefficients for the third and fourth wealth quartile are closest to the baseline results, while estimates for the bottom quartile are close to zero or reversed for three out of four outcomes. This pattern is not surprising given the low share of wealth held in stocks in that group (shown in the bottom row of Table A.14). It re-emphasizes that the typical complier is a retiree from the upper half of the wealth distribution and that the estimated average treatment effects are more representative for that group than for poor retirees.

5.7.2 Alternative definitions of stock market changes

In some years of the sample period there are substantial fluctuations in the weekly stock market data. It is questionable whether respondents are aware of (and care about) this week-to-week variation or whether effects are driven by stock market changes occurring over larger time periods. In the first three columns of Table A.15 and A.16, I compare the baseline results with the estimated effects of wealth shocks that are constructed using average changes in the S&P 500 across calendar years or entire interview waves in the data. Standard errors increase slightly, as one would expect, but point estimates remain largely the same, indicating that effects are not driven by within-wave variation.

The last column of Tables A.15 and A.16 show estimates based on a comprehensive "total return" version of the S&P500 that includes dividends, which is based on the assumption that all dividends are fully reinvested in the stock market. In practice, the total returns index adds up to about 4 percentage points to annual returns (while the variance of returns changes very little), with a limited impact on the identifying variation in wealth shocks, which is largely driven by the dramatic stock market booms and busts during the sample period. The "total return" estimates are therefore very similar to the baseline results.

5.7.3 Additional controls

In Table A.17, I explore the impact of the inclusion of additional control variables. In column (2), I include interaction terms of all socio-demographic controls with the individual specific stock market change, doubling the number of covariates. This addition slightly changes point estimates and standard errors, but these changes go in different directions for the different outcomes, and the overall pattern remains the same. In the last two columns of Table A.17, I show what happens when individual fixed effects are included. In a first differences specification, fixed effects absorb individual-specific trends, requiring three or more consecutive observations. In column (3), I run the baseline specification without fixed effects in the fixed effects subsample. Restricting the sample strains power, resulting in less significant estimates.[30] The inclusion of individual fixed effects in column (4), on the other hand, does not change estimates substantially compared to those in column (3).

[30]Note that the survival regressions require three consecutive observation periods already in the baseline specification (two periods to construct the wealth shock and an additional period to observe post-shock survival). Therefore, the fixed effects specification requires four consecutive periods for the survival regression, straining power in particular for this outcome.

6 Conclusion

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

Such impacts of wealth shocks on elderly health have been found so far only for poor retirees in poor countries. In contrast to the literature analyzing the wealth-health relationship, this paper documents that wealth shocks matter for the physical and mental health of wealthy retirees in a rich country. Policy makers considering pension reforms involving dramatic cuts for the elderly should take these results into account.

I uncover these results with a new measure to identify stock market fluctuations in the wealth of US retirees. This measure, the interaction of stock holdings with stock market changes, is of interest beyond the context of health economics. It could also be used to study, for example, the effects of unearned income on labor supply, savings, and in particular, on consumption.

The pattern of affected health conditions found in this study point to a story in which psychological factors play an important role. Psychological factors as central mechanism linking economic shocks and health outcomes are in line with the results of Sullivan and von Wachter (2009). They find strong mortality effects of lay-offs for displaced workers in the US and argue that psychological reactions are the most likely mechanism underlying these effects. These could be psychological reactions to the arrival of news about future consumption as well as reactions to actual changes in consumption. Applying the empirical strategy developed in this paper to data sets that allow to study consumption behavior in detail would be a promising path for future research. Of particular use would be consumption data in combination with information on individual stock portfolio compositions. Precise information on individual stock holdings would allow for the construction of high-frequency individual-specific wealth shocks, which would greatly increase the power of such analysis without the need for extended time series of stock market changes.

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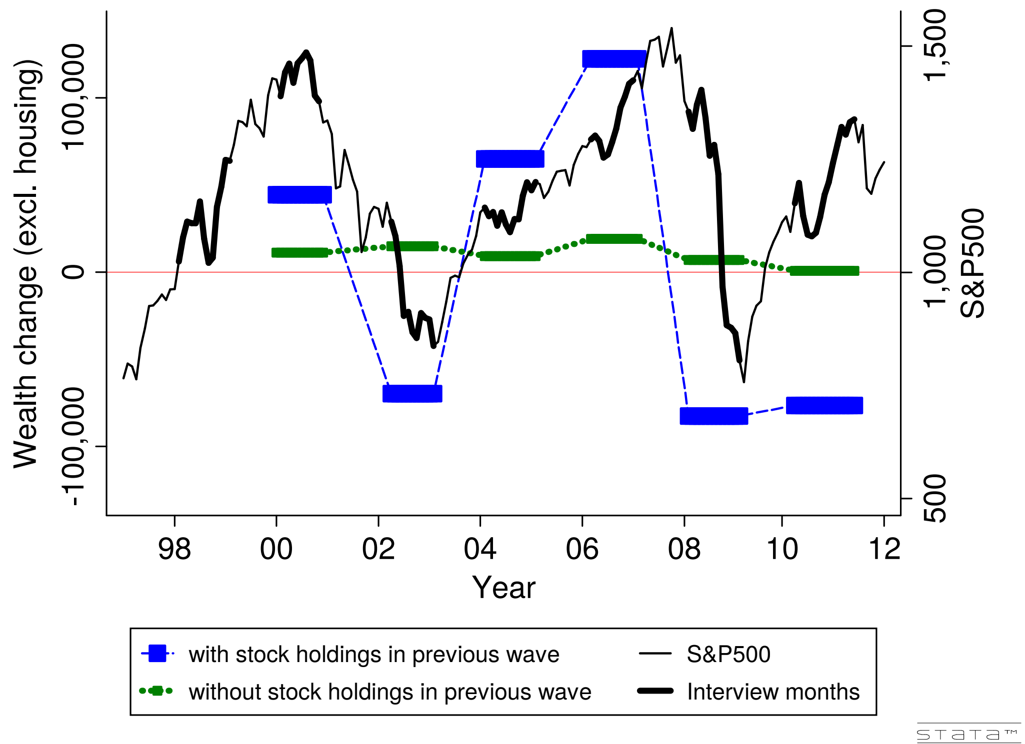
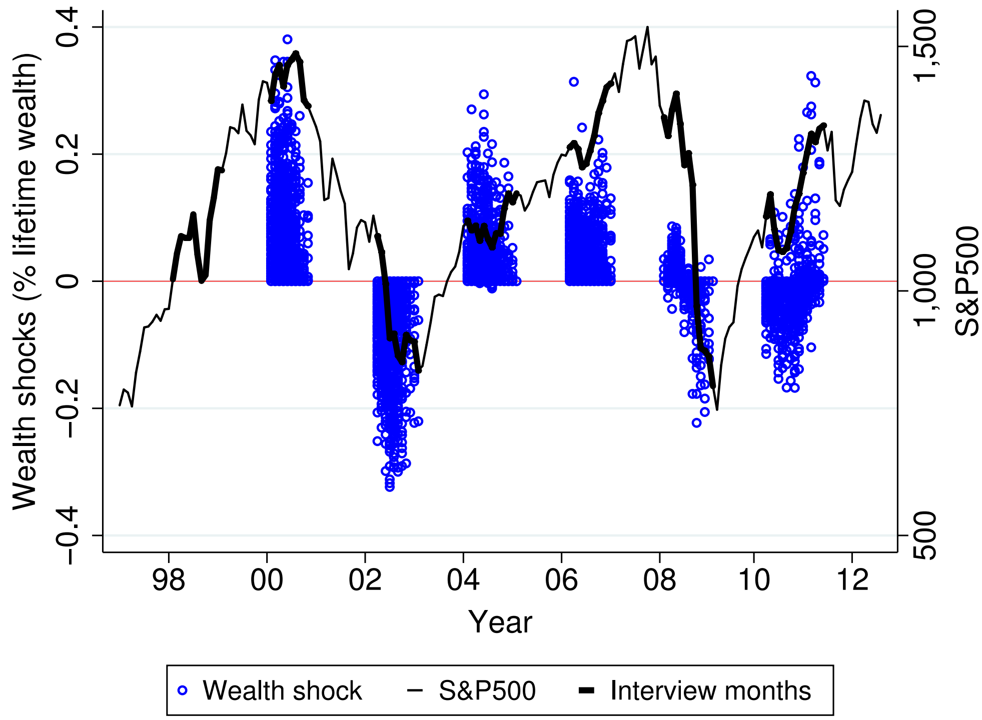
7 Tables and Figures

