Does Health Affect Portfolio Choice?

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Abstract

A number of recent studies find that poor health is empirically associated with a safer portfolio allocation. It is difficult to say, however, whether this relationship is truly causal. Both health status and portfolio choice are influenced by unobserved characteristics such as risk attitudes, impatience, information, and motivation, and these unobserved factors, if not adequately controlled for, can induce significant bias in the estimates of asset demand equations. Using the 1992–2006 waves of the Health and Retirement Study, we investigate how much of the connection between health and portfolio choice is causal and how much is due to the effects of unobserved heterogeneity. Accounting for unobserved heterogeneity with fixed effects and correlated random effects models, we find that health does not appear to significantly affect portfolio choice among single households. For married households, we find a small effect (about 2–3 percentage points) from being in the lowest of five self-reported health categories.

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1 Introduction

With the aging of the baby boom, more households are entering a period of life when health becomes more precarious. A sudden illness or the onset of a chronic condition can lead households to revise some of their plans for retirement—how they will spend their time, how much they would like to spend and save, and how much risk they are willing to bear. In this paper we focus on the effect of health on one key decision: how to allocate financial wealth between risky and safe assets. This potential link is important, because as populations age even relatively small changes in asset demand due to health shocks can aggregate up to important effects on asset markets, national savings, and wealth adequacy. As we discuss below, the effect of health on portfolio choice is theoretically ambiguous, depending on the relative sizes of potentially offsetting effects. Nonetheless, a number of recent studies find that poor health is empirically associated with a safer portfolio allocation. It is difficult to say, however, whether this relationship is truly causal. Both health status and portfolio choice are influenced by unobserved characteristics such as risk attitudes, impatience, information, and motivation, and these unobserved factors, if not adequately controlled for, can induce significant bias in the estimates of asset demand equations. Using the 1992–2006 waves of the Health and Retirement Study, we investigate how much of the connection between health and portfolio choice is causal and how much is due to the effects of unobserved heterogeneity.

Why might one expect health to affect household portfolios? The life-cycle model suggests three primary channels through which health shocks could influence the share of risky assets: preferences, background risk, and expectations. The direction of the first channel depends critically on the complementarity of health with consumption and leisure. As pointed out by Edwards (2006, 2007, 2008), health shocks could either increase or decrease the marginal utility of consumption. Health and consumption could be complements if improved health allows individuals to take advantage of costly leisure activities, such as going out to the movies, taking a trip, or going out to eat. On the other hand, worsening health could increase the marginal value of labor-saving consumption, such as taxi rides or cleaning services. Finkelstein, Luttmer, and Notowidigdo (2009) summarize the empirical research on the state dependence of the marginal utility of consumption on health and find mixed evidence. While some studies conclude that sickness increases marginal utility (e.g., Edwards (2008)), others find negative state dependence (e.g., Finkelstein, Luttmer, and Notowidigdo (2008)). The complementarity of health and consumption remains an unsettled question.

The second channel arises because health shocks are frequently accompanied by large out-of-pocket medical costs. These unexpected costs constitute a form of background risk, which has been shown to affect optimal portfolio decisions (see, e.g., Kimball and Elmendorf (2000), Viceira (2001), and Haliassos and Michaelides (2003)). Given the growing body of evidence documenting the importance of medical costs for precautionary saving (see, e.g., Hubbard, Skinner, and Zeldes (1995), Palumbo (1999), and French and Jones (2004)), it is natural to expect uncertain medical costs to influence portfolio decisions as well. And indeed, Pang and Warshawsky (2008) examine a numerical model of health expenses and portfolio choice and find that medical expense risk generates a shift away from risky assets. These numerical results are consistent with a recent empirical study finding that better-insured households tend to invest a larger share of their savings in risky assets (Goldman and Maestas, 2007). It should be noted that much of the research on medical expense risk assumes that medical expenses follow an exogenous process. Yogo (2009), however, explicitly recognizes that some medical spending represents an endogenous investment in health, which can *reduce* a portion of the background risk associated with uncertain health.

Finally, health could affect portfolio choice by altering life expectancy. Bodie, Merton, and Samuelson (1992) show that horizon length can affect asset allocation, particularly when there is a sizable stream of future income (e.g., Social Security or pension income) that can substitute for bonds in a household's total wealth portfolio. Intuitively, when expected annuity income is large relative to financial wealth, fluctuations in asset returns have a comparatively smaller effect on the marginal utility of consumption and therefore the valuation of risk in a given portfolio. In a simplified world of fixed horizon length, shorter horizons reduce the present value of these income streams and therefore tend to push households toward safer portfolio decisions. In the real world, however, health shocks are likely to affect both the mean and the variance of the survival function. In that case, it can be shown that, *holding horizon constant*, an increase in survival risk can actually increase the optimal risky share for some households (Love and Perozek, 2007).

It is therefore unclear, a priori, whether a negative health shock should increase or decrease the share of risky assets; it depends on the complementarity of health and consumption, the extent to which medical expenses are endogenous, and the impact of the variance of the survival function. While the net effect of health on portfolio choice is theoretically ambiguous, several studies find that poor health is empirically associated with a safer household portfolio allocation. Rosen and Wu (2004) find a link between poor health and a reduced probability of owning any stocks or mutual funds, as well as a reduced share of wealth held in risky assets. The authors conclude there is a relatively strong connection between health and portfolio choice. Their interpretation, however, depends on the strong assumption in their random effects specifications that unobserved variables are uncorrelated with observed variables. This crucial assumption would be violated if, for instance, any of the unobserved factors mentioned above—risk aversion, impatience, information, or motivation—were correlated with any of the controls on the right-hand side, such as age, education, income, or wealth. While commonly-used panel data sets, such as the Health and Retirement Study, include questions about risk preferences and planning horizon, these provide only imperfect proxies for the true variation in preferences across households. Given the likelihood that some of the remaining unobserved characteristics might be correlated with the observables, a number of authors have continued to investigate the link between health and portfolio choice.

Berkowitz and Qiu (2006), using the same data set as Rosen and Wu, find that the relationship between health and portfolio allocation mostly disappears after controlling adequately for differences in financial wealth between sick and healthy households. Their result does not dismiss the importance of health shocks for portfolio choice, but rather identifies an indirect channel through which it operates—namely, the tendency of health shocks to erode financial wealth and thereby induce a change in portfolio allocation. Fan and Zhao (2009), using an older data set, report a strong relationship between portfolio choice and health in linear OLS and random effects specifications, but find that the correlation disappears in a linear fixed effect specification. Coile and Milligan (2009) perform event studies of health changes and find a small negative effect of a chronic health shock on the probability of holding IRAs or stocks, though no effect on the marginal stock share of assets. Our approach is similar in spirit to theirs, in that our fixed effect regressions rely on health changes and subsequent portfolio changes to identify the causal effect of health on portfolio allocation.

Edwards (2008) considers a slightly different question and asks whether households adjust their portfolios in response to self-perceived health risk. Edwards replicates the random effects approach of Rosen and Wu and also estimates IV Tobit models in order to address measurement error and the potential endogeneity of health status. Edwards finds a tendency for households to move toward safer assets in response to worsening health and worsening perceived health risk. The IV approach, however, imposes its own strong assumption—that the instruments are uncorrelated with the set of unobserved factors that affect portfolio choice. As with the random effects approach, this assumption could be problematic if, for example, instruments such as lagged health risk and smoking were correlated with unobserved factors such as risk aversion, impatience, information, or motivation.[1]

In this paper, we explore how much of the connection between health and portfolio choice is causal, and how much is due to the effects of unobserved heterogeneity. In taking this approach, our motivation is similar to that of Fan and Zhao (2009), though there are several important differences. First, we use the 1992 through 2006 waves of the Health and Retirement Study (HRS), giving us the most recent and comprehensive data available on the health and wealth of older U.S. households. Second, we explore the effects of unobserved heterogeneity in detail, comparing the effects of health across random effects, correlated random effects, and censored fixed effects specifications. We begin by replicating the random effects specification of earlier studies, finding a relatively strong correlation between health and portfolio choice. We then investigate the effects of unobserved heterogeneity by estimating correlated random effects specifications (in which the random effect is allowed

[1] In related work, Michaud and van Soest (2008) find no evidence of reverse causality between health and wealth among older households in the Health and Retirement Study (HRS).

to depend on observed variables) and censored fixed effects models. We find that most of the effects of health on portfolio allocation disappear in these more detailed specifications. We conclude that controlling adequately for unobserved heterogeneity generally eliminates the connection between health and portfolio choice among single households. For married households, we find a small negative effect (about 2–3 percentage points) from being in the lowest of five self-reported health categories.

