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Recession Depression: Mental Health Effects of the 2008 Stock Market Crash

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Recession Depression: Mental Health Effects of the 2008 Stock Market Crash

Abstract

How do sudden, large wealth losses affect mental health? Most prior studies of the causal effects of material well-being on health use identification strategies involving income increases; these studies as well as prior research on stock market accumulations may not inform this question if the effect of wealth on health is asymmetric. We use exogenous variation in the interview dates of the 2008 Health and Retirement Study to assess the impact of large wealth losses on mental health among older U.S. adults. We compare cross-wave changes in wealth and health for respondents interviewed before and after the October 2008 stock market crash. We find that the crash reduced wealth and increased depressive symptoms and the use of anti-depressants. These results suggest that sudden wealth losses cause immediate declines in mental health; for example, a loss of $50,000 of non-housing wealth increases the likelihood of feeling depressed by 1.35 percentage points, or by 8%.

JEL-Code: I100, I110.

Keywords: mental health, depression, health status, wealth, income.

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1. Introduction

How do sudden, large wealth losses affect mental health? Despite a large literature on the relationship between socioeconomic status and health, the answer to this question is not well-known. Most prior studies that use exogenous shocks to identify the causal effects of income on health exploit natural experiments that increase income (see, e.g., Frijters et al., 2005; Gardner and Oswald, 2007; and Kim and Ruhm, 2012) or lead to wealth accumulations (e.g., Smith, 2004). These studies show that some types of income increases lead to mental health improvements, while receiving a bequest or experiencing gains in stock market wealth have no significant effects on health. The health effects of losing money are studied less often despite the fact that a number of life events such as divorce, widowhood, and expensive medical episodes can trigger significant declines in material well-being.

Research on the effects of wealth losses is especially important if the effect of wealth on health is asymmetric. Results from economics and psychology experiments provide ample evidence that individuals respond differently to losses than they do gains. For example, subjects in experiments often place a higher value on a good that they own compared to an identical good that they do not own (the endowment effect). One explanation for this effect is loss aversion, which explains that the disutility of losing a good is greater than the utility of obtaining it (e.g., Kahneman and Tversky, 1984). Furthermore, the effects of large wealth losses may be far more disruptive than generalizations of the effects of small gains or losses may suggest. It may take years of favorable stock market returns to recapture wealth losses, whereas small losses can be offset by an increase in work hours or a short-term change in consumption patterns. If the psychological or economic consequences of losses differ in these ways, then prior studies that focus on the effects of sudden gains in socioeconomic status may offer little guidance on the effects of losses on health.

In this study, we identify the effect of large wealth losses on mental health by focusing on the immediate and largely unexpected declines in the net worth of U.S. households brought on by the stock market crash that occurred in the fall of 2008. In the 8-day trading period that began on October 1, 2008, the closing value of the Dow Jones Industrial Average (DJIA) fell by nearly 2,400 points or roughly 22%. After a small uptick, the DJIA fell throughout the month; by the end of October, the index had reached its lowest level since 2003. By December 2008, retirement accounts had lost almost one-third of their value on average, and total losses were estimated at $2.8 trillion (Soto, 2008). Our identification strategy uses variation in the timing of interview dates for the 2008 Health and Retirement Study (HRS). Specifically, we examine respondent-specific changes in wealth and health between the 2006 and 2008 waves of the HRS, comparing changes for respondents whose 2008 interview took place before October of that year with the changes experienced by those interviewed after the October 2008 stock market crash. We allow the effects of the crash to vary by respondent's exposure, which we measure in pre-crash wealth in stocks and individual retirement accounts.

We find that respondents interviewed post-crash lost significantly more non-housing wealth between the two survey waves than respondents interviewed before the crash. Results from several falsification and placebo tests suggest that our post-crash indicator identifies the effects of the stock market crash separately from the effects of changes in the housing market or the unobservable traits of respondents interviewed later in the year. We find that respondents interviewed post-crash were also significantly more likely to feel depressed than those interviewed prior to October 2008. Further, the adverse effects on depression and anti-depressant use were largest among those with the largest wealth losses. Results from instrumental variables analysis suggest that large wealth losses have a causal effect on the likelihood of depression. For example, a loss of $50,000 of non-housing wealth increases the likelihood of feeling depressed by 1.35

percentage points, or by 8% relative to a mean rate of 16.6%. Our results provide evidence that large wealth losses have immediate effects on mental health and may serve as useful evidence when evaluating the effects of other events that reduce wealth, such as divorce, widowhood, or costly medical expenditures associated with a family member's illness.

2. Previous Literature

A key distinction between our work and prior studies in this literature is that we examine an exogenous event that reduced wealth while the identification strategies of most prior studies are based on income or wealth increases. These increases may result from either new public programs (Case, 2004; Frijters et al., 2005; Chung and Kim, 2011; Robert, 2011), lottery winnings (Lindahl, 2005; Gardner and Oswald, 2007; Apouey and Clark, 2010), or inheritances (Meer et al., 2003; Kim and Ruhm, 2012). Most of these studies find that income from new public programs improves health status or health or life satisfaction, or that winning the lottery improves mental health. In contrast, receiving a bequest appears to have no effect on various measures of health status, including self-reported health, ADL and IADL limitations, and depression (Meer et al., 2003; Kim and Ruhm, 2012), and wealth increases driven by stock market gains are not associated with improvements in physical health.[1]

Two studies that examine the effects of decreases in income are Snyder and Evans (2006) and Sullivan and van Wachter (2009). Snyder and Evans (2006) look at the effects of a legislative change affecting U.S. Social Security payments, and find that those receiving lower incomes as a result had significantly *lower* mortality rates. Since reductions in Social Security benefits may have increased

[1] There is some evidence that increases in income can worsen health behaviors. Kim and Ruhm (2012) report that receiving a bequest increased recreational drinking; similarly, Apouey and Clark (2010) report worsening health behaviors among lottery winners, like increased drinking and smoking.

employment incentives, these results could be explained by the social benefits of remaining in the labor force. Sullivan and van Wachter (2009) find that job displacement led to large increases in short-term mortality risk as well as smaller increases in long-term mortality risk. Their results are suggestive of the effects of income losses on health since short-term earnings losses were greater than long-term earnings losses, and since workers with larger earnings reductions experienced larger increases in mortality.

Our work differs from the prior studies of earnings decreases in several ways. First, we examine the immediate effects of wealth losses on mental health, as opposed to long-term or even one-year effects on mortality. This is because our research strategy compares HRS respondents affected by the 2008 stock market crash to those who were not affected based on the timing of the 2008 interviews; over a longer horizon, for example, by the 2010 interview, every individual in the HRS has been affected by the 2008 crash. Nonetheless, our focus on mental well-being, rather than mortality, is very appropriate for testing short-term effects. Mental well-being is likely impacted by material losses more immediately than mortality. Psychological research suggests that reductions in wealth increase stress levels, and higher levels of stress are associated with increases in depression and other mental illnesses (see, e.g., Rabkin and Struening, 1976; Schneiderman, Ironson, and Siegel, 2005). It may take as little as two weeks for depressive symptoms to emerge, and major life events such as assault, job loss, and financial or housing problems can have significant causal effects on psychological well-being even when measured within one month of the stressful life event (Dohrenwend, 1973; Kendler et al., 1999).[2] Because we focus on the immediate impacts on mental health, there is also less ambiguity about the

[2] Stress may subsequently worsen physical health as elevated stress hormones take their toll on the cardiovascular and immune systems, but these effects may not be immediate (see, e.g., McEwen 1998; Schneiderman, Ironson, and Siegel, 2005).

channels or mechanisms linking wealth to health compared to studies that focus on long-run risk of mortality (e.g., Snyder and Evans, 2006). Of course a downside to our approach is that there may be important differences in the short-run and long-run relationships between wealth and health. For example, looking at income increases, Evans and Moore (2011) and (2012) find that having more income causes consumption and activity levels to increase, and thereby leads to increases mortality in the short-term. As a result, our study is unable to examine whether the effects we observe are transient or permanent.

We also examine wealth fluctuations with the potential to affect a larger population of individuals compared to events like winning the lottery or receiving an inheritance. The 2008 stock market crash was unanticipated and affected asset levels for all stockholders. Because the effects of the crash were both large and sudden, our strategy differs from Smith (2004) who used stock market changes in the 1990s in an attempt to identify health effects. Smith found little evidence that exogenous wealth increases were timed with health improvement. As noted above, it is possible that the effects of wealth losses differ from those of wealth gains; if this is the case, our study of the events of the 2008 stock market crash may be especially informative.[3]

3. Methodological Approach

3.1. Identification Strategy

Our study employs data from several waves of the HRS, a nationally representative longitudinal survey of more than 22,000 Americans over the age of

[3] Deaton (2012) examines changes in self-reported well-being in the U.S. population from 2008 to 2011, a period that includes the 2008 stock market crash. Using daily polling data from the Gallup Organization, he finds a sharp decline in well-being around the fall of 2008 and a close association between well-being and the S&P 500 index between 2008 and 2010. However, the Gallup data do not include measures of wealth, so questions about the effects of the crash on wealth, the associations between wealth and health, and how these relationships vary by stock market exposure cannot be examined.

