Liquidity vs. Wealth: How Financial Resources Shape Responses to Health Shocks Among Older Workers

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Abstract

Using longitudinal data from the Health and Retirement Study (1992–2022), we examine how financial resources shape older workers' responses to health shocks. Focusing on workers aged 50–61 and using staggered difference-in-differences methods, we first confirm that health shocks reduce earnings, accelerate retirement, and lead to earlier Social Security claiming. We then investigate heterogeneity by wealth, finding that workers with limited liquid assets experience significantly worse outcomes. In contrast, we find little evidence of differential effects by total or non-housing wealth. These findings highlight the importance of liquidity, rather than overall wealth, in shaping workers' responses to health shocks.

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1 Introduction

Over the past several decades, older workers have come to represent a growing share of the U.S. labor force (U.S. Bureau of Labor Statistics, 2025). Population aging, increases in life expectancy, and cohort-specific changes in labor force attachment have all contributed to rising employment rates at older ages (Wilkins, 2025). At the same time, institutional features of the U.S. retirement system—most notably the gradual increase in the Social Security full retirement age and the decline of employer-provided retiree benefits—have lengthened the period during which many individuals rely on labor earnings as their primary source of income. As a result, shocks that disrupt employment late in the working life can have outsized consequences, affecting not only current well-being but also the accumulation of retirement resources and the timing of labor market exit (Chan & Stevens, 2004; Mahmoudi, 2023).

Health shocks are a particularly salient source of risk for older workers (Coile, 2004; J. P. Smith, 1999; Trevisan & Zantomio, 2016). Unlike younger individuals, workers approaching retirement face age-related declines in physical resilience, a reduced capacity to adjust by retraining or switching occupations, and a shorter remaining horizon over which to recover lost earnings. As a result, adverse health events late in the working life can have persistent effects on individuals' economic trajectories, as limited scope for adjustment amplifies the labor market consequences of even temporary declines in health.

Health shocks also differ in important ways from other commonly studied economic shocks. Prior to age 65, access to health insurance in the United States remains closely tied to employment, meaning that a negative health event can simultaneously increase medical needs while weakening the employment relationship that provides insurance coverage. Moreover, health shocks represent a direct hit to human capital: they may reduce both the quantity and the quality of work that individuals are able to perform, often in ways that are persistent rather than transitory. These risks are especially consequential given that many older households enter this period with limited liquid wealth. These factors suggest that the consequences of health shocks for older workers are likely to be heterogeneous, shaped

by demographic characteristics and, in particular, by the financial resources workers have available to absorb a shock.

In this paper, we explore heterogeneity in the impact of health shocks on older workers, focusing specifically on the role of financial resources. First, we construct a matched sample of workers in which members of the treated group, otherwise similar in observables to members of a comparison group, suffer a health shock while those in the comparison group do not, and we use a staggered difference-in-differences design to estimate the baseline effects of health shocks. These estimates serve to validate and update the existing literature on the effect of health shocks. Next, we use re-weighting methods to compare and contrast the responses of workers with varying degrees of financial resources.

Our primary contribution is to distinguish between the composition and level of household wealth by progressively disaggregating assets. The results suggest that liquidity plays a central role in shaping labor market adjustment after a health shock. Individuals in households with above-median liquid assets are significantly less likely to deviate from their pre-shock work expectations, retire, or claim Social Security benefits following a shock. In contrast, similar patterns do not emerge when wealth is measured more broadly using total net worth or non-housing wealth.

Our paper contributes to two main strands of literature. First, we add to a large body of work examining the labor market consequences of health shocks by focusing explicitly on heterogeneity in the impacts among older workers prior to Social Security and Medicare eligibility. Although previous studies document persistent reductions in employment and earnings, less is known about how these effects vary with the amount and composition of pre-shock wealth. We address this gap among older workers prior to Social Security and Medicare eligibility, extending the analysis beyond labor market outcomes to consider broader measures of well-being.

Second, our findings contribute to the literature on retirement behavior and household finance by highlighting the role of asset liquidity in shaping responses to late-career shocks.

Our results suggest that aggregate measures of wealth may obscure differences in workers' financial capacity to adjust to adverse health events. This distinction informs policy debates surrounding retirement security and the design of social insurance programs for an aging workforce.

This paper proceeds as follows. In section 2, we outline the previous literature on health shocks and further clarify our contribution. In section 3, we describe our data and the sample restrictions we apply. Section 4 outlines our methods, including the construction of our analytic sample, our difference-in-differences design, and re-weighting techniques. The results are presented in section 5. Section 6 considers the economic significance of our findings. Section 7 concludes.

2 Literature Review

There is a substantial body of work on the labor market consequences of health shocks. Across countries and institutional settings, a health shock is largely associated with reduced labor supply on the extensive and intensive margins, as well as decreased earnings (Bound et al. (1999), Currie & Madrian (1999), Au et al. (2005), Disney et al. (2006)). The negative effects on employment and income can persist for several years, making recovery particularly difficult for older workers who may be near the end of their working lives (García-Gómez et al. (2013), Jones et al. (2020), Dobkin et al. (2018)).

A health shock can also trigger broader physical and mental health declines. Trevisan & Zantomio (2016) find that health shocks increase functional difficulties and depressive symptoms. In addition, subjective longevity expectations — which strongly predict future mortality (V. K. Smith et al. (2001)) — decline immediately following a shock (McGarry (2022)). However, this effect may be short lived; Baji & Biro (2018) find that reported probabilities tend to revert to their pre-shock trajectories over time.

Especially for older workers in the United States, the labor supply response to a health

shock is shaped by age-eligibility thresholds that independently affect retirement timing. While labor supply declines sharply at ages tied to Social Security and private pension eligibility, health also plays an independent role in accelerating retirement (Dwyer & Mitchell (1999), McGarry (2004), Gorry (2024)). For individuals under 65 years old, their ability to leave the labor force may be constrained by their need to retain employer-sponsored health insurance until reaching the Medicare threshold (Strumpf, 2010). Consequently, older workers may need to rely on their own private retirement assets to bridge the gap between a health shock and the eventual receipt of public benefits.