2 Data

We use the 1992 to 2006 waves of the Health and Retirement Study (HRS), which provides the most recent and comprehensive longitudinal data on health status and household portfolios of older U.S. households.[2] The HRS began in 1992 by surveying households aged 51 to 61. In addition to re-interviewing those households every two years, the survey merged with a similar survey of households aged 70 and older, and subsequent waves added households from different cohorts. By the 1998 wave, the HRS represented all U.S. households over age 50. Pooling 19,011 households across the eight waves, we construct a sample of 98,318 household-years. Longitudinally, the panel is not balanced—about 63 percent of our households first entered after 1992, and about 45 percent dropped out before 2006. Fortunately, however, a balanced panel is not necessary for our purposes—our empirical strategy effectively identifies the relationship between health and portfolio choice by correlating changes in portfolio with changes in health, and as long as we observe sufficient longitudinal variation in these variables, a balanced panel is not required. Moreover, we have no reason to suspect that households who exit the sample early would have systematically different portfolio responses to health changes than surviving households.[3]

2.1 Wealth Measures

In our study, we define total wealth as the sum of nonfinancial wealth (real estate, businesses, and vehicles), retirement wealth (IRAs and defined contribution accounts including 401(k)s), and other financial wealth (including checking, savings, money market, CDs, bonds, stocks, mutual funds, annuities, and trusts). We define "stocks" as those held directly or in mutual funds and trusts, excluding stocks held in retirement accounts such as IRAs and 401(k)s. While many households own stocks in retirement accounts (particularly younger households), we focus on the narrower definition (excluding retirement accounts)

[2]Specifically, we use the RAND HRS, which provides longitudinally linked HRS records with imputations for missing data. See St.Clair (2008) for details.

[3]Since our identification strategy relies on correlating health changes to portfolio changes, survivor bias is not a major concern unless we believe the portfolio response to health changes would be different (in particular, larger) among the group that drops out. While it may be the case that attriting households have lower average wealth than surviving households, we have no evidence that they have larger portfolio responses to health shocks.

because it has less measurement error and thus offers a cleaner test of the relationship between health and portfolio choice.

Table 1 shows that wealth varies substantially by age and marital status. Median total wealth ranges from about \$70,000 among younger, single households to about \$288,000 among older, married households. When reporting on wealth data, medians are helpful because wealth distributions are typically very heavily skewed, resulting in large means relative to medians. However, medians cannot be decomposed into components that sum to the total. To get around this problem, we select a truncated sub-sample of households such that the mean of the sub-sample matches the median of the original sample. We then use means to decompose the sub-sample. This approach allows us to estimate the breakdown of median wealth into its components. Nonfinancial wealth is the largest component of wealth for all age and marital status groups. For younger households, retirement accounts represent the next largest source of wealth, while for older households non-retirement financial wealth is the second-largest component. As shown on the bottom line, median stock wealth varies from about \$4,000 among younger, single households to about \$29,000 for older, married households.

The second panel of the table shows how stock ownership and portfolio allocation vary across the age and marital status groups. The share of households holding stocks depends much more on marital status than age. About 21 to 23 percent of single households own stocks compared with about 38 to 39 percent of married households. A similar pattern emerges when looking at stocks as a share of financial assets: stocks are about 16 percent of financial assets among single households of any age, while they make up about 23 percent of financial assets among married households of any age. Safe assets—defined as checking, savings, and money market accounts, CDs, savings bonds and Treasury bills—make up the bulk of financial assets, particularly for single households.

2.2 Health Measures

We explore multiple definitions of health status. The HRS asks respondents (and spouses, if married) to self-report their health status, using the categories "excellent," "very good," "good," "fair," or "poor." This is the basis of the health variable used by many previous studies, including Rosen and Wu. For married couples, we categorize the household according to the less-healthy spouse. As shown in the bottom panel of Table 1, self-reported health status varies considerably across age and marital status groups. Among younger, single households, 41 percent report excellent or very good health, while among older, married households, only 21 percent report this level of health. Not surprisingly, self-reported health declines with age, and is lower among married couples (who are at "double jeopardy" of bad health outcomes according to our categorization).

As a less subjective measure of health, the HRS also asks respondents and spouses to report whether they have been diagnosed by a doctor with any of the following conditions:

high blood pressure, diabetes, cancer, lung disease, heart problems, stroke, psychiatric problems, and arthritis. For this measure, we categorize married couples by the spouse with the higher number of conditions.[4] As shown in the second panel of the table, this measure also varies significantly across age and marital status: about a quarter of younger, single households report no diagnosed conditions, while only three percent of older, married households fit this category.

In some specifications below, we also condition on the type of conditions reported. Some conditions are more acute, indicating immediate serious health threats, while others are more chronic, suggesting ongoing problems which may be serious but are perhaps less of an immediate health threat. We follow Coile and Milligan (2009) in categorizing heart problems, cancer, and stroke as acute conditions, and high blood pressure, diabetes, lung disease, psychiatric problems, and arthritis as chronic conditions. The results in the bottom panel of Table 1 indicate that households are more likely to experience chronic conditions than acute conditions, and again, that older and married households are more likely to report both types of conditions than younger, single households.

Our final measure of health status is annual out-of-pocket expenditure on medical care. This includes uninsured costs related to doctor visits, outpatient surgery, hospital and nursing home stays, prescription drugs, home health care, and special medical facilities or services.[5] For married couples, we categorize based on the higher of the spouses' medical expenses. As is well-known, the cross-sectional distribution of medical expenses is characterized by a very long upper tail (see e.g., French and Jones, 2004).[6] In our regression analysis, we therefore use the natural logarithm of OOP expenses. Medical expenses are different from the other health measures in that they are potentially more endogenous—they are likely to reflect a household's propensity to spend on discretionary medical care as well as underlying health status. We include them because they are an important theoretical channel of causal health effects; however, their potential endogeneity highlights the importance of accounting adequately for unobserved preference and attitudinal differences across households.

In most of the regression specifications below, we focus on the self-reported health measures for two reasons: to maintain comparability with previous studies, and because there is considerable information in how a household subjectively describes its health.[7]

[4] As with our definition of self-reported health, the rationale is to identify the presence of an unhealthy individual, rather than represent an average measure of health for the household or a sum total of conditions in the household.

[5] This variable is imputed by RAND in many cases. The imputed share ranges from 15% in 2000 to 100% in 1992. RAND imputes each component of costs separately. The imputation model predictors are age, age-squared, education, subjective health status, gender, marital status, race, whether an individual has any health insurance, whether an individual reported a hospital or nursing home stay, number of doctor visits, and whether the hospital, nursing, or doctor visit data are missing. See St.Clair (2008) for details.

[6] For example, in our HRS sample, while median OOP expenses are about \$1,300 over a two-year period, the 90th percentile is about \$7,300 and the 99th percentile is about \$38,000 for singles.

[7] For example, two households could receive the same diagnosis but report different effects on their self-report of health—either because the conditions are of varying severity, or because of differences in attitude

Nonetheless, as described below, we also estimate our regressions using doctor-diagnosed conditions as the measure of health, and our findings are very similar.