50. Respondents are interviewed every two years and answer a wide array of questions pertaining to their household wealth, incomes, health status and behaviors, among other topics. We use HRS data from the Institute for Survey Research at the University of Michigan combined with the RAND HRS Data, version K, which include detailed wealth measures of particular relevance to our analysis.

We use variation in respondents' 2008 interview dates to identify the effect of wealth changes on health. Figure 1 illustrates the daily closing values of the DJIA during the fielding of the 2008 wave of the HRS. While all of the HRS 2008 interviews took place during an economic recession, respondents interviewed on or after October 1 (roughly 8% of the sample in that year) reported information about their wealth and health in a very different financial climate than those interviewed in the weeks and months before. Our identifying assumption is that the October 2008 stock market crash acted as an exogenous shock to the wealth of a large fraction of Americans, especially those with individual retirement accounts (IRAs) and other holdings in equity markets. Hudomiet et al. (2011) use this same strategy to study the effect of the crash on households' expectations about market returns; they show that the 2008 interview date was exogenous to HRS respondents' prior stock market expectations.

Our estimation strategy uses first-differences models of wealth and health to test for the effects of the crash. By examining changes between 2006 and 2008, rather than simply comparing those interviewed before and after the crash, we difference out time-invariant characteristics of respondents that might be related to both date of interview and health outcomes. This is a key strength of our approach, since it addresses concerns that the respondents interviewed in the months near the end of the wave (and after the crash) are different from those interviewed earlier in some unobservable ways. In a related study, Ásgeirsdóttir et al. (2012) employ individual fixed effects models to examine the effect of

Iceland's financial crisis on health behaviors; the financial crisis is measured by an indicator for 2009. Unlike their study, our identification approach employs both a treatment group (respondents interviewed post October 1, 2008) and a comparison group (those interviewed earlier) to test the effects of the crash, and we allow the treatment intensity to vary by respondent's exposure to the crash. We describe these features in more detail below.[4]

We first use data on respondents in both 2006 and 2008 to estimate Equation (1) below:

$$(1)\quad \Delta W_i = \beta_0 + \beta_1 POST_i + \lambda \Delta X_i + \Delta \varepsilon_{i,}$$

Here ΔW_i is the change in wealth between the 2006 and 2008 waves of the HRS. Our focus on changes in the level of wealth is comparable to most prior studies in this area. For example, Smith (2004) examines the effect of absolute increases in stock market wealth on health conditions, and Chung and Kim (2011) focus on absolute increases in income on birth weight. While it may be the case that income or wealth changes are also perceived in relation to one's baseline level of income or wealth or in relation to the income or wealth of one's peers, most of the studies cited in Section 2 do not make either type of relative adjustment, and instead look at the effects of receiving any (or any large) inheritance, any medium-sized lottery prize, any pension, or any reduction in Social Security earnings. Our focus on absolute changes in wealth is thus consistent with many

[4] We choose to pool all respondents interviewed before and after the October 1, 2008 cutoff instead of using a regression discontinuity approach for several reasons. First, the sample size is too small to support such an approach. Second, as we show later, not all respondents interviewed after the crash are stockholders. This would imply that a fuzzy RD approach would be more appropriate; however, the fluctuations in the DJIA shown in Figure 1 indicate that wealth loss is unlikely to be a smooth function of days from the crash. Finally, the size of the response may be proportional to the size of the loss. We instead employ an IV approach that is closely related to a fuzzy RD approach but allows us to make causal statements about the magnitude of the wealth effect on health.

studies in this literature.[5] The key explanatory variable is *POST*, a dummy variable equal to 1 if the 2008 survey interview took place during or after October of that year (i.e., post-crash) and 0 otherwise. Observable differences in respondents that vary over time and may contribute to differences in wealth (such as changes in household income, employment, etc.) are represented by *X*.

We then allow the crash to have heterogeneous effects on wealth; in particular we examine whether respondents who had more "exposure" to the crash suffered greater wealth losses. We first estimate Equation (2) below:

$$(2)\quad \Delta W_i = \alpha_0 + \alpha_1 POST_i * STOCK + \alpha_2 POST, * NOSTOCK + \lambda \Delta X_i + \Delta \varepsilon_{i,}$$

Here, *STOCK* equals one if the respondent held any non-zero amount of wealth in stocks or IRAs, and zero otherwise; *NOSTOCK* equals one if the respondent held *no* wealth in stocks or IRAs, and zero otherwise. These variables measure exposure to the effects of the stock market crash; we anticipate that the crash will impact respondents who own stocks and IRAs and have less effect on those with no stock or IRA wealth.[6] We define the respondent's exposure using stock and IRA wealth in 2006 (prior to the crash); this addresses the concern that mental health issues affect the decision to invest in risky assets (Bogan and Fertig, 2012). Estimates of α_1 that are negative and larger (i.e., more negative) than the estimates of α_2 would support our identification strategy.

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[5] In contrast, Lindahl (2005) examines the health effects of percent increases in income; we compare our estimated effect sizes to his below. While a large literature also explores the health effects of relative income (income compared to one's peers, for example), we do not pursue that approach because of the challenge of identifying the appropriate reference groups.

[6] This exposure measure does not capture all of the ways a respondent's wealth could be affected by stock market events. Some respondents who have employer pensions or own mutual funds may also be exposed to the crash, and some of these respondents could have no direct holdings of stock or IRAs. However, the HRS data do not permit us to measure whether the respondent's pension or mutual fund consists of stocks or the percentage of the respondent's IRAs holdings that are held in stock, at least in our sample (Gustman et al. (2011) do so for a much smaller sample of HRS respondents). Exposure to the crash might also depend on the attributes of the respondent's pension plan, such as whether the plan is a defined contribution plan and how much of the respondent' portfolio is held in stocks.

As a second test for heterogeneous effects, we estimate Equation (3) below:

$$(3)\quad \Delta W_i = \delta_0 + \delta_1 POST_i{*}HISTOCK + \delta_2 POST_i{*}LOWSTOCK + \delta_3 POST_i{*}NOSTOCK + \lambda \Delta X_i + \Delta \varepsilon_{i,}$$

In this specification, we allow the effects of the crash to vary across three groups of respondents: those with no wealth in stocks and IRAs (*NOSTOCK*), those with some wealth in stocks and IRAs (*LOWSTOCK*), and those with more wealth in stocks and IRAs (*HISTOCK*). Specifically, *HISTOCK* equals one if the respondent had stock and IRA wealth levels at or above the median wealth of stockholders (in terms of wealth in stocks and IRAs, not their overall wealth), and zero otherwise; *LOWSTOCK* equals one if the respondent had stock and IRA wealth levels that placed them in the less wealthy half of stockholders (again, in terms of the amount of wealth in stocks and IRAs), and zero otherwise.[7] Again we expect that exposure increases with the amount of wealth held in stocks/IRA; this specification allows the effects of increased exposure to be non-linear. As in equation (2), respondent exposure is based on 2006 wealth holdings. In this case, support for our identification strategy would come from negative estimates of δ_1 that are larger (i.e., more negative) than estimates of δ_2 and δ_3.

To examine the effect of the crash-induced wealth losses on health, we use two approaches. In one approach, we estimate a series of models similar to those described above, in which the dependent variable is one of several measures of mental and physical health, and in which the key explanatory variable is an indicator representing a "post-crash" interview. Our preferred specification is represented by Equation (4) below:

[7] In results described below, we also tested an alternative definition of exposure to the crash. In these alternative specifications, we estimate equation (3) but instead define *LOWSTOCK* and *HISTOCK* based on the percentage of the respondent's portfolio that is held in stock/IRA assets.

(4) $\Delta H_i = \delta_0 + \delta_1 POST_i{*}HISTOCK + \delta_2 POST_i{*}LOWSTOCK + \delta_3 POST_i{*}NOSTOCK + \lambda \Delta X_i + \Delta \varepsilon_{i,}$

where *H* is one of several health measures. This reduced form specification allows the effects of the crash to vary by the respondent's exposure to the crash and to look for evidence that those respondents who experienced the largest wealth reductions as a result of the crash also experienced the largest health consequences from the crash. Our second approach provides direct evidence of the causal relationship between changes in wealth and changes in health from a set of instrumental variables models illustrated by equations (5) and (6) below:

(5) $\Delta W_i = \delta_0 + \delta_1 POST_i{*}HISTOCK + \delta_2 POST_i{*}LOWSTOCK + \delta_3 POST_i{*}NOSTOCK + \lambda \Delta X_i + \Delta\varepsilon_{i,}$

(6) $\Delta H_i = \alpha_0 + \beta\Delta\widehat{W}_i + \lambda\Delta X_i + \Delta\xi_i$

Note that the post-crash indicators serve as the instruments in the first-stage model of wealth.