While existing research tracks how health shocks vary by socioeconomic demographics, heterogeneity across distinct subpopulations remains under-explored. Heterogeneity analyses often emphasize gender differences, especially when considering the spillover effects of a health shock and household-level responses (Currie & Madrian (1999), Coile (2004)). Existing studies have also considered differences by education, generally concluding that less educated individuals suffer more negative effects after a health shock.[1] Given the strong positive association between health and wealth, some papers have studied heterogeneity by pre-shock income levels.[2] Yet, how these impacts vary by the specific amount or *type* of wealth remains unexamined. Furthermore, whereas the literature heavily prioritizes labor supply outcomes, a smaller set of studies has considered expenditure and consumption.[3] For example, Hurd & Rohwedder (2013) find that spending declines at retirement are concentrated among low-wealth households — a pattern they attribute mainly to early retirement driven by poor health. This finding suggests an important role for pre-shock wealth in shap-

[1]Lundborg et al. (2015) find that workers with low educational attainment experience greater relative decreases to income immediately after a health shock and in the long-run. Heinesen & Kolodziejczyk (2013) and Taskila-Brandt et al. (2004) find that a health shock leads to a higher risk of labor market exit for less educated individuals.

[2]J. P. Smith (1999) finds that higher-income households experience greater wealth losses; García-Gómez et al. (2013) using Dutch data, finds that lower-income households experience worse labor market consequences.

[3]Research on the impact of health shocks on asset portfolios has tended to focus on post-retirement shocks (e.g. Coile & Milligan (2009)).

ing the financial consequences of health-driven retirement, but we expand the scope further to evaluate multiple measures of well-being as well as different types of wealth.

3 Data

3.1 Dataset

We use all available waves of the Health and Retirement Study (HRS), a nationally representative biennial longitudinal panel survey of households aged 50 and above (1992-2022). Since its inception, the study has collected individual-level information on health, use of health services, employment transitions, retirement decisions, and pension arrangements, making it one of the most important data sources on older age U.S. households. Due to its longitudinal design, in which the sample is periodically replenished with new cohorts, it is particularly well-suited for studying issues that affect the lives and health of older workers. The sampling design selects individuals based on their birth year, and also interviews each respondent's spouse or partner regardless of age. Hereafter we use "spouse" to refer to married or unmarried partners. We include both household members in our analyses if they satisfy our eligibility criteria, outlined below.

3.2 Variables

3.2.1 Health shock

The literature employs many different ways of defining a health shock, but they all represent a negative, unplanned event that impacts an individual's health. Definitions used in past work include hospitalizations (García-Gómez et al. (2013), Dobkin et al. (2018)), sudden illness or injury (Doyle (2011), Fadlon & Nielsen (2021)), drops in self-reported health (Bound et al. (1999), Disney et al. (2006)), the onset of chronic conditions (García-Gómez (2011), Heinesen & Kolodziejczyk (2013)), or a combination of these events (Trevisan & Zantomio

(2016)). While some studies use administrative data to confirm health shocks, most rely on survey instruments. This literature frequently contrasts subjective self-rated health against objective health markers, both of which are collected through survey questionnaires. We follow the health markers approach to leverage greater reporting accuracy observed for severe, acute health events.[4] We define a health shock as the occurrence of any of the following since the previous survey wave: heart problem, stroke, or at least two nights spent in a hospital.[5] Spending at least two nights in the hospital is the most common of the three criteria; 85 percent of our treated group spent multiple nights in a hospital, while 27 percent experienced a heart problem, and five percent experienced a stroke. These categories are not mutually exclusive, and consequently the percentages sum to more than 100. In additional tests, we explore the robustness of our findings to alternative definitions.

3.2.2 Outcomes

Our outcome variables span health and well-being, financial circumstances, and labor market activity — all of which are likely to be affected by a health shock. We first examine several health outcomes, including activity limitations, whether health limits the ability to work, depressive symptoms, doctor visits, and out-of-pocket medical expenditures. We measure activity limitations through an indicator for any difficulty with activities of daily living (ADLs), encompassing five essential tasks: walking across a room, dressing, bathing, eating, getting in and out of bed, and using the toilet. Depressive symptoms are captured using the eight-item Center for Epidemiologic Studies Depression Scale (CES-D-8). We construct a binary indicator for depression using a threshold of four or more symptoms on the CES-D-8 scale.[6]

[4]See Bound (1991) and Baker et al. (2004) on the limitations of self-rated health in survey data; Baker et al. (2004) find that reporting error decreases with severity of the health condition.

[5]A heart problem includes a heart attack, coronary heart disease, angina, congestive heart failure, or other heart condition; a stroke includes possible strokes or transient ischemic attacks; the overnight hospital stays may or may not be consecutive.

[6]The CES-D-8 is better suited as a screening tool for the likelihood of clinical depression than as a continuous measure of depression severity. Wallace et al. (2000) recommend a cutoff point of 4 to best align with clinical diagnoses of depression.

On the financial side, we track individual earnings, income from Social Security, and pension or annuity income. Since our sample consists of pre-Medicare individuals, only some of the respondents become eligible to claim Social Security retirement benefits. Therefore, we consider two measures of Social Security income: first, Social Security Disability Insurance (SSDI) or Supplemental Security Income (SSI) only, and second, any income from Social Security, including retirement. For pension or annuity income, we use an indicator equal to 1 if the household receives any income from a pension or annuity, and 0 otherwise.

We capture labor market activity through reported employment status, hours worked, and job instability (measured via employer separations). To analyze retirement dynamics, we consider three outcomes. The first two come from self-reported retirement status: an indicator for being completely retired, and a broader indicator for being either completely or partially retired.[7] The third outcome captures unexpected early retirement, defined as leaving the labor force after the shock but before the age at which the respondent had planned, before the shock, to stop working. Incorporating pre-shock retirement expectations helps differentiate forced exits precipitated by a health shock from planned exits.[8]

Finally, a health shock may revise workers' beliefs about their future health and longevity, which shapes downstream labor supply and savings decisions. We include subjective expectations about survival probabilities to age 75.[9]

[7]The retirement outcomes are primarily based on responses to: "At this time do you consider yourself partly retired, completely retired, or not retired at all?" If the response to this question is unavailable, we use a multi-response employment status question. Under this alternative, complete retirement requires "retired" to be the only status selected, whereas complete or partial retirement allows "retired" to be selected alongside other statuses.