2.3 Descriptive Analysis

Table 2 shows stock ownership and allocation by our various health measures. There is clearly a positive empirical correlation between health and stock holding. As shown in the top panel, households in the top self-rated health group are about twice as likely to own stock as those in the lowest health group. Similarly, their stock share of financial assets is about twice as high as their less-healthy peers. The pattern is similar when looking at doctor diagnosed conditions (second panel), though the differences are not as large. The type of condition (acute or chronic) does not appear to matter with respect to portfolio allocation (third panel). Looking at out-of-pocket expenses, we see a different pattern: households in the highest spending group own more stock than households with less medical spending. The difference between the OOP pattern and the other health measures suggests that OOP may measure something quite different from health status—perhaps the ability or willingness to pay for medical care, or a preference for a greater amount or higher quality care.

The strong cross-sectional correlations between health status and stock-holding illustrated in Table 2 suggest an effect of health on portfolio choice. But the cross-sectional correlation could be deceiving if unobserved factors were actually driving both variables. Stock-holding is correlated with wealth, for example, and wealth is correlated with health status—not just because health shocks might reduce wealth, but because less healthy households are likely to be different than more healthy households in a number of ways, including unobserved factors such as risk aversion, impatience, information, and motivation.

One way to try to distinguish between causal health effects and correlation due to unobserved differences is to investigate whether *changes* in health lead to observable changes in portfolio allocation. This approach is similar in spirit to the event studies of Coile and Milligan (2009) and is effectively the strategy utilized by our regression specifications below. In order to identify health effects with this strategy, however, we need to observe sufficient longitudinal variation in health status and portfolio choice.

Table 3 shows empirical health transition matrices by marital status. The matrices show a relatively large amount of mobility across self-reported health statuses—particularly in the direction of declining health. For both married and single households, the least "stable" health states are the highest (e.g., excellent), while the most stable are the lowest (e.g., poor). In any case, it is clear that a fairly large number of transitions are observed. Indeed, some kind of health transition is observed for about half of households (not shown in the table), and a quarter experience two or more transitions.

toward the health shock (e.g., "stoic" vs. "vulnerable"). Similarly, in the spirit of the psychological literature on happiness (see, e.g., Gilbert 2006), individuals might adapt to income and health shocks over time, "resetting" to some accustomed level of well-being.

In a similar spirit, we can also compute the stock-owning matrices by marital status (not shown in the table). In this case, we are looking at transitions across the extensive margin—that is, households moving from owning stocks to not owning stocks, or vice versa. Naturally, this measure understates actual stock-holding changes because it ignores movements along the intensive margin. Nonetheless, it provides a simple, lower-bound metric for measuring stock changes. We find that the probability of exiting stock-holding status is about 23 percent among single households and about 27 percent among married households, while the probabilities of entering stock-holding are about 17 percent and 11 percent among singles and married couples, respectively. Overall, a stock-owning transition is observed for about 16 percent of single households and 25 percent of married households.

2.4 Controls

The regressions relate stock ownership and allocation to our health measures and other covariates. Controls include wealth, income, age, age squared, spouse's age, spouse's age squared, sex, education, race, and other household demographics. In models such as these there is often some concern about endogeneity since unobserved factors affecting stock holding and health can also affect wealth and income. To help address this issue, we first control separately for financial wealth, nonfinancial wealth, and income.[8] We treat these separately because the relationship between health and wealth could vary across types of wealth—indeed, Berkowitz and Qiu (2006) find that health changes affect financial wealth more than nonfinancial wealth, and that the effect of health on portfolio choice seems to disappear when differences in financial wealth are controlled for. Second, because married households may respond differently to health shocks than single households, we split the sample by martial status. Finally, and most importantly, we explicitly account for unobserved heterogeneity using correlated random effects models and censored fixed effects models, as described in the section below.

To control for the role of expected bequests in household portfolio decisions, we also include the subjective probability that the household will leave an inheritance of at least $100,000. About 54 percent of singles report a probability less than 20 percent of leaving such a bequest, 15 percent report a probability between 20 and 80 percent, and 31 percent report a probability higher than 80 percent. Couples are more likely to report a good chance of a bequest, with about a third reporting a low probability and just under half reporting a high probability.

Preference constitute an important dimension of heterogeneity across households. The literature on household portfolio choice highlights the central roles of risk aversion and planning horizon for allocation decisions. While preferences regarding risk and planning can only be imperfectly proxied, we include a couple of measures that may help control for

[8]Following Hochguertel (2003), we transform the wealth and income variables using the log odd function: $f(x) = \ln(x+1)$ if $x > 0$, and $f(x) = -\ln(-x+1)$ if $x \leq 0$.

preference heterogeneity across households. The first is an indicator variable for whether the mode across waves of the HRS financial respondent's categorical risk aversion (based on the HRS set of "income gamble" questions) is in the top two categories out of four possible. The second is an indicator variable for whether the financial respondent reported having a planning horizon longer than 5 years.

Since the effect of health on household decisions may depend critically on health insurance coverage, particularly for younger households who do not yet qualify for Medicare, we include a control for whether each member of the household has some form of health insurance (government, employer, or other). While about 98 percent of older households are insured, the figure drops to about 85 percent among younger households.

A concern pertaining to all studies of health and wealth is the possibility that causality might run in either direction. If wealth gains improve health, either through improving access to health care or through some other channel, a causal interpretation of the estimates on our health variables would be problematic. A recent study (Michaud and van Soest, 2008), however, tests for reverse causality using dynamic panel data models that control for unobserved heterogeneity and finds no evidence that *changes* in wealth drive changes in health. If wealth changes do not influence health, there is little reason to expect that portfolio changes would have an independent effect on health.

Finally, we note that, as in any panel study of wealth, we cannot separately identify cohort effects, age effects and time effects. Longitudinal wealth patterns presumably reflect all three effects, but panel methods only allow us to control for any two.[9] Most papers in the literature control for age and time effects, which are considered significant due to the first-order importance of life-cycle profiles and business cycle variations, respectively. We follow a similar approach—in our regressions, we include a quadratic in age and a full set of year dummies.

3 Estimation Strategy

Our empirical approach exploits the panel nature of the data to account for unobserved household-specific effects that might influence both portfolio choice and health status. Following earlier studies, we estimate two sets of models: first, binary response models that identify the effect of health on the extensive margin of stock ownership, and second, censored regression models that identify the effect of health on the intensive margin of stock allocation as a share of financial assets. The allocation models are censored because a significant share of households hold zero stocks. Each set (stock ownership and stock allocation) is made up of three model specifications: a random effects (RE) specification, a correlated random

[9]In our data, mean real financial wealth (in 2006 dollars) rises (non-monotonically) from about $74,000 in 1992 to about $147,000 in 2006. We attribute yearly variations (e.g., a decrease of about $13,000 in 2002, followed by an increase of about $30,000 in 2004) to movements in asset markets—e.g., from 2000 to 2002, stock markets fell significantly, while from 2002 to 2004 both stock markets and housing markets grew appreciably.

effects (CRE) specification, and a fixed effects (FE) specification. In order to benchmark our results to earlier studies, we begin with a random effects specification. We then apply a correlated random effects approach and a fixed effects estimator to test the robustness of the health effect to a careful treatment of unobserved heterogeneity.

3.1 Random Effects Logits and Tobits

We begin with a random effects logit model for stock ownership and a random effects Tobit model for marginal allocation. We focus on logits rather than probits in the ownership regressions for the sake of comparison across the three specifications (RE, CRE and FE).[10] As a robustness check, we also estimate the RE and CRE models using a probit specification, and find that the coefficient estimates and average partial effects closely match those in the logit specifications.

Underlying all of the specifications is a latent variable model of the form:

$$y_{it}^* = \mathbf{x}_{it}\beta + c_i + u_{it}, \tag{1}$$

where $\mathbf{x}_{it}$ is a vector of exogenous explanatory variables, c_i is an unobserved, time-invariant, household-specific effect, and u_{it} is an idiosyncratic error. In the ownership equations, the dependent variable y_{it} is an indicator for holding positive amounts of risky assets, such that $y_{it} = 1$ if $y_{it}^* > 0$. In the allocation regressions, y_{it}^* represents the desired portfolio share, but the observed risky asset shares lie between 0 and 1, so that $y_{it} = \max\{0, \min\{y_{it}^*, 1\}\}$.