3.2. Data

We estimate our regression models using two samples of HRS respondents constructed as follows. Of the combined respondents to the 2006 and 2008 waves (35,686 respondents), we define our sample as those respondents with non-proxy interviews in both waves (n=30,508), designated as the financial respondent in the household in both waves (n=20,708),[8] and born no later than 1953 (n=20,040).[9] The resulting sample includes 10,020 unique financial respondents and is our first

[8] There is one financial respondent for almost every household interviewed in the HRS. In one-respondent households, the sole respondent is the financial respondent; in two-respondent households, the financial respondent answered household-level financial questions on behalf of the entire household. This respondent was determined based on the answer to the question: "Which member of your immediate family is most knowledgeable about your family's assets, debts and retirement planning?"

[9] We exclude persons born after 1953 because the HRS was designed to be representative of individuals aged 51-61 and the youngest cohort added to the 2008 HRS core sample of respondents was born no later than 1954. This exclusion leads us to drop spouses of age-eligible persons who, while interviewed, were not the focus of the sample design.

estimation sample. We also define a second estimation sample that excludes respondents in the top 1% and the bottom 1% of 2006 non-housing wealth, leaving us with 19,642 respondents in both years, or 9,821 unique respondents.

Table 1 reports descriptive statistics for the first estimation sample by year of interview and for respondents interviewed after the 2008 crash. Our main measure of wealth is real non-housing wealth, reported in 2008 dollars. This is defined from the RAND HRS data as the total of non-housing assets less debt, and where non-housing assets are the amounts of wealth held in checking and savings accounts, CDs, savings bonds and Treasury bills, bonds, total IRA accounts, stocks, mutual funds, real estate other than housing, vehicles, businesses, plus other savings and assets. On average, real non-housing wealth declined from $344,491 in 2006 to $292,861 in 2008. As expected, the size of this decline was more severe among the respondents whose 2008 interview took place on or after October 1 of that year. For that group of 815 respondents, non-housing wealth fell by an average of 46%, from $506,150 in 2006 to $271,484 in 2008.[10] Mean changes in non-housing wealth are large in magnitude because they are concentrated among households with sufficient wealth to have stock market exposure. The mean household experienced losses of $51,630 from 2006 to 2008, while the median household reported losses of only $5,889.

Non-housing wealth is our preferred wealth measure because it includes IRA accounts which are likely to be impacted by the crash; we also prefer this broadly-defined measure in our instrumental variables analysis of the wealth effects on health. As a robustness check, we also examine financial wealth, defined in the RAND HRS data as total financial assets less debt, including

[10] Note that this is the combined decline in wealth from losses and sales. Respondents who sold stock since their 2006 interview can be identified in the 2008 wave, but the specific dates of stock sales are not identified; therefore we are unable to examine the link between decisions to sell stock and the stock market crash.

checking and savings accounts, CDs, savings bonds and Treasury bills, bonds, stocks, mutual funds, plus other savings and assets, but excluding IRA accounts. This exclusion makes financial wealth less suitable for our study, given that IRA account owners face considerable exposure to the market. Gustman et al. (2011) report that, among the early boomer HRS cohort, 73% of IRA assets were held in stocks (see Table 1 of their study).

We use six measures of HRS respondents' health. The first is the Center for Epidemiologic Studies Depression Scale or CES-D for depression. The HRS version of the CES-D includes eight items (instead of the 20 items in the full index) that ask respondents whether or not in the past week they felt depressed, happy, lonely, sad, etc.[11] Higher values of the index indicate worse mental well-being. Our second measure is a binary indicator equal to one if the respondent reported feeling depressed in the past week (and zero otherwise). The third measure is a binary indicator equal to one if the respondent reported regularly taking prescription medications to help relieve anxiety or depression. The next three measures are self-reported health (on a scale of 1 to 5 where 1 corresponds to poor and 5 corresponds to excellent), plus one binary indicator for fair/poor health and another binary indicator for excellent/very good health.

Among all financial respondents, there was a slight decline in depression between 2006 and 2008 according to both the CES-D index and the binary indicator for feeling depressed. For respondents interviewed post-crash, mean CES-D and the proportion depressed were higher than in the full sample, and both measures declined to a lesser extent across waves than in the full sample. For example, the share of all financial respondents who reported "feeling depressed" fell from 16.8% to 13.4% between 2006 and 2008. Among those whose 2008

[11] The other four items deal with whether "everything is an effort", whether "sleep is restless", and whether the respondent "enjoys life" and "cannot get going." The score is the sum of the number of negative symptoms reported, plus (1 minus "enjoys life") and (1 minus "feels happy").

interview took place post-crash, there was a somewhat smaller decline, from 18.2% to 16.6%. The proportion of respondents in fair or poor health increased across both waves, and the increase was somewhat larger among those interviewed post-crash relative to the full sample.

Table 1 also allows us to compare post-crash respondents to the full sample in terms of their demographic, economic, and other traits. Respondents interviewed later in the year in 2008 are younger, have higher household incomes, and are more likely to work and less likely to be retired. This is consistent with reports that respondents interviewed later in the wave are those who are hardest to schedule.[12] Respondents interviewed later in the wave are similar to the larger sample in terms of sex, race, education, marital status, and cognitive function. We looked for evidence of unobservable differences in current health status by comparing the percentage of 2008 financial respondents who died by 2010 by broadly-defined interview dates: 7.8% of respondents interviewed in the first six months of the 2008 wave (February-June 2008) had died by 2010, compared to 7.6% of respondents interviewed in the last six months of the 2008 wave (September 2008-February 2009). Nonetheless, to address concerns about differences in the post-crash group that might be correlated with wealth and/or health, our regression models include a number of controls for time-varying individual or household traits. The full set of explanatory variables includes measures of: cross-wave transitions in marital status, employment status, and retirement status[13]; changes in household income and its square, changes in household size and cognitive function; controls for the onset of various physical

[12] See Hudomiet et al. (2011), who also state that respondents interviewed later may be harder to locate and more reluctant to be interviewed (p. 402).

[13] Controls for both retirement and employment status are included because some respondents are employed during retirement. Cognitive impairment is associated with depression and may also be associated with changes in spending for nursing home care and thus wealth.

health conditions across waves,[14] the time between waves (measured in months and months squared), and two indicator variables to control for changes in the season in which the interview takes place.[15] As noted above, the first-difference specification controls for time-invariant traits that may be correlated with the post-crash indicator and our outcomes of interest, whether these traits are observed (such as race, sex, education) or unobserved (such as risk preference).[16]

4. Empirical Results

4.1. Effects of the Stock Market Crash on Wealth

Table 2 presents our main analysis of the effects of the crash on respondent wealth, where wealth is measured as total non-housing wealth. Columns (1) and (2) report the estimated post-crash coefficient from Equation (1) using the sample of all financial respondents and then excluding the top and

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[14] The physical health conditions are cancer, diabetes, heart disease, lung disease, stroke, arthritis, and hypertension. We include separate indicators for each condition. To preserve sample sizes we also include a set of controls for whether the respondent is missing data on a given indicator, and then recode the indicator to zero.

[15] The designation known as SAD, or seasonal affective disorder, links the onset of depressive symptoms to decreases in daylight timed with fall and winter months. For example, Tefft (2012) shows that more hours of darkness are associated with adverse mental health and employment outcomes. These controls help to account for the possibility that respondents in our sample were interviewed at different times of the year in the 2006 and 2008 waves and that the onset of seasonal depression might be correlated with being interviewed post-crash in 2008. It is not clear how much of a concern this should be, as the post-crash period includes both spring and winter months (October 2008 through April 2009). Nonetheless we include a dummy equal to one if the respondent's interview changed from a fall/winter month to a spring/summer month between 2006 and 2008 (and zero otherwise), and a second dummy equal to one if the respondent's interview changed from a spring/summer month to a fall/winter month across the two waves (and zero otherwise). Certain HRS design features (namely an oversample of both Floridians and Hispanics) also help to mitigate concerns of SAD for respondents who live in regions with perpetually summery weather. We obtained similar results when we used a set of month of interview dummy variables in place of the seasonal change in indicators.

[16] It is possible that some of the control variables are themselves affected by the crash (e.g., income). As a robustness check, we examine the sensitivity of our results to the exclusion of household income and other controls. These results are described below.

bottom 1% of the wealth distribution. In the full sample, the crash had a statistically significant effect on non-housing wealth, reducing it by more than $238,000. When we trim the sample to exclude the top and bottom 1% of respondents from the sample, the estimated crash effect is wealth loss of just under $44,000 but the effect is no longer statistically significant. The sizes of these effects are consistent with the asset declines in reported by Bricker et al. (2012).

The remaining columns of Table 2 illustrate the importance of allowing for heterogeneous effects. We first allow the effect the crash to vary between those with any stock/IRA wealth in 2006 (who comprise 47.2% of respondents) and those with no stock/IRA wealth (the remaining 52.8%), as in Equation (2). In columns (3) and (4) we report the post-crash coefficients from this equation for both the full sample and the trimmed sample. In both samples, the post-crash coefficient defined for stockholders (*POST*STOCK*) is negative and significant, and much larger in absolute value than the post-crash coefficient defined for non-stockholders. The estimated post-crash coefficient for non-holders is also statistically insignificant. The results for the trimmed sample, for example, suggest that the crash caused wealth declines of more than $81,000 among respondents with any stock/IRA wealth, but led to no significant changes in wealth for respondents without stocks or IRAs.