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



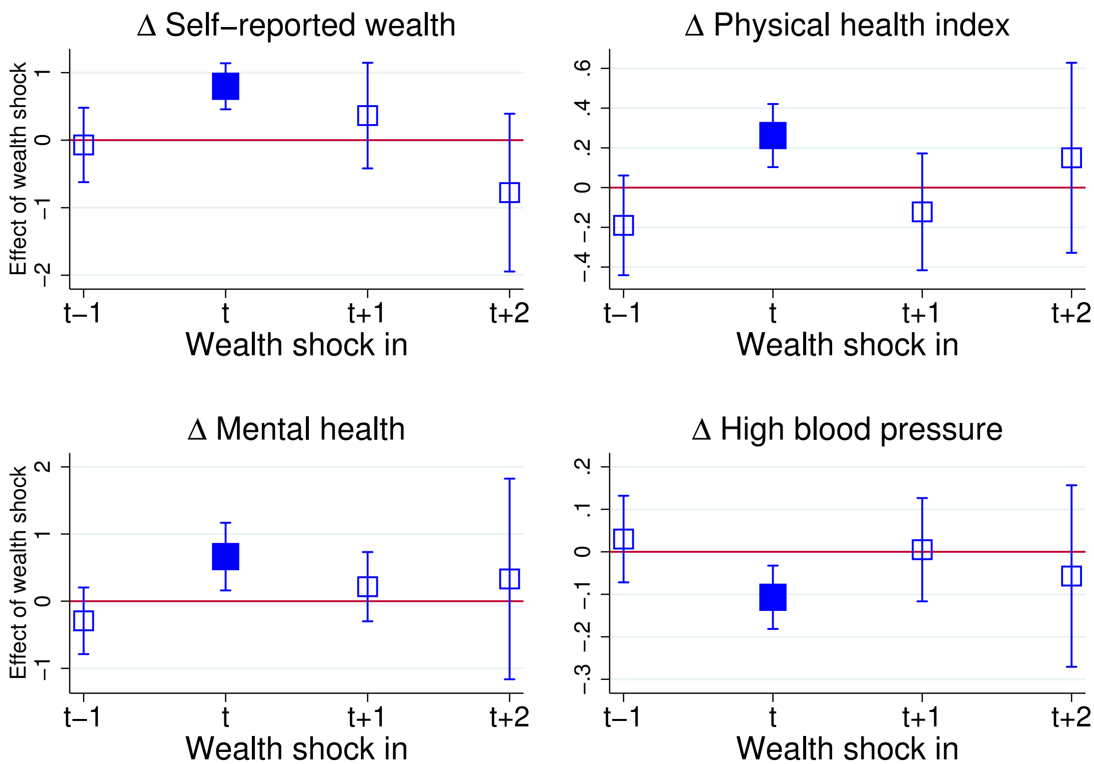
Average changes in reported wealth excluding housing wealth for retiree households with and without stocks in the previous period are plotted per HRS wave. The time period in each wave over which interviews were conducted is indicated by the length of the bars and by the bold sections of the S&P500 plot.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

Figure 2: Constructed Wealth Shocks and the S&P500



Constructed wealth shocks are plotted over time with the S&P500. The time period in each wave over which interviews were conducted is indicated by the bold sections of the S&P500 plot. 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.

Figure 3: Event studies for leads and lags of wealth shocks



Coefficients on leads and lags of wealth shocks in regressions for self-reported wealth, the physical and mental health index, and high blood pressure are reported along with 95% confidence intervals. Each plotted coefficient comes from a separate regression that includes lead or lag terms up to the one that is plotted. Corresponding regression results for these and further outcomes are reported in Appendix Tables A.6-A.8.