[8]Planned retirement age comes from the respondent's stated retirement plans in wave $t-1$, or, for those who report having no plans, from a follow-up question asking when they expect to stop working. Results are similar when we use only the stated retirement plans and drop the follow-up responses.

[9]Respondents are asked, "What is the percent chance that you will live to be 75 or more?"

4 Methods

4.1 Construction of analytic sample

To compute our baseline estimates, we construct a matched sample of workers who do and do not experience a health shock. Conceptually, we seek to compare a group of workers who experience a health shock with an otherwise identical group of workers who do not. Because we utilize longitudinal data, workers may experience health shocks in different survey waves, implying a staggered treatment design in which treatment timing varies across individuals.

A recent literature on staggered treatment adoption highlights the challenges associated with using longitudinal data in which already treated units serve as controls (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Roth et al., 2023; Sun & Abraham, 2021; Borusyak et al., 2024; De Chaisemartin & d'Haultfoeuille, 2020). To avoid the comparisons that this literature identifies as problematic, we follow Deshpande & Li (2019), Cengiz et al. (2019), Butters et al. (2022), and Wing et al. (2024) in creating a "stacked" sample. Specifically, we create five-wave event windows (spanning 8 years) centered around a potential health shock, as defined in Section 3.2.1, in the relative third wave, t, requiring that individuals experienced no health shock in either of the two preceding waves, $t-1$ and $t-2$. For individuals observed more than five consecutive survey waves, we duplicate their records so that multiple event windows can be included, up to their first observed health shock (if any).

Since our focus is on older workers who are not yet eligible for full Social Security benefits or Medicare, we require individuals to be at least 50 years old when reporting the health shock, and no more than 64 years old during the post-treatment period. This results in a treated sample consisting of individuals aged 50-61 years old at the time of a health shock. We further require respondents to work for pay and have positive earnings throughout the pre-treatment period.

Each event window can be assigned to one of three groups: treated, never treated, and

not-yet treated.[10] The treated group consists of individuals who experienced a health shock in t, and not in $t-2$ or $t-1$. Event windows serving as control are either never treated or not-yet treated. The not-yet treated group consists of individuals who appear in the treated group in a later wave, but who may be used as controls before their treated event window.[11] Individuals can contribute multiple control group windows to the final dataset, but they can only contribute one treated window. In the final stacked estimation sample, the individual-wave observations range from 1992 to 2022, capturing health shocks from 1996 to 2018.

One potential concern with the stacked difference-in-differences approach is that the relative representation of treated and control observations differs across event windows. While all individuals are subject to the same five-wave event-window construction and eligibility criteria, the number of treated and control observations varies across cohorts. As a result, some cohorts receive greater implicit weight in the pooled estimation than others. To correct for this imbalance, we follow Wing et al. (2024) and apply cohort-specific sample weights that align the distribution of treated and control observations within each event window with their distribution in the full stacked sample. These weights ensure that the estimated treatment effect reflects a consistent comparison between treated and control units across event windows. In practice, the corrective weights have little effect on the results. We therefore present unweighted results in the main text for simplicity and ease of interpretation and report weighted estimates as a robustness check.

Next, to ensure that the group of workers who experiences a health shock is as similar as possible to the group of workers who do not, we apply a weighting scheme to the comparison

[10]The never and not-yet treated groups can include both members of couple households when they meet our sample criteria, however, we drop all event windows after a spouse or partner has experienced a health shock. This ensures that households can contribute no more than a single event window to the treated group. If both members of a household experience health shocks in the same wave, both individuals are dropped from the sample.

[11]The event window for an individual in the not-yet treated group is distinct from the same individual's event window in the treated group. That is, $t+2$ in the not-yet treated group does not correspond to the same survey wave as $t-2$ in the treated group. Given the age restrictions we impose, there are very few not-yet treated observations.

group. For each wave of a health shock, we estimate propensity scores using logistic regression. Our model includes age, sex, race, marriage status, education, earnings, self-reported health, and type of health insurance, all measured in $t-1$.[12] We weight the control observations according to their propensity score, using inverse probability of treatment weighting (IPTW), and winsorize at the 1% level.

Table 1 shows summary statistics for the variables used in the matching model before and after re-weighting. Whereas the unweighted sample displays differences across several dimensions, the re-weighted sample achieves balance between workers who do and do not experience a health shock. There remain minor differences between the two groups, though these differences are notably smaller than in the unweighted sample and a Wald test fails to reject the null hypothesis of equality for each characteristic.

4.2 Difference-in-differences

With our matched sample, we produce baseline measures of the impact of health shocks using a difference-in-differences design. This approach accounts for time-invariant differences between workers who do and do not experience a health shock.

We estimate an empirical model of the following form:

$$Y_{it} = \beta_1(Post_t \times Treatment_i) + \beta_2 Post_t + \alpha_i + \theta_t + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome for worker i in wave t. $Post \times Treatment$ is an indicator equal to one for all periods following worker i's health shock, and zero otherwise. $Post$ is an indicator for periods $t = 0$ through $t = +2$. α_i are individual fixed effects, θ_t are interview year fixed effects, and ε_{it} is an error term. Because control individuals may appear multiple times

[12]Marriage status is an indicator for marriage or partnership. We include a four-category measure of educational attainment (less than high school or GED, high school diploma, some college, and college and above). Self-reported health is based on the respondent's general health status on a five-point scale (excellent, very good, good, fair, poor). We include binary indicators for different types of health insurance coverage, which include private, employer-provided (own or spouse's), government, other, and no insurance.

in the sample and households may contribute both respondent and spouse to the sample, standard errors are clustered at the household level to account for within-household serial correlation. β_1 is the coefficient of interest, capturing the average post-treatment effect of a health shock.