We then allow the effects of the crash to vary across respondents in the top half of the stock/IRA wealth distribution in 2006 (23.6% of respondents), those in the bottom half of the stock/IRA wealth distribution (another 23.6%), and those with no stock/IRA wealth (the remaining 52.8%). In columns (5) and (6) we report the post-crash coefficients from this equation for both the full sample and the trimmed sample. In both samples, the results show that the intensity of the crash effect varies by exposure. Effects are largest for those in the top half of the stock/IRA wealth distribution (e.g., in the trimmed sample, the crash caused

wealth losses of $223,581), and statistically insignificant for the other types of respondents. Results from χ^2 tests in both samples allow us to reject the null hypothesis that the post-crash coefficients are equal among the top half of stockholders and the non-stockholders ($p<0.001$). As a robustness check, we estimate the same set of models using financial wealth as a dependent variable; recall that this more narrowly focused measure includes stocks and mutual funds but excludes IRA accounts. As expected the coefficients of the post-crash indicator variable and interaction terms are smaller, but the pattern is the same as that seen for non-housing wealth (see Appendix Table 1). Clearly, the events on Wall Street had very different effects on wealth depending on a given respondent's exposure to the stock market.[17]

4.2. Falsification and Placebo Tests

While the stock market crash was closely timed with the start of October 2008 (see Figure 1), it is possible that our post-crash coefficient could be picking up effects of other coincident events or correlated factors, and not the effect of the crash, *per se*. One such event is the downturn in the housing market; this is particularly important for us to consider since the housing crisis itself may have

[17] We also estimated models of non-housing wealth where we allowed the effect of the crash to vary across respondents with no stock/IRA wealth, those with low stock/IRA wealth, and those with high stock/IRA wealth (as in Table 2, columns 5 and 6), but in which we defined "high" and "low" stock/IRA respondents based on the *share* of the respondent's wealth held in stocks/IRAs (instead of the level). That is, high stock respondents were those whose share of wealth in stocks/IRAs was above the median *share* among stock/IRA owners, and low stock respondents are those whose share of wealth was below the median. The share is defined as the amount of wealth in stocks and IRA divided by the total value of wealth including the value of a second home. Because this approach uses two wealth measures, it introduces some measurement error in the post interaction terms and yields a slightly smaller sample because of missing values in total wealth. We did the same exercise for the Appendix 1 models of financial wealth. The results are very similar to what we observe in columns (5) and (6) of Table 2 and Appendix 1 (in that the wealth effects of the crash is large, negative and significant only for those with high shares of stock), although the point estimates are smaller, which is consistent with measurement error. Results are available upon request.

led to worsening health conditions.[18] Another possibility is that there is something unobservable about the respondents interviewed late in the 2008 wave of the HRS that makes them more prone to wealth losses than the other respondents, regardless of what is happening on Wall Street. We next examine both of these possibilities carefully.

First, we consider whether the post-crash indicators could be picking up declines in housing wealth. This could be possible since our broadly-defined measure of non-housing wealth includes real estate other than housing. As a first step, we examine the timing of the housing crisis relative to October 2008 using data from the Federal Housing Finance Authority's House Price Index.[19] Figure 2A plots the index based on all transactions, which includes new home purchases and refinancing appraisals for existing homeowners; these data represent the most comprehensive information respondents might have about the value of their home. Figure 2B plots seasonally-adjusted data based on home sales only. In each graph, we show national trends (solid line) and the trend at the time of HRS interview (dashed line). The HRS trends are constructed by matching HRS respondents to housing information for their state of residence during the quarter of their interview. Both figures show that the HRS sample is representative of national trends, and moreover, that the housing crisis began much earlier than the October 2008 crash. Figure 2B suggests that, if anything, national housing prices were starting to recover in the post-crash period relative to the pre-crash period. As a result, respondents interviewed after the crash may report smaller changes in

[18] For example, Currie and Tekin (2011) find that the foreclosure crisis was associated with an increase in medical visits for anxiety, suicide attempts, and stress-related physical complaints.

[19] Figures 2A and 2B report the Purchase Only Index and All Transactions Index, US Summary and Census Divisions through 2012Q1 and States through 2012Q1, obtained from the Federal Housing Authority and available at http://www.fhfa.gov/Default.aspx?Page=87 (accessed 6 August 2012). The All Transactions Index includes data on refinancing and home purchases, while the Purchase Only Index includes only home purchases. National data are reported monthly, and state-level totals are available quarterly.

real estate values than pre-crash respondents. Thus, any confounding of our estimates from the foreclosure crisis and decline in housing wealth should bias us towards finding no effect of the crash.

We follow this by conducting a falsification test using housing wealth as defined by HRS respondents. For this exercise, we estimate the same series of models shown in Equations (1) through (3) where the dependent variable is housing wealth. Table 3 reports results from models where the dependent variable is the net value of the primary residence. We see no evidence that respondents interviewed after October 2008 saw significant declines in housing wealth. In all but one case, the coefficients of the post-crash indicator or the post-crash interaction terms are *positive*. We get similar results when we use a dependent variable defined as the gross value of the primary residence (results are available upon request).

Next, we consider the possibility that respondents interviewed late in 2008 differ in some unobservable way that makes them more prone to wealth losses across waves, regardless of events in the stock market. We estimate a series of placebo regressions similar to the specifications shown by Equations (1) through (3), but in which the dependent variable is the change in wealth between 2004 and 2006 and the *POST* indicator is, as before, defined from the date of interview in 2008. Evidence that a post-crash interview date is not associated with reductions in wealth would support our identification strategy.[20] Results from this exercise are reported in Table 4. There is no evidence that respondents with a post-crash interview in 2008 were more likely to experience wealth losses between the 2004 and 2006 waves. If anything, there is some evidence that respondents interviewed in October 2008 or later had larger wealth increases. The direction of this effect would thus bias *against* our observed findings in Table 2. We conduct a similar

[20] This placebo test is similar to the one reported in Hudomiet et al. (2011).

series of placebo tests in which the dependent variable is the change in financial wealth across the 2004 and 2006 waves. The results, reported in Appendix 2, are similar. In no case is having a post-crash interview date in 2008 associated with significant declines in wealth across the earlier waves. Therefore, we conclude that even though there may be some differences between respondents interviewed in October 2008 or later and those interviewed earlier in the year, these differences are expected to work against us in terms of finding post-crash wealth declines.

4.3. Wealth Effects of the Stock Market Crash on Health

Table 5 reports the reduced form estimates of the effects to the stock market crash on six different measures of mental health and self-reported health. We again estimate models using both the full sample and the trimmed sample. The first two columns report results from models of the CES-D index; recall that higher values of this index imply more depressive symptoms. The pattern of results is mixed. Respondents who owned no stock/IRA accounts were not affected by the crash, while the wealthier half of stockholders reported more depressive symptoms after the crash. However, the latter effect is not statistically significant, and the less wealthy half of stockholders reported significantly *fewer* depressive symptoms after the crash than the other groups.

The next four columns of Table 5 report results from models where the dependent variable is an indicator for feeling depressed or taking anti-depressant medications. These results show a pattern that is very consistent with those who are most exposed to the crash experiencing the greatest changes in health. For example, non-stockholders and the less-wealthy half of stockholders, who experienced no significant declines in wealth from the crash, experience no significant changes in the likelihood of feeling depressed or taking anti-depressants as a result of the crash. The wealthier half of respondents, who experienced statistically significantly and large wealth declines following the

crash, also experience statistically significant increases in the likelihoods of feeling depressed and taking anti-depressants as a result of the crash. For that group, the stock market crash increased the probability of feeling depressed by 8.2 percentage points, or nearly 50% from a base of 16.6 percent (Table 1, column 4). The crash also increased the probability that the wealthier half of respondents take anti-depressants by 6.2 percentage points, or nearly 35% from a base of 17.6 percent (Table 1, column 4). Additional analysis also shows a longer lag in anti-depressant use than in feelings of depression following the crash.[21]

In the remainder of Table 5, we report results from models of self-reported health, and two binary indicators defined from that measure (poor/fair health, and excellent/very good health). For models of self-reported health (columns 7 and 8), we see additional evidence that the effects of the crash on health are consistent with its effects on wealth. We find no evidence of health effects among respondents who did not own stocks or IRAs. Both groups of stockholders report significantly worse health, and the post-crash coefficient is larger for the wealthier half of stockholders than it is for the less wealthy half. Interestingly, the crash-induced reductions in health are coming from a decline in respondent reports of very good or excellent health, and not from an increase in fair or poor health. In the fair/poor health models, it appears that all respondents are uniformly affected by the crash; since wealth effects were clearly concentrated among the wealthier half of stockholders, this result is not consistent with the effects of crash-induced wealth losses. In models of excellent/very good health, the results show that the same respondents with the large wealth losses are most likely to report reductions in self-reports of strong health. This may be explained by the fact that respondents

[21] In results available upon request, we find a large increase in the likelihood of feeling depressed in the month of and the month following the crash; in contrast, increases in the likelihood of taking anti-depressants are larger at two to three months post-crash than in the month of and the month after the crash.

with more wealth (and more stock market exposure) are more likely to report excellent or very good health, and less likely to report fair or poor health.