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

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

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

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

Dep. var.: Reported wealth change	(1)	(2)	(3)	(4)
Predicted wealth shock	0.823***	0.798***		
= % in stocks[t-1]	[0.173]	[0.174]		
x Stock market change				
D(Any stocks[t-1])			0.213***	0.208***
x Stock market change			[0.041]	[0.041]
Stock market change	0.067	0.035	0.057	0.023
	[0.164]	[0.166]	[0.166]	[0.169]
Main effects	✓	✓	✓	✓
Demographic controls		✓		✓
N	31,672	31,672	31,672	31,672
R^2	0.007	0.012	0.006	0.012

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

Table 3: Baseline Regressions of Health Measures on Wealth Shocks

	OLS		Probit-adapted OLS	
Dependent variable	(1)	(2)	(3)	(4)
Δ Physical health index	0.264***	0.262***	0.201***	0.199***
N= 35,738	[0.082]	[0.081]	[0.063]	[0.063]
Δ Self-reported health	0.228*	0.247*	0.184*	0.201*
N= 41,692	[0.123]	[0.125]	[0.107]	[0.108]
Self-reported change in health	0.088	0.102	0.127	0.147
N= 41,692	[0.082]	[0.086]	[0.119]	[0.124]
Δ Mental health index	0.654**	0.664**	0.295**	0.300**
N= 37,034	[0.253]	[0.257]	[0.131]	[0.132]
Survival	0.080*	0.096**	0.150*	0.180**
N= 34,955	[0.048]	[0.044]	[0.089]	[0.082]
Main effects	✓	✓	✓	✓
Demographic controls		✓		✓
Standardized dependent variable			✓	✓

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

Table 4: Baseline Regressions of Health Conditions on Wealth Shocks

	OLS		Probit-adapted OLS	
Dependent variable	(1)	(2)	(3)	(4)
Δ High blood pressure	-0.108***	-0.107***	-0.176***	-0.174***
	[0.039]	[0.038]	[0.063]	[0.062]
Δ Heart disease	-0.068*	-0.068*	-0.111*	-0.111*
	[0.035]	[0.036]	[0.058]	[0.059]
Δ Stroke	-0.015	-0.017	-0.029	-0.034
	[0.025]	[0.025]	[0.049]	[0.048]
Δ Diabetes	-0.001	0.003	-0.002	0.005
	[0.023]	[0.024]	[0.040]	[0.041]
Δ Cancer	-0.033	-0.034*	-0.059	-0.061*
	[0.020]	[0.020]	[0.036]	[0.036]
Δ Arthritis	-0.039	-0.038	-0.065	-0.064
	[0.046]	[0.046]	[0.077]	[0.076]
Δ Lung disease	0.000	0.000	0.000	0.001
	[0.021]	[0.021]	[0.039]	[0.039]
Main effects	✓	✓	✓	✓
Demographic controls		✓		✓
Standardized dependent variable			✓	✓

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

Table 5: Linearity of Wealth Shock Effects

Dependent Variable	Δ Physical H Index (1)	Δ Self-rep. Health (2)	Δ Mental H Index (3)	Survival (4)
Stock market change	-0.036	-0.036	0.152	-0.017
(reference group)	[0.042]	[0.061]	[0.198]	[0.033]
Stock market change	0.073*	0.025	0.023	0.007
x D(1-10% stocks[t-1])	[0.044]	[0.080]	[0.097]	[0.019]
Stock market change	0.138***	0.065	0.237**	0.035**
x D(>10% stocks[t-1])	[0.030]	[0.045]	[0.108]	[0.017]
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓

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

Table 6: Regressions of Health Measures on Wealth Shocks Interacted with Age and Gender

	Interaction category					
	Sign of wealth shock		Gender		Age	
Dependent Variable	Shock effect (1)	x (Shock>0) (2)	Shock effect (3)	x (Female) (4)	Shock effect (5)	x (Age>79) (6)
Δ Physical health index	0.584**	-0.237	0.275*	-0.031	0.134	0.311*
	[0.246]	[0.339]	[0.156]	[0.176]	[0.112]	[0.174]
Δ Self-reported health	-0.172	0.512	0.120	0.189	0.206	0.116
	[0.610]	[0.717]	[0.235]	[0.326]	[0.143]	[0.276]
Δ Mental health index	1.114*	0.176	0.256	0.674	0.531	0.322
	[0.635]	[0.802]	[0.339]	[0.523]	[0.338]	[0.482]
Survival	0.173	0.026	0.102	-0.009	-0.003	0.238**
	[0.164]	[0.194]	[0.066]	[0.069]	[0.042]	[0.091]
Controls (interacted)						
Main effects	✓		✓		✓	
Demographic controls	✓		✓		✓	

Shown are the coefficients on wealth shocks and the coefficients on wealth shock interacted with three different subgroup dummies: positive shocks, female, and age above 79. All controls are interacted with the respective subgroup dummy. Further comments as in Table 3.

Table 7: Inclusion of Placebo Shocks

	Baseline (1)	Placebo u-rate (2)	Placebo bond (3)	Horse race (4)
Dep. var.: Δ Index of Health Conditions				
Wealth shock	0.262***			0.292*
	[0.081]			[0.151]
Placebo u-rate shock		-0.103		0.011
		[0.063]		[0.119]
Placebo bond shock			-0.497	-0.621
			[0.392]	[0.390]
Dep. var.: Δ Self-reported health				
Wealth shock	0.247*			0.220
	[0.125]			[0.215]
Placebo u-rate shock		-0.206**		-0.030
		[0.080]		[0.164]
Placebo bond shock			-0.047	-0.153
			[0.629]	[0.634]
Dep. var.: Δ Mental health index				
Wealth shock	0.664**			1.035***
	[0.257]			[0.331]
Placebo u-rate shock		-0.170		0.397*
		[0.165]		[0.223]
Placebo bond shock			2.099**	1.717*
			[0.936]	[0.960]
Dep. var.: Survival				
Wealth shock	0.096**			0.205**
	[0.044]			[0.088]
Placebo u-rate shock		0.030		0.122
		[0.044]		[0.091]
Placebo bond shock			0.116	0.085
			[0.166]	[0.165]
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓

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

Table 8: Benchmark Regressions of Health Measures on Ln of Lifetime Wealth

	Baseline	OLS Benchmark		2SLS
	$\Delta H_{i,t}$ on $\frac{\Delta S\&P_t}{S\&P_{t-1}} \frac{s_{i,t-1}}{W_{i,t-1}}$	$H_{i,t}$ on $lnW_{i,t}$	$\Delta H_{i,t}$ on $\frac{\Delta W_{i,t}}{W_{i,t-1}}$	$\Delta H_{i,t}$ on $\frac{\Delta W_{i,t}}{W_{i,t-1}}$ IV: $\frac{\Delta S\&P_t}{S\&P_{t-1}} \frac{s_{i,t-1}}{W_{i,t-1}}$
Dependent Variable	(1)	(2)	(3)	(4)
Physical health index	0.262***	0.220***	0.004	0.340***
	[0.081]	[0.013]	[0.003]	[0.127]
Self-reported health	0.247*	0.334***	0.019***	0.306*
	[0.125]	[0.009]	[0.005]	[0.164]
Mental health index	0.664**	0.479***	0.016	0.769**
	[0.257]	[0.017]	[0.011]	[0.341]
Survival	0.096**	0.031***	0.000	0.119**
	[0.044]	[0.002]	[0.002]	[0.060]
First stage F-statistic				25.13
Main effects	✓			✓
Demographic controls	✓			✓
Male, age, cohort		✓	✓	

Column (1) shows the baseline estimates as in Table 3. Columns (2) and (3) show OLS regressions of health measures on log wealth and health changes on percentage wealth changes, respectively. Column (4) shows the 2SLS coefficients on changes in reported wealth with constructed wealth shocks as instrument. Dependent variables are regressed in first differences in columns (1), (3), and (4), and in levels in column (2) (except for survival, which is not transformed between columns). In columns (2) and (3) only gender, age, and cohort controls are included, so that lifetime wealth proxies for socio-economic status within these groups. The inclusion of further controls decreases the coefficient on lifetime wealth. Further comments as in Table 3.

Table 9: Benchmark Regressions of Health Conditions on Ln of Lifetime Wealth

	Baseline	OLS Benchmark		2SLS
	$\Delta H_{i,t}$ on $\frac{\Delta S\&P_t}{S\&P_{t-1}} \frac{s_{i,t-1}}{W_{i,t-1}}$	$H_{i,t}$ on $lnW_{i,t}$	$\Delta H_{i,t}$ on $\frac{\Delta W_{i,t}}{W_{i,t-1}}$	$\Delta H_{i,t}$ on $\frac{\Delta W_{i,t}}{W_{i,t-1}}$ IV: $\frac{\Delta S\&P_t}{S\&P_{t-1}} \frac{s_{i,t-1}}{W_{i,t-1}}$
Dependent Variable	(1)	(2)	(3)	(4)
High blood pressure	-0.107***	-0.052***	0.001	-0.139**
	[0.038]	[0.004]	[0.001]	[0.054]
Heart disease	-0.068*	-0.031***	-0.003***	-0.088*
	[0.036]	[0.004]	[0.001]	[0.049]
Stroke	-0.017	-0.031***	-0.000	-0.023
	[0.025]	[0.003]	[0.001]	[0.032]
Diabetes	0.003	-0.062***	0.000	0.004
	[0.024]	[0.004]	[0.001]	[0.030]
Cancer	-0.034*	0.023***	-0.000	-0.045
	[0.020]	[0.003]	[0.001]	[0.028]
Arthritis	-0.038	-0.035***	-0.002*	-0.050
	[0.046]	[0.004]	[0.001]	[0.059]
Lung disease	0.000	-0.036***	0.000	0.000
	[0.021]	[0.003]	[0.001]	[0.027]
Main effects	✓			✓
Demographic controls	✓			✓
Male, age, cohort		✓	✓	

Column (1) shows the baseline estimates as in Table 3. Columns (2) and (3) show OLS regressions of health measures on log wealth and health changes on percentage wealth changes, respectively. Column (4) shows the 2SLS coefficients on changes in reported wealth with constructed wealth shocks as instrument. For further comments, see Table 8.

Table 10: Alternative sample specifications

Dependent Variable	Baseline (1)	Including non-retirees (2)	Excluding HH $<$ 65 (3)	Singles only (4)	Financial resp. only (5)	Excluding 12/07 - 6/09 (6)
Δ Physical health index	0.262^{***}	0.221^{***}	0.325^{***}	0.453^{***}	0.311^{***}	0.263^{***}
	[0.081]	[0.080]	[0.080]	[0.114]	[0.094]	[0.082]
N	35,738	55,060	28,285	17,094	26,851	29,837
Δ Self-reported health	0.247^{*}	0.201	0.256^{*}	0.129	0.235^{*}	0.248^{**}
	[0.125]	[0.133]	[0.137]	[0.203]	[0.141]	[0.123]
N	41,692	63,229	33,236	20,318	31,583	34,848
Δ Mental health index	0.664^{**}	0.452^{**}	0.756^{**}	0.834^{*}	0.545^{*}	0.695^{***}
	[0.257]	[0.224]	[0.326]	[0.489]	[0.314]	[0.259]
N	37,034	56,892	29,240	17,747	28,384	30,746
Survival	0.096^{**}	0.082^{**}	0.089^{*}	0.101	0.136^{**}	0.100^{**}
	[0.044]	[0.033]	[0.047]	[0.085]	[0.054]	[0.045]
N	34,955	52,934	27,573	16,943	26,341	28,437
Main effects	✓	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓

Column (1) shows the baseline estimates as in Table 3. Column (2): non-retired individuals are included (as long as some kind of retirement income is reported for HH). (3): HHs are excluded if either financial respondent or spouse or both are below age 65. (4): Only single HH included. (5): Only financial respondents are included. (6): Wave 9, covering the financial crisis, is excluded. Further comments as in Table 3.

A Appendix

A.1 The role of stock market expectations

Constructed wealth shocks under- or overestimate actual wealth shocks if retirees' expectations of stock market returns systematically differ from zero. Since 2002 the HRS includes a question about the likelihood that the stock market increases within the following year. Figure A.5 in the Appendix plots monthly averages for this question together with the S&P500. Expectations are strikingly low: even those with stocks expect on average only a 45-60% chance that the stock market will increase. Furthermore, expectations seem to be slightly correlated with the stock market. Following Dominitz and Manski (2007) I transform expected probabilities about stock market increases into expected stock market returns and adjust for them when constructing wealth shocks. As expectations are only marginal compared to actual stock market changes, their inclusion decreases estimates only slightly. For better comparability of my results with other studies I therefore do not include expectations in the baseline regressions.