In separate figures, we also report results from a related event study model of the following form:

$$Y_{it} = \sum_{k=-2}^{+2} Treatment_{it}\delta_k + \alpha_i + \theta_t + \varepsilon_{it} \tag{2}$$

with the parameters δ_k capturing the impact of health shocks in the periods before and after they are reported.

4.3 Heterogeneity and re-weighting

Our baseline weighting ensures that those individuals who do and do not experience a health shock are as similar as possible on observables. Examining heterogeneity by financial resources introduces an additional complication, however. Workers with high and low levels of wealth may differ systematically in other pre-treatment characteristics, such as education or prior earnings, that could themselves shape their responses to a health shock. Without accounting for these differences, variation in responses across wealth groups could reflect differences in their pre-treatment characteristics rather than in their financial resources.

To isolate heterogeneity along a particular dimension, we therefore apply an additional re-weighting procedure that balances individuals not only across treatment status, but also across the financial characteristic of interest. This creates four groups rather than two—for example, treated and control workers with high and low wealth—and re-weights them to be comparable on observed pre-treatment characteristics. The resulting estimates should therefore be interpreted as composition-adjusted heterogeneity: they ask whether the effect of a health shock differs by financial resources among workers who are otherwise comparable

on observed characteristics. This approach allows us to assess whether differential responses associated with financial resources persist after accounting for observable differences between the groups. We implement a weighting procedure similar to that used by Krolikowski et al. (2020), which extends conventional re-weighting methods to comparisons across multiple groups (DiNardo et al., 1996; Fortin et al., 2011).

We estimate new propensity scores using multinomial logistic regression across four groups defined by the interaction of health shock status and the characteristic of interest. Rather than a binary comparison, our multinomial approach designates a reference group to facilitate comparisons across all four categories. The remaining three subgroups are re-weighted to look similar to the reference group. As with the baseline weights, we winsorize the weights for the heterogeneity analysis at the 1 percent level.

To illustrate, we stratify by total wealth at the median and compare effects of a health shock for individuals above and below the median wealth level, as summarized in Table 2.[13] The first six columns show summary statistics for workers who do and do not experience a health shock, and who have above- and below-median wealth. Columns 7-12 show these same four groups after weighting. There are some sizable differences across the groups prior to weighting, but these differences are largely eliminated in the re-weighted sample.

Following re-weighting, we estimate a model similar to Equation 1 but with an additional set of interaction terms:

$$\begin{aligned} Y_{it} = \beta_1(Post_t \times Treatment_i) + \beta_2 Post \\ + \gamma_1(H_i \times Post_t \times Treatment_i) + \gamma_2(H_i \times Post_t) + \alpha_i + \theta_t + \varepsilon_{it} \end{aligned} \tag{3}$$

where H_i is a dimension of heterogeneity for worker i, such as above-median wealth. The

[13]Because wealth in the HRS is measured at the household level, we adjust for household size before stratifying. For respondents in married or partnered households, we divide total wealth by $\sqrt{2}$, following the equivalence scale adjustment commonly used to account for economies of scale in household consumption (Garner et al. (2003); Short et al. (1999)). Single-person households are left unadjusted. We apply the same adjustment to each of the financial resource measures we stratify by: total wealth, non-housing wealth, and liquid assets.

coefficient β_1 captures the average post-treatment effect of a health shock for workers with $H_i = 0$, while γ_1 captures the differential effects for workers with $H_i = 1$.

5 Results

5.1 Baseline effects of a health shock

Table 3 shows the estimated effects of a health shock on older workers across a broad set of outcomes. As noted earlier, we also ran a version that applied corrective weights to address differences in the implicit weighting of treated and control trends across cohorts in the pooled estimation (following Wing et al. (2024)), but this made little difference to our results (see Appendix Table A1). The results in Table 3 confirm that physical health deteriorates after a shock, with a 6 percentage point increase in individuals reporting at least one activity limitation and a 13 percentage point increase in those stating that their health limits their ability to work. Depression is also significantly more prevalent post-shock. Consistent with the severity of these health events, both the number of doctors visits and out-of-pocket medical expenditures increase.

A subset of these outcomes are highlighted in Figure 1, which shows event study plots based on equation 2. The horizontal axis denotes time relative to the health shock, normalized to period t=0. Out-of-pocket medical expenditures more than double in the wave immediately following the health shock (an increase of 70 log points), before returning to pre-shock levels by the end of the event window (Figure 1a). Although our empirical design assumes that individuals are on similar pre-treatment trajectories, there is some evidence that activity limitations evolved differently between the treatment and comparison groups before the health shock (Figure 1b).

The adverse impacts of health shocks also carry over into the labor market. Table 3 shows a significant decline in earnings, reflecting fewer work hours or not working, rather than a reduction in wages. A significantly higher share of individuals report separating from

an employer. As depicted in Figure 1c, earnings experience a sharp initial drop that does not recover over the observation period. At the same time, health shocks lead to a greater reliance on non-labor income. Workers experiencing a health shock are more likely to claim SSDI/SSI or any Social Security, or receive pension and annuity income.

The decline in earnings and work hours combined with an increased reliance on retirement income reflect a fundamental shift in labor market attachment. Accordingly, Table 3 shows an increase in self-reported retirement following a health shock (also shown in Figures 1e, 1f), with affected workers more likely to retire earlier than their pre-shock expectations.[14] Rather than taking temporary periods of medical leave, our findings suggest that health shocks trigger permanent labor market exits among older workers. Finally, the bottom row of Table 3 shows that a health shock reduces subjective longevity expectations.

Although these baseline estimates document the substantial impacts that health shocks impose on older workers, these average effects may mask considerable underlying variation. The degree of physical decline, the severity of financial strain, and the ability to remain in the labor market likely depend critically on individual circumstances. Therefore, we turn to heterogeneity analyses to examine whether specific sub-populations of older workers are more vulnerable to the effects of a health shock.