To directly examine the causal relationship between wealth and health, we estimate a set of first difference instrumental variables models; results are reported in Table 6. For ease of presentation, we report coefficient estimates for wealth changes in units of $10,000. The results confirm the pattern suggested in the first-stage and reduced form models presented earlier, in that decreases in wealth increase the CES-D index, increase the likelihood of feeling depressed, increase the likelihood of taking anti-depressants, and lower the likelihood of being in excellent or very good health. The first-stage F-statistics are supportive of the instruments defined from the crash, although overidentification tests are more supportive of the exogeneity of the instruments in the models for mental health outcomes (columns 1-6). The mental health consequences of large wealth losses are non-trivial. For example, a loss of $50,000 raises the likelihood of feeling depressed by 1.35 percentage points, or 8%.

To benchmark the size of this effect, we compare our results to those reported in Lindahl (2005), which estimates the effect of a percent change in income on an index of bad health. For this exercise, we estimate our IV models using the log of wealth as the endogenous regressor.[22] We find that a 10% increase in wealth reduces the likelihood of depression by 3.4% of a standard deviation and increases the likelihood of excellent/very good health by 3.8% of a standard deviation; the estimated effects on anti-depressant use and the continuous measure of self-reported health are smaller, at 2.5% and 2.2% of a standard deviation, respectively. In the full sample of survey respondents, Lindahl finds that a 10% increase in income reduces bad health by 1 to 2 percent of a

[22] This sample is about 20% smaller than the sample shown in Table 6 because we lose respondents with negative or zero wealth in either wave. Results are available upon request.

standard deviation.[23] Our effect sizes may be larger for a number of reasons (e.g., differences in the traits of the survey respondents). Another difference is that variation in wealth in our study is coming from changes over time, in particular, decreases in wealth, on average, while Lindahl uses cross-sectional variation in income. Thus, it is possible that some of the difference is driven by our examination of wealth losses.

We tested the robustness of the health findings to various specification changes. First, we examined how sensitive our results were to alternate definitions of the treatment and control groups, since Figure 1 suggests that the DJIA began to experience some declines in June (although these declines were much smaller than those experienced in the fall 2008). In one specification we used all respondents and defined the treatment group as those interviewed from June 2008 on; we obtained similar results but the size of the crash effects on wealth, depression and self-rated health were smaller, as would be expected. In another specification we excluded respondents interviewed between June and September and defined the treatment group as those interviewed from October 2008 on; we obtained larger point estimates for the effects of the crash on depression and excellent/very good self-reported health, but in the latter case, the standard errors increased (perhaps not surprising since the sample size fell by 37%). In one set of models, we dropped controls for the household income and physical health conditions, given the possible endogeneity of the variables. In another set of models, we dropped all controls including those for changes in cognitive impairment and transitions in marital status, work and retirement. We added a control of the onset of mental health chronic conditions, which we originally excluded out of concerns about endogeneity. In almost all cases, the results from

[23] See Table A2. Among lottery winners, Lindahl reports larger estimates ranging from .04 to .05 standard deviations.

these specification changes are substantively the same as those reported in Tables 5 and 6.[24]

Finally, we estimated a set of placebo regressions similar to those shown for wealth; these use the changes in health measures between 2004 and 2006 as the dependent variables, and a set of "fake" post-crash variables as the key explanatory variables. Table 7 shows the results; since the 2004 wave of the HRS does not contain the question on anti-depressant use we are unable to conduct a placebo test for this measure. Evidence that the fake post-crash variables worsened health among respondents with stock and IRA holdings would call our identification strategy into question. In contrast, we see no evidence of that sort. Respondents interviewed later in 2008 saw no indication of worsening health between the 2004 and 2006 waves. That said, we do not see improvements in health for the wealthiest half of stockholders despite the fact that we see positive impacts on wealth for this group in the Table 4 models. This may be explained by asymmetric effects of wealth on health, which may also help to reconcile our results with those from Smith (2004), who reported that the gradual stock market wealth accumulations in the 1990s were not associated with improvements in health.

5. Discussion

In this study we use exogenous variation in wealth brought about the stock market crash that took place in the fall of 2008 to understand the causal effect of wealth loss on health. The crash of 2008 caused immediate and largely unexpected declines in the net worth of U.S. households. Estimates of aggregate portfolio losses were massive, at close to one-third of the value of retirement

[24] In the IV first-difference model of antidepressant use, the wealth effect becomes insignificant upon controlling for psychiatric conditions, which is not surprising given the potential for this variable to be affected by the crash and correlated with the outcome.

accounts and a total of $2.8 trillion (Soto, 2008). Several existing studies examine how the financial crisis of 2008 altered the retirement decisions and expected retirement incomes of older adults (Coile and Levine, 2009) and affected the age that older workers expect to retire (McFall, 2011). Our study examines the immediate wealth and health effects among HRS respondents interviewed post-crash. We contribute to the literature on the health consequences of adverse economic shocks, which includes studies of the recent U.S. foreclosure crisis by Currie and Tekin (2011) and Burgard et al. (2012) and research on Iceland's financial crisis by Ásgeirsdóttir et al. (2012). Like these other works, our study finds that severe macroeconomic events have significant consequences for health.

Moreover, we exploit the exogenous variation in wealth created by the crash to address a question of long-standing interest in the health economic and social epidemiology literatures: what is the causal effect of wealth on health? Our study adds to a number of careful studies in the area but is somewhat unique in that we examine the immediate effect of sudden wealth *losses* on psychological well-being. Other studies that examine negative income shocks focus on mortality; and other studies that examine mental health consequences examine the effects of increase income from lottery winnings.

We find new evidence that large wealth losses cause immediate reductions in mental health. Those respondents with the greatest exposure to the stock market suffered the most significant wealth losses and the most significant increases in depression and the likelihood of anti-depressant use. Instrumental variables models of health suggest that the effects of large wealth losses are non-trivial. In our IV models, we estimate that a loss of $50,000 raises the likelihood of feeling depressed by 1.35 percentage points, or 8%. In our reduced form specifications, we estimate that respondents most affected by the crash (who lost nearly $250,000 as a result), had nearly a 50% increase in the likelihood of feeling depressed and a 35% increase in the likelihood of taking anti-depressants. One

limitation of our analysis is that the HRS data do not permit us to examine whether the effects we observe are transient or permanent.

These findings may well be useful in examining the immediate mental health consequences of other costly life events. Events like divorce, widowhood, and expensive medical crises all have the potential to result in large wealth losses, but for obvious reasons the health consequences of these wealth losses may be difficult to disentangle from the effects of the unobserved factors that precipitate them. For example, a spouse's death or a marital dissolution may be caused by the same factors that worsen the individual's health. Further, a medical crisis may cause both direct wealth effects and direct health effects whether the crisis affects one's own health (as a new diagnosis of a costly disease might) or the health status of a loved one (as the need for costly long-term care might). Our results suggest that costly life events that reduce wealth and savings may have independent effects on mental health.

Several policy implications follow from our results. First, our results offer a new dimension on which to evaluate the benefits of publicly-financed insurance programs against sudden large losses. For example, Social Security survivor benefits and military survivor benefit plans may have the added value of alleviating the mental health consequences of widowhood. Further, health insurance coverage that offers protection from large financial losses and even personal bankruptcy may have the added benefit of increasing psychological well-being, separately from any physical or mental health benefits associated with increased access to healthcare. Second, our results suggest that there may be direct benefits from providing mental health screenings for individuals following job loss or other sudden, stressful events. Limiting the damage of stressful life events through early access to treatment may prevent a laid-off worker or a newly widowed spouse from becoming severely depressed and may increase the ability to return to the workforce.