A.2 Further details on the construction of lifetime wealth

The HRS reports the subjective probability of living to reach a certain age and Hurd and McGarry (2002) show these subjective probabilities are predictive of the respondent's remaining life time. I do not include this information because subjective survival probabilities are only available for age 75 and 85 and I would have to make assumptions about how subjective probabilities at other ages relate to these reports. However, the inclusion of such subjective survival probabilities may be a worthwhile extension for future research.

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

Different life expectancies within households, i.e. within couples, are a further complication. Typically, wives can expect to survive their husbands, but it would be demanding to calculate all different survival constellations and the corresponding exact survivor benefit amounts. For simplicity, a couple's lifetime 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 lifetime wealth formula for couples does not affect the pattern of the estimated effects (see Table 10).

A.3 Estimating effects on physical health conditions in survival models

As discussed in the data section, the questions on physical health conditions ask whether a condition has ever been diagnosed. Taken literally, these questions imply a survival process. Once a respondent replied "yes" to the question, there should not be any reversal to "no" in a future period. In the data, these reversals occur and they seem non-random, which suggests that not all respondents understand the question in this way and that these reversals contain information about people's current health status. However, one can create measures of health conditions that are switched on if a respondent ever replied "yes" and estimate effects on these outcomes using survival models. Note that this transformation implies a loss of information not only because it eliminates reversals from "yes" to "no". It also eliminates future switches back from "no" to "yes", which might contain further information about actual health changes (e.g. the sequence "no"-"yes"-"no"-"no"-"yes" is transformed to "no"-"yes"-"yes"-"yes"-"yes").

Table A.5 shows hazard ratios estimated using the following Cox proportional hazard model:

$$h(t) = h_0(t)exp(\beta \frac{s_{i,t-1}}{W_{i,t-1}} \frac{\Delta SP_t}{SP_{t-1}} + \gamma \frac{s_{i,t-1}}{W_{i,t-1}} + \delta X_{i,t}) \tag{A.1}$$

where $h_0(t)$ is the baseline hazard function and the variables in the exponent are the same as in the baseline specification. Survival time is the time until a respondent affirms the diagnosis of a health condition for the first time, with individuals never reporting a health condition treated as censored observations. The Cox proportional hazard model assumes that the hazard rate of developing a health condition is multiplicatively shifted by changes in the right-hand-side variables, i.e. exogenous wealth shocks and included covariates in this case.

Appendix Table A.5 compares the baseline regression estimates in column (1) and (2) with the estimated hazard ratios based on equation (A.1) in columns (3) and (4). For high blood pressure, the hazard ratio is smaller than one and significant at the 5% level, in line with the negative impact of wealth shocks estimated in the baseline regressions. The estimated hazard ratio in the heart disease regression is smaller than one, too, but it is not significantly different from zero.

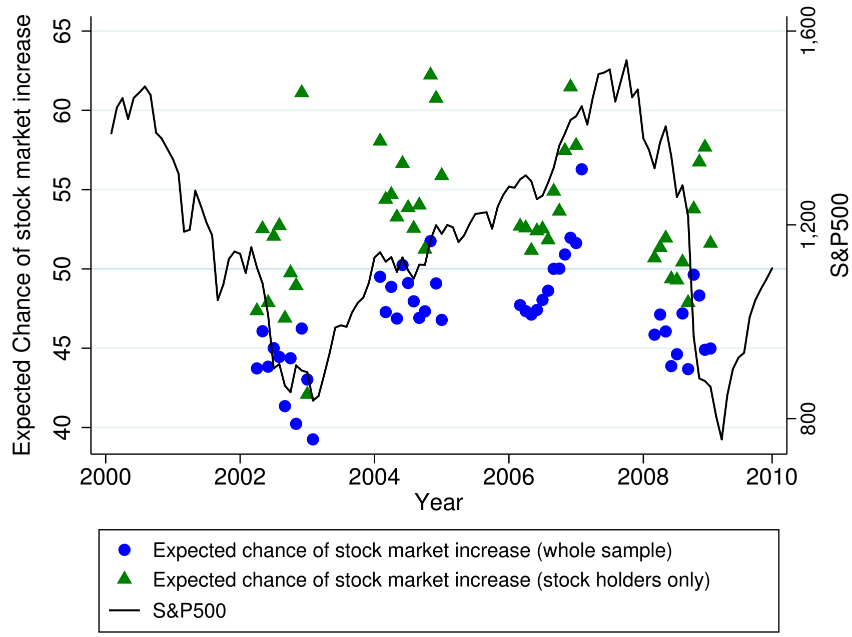
A.4 Effects on nutrition and health inputs

As discussed in the introduction, calorie intake and health inputs are central mechanisms through which wealth affects health in poor countries (Jensen and Richter 2004; Case 2004) but they might be less relevant for wealthy retirees in the US. The HRS reports respondents' body mass index (BMI) and the number of doctor visits as well as out-of-pocket medical expenditure (OOP), which allows to directly test for the role of these potential mechanisms. Table A.18 in the Appendix shows that indeed wealth shocks do not significantly affect any of these three measures.

Notice however that there could be opposing effects at work that might cancel out in the regression. People might be cutting back on food expenditures as a response to a negative wealth shock. But 'cheaper calories' often come in the form of inferior food that remains stored in body fat to a greater extent than higher quality food. In this case, cutting back on food expenditure might even increase people's BMI. The effect on health inputs is ambiguous, too. If wealth shocks make you sick, you might end up going to the doctor more often, even if this might imply higher OOP expenditures, e.g. because premium health care coverage is not affordable anymore. Therefore, the results in Table A.18 should not be interpreted as evidence that wealth shocks do not affect people's nutrition behavior or the optimal receipt of health inputs. However, it seems unlikely that these are the main mechanisms underlying the strong short-term effects of wealth shocks on physical and mental health that we observe in the data.

A.5 Appendix Figures

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



Notes: 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.