5.2 Heterogeneity by financial resources

We analyze heterogeneity in treatment effects, focusing specifically on the role of financial resources. In appendix tables, we also explore heterogeneity by gender, age, and education using the methodology described in Section 4.3. Along these dimensions, we find only relatively minor differences across groups, and these differences are in line with the previous literature. Women are more likely to experience depression following a health shock (Table A2), younger workers are less likely to claim SSDI/SSI (Table A3), while having a college education offers a partial buffer against health limiting work (Table A4).

[14] We also examined the subjective expectation of working after reaching age 65 and found a strongly significant negative effect, consistent with Gorry (2024).

Table 4 shows the results splitting the sample at the median of total wealth. We find no statistically significant difference between high- and low-wealth individuals. Wealthier individuals fare no better than others in responding to health shocks when wealth is measured using a comprehensive definition of all assets and liabilities.

While total wealth captures the full scope of a household's balance sheet, it conflates assets that differ substantially in their liquidity and therefore their availability as a buffer against an unexpected health shock. To more closely proxy the financial resources households can mobilize in response to a health shock, we stratify by two additional types of financial resources: non-housing wealth and liquid assets.

Non-housing wealth is total wealth, excluding primary and secondary residence equity. Housing wealth is the largest component of wealth for most households in our sample, and it is largely illiquid. To the extent that wealth moderates the consequences of a health shock through a consumption-smoothing channel, non-housing wealth is the more proximate buffer as it can be drawn down more quickly. Nonetheless, the results shown in Table 5 are broadly consistent with the null results for total wealth. Only one coefficient is significant; those with above-median non-housing wealth are less likely to stop working earlier than expected. Once again, wealth does not appear to be a significant buffer for older workers.

Finally, in Table 6, we classify households according to liquid assets. Liquid assets include stocks and mutual funds, checking and savings accounts, CDs, government savings bonds, Treasury bills, and bonds and bond funds. In this case, we find stronger and more consistent evidence of heterogeneity.[15] Event study plots for selected outcomes are displayed in Figure 2. Generally, households with above-median liquid assets are substantially buffered from the negative consequences of a health shock. There are some marginally significant differences in health outcomes, namely fewer work-limiting health conditions, but the main results lie in the financial and labor market effects. Above-median workers are five percentage points

[15]The results are very similar if we subtract the value of all non-housing debt from the liquid asset measure. Ideally, the measure of liquidity would subtract all near-term non-housing debt. However, while the HRS disaggregates debt components in recent survey waves (providing information on, e.g. credit card debt), we do not have this information in all years of our sample.

more likely to be working for pay and consequently see smaller earnings declines. Relative to their low-liquid asset counterparts, they are five percentage points less likely to receive any Social Security income and six percentage points less likely to be completely or partly retired. Consistent with these retirement effects, workers with above-median liquid assets are also three percentage points less likely to stop working earlier than they had expected before the health shock.

Because wealth and income are highly correlated, we also examine whether our conclusions regarding liquid assets are merely driven by differences in pre-treatment income. Under our baseline matching results, there are in fact statistically significant differences in income for those in the above- and below- median wealth categories; wealth and income, after all, are highly correlated. To address this concern, we pursue an alternative matching strategy whereby we match solely on pre-treatment household income. We again reach very similar findings (Table A5), suggesting that our conclusion regarding the importance of liquid assets holds whether or not those assets are the product of higher incomes or not. All of our main results for earnings, receipt of Social Security, and retirement remain consistent.

5.3 Additional robustness checks

In this section, we assess potential threats to our conclusion that liquidity plays a primary role in mitigating the consequences of health shocks. First, we explore whether the likelihood of experiencing a health shock is correlated with asset composition. In columns (1) and (2) of Table 7, we regress an indicator variable for health shocks on a set of pre-treatment characteristics, including the level of various types of assets (liquid, housing, retirement, and other). In column 1, we measure the variables using continuous measures of pre-treatment assets. In column 2, we use binary indicators of above-median. None of the coefficients are statistically significant, suggesting that the likelihood of experiencing a health shock is not systematically related to an individual's pre-treatment assets.

Next, we consider the possibility that pre-treatment liquid assets are correlated with the

severity of the shock. If "wealthier" individuals, as measured by their amount of liquid assets, tend to experience less severe health shocks, then this would confound the interpretation of the effects of liquidity. In columns (3) and (4) of Table 7, we regress the number of nights in the hospital – a proxy for the severity of the health shock – on the same measures of asset levels as in columns 1-2. Once again, the measure of liquid assets is not significant in either specification, though there appears to be some association between the severity of a health shock and the level of retirement balances (column 3) and an indicator for above-median housing assets (column 4).

Finally, we also consider an alternate definition of a health shock. In a new specification, we instead define a health shock as a diagnosis of one of the following health conditions: heart problem, stroke, cancer, or lung disease. Table 8 shows the estimates for the heterogeneity by liquid assets using this new definition of a health shock. Broadly, the pattern of results is consistent with our preferred measure; under both definitions, higher pre-shock liquid assets leads to lower earnings declines, lower probability of receiving Social Security, and lower likelihood of retirement.

6 Discussion

A health shock generates large and statistically significant increases in retirement transitions among workers who were not retired in the pre-treatment period. In our baseline results, the probability of complete retirement increases by 7 percentage points. By comparison, the probability of retirement among workers who do not experience a health shock increases by 11 percentage points over the same period, as captured by the coefficient on *Post* in equation 1. Thus, a health shock increases the probability of retirement by nearly 60 percent relative to the counterfactual rate. Effects on complete or partial retirement are of the same magnitude. A health shock also increases the probability of receiving any Social Security income by 7 percentage points, representing a 70 percent increase relative to the counterfactual rate

of just over 10 percent. These estimates indicate that health shocks substantially accelerate labor force exit among affected workers. Our baseline estimates are broadly consistent with prior work documenting accelerated retirement following a health shock among older workers (Coile (2004), Dwyer & Mitchell (1999)), while the magnitude of the Social Security claiming effect aligns with evidence that health shocks alter retirement expectations (McGarry (2004)).

These aggregate effects mask substantial heterogeneity across financial resources. Among our reference group of households with below-median liquid assets, the health shock increases the probability of complete retirement by 7 percentage points, complete or partial retirement by 8 percentage points, and Social Security income receipt by 8 percentage points. Above-median liquid asset households show significantly muted responses across all three measures.