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Figure 1. DJIA Daily Closing Value by 2008 HRS Fielding Date

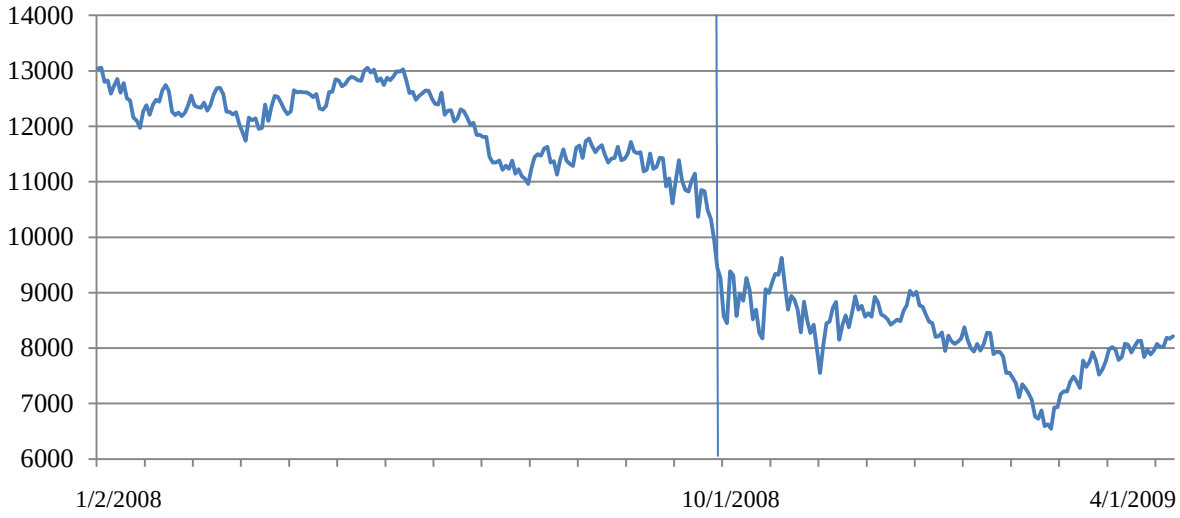

Figure 2A. FHFA House Price Index, All Transactions

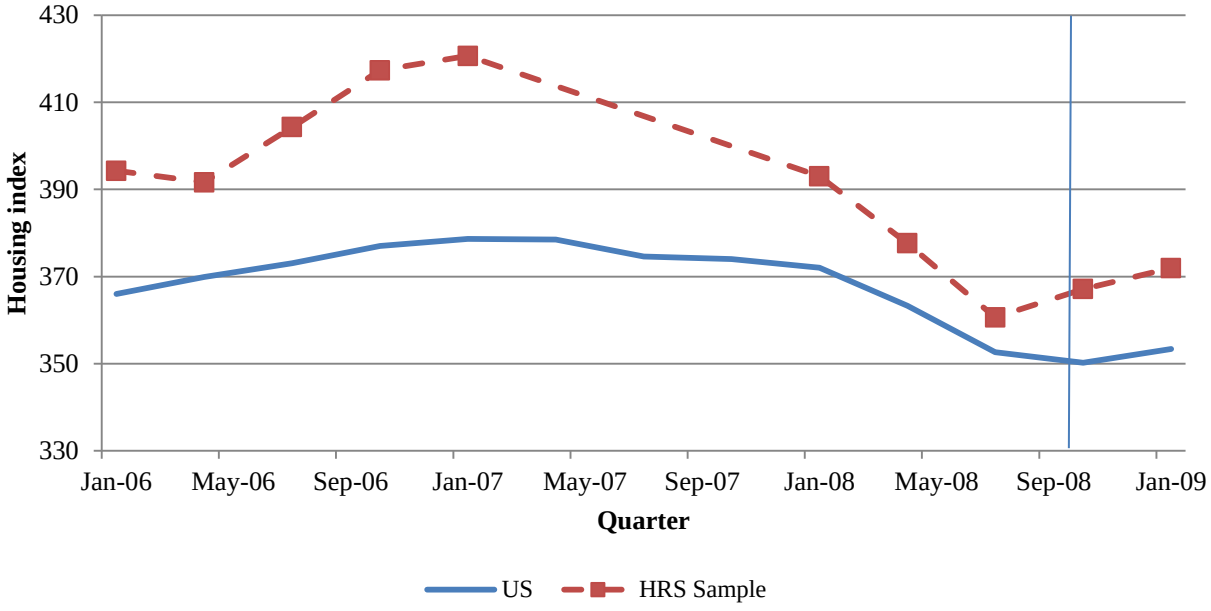

Figure 2B. FHFA House Price Index, Purchase Only, Adjusted

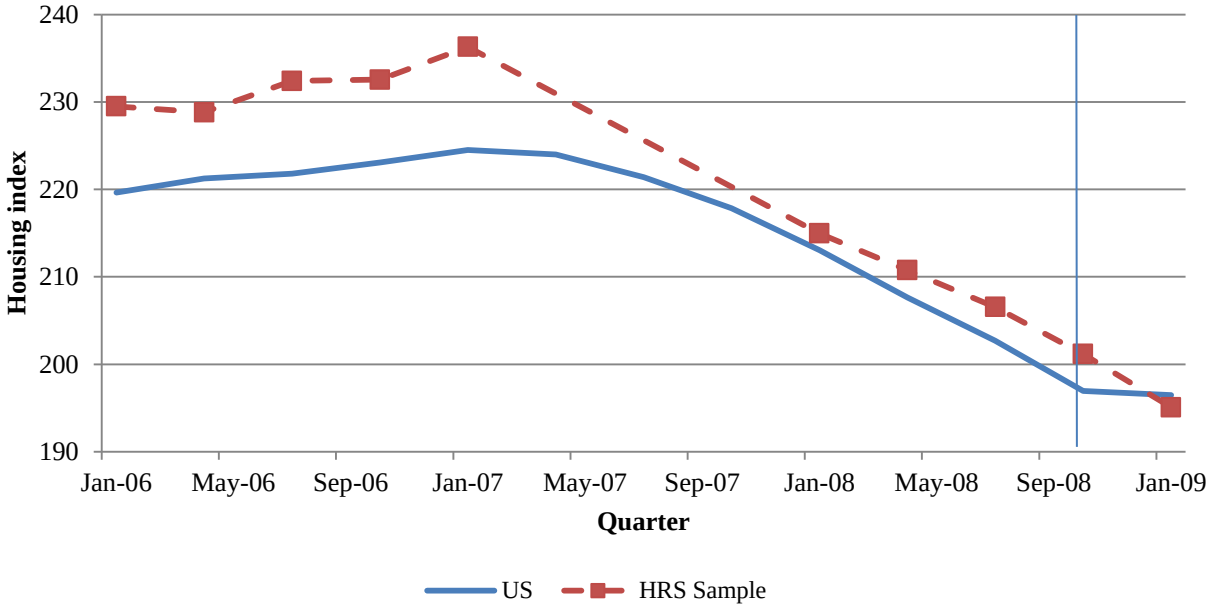

Table 1. Descriptive Statistics for HRS Financial Respondents

	Full Sample (n=10,020)		Interviewed Post-Crash (n=815)	
	2006	2008	2006	2008
Non-housing wealth (2008 $)	344,491	292,861	506,150	271,484
	(1,912,915)	(974,735)	(3,863,789)	(739,056)
Financial wealth (2008 $)	147,471	133,699	140,331	113,830
	(1,115,278)	(546,440)	(634,355)	(394,535)
CES-D	1.55	1.50	1.67	1.62
Feels depressed	16.8%	13.4%	18.2%	16.6%
Rx for depression	15.3%	15.5%	16.7%	17.6%
Self-Reported (SR) health (1=exc., 5=poor)	2.83	2.94	2.81	2.94
Poor/fair SR health	28.3%	30.6%	27.5%	33.4%
Excellent/very good SR health	40.3%	37.3%	40.5%	38.7%
Age	68.73	70.7	65.99	68.31
	(9.87)	(9.89)	(9.67)	(9.73)
Female	0.57	0.57	0.59	0.59
Black	0.16	0.16	0.15	0.15
Hispanic	0.09	0.09	0.10	0.10
Other race	0.04	0.04	0.06	0.06
Less than high school	0.21	0.21	0.21	0.21
Some college	0.04	0.04	0.05	0.05
College degree	0.12	0.12	0.12	0.12
Advanced degree	0.10	0.10	0.10	0.10
Other education	0.003	0.003	0.007	0.007
Education missing	0.001	0.001	0.002	0.002
Never married	0.04	0.05	0.05	0.05
Widowed	0.28	0.30	0.24	0.27
Separated/divorced	0.17	0.17	0.18	0.18
Marital status missing	0	0.0004	0	0.002
Household income (in 2008 $)	60,080	62,102	70,812	75,348
	(184,595)	(610,002)	(120,803)	(272,277)
No. persons in household	2.00	1.98	2.13	2.09
	(1.14)	(1.12)	(1.28)	(1.34)
Cognitive impairment category	1.23	1.26	1.22	1.25
	(0.50)	(0.54)	(0.50)	(0.54)

Notes: The sample size in the column header row refers to the number of respondents with non-missing data on wealth. Missing data on health measures results in fewer observations.