A.6 Appendix Tables

Table A.1: Summary Statistics Demographic Controls

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

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

Table A.2: Summary Statistics Wealth Measures

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

For comments, see notes under Table 1.

Table A.3: Summary Statistics of Health Measures

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

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

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

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

Notes: The coefficient on constructed wealth shocks (*'%wealth in stocks[t-1] x stock market change*') is displayed. Main effects' are the lagged fraction of wealth held in stocks, a dummy for lagged stock ownership, the stock market change, and year-month dummies. 'Demographic controls' are dummies for gender (1), age group (12), cohort (10), race (2), region (4), degree (4), and lagged marital status (7). Standard errors in brackets are multi-level clustered by household and interview month. For details on the coding of the items, see the data sections.

Table A.5: Estimating effects on physical health conditions using survival models

	Baseline		Cox prop. hazard	
Dependent variable	(1)	(2)	(3)	(4)
Δ High blood pressure	-0.108***	-0.107***	0.165**	0.200**
	[0.039]	[0.038]	[0.147]	[0.162]
Δ Heart disease	-0.068*	-0.068*	0.812	0.761
	[0.035]	[0.036]	[0.887]	[0.714]
Δ Stroke	-0.015	-0.017	0.365	0.393
	[0.025]	[0.025]	[0.653]	[0.579]
Δ Diabetes	-0.001	0.003	1.125	1.478
	[0.023]	[0.024]	[1.418]	[1.713]
Δ Cancer	-0.033	-0.034*	0.193	0.227
	[0.020]	[0.020]	[0.198]	[0.216]
Δ Arthritis	-0.039	-0.038	0.515	0.572
	[0.046]	[0.046]	[0.617]	[0.605]
Δ Lung disease	0.000	0.000	2.408	2.362
	[0.021]	[0.021]	[3.950]	[3.581]
Main effects	✓	✓	✓	✓
Demographic controls		✓		✓
Survival model			✓	✓

Notes: Column (1) and (2) show the baseline estimates as in Table 3. Column (3) and (4) report hazard ratio estimated in Cox proportional hazard models described in Appendix section A.3. Further comments as in Table 3.

Table A.6: Event study regressions for main outcomes

	(1)	(2)	(3)	(4)	(5)
Dep. var.: Δ Reported wealth change					
Wealth shock (t-2)	-0.138				
	[0.377]				
Wealth shock (t-1)	-0.131	-0.071			
	[0.516]	[0.281]			
Wealth shock in t	-0.153	0.499***	0.798***	0.846***	0.623**
	[0.344]	[0.188]	[0.174]	[0.201]	[0.284]
Wealth shock (t+1)				0.364	0.074
				[0.399]	[0.500]
Wealth shock (t+2)					-0.777
					[0.596]
N	12,036	19,567	31,672	19,530	11,987
Dep. var.: Δ Physical health index					
Wealth shock (t-2)	0.109				
	[0.166]				
Wealth shock (t-1)	-0.231	-0.190			
	[0.194]	[0.128]			
Wealth shock in t	-0.358*	0.084	0.262***	0.218**	0.066
	[0.192]	[0.107]	[0.081]	[0.104]	[0.146]
Wealth shock (t+1)				-0.122	-0.244
				[0.150]	[0.190]
Wealth shock (t+2)					0.150
					[0.244]
N	13,256	21,894	35,738	21,986	13,440
Dep. var.: Δ Self-reported health					
Wealth shock (t-2)	0.360				
	[0.262]				
Wealth shock (t-1)	0.235	-0.130			
	[0.349]	[0.206]			
Wealth shock in t	0.840**	0.188	0.247*	0.290*	0.294**
	[0.404]	[0.148]	[0.125]	[0.162]	[0.144]
Wealth shock (t+1)				0.224	0.293
				[0.164]	[0.250]
Wealth shock (t+2)					0.311
					[0.305]
N	15,416	25,374	41,692	25,389	15,440

Notes: Comments below Table A.7.

Table A.7: Event study regressions for main outcomes, continued

	(1)	(2)	(3)	(4)	(5)
Dep. var.: Δ Mental health index					
Wealth shock (t-2)	-0.314				
	[0.519]				
Wealth shock (t-1)	-0.194	-0.292			
	[0.222]	[0.253]			
Wealth shock in t	-0.562	-0.071	0.664**	0.913***	0.506
	[0.663]	[0.303]	[0.257]	[0.284]	[0.407]
Wealth shock (t+1)				0.216	-0.002
				[0.263]	[0.459]
Wealth shock (t+2)					0.331
					[0.762]
N	13,829	22,636	37,034	23,235	14,344
Dep. var.: Survival					
Wealth shock (t-2)	0.181**				
	[0.088]				
Wealth shock (t-1)	0.112	0.000			
	[0.108]	[0.061]			
Wealth shock in t	0.156	0.038	0.096**	0.000	0.000
	[0.190]	[0.087]	[0.044]	[.]	[.]
Wealth shock (t+1)				0.000	0.000
				[.]	[.]
Wealth shock (t+2)					0.000
					[.]
N	11,879	20,603	34,955	25,447	15,468

Notes: Regressions with different sets of leads and lags of wealth shocks are displayed. Each column represents one regression. All regressions include main effects, as well as the respective lead and lag versions of the main effects (depending on which leads and lags of the wealth shocks are included), and demographic controls. Figure 3 plots the coefficients on the diagonal for the most affected outcomes. For survival, lead regressions cannot be estimated as everyone observed with a future wealth shock has survived.