On their own, increased rates of retirement are difficult to interpret normatively. Retirement is a normal labor market transition, and workers who retire following a health shock may do so willingly after reassessing their priorities or concluding that they have accumulated sufficient resources to exit comfortably. The retirement transitions we document could therefore reflect a rational updating of retirement timing rather than a forced or undesirable exit. To distinguish between these interpretations, we examine whether affected workers report stopping work earlier than they had planned. We find that a health shock increases the probability of reporting an earlier-than-planned work exit by 3 percentage points. Among above-median liquid asset households, this effect is almost completely offset. This suggests that the retirement transitions we document among the low liquid assets group are actually representative of an unwanted acceleration of labor market exit driven by the health shock.

The Social Security claiming result deserves particular attention given its long-run implications. Workers who claim retirement benefits before reaching full retirement age permanently reduce their monthly benefit, but a health shock may also push workers toward SSDI or SSI; these programs serve as a safety net, but also signal a more severe and permanent labor market exit. Regardless of the exact type of benefit claimed, a health shock accelerates

workers' reliance on Social Security, and this result is most acute for workers with the fewest liquid assets. To the extent that Social Security becomes the primary income source for affected workers sooner than planned, the effects documented here are likely to compound existing inequalities in retirement income security.

Why might liquidity play such an operative role? One possible explanation for our findings is that liquid assets provide workers with the flexibility to absorb the uncertainty created by a health shock without making irreversible labor market adjustments. Following a shock, individuals may face uncertainty about the persistence of their condition, future medical needs, and their ability to remain employed. Because liquid assets can be accessed immediately, households with greater liquidity may be better positioned to smooth consumption and meet unexpected expenses while waiting to learn more about the trajectory of their health condition. This flexibility preserves the option to continue working rather than retiring or claiming Social Security benefits prematurely. While one might expect this mechanism to operate through higher medical spending, we do not find evidence of this pattern. However, liquidity may matter through expected rather than realized expenditures, allowing workers to maintain labor force attachment because they are less concerned about potential future costs. Moreover, our annual data may not capture short-lived disruptions in spending that occur immediately following a health shock and are subsequently smoothed over the course of the year.

7 Conclusion

This paper provides new evidence on the effects of health shocks on older workers, examining an expanded set of outcomes spanning health, labor supply, income and retirement measures. Using a staggered difference-in-differences design with propensity score weighting, we find that health shocks generate significant effects on retirement among workers who were not retired prior to the shock. The probability of complete retirement increases by nearly 60

percent relative to the counterfactual rate, with comparable increases in complete or partial retirement and Social Security claiming. These retirement transitions are accompanied by rising out-of-pocket medical expenditures and deteriorating health, both of which compound the burden of a health shock in older ages.

Beyond these baseline findings, this paper makes a meaningful contribution by documenting heterogeneity in the effects of health shocks along a dimension that has received limited prior attention. We find that liquid assets are the primary dimension along which heterogeneity emerges. These results point to liquidity as the operative channel through which financial resources bolsters health shock resilience. Workers with above-median liquid assets experience significant attenuation of the increase in retirement, suggesting that readily available financial assets act as a buffer protecting individuals from exiting the labor market ahead of schedule. One possible explanation is that liquid assets provide workers with the flexibility to absorb the uncertainty created by a health shock without making irreversible labor market decisions. By preserving the option to remain employed while health conditions and future financial needs become clearer, liquidity appears to be more important than overall wealth in shaping labor market adjustment following adverse health events.

These findings carry implications for retirement policy and the financial security of older workers. Policies aimed at building retirement wealth may do relatively little to protect workers from the labor market consequences of a health shock if the assets they accumulate are locked behind early withdrawal penalties or illiquid by design. Encouraging precautionary liquid savings among workers approaching retirement, whether through dedicated emergency savings vehicles, more flexible access to retirement assets for health-related expenses, or other mechanisms that reduce the cost of accessing funds in a crisis, may meaningfully reduce the vulnerability of older workers to adverse health events.

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Table 1: Sample means by health shock status before and after re-weighting by propensity score

	Unweighted			Weighted		
	Shock	No shock	p	Shock	No shock	p
Age (years), t-1	54.97	54.83	[0.08]	54.97	54.97	[1.00]
Male	0.41	0.39	[0.16]	0.41	0.41	[0.95]
Black	0.17	0.16	[0.71]	0.17	0.17	[0.98]
Married or partnered, t-1	0.75	0.78	[0.04]	0.75	0.76	[0.89]
Education, t-1						
HS diploma	0.32	0.28	[0.01]	0.32	0.32	[0.97]
Some college	0.25	0.26	[0.52]	0.25	0.25	[0.98]
College and above	0.26	0.32	[0.00]	0.26	0.26	[0.99]
Health status, t-1						
Very good health	0.34	0.42	[0.00]	0.34	0.34	[0.97]
Good health	0.36	0.27	[0.00]	0.36	0.36	[0.93]
Fair health	0.14	0.07	[0.00]	0.14	0.14	[0.90]
Poor health	0.01	0.01	[0.02]	0.01	0.01	[0.77]
Health insurance, t-1						
Government health insurance	0.05	0.03	[0.02]	0.05	0.05	[0.92]
Private health insurance	0.00	0.00	[0.98]	0.00	0.00	[1.00]
Employer health insurance	0.85	0.85	[0.91]	0.85	0.85	[0.95]
Other health insurance	0.07	0.06	[0.25]	0.07	0.07	[0.85]
No health insurance	0.08	0.09	[0.20]	0.08	0.08	[0.93]
Earnings (log $), t-1	10.64	10.71	[0.01]	10.64	10.64	[0.98]
# of event windows	1,099	9,682		1,099	9,682	
# of unique individuals	1,099	4,854		1,099	4,854	

Note: Weights are determined by propensity score re-weighting. All covariates listed are included in the matching model. We report means and the p-value of a Wald test of equality with the treated group. Units are binary unless otherwise noted. Earnings are shown in log 2020 dollars. Our stacking approach allows for the same respondent to appear more than once as a control. Number of unique individuals reports the number of distinct respondents, counting each person only once.