It can be seen from the specification above that failing to account for unobserved effects will generally lead to inconsistent coefficient estimates unless the omitted variables are perfectly uncorrelated with any of the independent variables in the regression. The inconsistency comes from a violation of the orthogonality assumption applied to the composite error in the latent variable model above. In a pooled regression, $E(\mathbf{x'}_{it}|(c_i + u_{it}))$ will generally be nonzero if the unobserved factor c_i is correlated with the observables $\mathbf{x'}_{it}$. The standard random effects models obtain coefficient estimates by integrating the unobserved factor c_i out of the likelihood function. However, consistency requires that $E(c_i \mid \mathbf{x}_{i1}, ..., \mathbf{x}_{iT}) = E(c_i) = 0$, which is violated whenever there is correlation between c_i and any of the $\mathbf{x}_{it}$'s. That is, the random effects estimator is only consistent in the special case that the unobserved effect is uncorrelated with the observables—which is unlikely in this context because key unobserved parameters such as risk preference, impatience, and longevity are likely to be correlated with education, wealth, and other observables. To some extent, we can control for these factors by including proxy variables, such as subjective survival probabilities, but these are at best imperfectly measured, and the omitted variables problem remains. A more promising approach would be to attempt to systematically account for the correlation

[10] RE and CRE models can be estimated with either a probit or a logit specification, while the conditional fixed-effect logit model discussed below requires a logit specification.

between the unobserved random effect and the observables (the CRE approach), or better yet, to difference out the individual effects via a fixed effects strategy.[11]

3.2 Correlated Random Effects Logits and Tobits

Our next step will be to move beyond earlier studies by specifically addressing the correlated unobservables problem. First we systematically account for the potential correlation between unobserved and observed variables using a CRE specification in the spirit of Chamberlain (1984) and Mundlak (1978).[12] The CRE approach allows the unobserved household-specific effect c_i to depend on observed characteristics:

$$c_i = \Psi + \bar{\mathbf{x}}_i \lambda + a_i, \;\; a_i \mid \mathbf{x_i} \sim N(0, \sigma_a^2),$$

where the vector $\bar{\mathbf{x}}_i$ contains the means of the time-varying regressors, Ψ is a constant, and a_i is the independent portion of the individual effect.[13]

If health status is associated with stock-holding in the CRE specification, we will have stronger evidence that the correlation is indicative of a causal effect of health, rather than unobserved heterogeneity. Note that the CRE model imposes a linear relationship between unobservables and observables and requires the conditional variance of the unobserved effect to be constant. If these conditions do not hold, our omitted variable problem could remain. Thus, our final specification attempts to solve the problem more completely by differencing out individual effects altogether using a fixed effect approach.

3.3 Conditional Fixed Effect Logit and Censoring Models

In general, fixed effect differencing cannot be applied to nonlinear models such as ours, because the nonlinearity implies that differencing would not remove the individual effect. However, Chamberlain (1980) showed that in the binary choice case, a logit specification in which the likelihood function is conditioned on the number of observations with $y_{it} = 1$ can be constructed in a way that effectively removes unobserved heterogeneity from the choice probabilities. This estimator, called the conditional fixed effect logit estimator, can be used to obtain fixed effect estimates from longitudinal binary choice data, such as stock ownership.

Looking at stock allocation (i.e., the share of financial assets held in stock), we face the usual problem that a large number of households are censored at zero.[14] Honoré (1992)

[11]Another strategy to account for the omitted variables problem, pursued by Edwards (2008), would be an instrumental variables approach. The challenge with this approach is finding instruments that meet the exogeneity requirement—variables that are strongly correlated with health status but not portfolio choice.

[12]For recent applications of this model to portfolio choice, see Bakija (2004) and van Soest and Kapteyn (2006). Wooldridge (2002) provides a detailed discussion of the CRE approach.

[13]An alternative specification would allow c_i to depend on the time-varying x_{it}'s, rather than just their means. The motivation for our specification is that it economizes on degrees of freedom.

[14]The stock share is also censored at 100% (ignoring leverage), but this is rarely binding in practice. In

develops a censored fixed effects (CFE) specification that can be used in this situation.[15] The CFE technique produces consistent estimates of a censored regression model with fixed effects, and with minimal restrictions on the distribution of the error term (e.g., it need not be normal). However, the differencing out of the fixed effect means that marginal effects cannot be computed.[16] Nevertheless, this estimator provides a very useful way to test the robustness of the relationship between health and portfolio allocation after removing the effects of unobserved heterogeneity.[17] Further, as we will see in the results section, the coefficient estimates in the fixed effects specification are quite close to those in the correlated random effects specification, from which we *can* obtain marginal effects.

4 Estimation Results

4.1 Parsimonious Specifications

We begin with relatively parsimonious specifications that control for only a limited set of demographic covariates. We start here in order to get a sense of the overall impact of health on portfolio decisions before controlling for correlated sources of unobserved heterogeneity and without "controlling away" some of the channels of influence. If health appears unimportant even before we control for unobserved heterogeneity and indirect financial channels in the spirit of Berkowitz and Qiu (2006), then one might wonder whether the more detailed investigations of unobserved heterogeneity are warranted. Moreover, we may be interested in the "total correlation" between health and portfolio choice, including through any indirect channels such as effects on financial wealth, income, and expected bequests. In this sense, our parsimonious specification is similar to the event studies of Coile and Milligan (2009), who look at the overall effects of health shocks on portfolio allocation.

Table 4 presents coefficient estimates and selected average partial effects for the parsimonious stock ownership regressions, including the RE, CRE, and conditional fixed effects logit specifications.[18] The RE and CRE regressions include a full set of year dummies, as

our data, the stock share is zero for 67 percent of the sample, between 0% and 100% for 32 percent, and clustered at 100% for less than 1 percent.

[15] Honoré and Leth-Petersen (2006) generalize the one-sided least absolute deviation estimator in Honoré (1992) to handle the case of two-sided censoring. See Alan and Leth-Petersen (2006) and Hochguertel (2003) for applications to portfolio models.

[16] The marginal effects depend on the unobserved fixed effects, but Honoré's estimator strips these away and estimates the coefficients using only time variation in the regressors. As Honoré (2008) discusses, it is possible to recover interesting marginal effects from the censored fixed effects regression even when the unobservables might be correlated with the independent variables. In general, however, the marginal effects would not be directly comparable to those in our random effects and correlated random effects specifications.

[17] We implement Honoré's one-sided censored estimator using Gauss code available at his website: `http://www.princeton.edu/~honore/programs/pantob`.

[18] The reported standard errors are not adjusted for clustering because there are currently no implementations of cluster-adjusted standard errors in Stata's xtlogit, xtprobit, and xttobit commands. Nevertheless, to investigate the potential bias induced by serial correlation, we estimated linear panel regressions for each of our random effects, correlated random effects, and fixed effects specifications and compared standard errors both with and without adjustments for clustering. Although the standard errors were higher when we adjust

well as age, age squared, gender, and indicators for retirement, the presence of children, high school degree, college degree, and race/ethnicity. To conserve on space, we report only the estimates for age, age squared, and retirement.

The first column of Table 4, showing the RE estimates, strongly supports a significant connection between health and stock ownership. The average partial effects (which are similar in magnitude to the marginal effects, not shown) indicate that a self-reported health status of "poor," the worst health category, is associated with a reduction in the ownership probability of about 7 percentage points for singles and about 17 percentage points for married couples. Similarly, "fair" health, the second-worst category, is associated with a reduction of 5 percentage points and 10 percentage points, respectively. If these values represent the true underlying effects, health would appear to be a key determinant of stock ownership, with declines in health leading to less risky allocations.