Table A.8: Event study regressions for hypertension, heart problems, and cancer

	(1)	(2)	(3)	(4)	(5)
Dep. var.: Δ High blood pressure					
Wealth shock (t-2)	-0.073				
	[0.068]				
Wealth shock (t-1)	-0.017	0.030			
	[0.081]	[0.052]			
Wealth shock in t	-0.157*	-0.124**	-0.107***	-0.054	-0.009
	[0.093]	[0.051]	[0.038]	[0.058]	[0.066]
Wealth shock (t+1)				0.005	0.030
				[0.062]	[0.089]
Wealth shock (t+2)					-0.057
					[0.109]
N	13,256	21,894	35,738	21,986	13,440
Dep. var.: Δ Heart disease					
Wealth shock (t-2)	-0.047				
	[0.068]				
Wealth shock (t-1)	0.067	0.014			
	[0.075]	[0.052]			
Wealth shock in t	0.109	-0.079	-0.068*	-0.091**	-0.132**
	[0.115]	[0.052]	[0.036]	[0.035]	[0.052]
Wealth shock (t+1)				-0.028	-0.062
				[0.046]	[0.062]
Wealth shock (t+2)					-0.133
					[0.108]
N	13,256	21,894	35,738	21,986	13,440
Dep. var.: Δ Cancer					
Wealth shock (t-2)	0.006				
	[0.053]				
Wealth shock (t-1)	0.007	0.058			
	[0.052]	[0.041]			
Wealth shock in t	0.020	0.010	-0.034*	-0.017	0.014
	[0.061]	[0.031]	[0.020]	[0.025]	[0.044]
Wealth shock (t+1)				0.034	0.048
				[0.032]	[0.036]
Wealth shock (t+2)					0.063
					[0.073]
N	13,256	21,894	35,738	21,986	13,440

Notes: Comments below Table A.7.

Table A.9: 4-year wealth shocks

	Baseline (1)	4 yr sample (2)	4 yr shock (3)	Both shocks (4)
Dep. var.: Δ Physical health index				
Wealth shock (2 yr)	0.262***	0.166		0.152
	[0.081]	[0.082]		[0.121]
Wealth shock (4 yr)			0.067	0.030
			[0.082]	[0.090]
N	35,738	23,064	23,064	23,064
p-value ($\beta_{2yr} \neq \beta_{4yr}$)				0.515
Dep. var.: Δ Mental health index				
Wealth shock (2 yr)	0.664**	0.245		0.181
	[0.257]	[0.210]		[0.357]
Wealth shock (4 yr)			0.176	0.132
			[0.210]	[0.243]
N	37,034	23,895	23,895	23,895
p-value ($\beta_{2yr} \neq \beta_{4yr}$)				0.926
Dep. var.: Survival				
Wealth shock (2 yr)	0.096**	0.032		0.031
	[0.044]	[0.044]		[0.074]
Wealth shock (4 yr)			0.009	0.002
			[0.044]	[0.048]
N	34,955	21,955	21,955	21,955
p-value ($\beta_{2yr} \neq \beta_{4yr}$)				0.781
Dep. var.: Δ High blood pressure				
Wealth shock (2 yr)	-0.107***	-0.151***		-0.126**
	[0.038]	[0.039]		[0.057]
Wealth shock (4 yr)			-0.085**	-0.055
			[0.039]	[0.040]
N	35,738	23,064	23,064	23,064
p-value ($\beta_{2yr} \neq \beta_{4yr}$)				0.394
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓

Notes: Column (1) shows the baseline results. Column (2) reports baseline estimates in the subsample with non-missing 4-yr wealth shocks. Column (3) shows the coefficient on 4-yr wealth shocks. Regressions in column (4) include both 2-yr and 4-yr wealth shocks. Further comments as in Table 3.

Table A.10: Testing effect symmetry using dummies for stock market increases and decreases

Dependent Variable	Δ Physical H Index (1)	Δ Self-rep. Health (2)	Δ Mental H Index (3)	Survival (4)
D(>10% stock market *increase*)	0.009	-0.016	0.010	-0.003
	[0.010]	[0.016]	[0.047]	[0.009]
D(<-10% stock market *decrease*)	0.007	0.004	-0.060	-0.025
	[0.014]	[0.034]	[0.064]	[0.021]
% stocks[t-1]	0.024	-0.086	-0.104	-0.004
	[0.041]	[0.064]	[0.090]	[0.024]
D(>10% stock market *increase*)	0.013	0.061	0.173	0.027
x (% stocks[t-1])	[0.046]	[0.081]	[0.114]	[0.032]
D(>10% stock market *decrease*)	-0.118**	-0.031	-0.055	-0.016
x (% stocks[t-1])	[0.045]	[0.082]	[0.156]	[0.030]
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓

Notes: D(*>10% stock market change*) and D(*<-10% stock market change*) are dummy variables that indicate stock market changes of more than 10% or less than -10%, respectively. *'% stocks[t-1]'* is the lagged fraction of lifetime wealth held in stocks. Further comments as in Table 3.

Table A.11: 2SLS Regressions with Initial Stock Holdings as Instrument for Actual Stock Holdings

Dependent Variable	Baseline (1)	IV sample (2)	$\frac{\Delta S\&P_t}{S\&P_{t-1}}[\frac{s_i}{W_i}]^{1998}$ as IV for constructed wealth shocks (3)
Δ Physical health index	0.262***	0.315***	0.397***
	[0.081]	[0.094]	[0.135]
N		21,953	21,953
Δ Self-reported health	0.247*	0.261*	0.133
	[0.125]	[0.136]	[0.194]
N		25,968	25,968
Δ Mental health index	0.664**	0.460	0.744**
	[0.257]	[0.297]	[0.355]
N		22,760	22,760
Survival	0.096**	0.086*	0.052
	[0.044]	[0.048]	[0.047]
N		23,100	23,100
Main effects	✓	✓	✓
Demographic controls	✓	✓	✓

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

Table A.12: Balancing regressions

Dependent variable	Male (1)	Black (2)	Age <= 70 (3)	>= 80 (4)	> 12 yrs of education (5)	Region Midwest (6)
Predicted wealth shock	-0.004	-0.024	0.000	0.017	0.090	0.001
	[0.045]	[0.020]	[0.037]	[0.042]	[0.090]	[0.063]
Mean dep. var.	0.366	0.142	0.312	0.332	0.325	0.246
Controls						
Main effects	✓	✓	✓	✓	✓	✓
Demographics (excl. dep. var.)	✓	✓	✓	✓	✓	✓

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

Table A.13: Regressions of Health Measures on Changes in Reported Stock Wealth

	Specification of wealth shock			
	Baseline	Using changes in *reported* stock wealth		
	$\frac{\Delta S\&P_t}{S\&P_{t-1}} \frac{s_{i,t-1}}{W_{i,t-1}}$	$\frac{\Delta s_{i,t}}{1{,}000{,}000}$	$\frac{\Delta s_{i,t}}{W_{i,t-1}}$	$\frac{\Delta S\&P_t}{S\&P_{t-1}} \frac{s_{i,t-1}}{W_{i,t-1}}$ as IV for $\frac{\Delta s_{i,t}}{W_{i,t-1}}$
Dependent Variable	(1)	(2)	(3)	(4)
Δ Physical health index	0.262***	0.002	0.001	0.450***
	[0.081]	[0.003]	[0.008]	[0.165]
Δ Self-reported health	0.247*	0.006	0.022	0.389
	[0.125]	[0.005]	[0.014]	[0.238]
Δ Mental health index	0.664**	0.005	0.004	1.207**
	[0.257]	[0.008]	[0.022]	[0.501]
Survival	0.096**	-0.002	-0.010*	0.160*
	[0.044]	[0.002]	[0.006]	[0.086]
First stage F-statistic				32.07
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓

Notes: 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; $W_{i,t-1}$ = lifetime wealth (see Data section). Further comments as in Table 3.