Table 2. Changes in Non-Housing Wealth from 2006 to 2008

	Average Crash Effect		Crash Effects by Stockholder Status		Crash Effects by Level of Stock	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-238,309**	-43,798				
	(107,575)	(39,183)				
Post *Stock			-511,926***	-81,489*		
			(129,554)	(47,409)		
Post *No stock			-36,560	-16,653		
			(119,993)	(43,642)		
Post *High stock					-1,084,033***	-223,581***
					(159,783)	(59,309)
Post *Low stock					76,963	55,413
					(161,416)	(58,534)
Post *No stock					-38,264	-17,078
					(119,772)	(43,607)
$\chi^2_{(\text{Post Stock=Post No})}$			14.32 (0.00)	1.99 (0.16)		
$\chi^2_{(\text{Post Low=Post No})}$					0.53 (0.47)	1.59 (0.21)
$\chi^2_{(\text{Post Hi =Post No})}$					44.68 (0.00)	12.65 (0.00)
Exclude top and bottom 1% of households	No	Yes	No	Yes	No	Yes
Sample Size	10,020	9,821	10,020	9,821	10,020	9,821

Notes: Samples are restricted to non-proxy interviews, financial respondents, and those born before 1954. Where indicated, the sample excludes households in the top and bottom 1% of the assets distribution in 2006. "Post" equals 1 for respondents interviewed in or after October 2008 and 0 for all other 2008 respondents and all 2006 respondents. In columns (3) and (4), stockholders are defined as having any non-zero amount of stock or IRA holdings in 2006. In columns (5) and (6), high stock is defined as having stock/IRA wealth equal to or above the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, low stock is defined as having stock/IRA wealth below the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, and no stock is defined as having no stock/IRA wealth in 2006. All models include controls for changes in marital status, work status, retirement status, changes in household income and its square, changes in household size, the onset of new physical health conditions, changes in cognitive function, the number of months between waves and its square, and changes in the season of interview. Statistical significance is indicated by *** for 0.01 level, ** for 0.05 level, and * for 0.10 level; *p*-values from chi-square tests are presented in parentheses.

Table 3. Falsification Test of Changes in Housing Wealth from 2006 to 2008

	Average Crash Effect		Crash Effects by Stockholder Status		Crash Effects by Level of Stock	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	52,120	38,679				
	(58,286)	(54,014)				
Post *Stock			30,708	12,453		
			(70,244)	(65,358)		
Post *No stock			67,907	57,567		
			(65,060)	(60,165)		
Post *High stock					23,274	-1,208
					(86,794)	(81,830)
Post *Low stock					38,360	25,616
					(87,681)	(80,760)
Post *No stock					67,885	57,526
					(65,060)	(60,165)
$\chi^2_{(\text{Post Stock=Post No})}$			0.30 (0.59)	0.51 (0.48)		
$\chi^2_{(\text{Post Low=Post No})}$					0.12 (0.73)	0.16 (0.69)
$\chi^2_{(\text{Post Hi =Post No})}$					0.28 (0.60)	0.54 (0.46)
Exclude top and bottom 1% of households	No	Yes	No	Yes	No	Yes
Sample Size	10,020	9,821	10,020	9,821	10,020	9,821

Notes: Samples are restricted to non-proxy interviews, financial respondents, and those born before 1954. Where indicated, the sample excludes households in the top and bottom 1% of the assets distribution in 2006. "Post" equals 1 for respondents interviewed in or after October 2008 and 0 for all other 2008 respondents and all 2006 respondents. In columns (3) and (4), stockholders are defined as having any non-zero amount of stock or IRA holdings in 2006. In columns (5) and (6), high stock is defined as having stock/IRA wealth equal to or above the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, low stock is defined as having stock/IRA wealth below the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, and no stock is defined as having no stock/IRA wealth in 2006. All models include controls for changes in marital status, work status, retirement status, changes in household income and its square, changes in household size, the onset of new physical health conditions, changes in cognitive function, the number of months between waves and its square, and changes in the season of interview. Statistical significance is indicated by *** for 0.01 level, ** for 0.05 level, and * for 0.10 level; *p*-values from chi-square tests are presented in parentheses.

Table 4. Placebo Effects on Changes in Non-Housing Wealth from 2004 to 2006

	Average Crash Effect		Crash Effects by Stockholder Status		Crash Effects by Level of Stock	
	(1)	(2)	(3)	(4)	(5)	(6)
Fake Post	133,220*	6,422				
	(68,296)	(34,677)				
Fake Post *Stock			420,932***	96,007*		
			(101,908)	(52,096)		
Fake Post *No stock			-78,004	-57,959		
			(88,004)	(44,529)		
Fake Post *High stock					965,278***	278,050***
					(140,500)	(72,507)
Fake Post *Low stock					-149,816	-88,357
					(143,781)	(72,956)
Fake Post *No stock					-78,364	-58,097
					(87,863)	(44,499)
$\chi^2_{\text{(Post Stock=Post No)}}$			14.45 (0.00)	5.31 (0.02)		
$\chi^2_{\text{(Post Low=Post No)}}$					0.19 (0.67)	0.13 (0.72)
$\chi^2_{\text{(Post Hi =Post No)}}$					40.99 (0.00)	16.12 (0.00)
Exclude top and bottom 1% of households	No	Yes	No	Yes	No	Yes
Sample Size	9,828	9,629	9,828	9,629	9,828	9,629

Notes: Samples are restricted to non-proxy interviews, financial respondents, and those born before 1954. Where indicated, the sample excludes households in the top and bottom 1% of the assets distribution in 2006. "Fake Post" equals 1 for 2006 respondents whose 2008 interview took place during or after October 2008 and 0 for all other 2006 respondents and all 2004 respondents. In columns (3) and (4), stockholders are defined as having any non-zero amount of stock or IRA holdings in 2006. In columns (5) and (6), high stock is defined as having stock/IRA wealth equal to or above the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, low stock is defined as having stock/IRA wealth below the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, and no stock is defined as having no stock/IRA wealth in 2006. All models include controls for changes in marital status, work status, retirement status, changes in household income and its square, changes in household size, the onset of new physical health conditions, changes in cognitive function, the number of months between waves and its square, and changes in the season of interview. Statistical significance is indicated by *** for 0.01 level, ** for 0.05 level, and * for 0.10 level; *p*-values from chi-square tests are presented in parentheses.

Table 5. Changes in Health from 2006 to 2008

	CES-D		Depression		Rx for Depression	
	(1)	(2)	(3)	(4)	(5)	(6)
Post *High stock	0.195	0.189	0.082**	0.073*	0.062**	0.063**
	(0.169)	(0.174)	(0.036)	(0.038)	(0.026)	(0.027)
Post *Low stock	-0.335**	-0.328*	0.013	0.013	0.041	0.040
	(0.171)	(0.171)	(0.037)	(0.037)	(0.026)	(0.027)
Post *No stock	-0.024	-0.036	0.009	0.006	0.020	0.011
	(0.127)	(0.128)	(0.027)	(0.028)	(0.020)	(0.020)
$\chi^2_{\text{(Post Low=Post No)}}$	3.43 (0.06)	3.0 (0.08)	0.01 (0.91)	0.03 (0.86)	0.67 (0.41)	1.17 (0.28)
$\chi^2_{\text{(Post Hi =Post No)}}$	1.75 (0.19)	1.73 (0.19)	4.20 (0.04)	3.29 (0.07)	2.70 (0.10)	3.85 (0.05)
Exclude top and bottom 1% of households	No	Yes	No	Yes	No	Yes
Sample Size	9,871	9,677	10,005	9,806	9,994	9,795
	Self-Reported Health 1 (excellent) thru 5 (poor)		Fair/Poor Health		Excellent/Very Good Health	
	(7)	(8)	(9)	(10)	(11)	(12)
Post *High stock	0.184**	0.203**	0.062*	0.058	-0.074*	-0.090**
	(0.078)	(0.080)	(0.038)	(0.039)	(0.041)	(0.043)
Post *Low stock	0.154**	0.162**	0.071*	0.070*	-0.024	-0.028
	(0.079)	(0.080)	(0.038)	(0.038)	(0.042)	(0.042)
Post *No stock	0.032	0.036	0.055*	0.052*	0.067**	0.062**
	(0.059)	(0.059)	(0.028)	(0.029)	(0.031)	(0.031)
$\chi^2_{\text{(Post Low=Post No)}}$	2.50 (0.11)	2.63 (0.10)	0.18 (0.67)	0.25 (0.62)	4.93 (0.03)	4.77 (0.03)
$\chi^2_{\text{(Post Hi =Post No)}}$	3.92 (0.05)	4.47 (0.03)	0.04 (0.84)	0.03 (0.86)	12.1 (0.00)	13.3 (0.00)
Exclude top and bottom 1% of households	No	Yes	No	Yes	No	Yes
Sample Size	9,999	9,800	9,999	9,800	9,999	9,800

Notes: Samples are restricted to non-proxy interviews, financial respondents, and those born before 1954. Where indicated, the sample excludes households in the top and bottom 1% of the assets distribution in 2006. "Post" equals 1 for respondents interviewed in or after October 2008 and 0 for all other 2008 respondents and all 2006 respondents. High stock is defined as having stock/IRA wealth equal to or above the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, low stock is defined as having stock/IRA wealth below the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, and no stock is defined as having no stock/IRA wealth in 2006. All models include controls for changes in marital status, work status, retirement status, changes in household income and its square, changes in household size, the onset of new physical health conditions, changes in cognitive function, the number of months between waves and its square, and changes in the season of interview. Statistical significance is indicated by *** for 0.01 level, ** for 0.05 level, and * for 0.10 level; *p*-values from chi-square tests are presented in parentheses.