But a key finding is that even in the parsimonious specification, most of the health effect disappears in the CRE and FE specifications. That is, adequately accounting for unobserved heterogeneity substantially reduces the correlation between health status and portfolio choice. Hausman tests clearly reject the RE specification. In the CRE and FE specifications the average partial effects are essentially zero for singles, and much smaller in absolute value for married couples. However, among married couples, we do find a link between health and stock ownership for households in the lowest health category. The average partial effect of "poor" health (relative to "excellent" health) for married couples is about -7 percentage points in the CRE model. Recall that the FE model does not allow the calculation of marginal effects; however, the FE coefficient on "poor" health is a bit smaller than the CRE coefficient.

The analogous stock allocation regressions, reported in Table 5, tell a similar story. The marginal effects in the baseline RE Tobit specification indicate that "poor" health is associated with a statistically significant reduction in the stock share of about 4 percentage points for singles and 7 percentage points for married couples relative to the omitted category of "excellent" health. The connection is much weaker, however, once we control for unobserved heterogeneity in the CRE Tobit and CFE specifications. Again the only economically and statistically significant result is for married couples in the lowest health category, for whom the average partial effect is about 3 percentage points in the CRE specification. The CFE coefficient is a bit larger than the CRE coefficient.

As mentioned above, we also estimate our regressions using doctor diagnosed conditions as the health measure. In order to save space, we do not report the table, but the results are broadly similar. In the ownership regressions, we again find that the CRE and FE specifications result in substantially smaller health coefficients than the RE specification.

for clustering, the differences in the magnitudes were very small, on the order of 2–3 percent. While this does not constitute a formal test, we do not believe that serial correlation is likely to be an important factor in our analysis. In addition, since our results generally suggest that health does *not* affect portfolio choice, we are, if anything, stacking the deck against our result by reporting unadjusted standard errors.

For example, the average partial effect on chronic health conditions in the RE specification is -0.0289 (s.e. 0.0063) for singles and -0.0509 (s.e. 0.0096) for married couples. In the CRE specification, the analogous partial effects are -0.0168 (s.e. 0.0072) for singles and -0.0362 (s.e. 0.0105) for married couples. The average partial effects on acute conditions show a similar pattern. Thus, the results from these alternative regressions indicate a relatively small effect of health on stock ownership. The allocation regressions indicate an even smaller health effect: all of the marginal effects on chronic and acute conditions in the CRE Tobit specifications are smaller than 1 percentage point in absolute value.

What we learn from the parsimonious specification is that even allowing for indirect effects of health on portfolio choice (i.e, through financial wealth, as in Berkowitz and Qiu (2006)), the only evidence of health effects once we adequately account for unobserved heterogeneity is among married households in the lowest health category. For this group, the average partial effect on stock allocation is about -3 percentage points.

4.2 Full Specification of Stock Ownership

Table 6 presents the results from the full specifications of stock ownership for singles and married couples. In this specification we add controls for out-of-pocket medical costs, the presence of health insurance (interacted with age>=65), the probability of leaving a bequest, financial and non-financial assets, income, and whether the respondent is retired. We find that financial wealth, income, and education (not shown) are all strongly and positively associated with stock ownership, consistent with previous studies and likely reflecting factors such as financial and/or informational barriers to entry in asset markets. In addition, the RE specification indicates that households expecting to leave a bequest are somewhat more likely to own stock (this effect largely disappears in the CRE and FE specifications).

In the RE specification, "poor" health is again negatively associated with stock ownership, but the magnitudes are significantly lower than those in the parsimonious regressions above. That is, differences in financial resources and expectations across and within households appear to explain much of the connection between self-reported health and the decision to hold stock.

For purposes of comparison with Rosen and Wu (2004), we also estimate regressions using an indicator of fair or poor self-reported health, and obtain very similar results.[19] We also find that higher OOP medical expenses are associated with higher probabilities of stock ownership, which is consistent with our descriptive evidence that suggested OOP expenses might have discretionary or "luxury good" aspects.

In the CRE specification, the magnitudes on the health variables are smaller in absolute value, and none of the estimates is statistically significant.[20] Wald joint-significance tests

[19]In our RE specification, the average partial effect is about -1 percentage point for singles and -2 percentage points for married couples.

[20]For brevity, we report only the slope coefficients on the regressors of interest and not the estimated correlations between the unobservables and the independent variables. It is worth noting, however, that

on the four health indicators, reported at the bottom of the table, strongly reject the importance of self-reported health at the 5-percent level. In addition, the estimates on out-of-pocket medical costs fall by half to a third and lose statistical significance.

The FE results in the right-hand panel of the table show roughly similar results—most of the health coefficients are smaller than in the RE specification, and statistically insignificant. However, in the FE specification, married households in the lowest health group show a small, statistically significant difference, relative to those in the top health group. These results again suggest that much of the correlation between health and portfolio choice—particularly for singles—is due to unobserved heterogeneity across households, rather than a causal link. Among married households in the worst health, however, we find a small health effect.

One caveat is that both the FE and CRE estimates may be sensitive to measurement error, particularly if there is comparatively limited "within" variation (Bound, Brown, and Mathiowetz, 2001). In our case, the effect of health on ownership is identified by changes in health and stock ownership over our 12-year sample period. We observe a reasonable number of transitions in and out of stock ownership; nonetheless, the cautious interpretation of our results is as evidence against evidence—we find little support for a significant causal channel running from health to stock ownership.

4.3 Full Specification of Stock Allocation

Table 7 shows the results from our full specifications of stock allocation. Again, these specifications include controls for out-of-pocket medical costs, the presence of health insurance (interacted with age>=65), the probability of leaving a bequest, financial and non-financial assets, income, and whether the respondent is retired. The left-hand panel reports the results for our baseline RE Tobit regression of stock allocation. In this specification we find a clear and precisely measured relationship between health status and portfolio allocation for married couples, but only a small effect for singles. The marginal effects indicate that "poor" health is associated with a 3-percentage-point decline in the stock share of married couples, even after controlling for indirect causal channels of health, such as assets, income, out-of-pocket medical expenses, and expected bequests. "Fair" health also exhibits a significantly negative effect of about a 1.5-percentage-point decline for married couples. For comparison purposes, we also estimated the regression using the Rosen and Wu indicator for being in "fair" or "poor" health and found a marginal effect of about -1.1 percentage points.

In contrast to the negative effects on our self-reported health variables, we find a positive relationship between OOP expenses and the stock share, particularly among single

many of the latter estimates share the signs and significance levels of the estimates in the RE regression, suggesting that the random effects and observables are indeed correlated—a significant violation of a key assumption in the standard RE specification that results in inconsistent estimates.

households. A mechanical explanation for this relationship is that some households may be reluctant to finance out-of-pocket expenses out of stocks and choose instead to pay for them out of more liquid assets. In this case, the share of stock could rise even if the total value of stocks remains unchanged. In addition, this result might be related to the discretionary nature of some medical costs: if households can choose different levels of medical care, high out-of-pocket medical expenses might be correlated with unobserved shocks to future income and non-medical expenses.[21]

But again, we cannot impose a causal interpretation on these results if we suspect correlation between the unobserved random effect and observables. In the CRE Tobit specification, shown in the middle panel of Table 7, there is a sharp decrease in the absolute value of the health coefficients among married households (the results for singles remain small and statistically insignificant). In this specification, only "poor" health exhibits a statistically significant effect, and its marginal effect is a relatively modest 1.8 percentage points—about half as large in absolute value as in the RE specification. In addition, the strong, positive coefficient estimates on out-of-pocket expenses found in the baseline random effects specification vanish once we move to the CRE Tobit and CFE regressions. Thus, we again find that much of the apparent correlation between health and stock share seems to be due to unobserved heterogeneity rather than to represent a significant causal link.