Table A.14: Regressions by wealth quartile

Dependent Variable	Baseline (1)	Bottom quartile (2)	Second quartile (3)	Third quartile (4)	Top quartile (5)
Δ Physical health index	0.262***	0.113	0.733	0.428**	0.126
	[0.081]	[0.708]	[0.454]	[0.185]	[0.135]
N	35,738	8,382	8,718	8,820	9,008
Δ Self-reported health	0.247*	-0.323	1.123**	0.072	0.127
	[0.125]	[1.116]	[0.550]	[0.342]	[0.240]
N	41,692	10,185	10,192	10,206	10,208
Δ Mental health index	0.664**	-2.633	0.024	0.949	0.699*
	[0.257]	[2.187]	[0.776]	[0.741]	[0.379]
N	37,034	8,163	9,101	9,399	9,624
Survival	0.096**	0.004	0.044	0.118	0.134**
	[0.044]	[0.476]	[0.171]	[0.124]	[0.052]
N	34,955	8,590	8,587	8,479	8,514
Main effects	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓
Percent life-time wealth in stocks	0.068	0.006	0.024	0.067	0.181

Notes: The sample is split into quartiles based on households' lagged lifetime wealth. The coefficient on constructed wealth shocks (*'%wealth in stocks[t-1] x stock market change*') is displayed. Main effects' are the lagged fraction of wealth held in stocks, a dummy for lagged stock ownership, the stock market change, and year-month dummies. 'Demographic controls' are dummies for gender (1), age group (12), cohort (10), race (2), region (4), degree (4), and lagged marital status (7). Standard errors in brackets are multi-level clustered by household and interview month.

Table A.15: Alternative definitions of stock market changes

Dependent Variable	(1)	(2)	(3)	(4)
Δ Physical health index	0.262***	0.276***	0.294***	0.249***
	[0.081]	[0.086]	[0.086]	[0.079]
Δ Self-reported health	0.247*	0.204	0.253*	0.232*
	[0.125]	[0.130]	[0.135]	[0.120]
Δ Mental health index	0.664**	0.638**	0.635**	0.655**
	[0.257]	[0.285]	[0.285]	[0.251]
Survival	0.096**	0.080*	0.083*	0.092**
	[0.044]	[0.045]	[0.046]	[0.043]
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓
Stock market change averaged by year		✓		
Stock market change averaged by wave			✓	
Stock market change incl. dividends				✓

Notes: Column (1) shows the baseline results. Column (2) reports the coefficients of wealth shocks that are constructed using average annual changes in the stock market. In Column (3) stock market changes are averaged across entire waves instead of years. Regressions in column (4) use wealth shocks that are based on a "total returns" version of the S&P500 that includes dividends. Further comments as in Table 3.

Table A.16: Alternative definitions of stock market changes

Dependent Variable	(1)	(2)	(3)	(4)
Δ High blood pressure	-0.107***	-0.095**	-0.094**	-0.101***
	[0.038]	[0.039]	[0.039]	[0.038]
Δ Heart disease	-0.068*	-0.068*	-0.073*	-0.066*
	[0.036]	[0.038]	[0.038]	[0.035]
Δ Stroke	-0.017	-0.017	-0.027	-0.015
	[0.025]	[0.025]	[0.025]	[0.025]
Δ Diabetes	0.003	-0.007	-0.005	0.004
	[0.024]	[0.023]	[0.022]	[0.023]
Δ Cancer	-0.034*	-0.019	-0.021	-0.033*
	[0.020]	[0.022]	[0.022]	[0.020]
Δ Arthritis	-0.038	-0.062	-0.063	-0.040
	[0.046]	[0.044]	[0.043]	[0.044]
Δ Lung disease	0.000	-0.007	-0.011	0.002
	[0.021]	[0.020]	[0.020]	[0.020]
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓
Stock market change averaged by year		✓		
Stock market change averaged by wave			✓	
Stock market change incl. dividends				✓

Notes: Comments as in Table A.15.

Table A.17: Including additional controls

Dependent Variable	(1)	(2)	(3)	(4)
Δ Physical health index	0.262***	0.209***	0.166*	0.246**
	[0.081]	[0.079]	[0.089]	[0.110]
N	35,738	35,738	31,914	31,914
Δ Self-reported health	0.247*	0.190	0.159	0.172
	[0.125]	[0.153]	[0.127]	[0.180]
N	41,692	41,692	38,034	38,034
Δ Mental health index	0.664**	0.841***	0.542**	0.353
	[0.257]	[0.287]	[0.253]	[0.610]
N	37,034	37,034	33,605	33,605
Survival	0.096**	0.081*	-0.024	-0.057
	[0.044]	[0.043]	[0.043]	[0.064]
N	34,955	34,955	31,337	31,337
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓
Demographic controls, interacted with stock market change		✓		
Fixed effects sample			✓	✓
Individual fixed effects				✓

Notes: Column (1) shows the baseline results. The included 'demographic controls' are dummies for gender (1), age group (12), cohort (10), race (2), region (4), degree (4), and lagged marital status (7). In column (2), these demographic controls are interacted with the stock market change. Column (3) shows the baseline specification in the subsample of individuals that are observed with at least two changes (at least three consecutive observations). Column (4) includes person fixed effects. Further comments as in Table 3.

Table A.18: Regressions of Potential Mechanisms on Wealth Shocks

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

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