Table 2: Sample means by health shock status and median total wealth before and after re-weighting by propensity score

	Unweighted							Weighted						
	Above median			Below median				Above median			Below median			
	Shock	No shock	p	Shock	p	No shock	p	Shock	No shock	p	Shock	p	No shock	p
Age (years), t-1	55.04	54.82	[0.07]	54.92	[0.44]	54.85	[0.11]	55.04	55.05	[0.95]	54.94	[0.58]	55.01	[0.79]
Male	0.43	0.40	[0.20]	0.39	[0.26]	0.37	[0.03]	0.43	0.42	[0.89]	0.43	[0.92]	0.43	[0.93]
Black	0.07	0.08	[0.48]	0.24	[0.00]	0.25	[0.00]	0.07	0.07	[0.96]	0.06	[0.48]	0.07	[0.93]
Married or partnered, t-1	0.87	0.88	[0.69]	0.67	[0.00]	0.69	[0.00]	0.87	0.87	[0.85]	0.89	[0.40]	0.87	[0.91]
Education, t-1														
HS diploma	0.29	0.24	[0.04]	0.35	[0.03]	0.33	[0.08]	0.29	0.28	[0.90]	0.28	[0.87]	0.29	[0.95]
Some college	0.26	0.25	[0.88]	0.24	[0.63]	0.26	[0.76]	0.26	0.26	[0.98]	0.26	[0.84]	0.26	[0.94]
College and above	0.39	0.45	[0.01]	0.16	[0.00]	0.18	[0.00]	0.39	0.39	[0.99]	0.38	[0.88]	0.39	[0.93]
Health status, t-1														
Very good health	0.39	0.46	[0.00]	0.30	[0.00]	0.37	[0.46]	0.39	0.39	[0.96]	0.40	[0.68]	0.39	[0.91]
Good health	0.31	0.21	[0.00]	0.39	[0.01]	0.34	[0.24]	0.31	0.31	[1.00]	0.31	[0.94]	0.32	[0.87]
Fair health	0.08	0.04	[0.00]	0.19	[0.00]	0.10	[0.17]	0.08	0.08	[0.95]	0.09	[0.68]	0.08	[0.97]
Poor health	0.01	0.00	[0.26]	0.02	[0.04]	0.01	[0.42]	0.01	0.01	[0.85]	0.01	[0.71]	0.01	[0.88]
Health insurance, t-1														
Government health insurance	0.05	0.02	[0.02]	0.05	[0.62]	0.05	[0.86]	0.05	0.05	[0.91]	0.04	[0.41]	0.05	[0.87]
Private health insurance	0.00	0.00	[0.57]	0.00	[0.74]	0.00	[0.91]	0.00	0.00	[0.96]	0.00	[0.86]	0.00	[0.86]
Employer health insurance	0.91	0.92	[0.64]	0.81	[0.00]	0.78	[0.00]	0.91	0.91	[0.98]	0.93	[0.51]	0.91	[0.75]
Other health insurance	0.06	0.06	[0.88]	0.08	[0.30]	0.06	[0.88]	0.06	0.06	[0.95]	0.05	[0.64]	0.06	[0.69]
No health insurance	0.03	0.03	[0.96]	0.12	[0.00]	0.15	[0.00]	0.03	0.03	[0.87]	0.03	[0.87]	0.03	[0.83]
Earnings (log $), t-1	10.89	10.95	[0.21]	10.45	[0.00]	10.47	[0.00]	10.89	10.88	[0.83]	10.76	[0.03]	10.83	[0.17]
# of event windows	466	4,922		633		4,760		466	4,922		633		4,760	
# of unique individuals	466	2,617		633		2,616		466	2,617		633		2,616	

Note: Wealth is split at the sample median level of total wealth. Weights are determined by propensity score re-weighting. We report means and the p-value of a Wald test of equality with the group that has above-median wealth and experiences a health shock. Units are binary unless otherwise noted. Earnings are shown in log 2020 dollars. Our stacking approach allows for the same respondent to appear more than once as a control. Number of unique individuals reports the number of distinct respondents, counting each person only once.

Table 3: Baseline effects of a health shock

	Pre-shock control mean	Treat*Post (SE)	N
Activity limitations	0.03	0.058***	53,905
		(0.009)	
Health limits work	0.07	0.132***	53,430
		(0.011)	
Depression	0.10	0.034***	41,650
		(0.010)	
Doctor visits (log count)	1.48	0.356***	53,175
		(0.024)	
Medical expenditures (log $)	6.45	0.282***	38,740
		(0.078)	
Earnings (log $)	10.63	-0.761***	53,905
		(0.113)	
Hours worked, if working (# hours)	2,122.30	-48.570**	36,955
		(23.642)	
Working for pay	1.00	-0.091***	53,785
		(0.012)	
Wage, if working ($/hour)	26.20	-0.298	29,060
		(0.629)	
Left employer	0.00	0.069***	53,565
		(0.014)	
Receiving SSDI/SSI	0.00	0.057***	53,725
		(0.006)	
Receiving any Social Security income	0.00	0.073***	53,155
		(0.010)	
Receiving pension or annuity income	0.11	0.021**	53,905
		(0.010)	
Completely retired	0.00	0.067***	53,905
		(0.010)	
Completely or partly retired	0.00	0.072***	51,205
		(0.011)	
Stopped working earlier than expected	0.00	0.029***	53,905
		(0.007)	
Live to 75+ (% chance)	67.40	-1.707**	44,955
		(0.700)	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using inverse probability of treatment weights, estimated via propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for receiving pension or annuity income. Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.