As a robustness check, we also estimated all of our regressions using the alternative measure of acute and chronic health conditions, described in Section 2.2. Table 8 reports the coefficient estimates and marginal effects for the health variables of interest. The average partial effects in the ownership regressions indicate that the presence of acute or chronic conditions each reduce the probability of stock ownership by about 1 percentage point. In the allocation regressions, however, health conditions appear to have no statistically significant impact on the share of risky assets.

One interpretation of our results is that while health may be useful for explaining cross-sectional differences across households, it does not exert a strong causal influence on portfolio changes over time. An alternative explanation is that health changes do matter for portfolio choice, but that the effect is not contemporaneous with the change in health status. For example, individuals currently in poor (or good) health may have adjusted their portfolios the moment—potentially years in the past—that they learned of their expected health outcomes. Alternatively, it may take households a long time to adjust to health shocks. For these stories to be materially affecting our results, however, household expectations or lagged responses would have to take place more than two years in advance of, or following, the health shock (since the HRS waves are two years apart). Nevertheless,

[21] A related issue concerns accidental portfolio re-balancing due to fluctuations in the stock market. Because of trading costs and other frictions, households may not continuously re-balance their portfolios in response to market fluctuations (see, e.g., Liu (2004)). Nevertheless, as long as health shocks cause *some* households to hit their decision threshold, we should still be able to pick up such a relationship in our empirical estimates.

as a robustness check, we also estimated our regressions with lagged values of our health measures and found that the lags made very little difference in our results.

5 Conclusion

We test whether the relationship between health and portfolio choice persists after accounting carefully for the effects of unobserved heterogeneity, and we conclude that for the most part, it does not. Once we account for unobserved effects through a correlated random effects model or a fixed effects estimator, the estimates on the health variables are generally small and statistically insignificant. Among single households, we find no evidence that health exhibits a causal effect on either the extensive margin of stock ownership or on the intensive margin of asset allocation. For married households, we find a small effect (about 2–3 percentage points) from being in the lowest of five self-reported health categories.

One explanation for our results is that the causal effect of health changes on portfolio choice is simply very small. However, there are other possible explanations, including measurement error in our health variables, the role of expectations or long time lags in responses, and heterogeneity across similar households in the relationship between health and portfolios. Finally, our results could be reflecting the ambiguous theoretical relationship between health and portfolio choice. The theoretical effect of health on allocation depends on how health affects the marginal utility of consumption. Since this derivative can plausibly take either a positive or a negative sign, the net effect of health is ambiguous—some households might respond to worsening health by increasing their stock share, while others might move toward safer assets. Or, if the opposing forces of health on desired consumption affect each household's utility in the same way, our finding could reflect genuine ambivalence at the household level.

No matter what the interpretation, though, our findings indicate that the empirical relationship between health and portfolio choice is significantly less clear than some previous studies suggest. If such a causal relationship exists, it appears to operate largely for married households in the worst health, and with a relatively small overall effect.

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Table 1: Summary of Wealth and Health

Variable	Single <65	Single ≥65	Married <65	Married ≥65
Median Wealth, 1000s of 2006 Dollars				
Total Wealth	70	94	258	288
Nonfinancial	48	61	157	169
Retirement	12	6	58	40
Financial	9	27	43	78
Stocks	4	7	18	29
Ownership and Allocation				
Owns Any Stocks	.21	.23	.38	.39
Mean portfolio share				
Stocks	.16	.16	.23	.23
Bonds	.01	.02	.02	.03
Safe Assets	.74	.78	.65	.68
Other Assets	.09	.04	.10	.06
Health				
Self-reported Health Status				
Excellent or Very Good	0.41	0.32	0.34	0.21
Good	0.28	0.32	0.36	0.35
Fair or Poor	0.31	0.36	0.30	0.44
Number of Diagnosed Conditions				
None	0.26	0.11	0.14	0.03
One to Three	0.64	0.71	0.76	0.74
Four or More	0.10	0.18	0.10	0.23
Type of Diagnosed Conditions				
Acute	0.23	0.46	0.34	0.65
Chronic	0.70	0.86	0.80	0.95
Out of Pocket Medical Expenses	1,408	1,667	2,134	2,509

Pooled sample of HRS households from 1992 to 2006, representing 98,318 household-years and weighted using the HRS sampling weights. For median wealth, means are taken on subsets selected so that the mean total wealth of the subset equals the median total wealth of the original sample. Nonfinancial wealth includes real estate, businesses, and vehicles. Retirement wealth includes IRAs and DC accounts. Financial wealth sums checking, savings, money market, CDs, bonds, stocks, mutual funds, annuities, and trusts. Stocks includes stock held directly or in mutual funds or trusts, but excludes stocks in ret. accounts. Diagnosed conditions number 0–8 of the following conditions: high blood pressure, diabetes, cancer, lung disease, heart problems, stroke, psychiatric problems, arthritis. Acute conditions include heart problems, cancer, and stroke. Chronic conditions include high blood pressure, diabetes, lung disease, psychiatric problems, and arthritis. Acute and chronic conditions are conditional on having at least one diagnosed condition (households can have none, one, or both of these types). Annual out-of-pocket medical expenses, in real 2006 dollars.

Table 2: Stock Ownership and Allocation, by Health Status

	Percent			
	Owning		Allocation	
Variable	Single	Married	Single	Married
1. Self-reported Health Status				
Excellent or Very Good	31	51	20	30
Good	22	40	15	23
Fair or Poor	13	26	11	17
2. Number of Diagnosed Conditions				
None	26	43	17	25
One to Three	23	39	16	24
Four or More	17	32	13	20
3. Type of Diagnosed Conditions				
Acute	21	37	15	22
Chronic	21	37	15	23
4. Out of Pocket Medical Expenses				
Lowest Third	14	35	11	22
Middle Third	26	40	17	24
Highest Third	27	39	18	24

The table presents stock ownership fractions and allocation percentages from a pooled sample of HRS households from 1992 to 2006, representing 98,318 household-years. Means calculated using HRS sample weights. The distribution of out-of-pocket medical costs is conditional on marital-age group.

Table 3: Health Status Transition Matrices

Prior Wave	*Current Wave*				
	Married Couples				
	Excellent	Very good	Good	Fair	Poor
Excellent	36.52	42.53	16.54	3.54	0.87
Very good	6.67	49.89	34.34	7.50	1.60
Good	1.27	17.88	54.75	21.48	4.62
Fair	0.32	4.48	25.79	52.08	17.33
Poor	0.31	1.27	6.98	31.35	60.10
Total	4.02	21.49	35.96	25.83	12.69
	Singles				
	Excellent	Very good	Good	Fair	Poor
Excellent	45.45	32.25	15.85	4.55	1.90
Very good	10.64	47.45	29.87	8.98	3.06
Good	3.72	19.84	47.98	22.64	5.82
Fair	1.31	6.57	23.72	49.94	18.45
Poor	0.84	3.30	9.87	30.06	55.93
Total	8.86	22.58	30.11	24.71	13.74

This table reports the transition matrix for the self-reported health measure in the 1992–2006 waves of the HRS. For married couples, self-reported health is taken to be the worse health state of the respondent and the spouse.