Table 6. IV First Differences Models:
Effects of Changes in Non-Housing Wealth on Changes in Health

	CES-D		Depression		Rx for Depression	
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Housing Wealth (10,000s of $)	-0.0026*	-0.0146*	-0.0007**	-0.0027*	-0.0004*	-0.0017
	(0.0014)	(0.0078)	(0.0003)	(0.0016)	(0.0002)	(0.0011)
Exclude top/bottom 1% of households	No	Yes	No	Yes	No	Yes
First stage F-statistic	18.39	5.68	18.68	6.28	18.65	6.28
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Test of over-identifying restrictions	3.48	1.77	0.219	0.56	2.79	3.37
	(0.18)	(0.41)	(0.90)	(0.76)	(0.25)	(0.19)
Sample Size	9,871	9,677	10,005	9,806	9,994	9,795
	Self-Reported Health 1 (excellent) thru 5 (poor)		Fair/Poor Health		Excellent/Very Good Health	
	(7)	(8)	(9)	(10)	(11)	(12)
Non-Housing Wealth (10,000s of $)	-0.0012*	-0.0050	-0.0002	-0.0007	0.0008**	0.0036**
	(0.0007)	(0.0033)	(0.0003)	(0.0015)	(0.0004)	(0.0018)
Exclude top/bottom 1% of households	No	Yes	No	Yes	No	Yes
First-stage F-statistic	18.83	6.34	18.83	6.34	18.83	6.34
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Test of over-identifying restrictions	4.25	5.71	5.19	4.94	7.44	8.29
	(0.12)	(0.06)	(0.07)	(0.08)	(0.02)	(0.02)
Sample Size	9,999	9,800	9,999	9,800	9,999	9,800

Notes: Samples are restricted to non-proxy interviews, financial respondents, and those born before 1954. Where indicated, the sample excludes households in the top and bottom 1% of the assets distribution in the base year. All models include controls for changes in marital status, work status, retirement status, changes in household income and its square, changes in household size, the onset of new physical health conditions, changes in cognitive function, the number of months between waves and its square, and changes in the season of interview. The first-stage instruments are *POST*NOSTOCK*, *POST*HIGHSTOCK* and *POST*LOWSTOCK*, as defined in the text and the notes to Table 5. Statistical significance is indicated by *** for 0.01 level, ** for 0.05 level, and * for 0.10 level; *p*-values from chi-square tests are presented in parentheses.

Table 7. Placebo Effects on Changes in Health from 2004 to 2006

	CES-D		Depression		SR Health 1 (excellent) thru 5 (poor)		Fair/Poor Health		Excellent/Very Good Health	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fake Post *High stock	-0.110	-0.138	-0.047	-0.043	-0.003	-0.046	-0.023	-0.022	-0.030	-0.013
	(0.143)	(0.148)	(0.031)	(0.032)	(0.067)	(0.069)	(0.031)	(0.032)	(0.036)	(0.037)
Fake Post *Low stock	0.023	0.013	-0.064**	-0.066**	-0.016	-0.002	0.001	0.009	0.032	0.027
	(0.145)	(0.148)	(0.032)	(0.032)	(0.068)	(0.069)	(0.032)	(0.032)	(0.037)	(0.037)
Fake Post *No stock	-0.243***	-0.232***	-0.047**	-0.045**	0.012	0.007	-0.014	-0.020	-0.029	-0.028
	(0.089)	(0.091)	(0.020)	(0.020)	(0.042)	(0.042)	(0.020)	(0.020)	(0.022)	(0.023)
$\chi^2_{\text{(Post Low=Post No)}}$	2.50	2.05	0.20	0.34	0.13	0.01	0.16	0.58	2.12	1.63
	(0.11)	(0.15)	(0.65)	(0.56)	(0.72)	(0.91)	(0.69)	(0.45)	(0.15)	(0.20)
$\chi^2_{\text{(Post Hi =Post No)}}$	0.63	0.30	0.00	0.00	0.04	0.45	0.06	0.00	0.00	0.12
	(0.43)	(0.58)	(0.99)	(0.96)	(0.84)	(0.50)	(0.80)	(0.95)	(0.97)	(0.73)
Exclude top and bottom 1% of households	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Sample Size	9,672	9,477	9,664	9,469	9,810	9,612	9,810	9,612	9,810	9,612

Notes: Samples are restricted to non-proxy interviews, financial respondents, and those born before 1954. Where indicated, the sample excludes households in the top and bottom 1% of the assets distribution in 2006. "Fake Post" equals 1 for 2006 respondents whose 2008 interview took place during or after October 2008 and 0 for all other 2006 respondents and all 2004 respondents. High stock is defined as having stock/IRA wealth equal to or above the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, low stock is defined as having stock/IRA wealth below the median level of stocks/IRA wealth among respondents who own stocks/IRAs in 2006, and no stock is defined as having no stock/IRA wealth in 2006. All models include controls for changes in marital status, work status, retirement status, changes in household income and its square, changes in household size, the onset of new physical health conditions, changes in cognitive function, the number of months between waves and its square, and changes in the season of interview. Statistical significance is indicated by *** for 0.01 level, ** for 0.05 level, and * for 0.10 level; *p*-values from chi-square tests are presented in parentheses.

Appendix Table 1. Changes in Financial Wealth from 2006 to 2008

	Average Crash Effect		Crash Effects by Stockholder Status		Crash Effects by Level of Stock	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	4,937*	-20,027				
	(60,610)	(20,050)				
Post *Stock			-44,186	-41,200*		
			(73,040)	(24,259)		
Post *No stock			41,457	-4,778		
			(67,650)	(22,331)		
Post *High stock					-159,434*	-113,990***
					(90,228)	(30,348)
Post *Low stock					74,443	28,930
					(91,150)	(29,951)
Post *No stock					40,814	-4,995
					(67,634)	(22,313)
$\chi^2_{\text{(Post Stock=Post No)}}$			1.45 (0.23)	2.40 (0.12)		
$\chi^2_{\text{(Post Low=Post No)}}$					0.14 (0.71)	1.33 (0.25)
$\chi^2_{\text{(Post Hi =Post No)}}$					5.14 (0.02)	13.46 (0.00)
Exclude top and bottom 1% of households	No	Yes	No	Yes	No	Yes
Sample Size	10,020	9,821	10,020	9,821	10,020	9,821

Notes: Samples are restricted to non-proxy interviews, financial respondents, and those born before 1954. Where indicated, the sample excludes households in the top and bottom 1% of the assets distribution in 2006. "Post" equals 1 for respondents interviewed in or after October 2008 and 0 for all other 2008 respondents and all 2006 respondents. In columns (3) and (4), stockholders are defined as having any non-zero amount of stock or IRA holdings in 2006. In columns (5) and (6), high stock is defined as having stock equal to or above the median level of stocks among respondents who own stock in 2006. All models include controls for changes in marital status, work status, retirement status, changes in household income and its square, changes in household size, the onset of new physical health conditions, changes in cognitive function, the number of months between waves and its square, and changes in the season of interview. Statistical significance is indicated by *** for 0.01 level, ** for 0.05 level, and * for 0.10 level; *p*-values from chi-square tests are presented in parentheses.

Appendix Table 2. Placebo Effects on Changes in Financial Wealth from 2004 to 2006

	Average Crash Effect		Crash Effects by Stockholder Status		Crash Effects by Level of Stock	
	(1)	(2)	(3)	(4)	(5)	(6)
Fake Post	6,405	-5,844				
	(42,444)	(18,811)				
Fake Post *Stock			29,065	26,146		
			(63,379)	(28,265)		
Fake Post *No stock			-10,231	-28,834		
			(54,732)	(24,159)		
Fake Post *High stock					125,967	71,639*
					(87,509)	(39,360)
Fake Post *Low stock					-72,536	-19,927
					(89,553)	(39,604)
Fake Post *No stock					-10,295	-28,868
					(54,724)	(24,156)
$\chi^2_{\text{(Post Stock=Post No)}}$			0.23 (0.63)	2.30 (0.13)		
$\chi^2_{\text{(Post Low=Post No)}}$					0.36 (0.55)	0.04 (0.84)
$\chi^2_{\text{(Post Hi =Post No)}}$					1.80 (0.18)	4.89 (0.03)
Exclude top and bottom 1% of households	No	Yes	No	Yes	No	Yes
Sample Size	9,828	9,629	9,828	9,629	9,828	9,629

Notes: Samples are restricted to non-proxy interviews, financial respondents, and those born before 1954. Where indicated, the sample excludes households in the top and bottom 1% of the assets distribution in 2006. "Fake Post" equals 1 for 2006 respondents whose 2008 interview took place during or after October 2008 and 0 for all other 2006 respondents and all 2004 respondents. In columns (3) and (4), stockholders are defined as having any non-zero amount of stock or IRA holdings in 2006. In columns (5) and (6), high stock is defined as having stock equal to or above the median level of stocks among respondents who own stock in 2006. All models include controls for changes in marital status, work status, retirement status, changes in household income and its square, changes in household size, the onset of new physical health conditions, changes in cognitive function, the number of months between waves and its square, and changes in the season of interview. Statistical significance is indicated by *** for 0.01 level, ** for 0.05 level, and * for 0.10 level; *p*-values from chi-square tests are presented in parentheses.