Table 4: Stock Ownership Logits: Only Demographic Controls

	RE		CRE		FE	
Explanatory Variable	Single	Married	Single	Married	Single	Married
SRH = Very good	0.0412	-0.1746*	0.0864	-0.0832	0.1688*	-0.0989
	(0.0729)	(0.0807)	(0.1338)	(0.0854)	(0.0817)	(0.0869)
Good	-0.2318**	-0.4105***	0.0277	-0.1528	0.0897	-0.1744
	(0.0769)	(0.0835)	(0.1447)	(0.0903)	(0.0896)	(0.0924)
Fair	-0.4757***	-0.6039***	-0.2181	-0.1257	0.0856	-0.1275
	(0.0871)	(0.0902)	(0.1659)	(0.0990)	(0.1051)	(0.1020)
Poor	-0.7662***	-1.0338***	-0.0529	-0.3628**	0.0251	-0.3645**
	(0.1085)	(0.1044)	(0.2023)	(0.1165)	(0.1372)	(0.1226)
Average Partial Effects						
SRH = Very good	0.0045	-0.0290***	0.0146	-0.0149**	.	.
Good	-0.0240***	-0.0669***	0.0047	-0.0273***	.	.
Fair	-0.0466***	-0.0967***	-0.0367***	-0.0225***	.	.
Poor	-0.0702***	-0.1572***	-0.0089	-0.0647***	.	.
Statistics						
Rho	0.6939	0.6555	0.6935	0.6532	.	.
Observations	37962	43505	12971	43505	11689	19519
Log-likelihood value	-15135	-21001	-5661	-20862	-4395	-7456
Health vars $\chi^2(4)$	97.10	143.57	7.12	12.52	5.41	12.47
Mean health vars $\chi^2(4)$	.	.	135.24	243.90	.	.
Hausman (single): $\chi^2(4) = 114.57$. Hausman (married): $\chi^2(4) = 187.51$.						

The dependent variable is an indicator variable for whether the household owns a positive amount of stocks outside of retirement accounts. Standard errors are in parentheses below the estimates. All regressions include year dummies. To save room, we do not report selected controls (age, age squared, spouse's age, spouse's age squared, retired, gender, children, high school college, nonwhite, and Hispanic) or those of the mean time-varying independent variables. "Rho" is the variance share of unobserved heterogeneity. Wald joint-significance tests are included for the set of four health indicators, and the set of four mean health indicators in the CRE regressions. Hausman tests are restricted to the four SRH variables. Statistical significance (not reported for FE coefficients) is indicated as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 5: Stock Allocation Tobits: Only Demographic Controls

	RE		CRE		FE	
Explanatory Variable	Single	Married	Single	Married	Single	Married
SRH = Very good	0.0098	-0.0300*	0.0236	-0.0154	0.0542**	-0.0214
	(0.0130)	(0.0138)	(0.0243)	(0.0148)	(0.0179)	(0.0158)
Good	-0.0557***	-0.0716***	-0.0026	-0.0226	0.0288	-0.0262
	(0.0135)	(0.0142)	(0.0263)	(0.0157)	(0.0205)	(0.0170)
Fair	-0.1076***	-0.1172***	-0.0357	-0.0217	0.0282	-0.0234
	(0.0151)	(0.0154)	(0.0301)	(0.0174)	(0.0252)	(0.0194)
Poor	-0.1761***	-0.2150***	0.0139	-0.0765***	0.0431	-0.0950**
	(0.0187)	(0.0180)	(0.0364)	(0.0207)	(0.0345)	(0.0265)
Marginal Effects						
SRH = Very good	0.0027	-0.0112*	0.0071	-0.0058	.	.
Good	-0.0147***	-0.0268***	-0.0008	-0.0085	.	.
Fair	-0.0273***	-0.0426***	-0.0104	-0.0082	.	.
Poor	-0.0416***	-0.0721***	0.0041	-0.0278***	.	.
Statistics						
Rho	0.4650	0.5219	0.4916	0.5159	.	.
Observations	37962	43505	12971	43505	8660	8924
Log-likelihood value	-19530	-25936	-7227	-25792	.	.
Health vars $\chi^2(4)$	178.01	217.66	10.11	17.40	.	.
Mean health vars $\chi^2(4)$	.	.	169.79	291.71	.	.

The dependent variable is the financial wealth share of stocks held outside of retirement accounts. Standard errors are reported in parentheses below the coefficient estimates. To save room, we do not report selected controls (age, age squared, spouse's age, spouse's age squared, retired, gender, children, high school college, nonwhite, and Hispanic) or those of the mean time-varying independent variables. The marginal effects are evaluated at the means of the independent variables. "Rho" is the variance share of unobserved heterogeneity. Wald joint-significance tests are included for the set of four health indicators, and the set of four mean health indicators in the CRE regressions. Statistical significance is indicated as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: Stock Ownership Logits: Full Specification

	RE		**CRE**		**FE**	
Explanatory Variable	Single	Married	Single	Married	Single	Married
SRH = Very good	0.0050	-0.0701	0.0710	-0.0761	0.0442	-0.1030
	(0.0847)	(0.0900)	(0.1569)	(0.0973)	(0.0999)	(0.1004)
Good	-0.0509	-0.1992*	0.2145	-0.1171	0.0848	-0.1590
	(0.0898)	(0.0925)	(0.1717)	(0.1027)	(0.1118)	(0.1068)
Fair	-0.1167	-0.2696**	0.0066	-0.0659	0.1589	-0.0996
	(0.1042)	(0.1000)	(0.2006)	(0.1129)	(0.1364)	(0.1185)
Poor	-0.2021	-0.5057***	0.0773	-0.2525	0.0311	-0.3144*
	(0.1402)	(0.1168)	(0.2560)	(0.1336)	(0.1918)	(0.1443)
Log O.O.P. med costs	0.0661***	0.0281*	0.0204	0.0135	0.0270	0.0069
	(0.0119)	(0.0126)	(0.0236)	(0.0138)	(0.0148)	(0.0148)
Prob. leave beq	0.0033***	0.0025***	0.0026	0.0003	0.0010	0.0002
	(0.0007)	(0.0006)	(0.0014)	(0.0007)	(0.0009)	(0.0007)
Log HH fin. assets	1.1241***	1.1505***	1.0166***	1.0456***	0.8573***	0.9516***
	(0.0220)	(0.0180)	(0.0388)	(0.0191)	(0.0261)	(0.0207)
Log HH non-fin. assets	0.0404***	0.0102	0.0311	-0.0094	0.0137	-0.0084
	(0.0090)	(0.0102)	(0.0161)	(0.0103)	(0.0123)	(0.0123)
Log HH income	0.1547***	0.1255***	0.2009***	0.1017***	0.0931**	0.1093***
	(0.0290)	(0.0280)	(0.0522)	(0.0299)	(0.0339)	(0.0323)
Retired	-0.0877	0.0808	-0.0855	0.0352	-0.0964	0.0878
	(0.0681)	(0.0567)	(0.1305)	(0.0634)	(0.0882)	(0.0684)
Age	-0.0898*	-0.0793*	-0.0496	-0.0909	-0.2407*	0.0575
	(0.0371)	(0.0328)	(0.0773)	(0.0477)	(0.1069)	(0.0832)
Age sq./100	0.0578*	0.0499	0.0184	0.0569	0.1211**	0.0264
	(0.0254)	(0.0256)	(0.0574)	(0.0357)	(0.0393)	(0.0428)
Risk averse	-0.0271	-0.1049	-0.0930	-0.1464*	.	.
	(0.0917)	(0.0678)	(0.1472)	(0.0705)	.	.
Planner	0.2203*	0.1968**	0.0141	0.0926	.	.
	(0.1003)	(0.0695)	(0.1522)	(0.0710)	.	.
			Average Partial Effects			
SRH = Very good	0.0004	-0.0073**	0.0066	-0.0075**	.	.
Good	-0.0044	-0.0207***	0.0198***	-0.0116***	.	.
Fair	-0.0102*	-0.0279***	0.0006	-0.0065*	.	.
Poor	-0.0175**	-0.0520***	0.0071	-0.0249***	.	.
			Statistics			
Rho	0.6408	0.5956	0.6479	0.5993	.	.
Observations	32661	42376	11519	42376	9843	18947
Log-likelihood value	-10500	-16555	-3951	-16414	-2796	-5479
χ^2	3460	5138	1204	4839	1936	3724
Mean predicted prob	0.26	0.27	0.27	0.27	.	.
% correctly predict own	.2036	.2098	.2090	.2122	.	.
% correctly predict not	.4111	.4031	.4061	.4022	.	.
Health vars $\chi^2(4)$	3.71	28.58	3.36	5.83	1.81	7.12
Mean health vars $\chi^2(4)$	.	.	13.18	24.90	.	.
Hausman (single): $\chi^2(4) = 11.89$. Hausman (married): $\chi^2(4) = 24.95$.						

The dependent variable is an indicator for whether the household owns stocks outside of retirement accounts. Standard errors are in parentheses. All regressions include year dummies. To save room, we do not report selected coefficient estimates (gender, children, high school college, nonwhite, and Hispanic, spouse's age, spouse's age squared, health insurance, and health insurance interacted with age>= 65) or those of the mean time-varying independent variables. "Rho" is the variance share of unobserved heterogeneity. The percent correctly predicted uses the fraction of successes in the sample as the threshold. Wald joint-significance tests are included for the model as a whole, the set of four health indicators, and the set of four mean health indicators in the CRE regressions. Hausman tests are restricted to the four SRH variables. Statistical significance (not reported for FE coefficients) is indicated as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7: Stock Allocation Tobits: Full Specification

	RE		**CRE**		**FE**	
Explanatory Variable	Single	Married	Single	Married	Single	Married
SRH = Very good	0.0084	-0.0130	0.0191	-0.0160	0.0214	-0.0232
	(0.0131)	(0.0129)	(0.0239)	(0.0140)	(0.0156)	(0.0136)
Good	-0.0114	-0.0265*	0.0200	-0.0149	0.0145	-0.0218
	(0.0138)	(0.0133)	(0.0262)	(0.0149)	(0.0178)	(0.0147)
Fair	-0.0159	-0.0430**	0.0012	-0.0118	0.0286	-0.0177
	(0.0159)	(0.0145)	(0.0306)	(0.0165)	(0.0217)	(0.0169)
Poor	-0.0271	-0.0915***	0.0380	-0.0515**	0.0425	-0.0759**
	(0.0213)	(0.0172)	(0.0389)	(0.0199)	(0.0316)	(0.0227)
Log O.O.P. med costs	0.0122***	0.0040*	0.0039	0.0007	0.0020	-0.0012
	(0.0019)	(0.0019)	(0.0037)	(0.0021)	(0.0028)	(0.0024)
Prob. leave beq	0.0004***	0.0003**	0.0002	-0.0001	-0.0001	-0.0003*
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0001)
Log HH fin. assets	0.1883***	0.1841***	0.1729***	0.1713***	0.1627***	0.1638***
	(0.0029)	(0.0023)	(0.0053)	(0.0025)	(0.0046)	(0.0034)
Log HH non-fin. assets	0.0050***	-0.0010	0.0033	-0.0036*	0.0028	-0.0032
	(0.0013)	(0.0015)	(0.0024)	(0.0015)	(0.0027)	(0.0021)
Log HH income	0.0117**	0.0082*	0.0160*	0.0070	-0.0038	0.0041
	(0.0043)	(0.0040)	(0.0076)	(0.0044)	(0.0062)	(0.0053)
Retired	-0.0143	0.0069	-0.0150	0.0003	-0.0072	0.0022
	(0.0103)	(0.0084)	(0.0198)	(0.0095)	(0.0147)	(0.0104)
Age	-0.0132*	-0.0163***	-0.0322**	-0.0153*	-0.0321*	-0.0225*
	(0.0055)	(0.0049)	(0.0111)	(0.0072)	(0.0184)	(0.0130)
Age sq./100	0.0077*	0.0115**	0.0230**	0.0112*	0.0307***	0.0202**
	(0.0037)	(0.0038)	(0.0083)	(0.0054)	(0.0074)	(0.0057)
Risk averse	0.0002	-0.0096	-0.0067	-0.0175	.	.
	(0.0121)	(0.0095)	(0.0195)	(0.0097)	.	.
Planner	0.0275*	0.0267**	-0.0129	0.0136	.	.
	(0.0132)	(0.0098)	(0.0205)	(0.0098)	.	.
			Marginal Effects			
SRH = Very good	0.0017	-0.0047	0.0045	-0.0057	.	.
Good	-0.0023	-0.0095*	0.0047	-0.0054	.	.
Fair	-0.0032	-0.0152**	0.0003	-0.0042	.	.
Poor	-0.0054	-0.0309***	0.0092	-0.0179**	.	.
			Statistics			
Rho	0.3975	0.4472	0.4140	0.4422	.	.
Observations	32661	42376	11519	42376	7555	8713
Log-likelihood value	-13946	-21159	-5259	-21054	.	.
Wald χ^2	6476	9730	2667	10092	.	.
Health vars $\chi^2(4)$	6.26	40.35	2.14	9.74	.	.
Mean health vars $\chi^2(4)$	.	.	23.13	37.05	.	.

The dependent variable is the financial wealth share of stocks held outside of retirement accounts. To save room, we do not report the coefficient estimated of selected controls (gender, children, high school college, nonwhite, and Hispanic, spouse's age, spouse's age squared, health insurance, and health insurance interacted with age>= 65) or those of the mean time-varying independent variables. The marginal effects are evaluated at the means of the independent variables. "Rho" is the variance share of unobserved heterogeneity. Wald joint-significance tests are included for the model as a whole, the set of four health indicators, and the set of four mean health indicators in the CRE regressions. Statistical significance is indicated as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8: Alternative Health Measure: Acute and Chronic Conditions

	RE		CRE		FE	
Explanatory Variable	Single	Married	Single	Married	Single	Married
Stock Ownership Logit Regressions						
Coefficient Estimates						
Acute	0.1253	-0.0649	0.1073	-0.1066	0.1511	-0.0906
	(0.0683)	(0.0498)	(0.1359)	(0.0602)	(0.0988)	(0.0645)
Chronic	-0.1126	-0.1370*	-0.0888	-0.1220	-0.0702	-0.1373
	(0.0753)	(0.0642)	(0.1622)	(0.0723)	(0.0992)	(0.0763)
Avg. partial effects						
Acute	0.0110***	-0.0067***	0.0098*	-0.0105***	.	.
	(0.0031)	(0.0015)	(0.0044)	(0.0017)	.	.
Chronic	-0.0098**	-0.0141***	-0.0081	-0.0120***	.	.
	(0.0033)	(0.0019)	(0.0052)	(0.0020)	.	.
Rho	0.6411	0.5966	0.6491	0.6006	.	.
Observations	32661	42376	11519	42376	9843	18947
Hausman (single): $\chi^2(2) = 0.76$. Hausman (married): $\chi^2(2) = 0.82$.						
Stock Allocation Tobit Regressions						
Coefficient Estimates						
Acute	0.0215*	-0.0094	0.0320	-0.0131	0.0204	-0.0038
	(0.0100)	(0.0073)	(0.0207)	(0.0091)	(0.0178)	(0.0104)
Chronic	0.0010	-0.0083	-0.0153	-0.0040	0.0281	0.0044
	(0.0113)	(0.0096)	(0.0246)	(0.0109)	(0.0170)	(0.0119)
Marginal effects						
Acute	0.0044*	-0.0034	0.0076	-0.0047	.	.
	(0.0021)	(0.0026)	(0.0050)	(0.0033)	.	.
Chronic	0.0002	-0.0030	-0.0036	-0.0015	.	.
	(0.0023)	(0.0035)	(0.0059)	(0.0040)	.	.
Rho	0.3977	0.4480	0.4145	0.4445	.	.
Observations	32661	42376	11519	42376	7555	8713

The table displays the coefficient estimates and marginal effects from logit and Tobit regressions similar to those reported in Tables 6 and 7, with acute and chronic health condition variables replacing those for self-reported health. "Acute" is an indicator for whether the respondent (or spouse) currently suffers from heart problems, cancer, or stroke. "Chronic" is an indicator for whether the respondent (or spouse) suffers from high blood pressure, diabetes, lung disease, psychiatric problems, or arthritis. Because the coefficient estimates on the other controls are in line with the earlier regressions, we only report estimates for the health conditions. "Rho" is the variance share of unobserved heterogeneity. The Hausman tests in the logit regressions are restricted to the two health variables. Statistical significance is indicated as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.