Table 4: Heterogeneity by total wealth

	Treat*Post (SE)	Treat*Post*Above median (SE)	N
Activity limitations	0.046***	-0.002	53,905
	(0.013)	(0.017)	
Health limits work	0.107***	-0.007	53,430
	(0.017)	(0.023)	
Depression	0.036**	-0.021	41,650
	(0.015)	(0.020)	
Doctor visits (log count)	0.309***	-0.012	53,175
	(0.042)	(0.053)	
Medical expenditures (log $)	0.527***	-0.235	38,740
	(0.141)	(0.175)	
Earnings (log $)	-0.433**	-0.076	53,905
	(0.187)	(0.253)	
Hours worked, if working (# hours)	-27.682	-28.617	36,955
	(39.771)	(53.684)	
Working for pay	-0.073***	0.021	53,785
	(0.020)	(0.026)	
Wage, if working ($/hour)	0.923	-1.558	29,060
	(1.480)	(1.907)	
Left employer	0.021	0.022	53,565
	(0.026)	(0.033)	
Receiving SSDI/SSI	0.042***	-0.007	53,905
	(0.009)	(0.012)	
Receiving any Social Security income	0.068***	-0.028	53,905
	(0.017)	(0.021)	
Receiving pension or annuity income	0.024	-0.007	53,905
	(0.021)	(0.027)	
Completely retired	0.058***	-0.014	53,905
	(0.017)	(0.023)	
Completely or partly retired	0.072***	-0.022	51,205
	(0.021)	(0.027)	
Stopped working earlier than expected	0.032**	-0.023	53,905
	(0.013)	(0.016)	
Live to 75+ (% chance)	-1.834	-0.340	44,955
	(1.196)	(1.497)	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using weights constructed from estimated propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for receiving pension or annuity income. Treat*Post reports the effect of experiencing a health shock in the post-shock period. Treat*Post*Above median reports the additional differential effect for respondents above the sample median of total wealth. Total wealth is the sum of all wealth components (residential housing, other real estate, vehicles, businesses, retirement accounts, stocks and mutual funds, checking and savings accounts, CDs, government savings bonds, Treasury bills, bonds and bond funds, and other savings) minus all debt (mortgages, credit card, medical, education, and other debt). Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.

Table 5: Heterogeneity by non-housing wealth

	Treat*Post (SE)	Treat*Post*Above median (SE)	N
Activity limitations	0.043***	-0.001	53,905
	(0.013)	(0.017)	
Health limits work	0.111***	-0.013	53,430
	(0.017)	(0.023)	
Depression	0.026	-0.001	41,650
	(0.016)	(0.020)	
Doctor visits (log count)	0.295***	0.018	53,175
	(0.043)	(0.054)	
Medical expenditures (log $)	0.529***	-0.253	38,740
	(0.144)	(0.178)	
Earnings (log $)	-0.333*	-0.283	53,905
	(0.185)	(0.250)	
Hours worked, if working (# hours)	-34.223	-10.533	36,955
	(40.857)	(53.234)	
Working for pay	-0.066***	0.007	53,785
	(0.020)	(0.026)	
Wage, if working ($/hour)	0.478	-0.535	29,060
	(1.446)	(1.888)	
Left employer	0.011	0.040	53,565
	(0.025)	(0.033)	
Receiving SSDI/SSI	0.040***	-0.004	53,905
	(0.009)	(0.012)	
Receiving any Social Security income	0.067***	-0.021	53,905
	(0.017)	(0.021)	
Receiving pension or annuity income	0.028	-0.011	53,905
	(0.023)	(0.028)	
Completely retired	0.059***	-0.015	53,905
	(0.018)	(0.023)	
Completely or partly retired	0.073***	-0.025	51,205
	(0.022)	(0.028)	
Stopped working earlier than expected	0.038***	-0.036**	53,905
	(0.013)	(0.015)	
Live to 75+ (% chance)	-1.738	-0.435	44,955
	(1.255)	(1.533)	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using weights constructed from estimated propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for receiving pension or annuity income. Treat*Post reports the effect of experiencing a health shock in the post-shock period. Treat*Post*Above median reports the additional differential effect for respondents above the sample median of non-housing wealth. Non-housing wealth is the sum of all non-housing wealth components (non-residential real estate, vehicles, businesses, retirement accounts, stocks and mutual funds, checking and savings accounts, CDs, government savings bonds, Treasury bills, bonds and bond funds, and other savings) minus non-housing debt (credit card, medical, education, and other debt). Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.

Table 6: Heterogeneity by liquid assets

	Treat*Post (SE)	Treat*Post*Above median (SE)	N
Activity limitations	0.047***	-0.001	53,905
	(0.012)	(0.017)	
Health limits work	0.125***	-0.038*	53,430
	(0.018)	(0.023)	
Depression	0.027*	-0.011	41,650
	(0.015)	(0.019)	
Doctor visits (log count)	0.309***	-0.004	53,175
	(0.039)	(0.051)	
Medical expenditures (log $)	0.462***	-0.133	38,740
	(0.129)	(0.161)	
Earnings (log $)	-0.657***	0.397*	53,905
	(0.174)	(0.239)	
Hours worked, if working (# hours)	-26.441	-11.626	36,955
	(36.558)	(49.094)	
Working for pay	-0.087***	0.053**	53,785
	(0.018)	(0.024)	
Wage, if working ($/hour)	0.019	0.210	29,060
	(1.168)	(1.638)	
Left employer	0.030	0.006	53,565
	(0.023)	(0.031)	
Receiving SSDI/SSI	0.048***	-0.021**	53,905
	(0.009)	(0.011)	
Receiving any Social Security income	0.076***	-0.046**	53,905
	(0.015)	(0.019)	
Receiving pension or annuity income	0.024	-0.005	53,905
	(0.019)	(0.025)	
Completely retired	0.073***	-0.047**	53,905
	(0.015)	(0.021)	
Completely or partly retired	0.084***	-0.058**	51,205
	(0.019)	(0.025)	
Stopped working earlier than expected	0.033***	-0.028**	53,905
	(0.010)	(0.014)	
Live to 75+ (% chance)	-2.166**	0.505	44,955
	(1.103)	(1.434)	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using weights constructed from estimated propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for receiving pension or annuity income. Treat*Post reports the effect of experiencing a health shock in the post-shock period. Treat*Post*Above median reports the additional differential effect for respondents above the sample median of liquid assets. Liquid assets are defined as stocks and mutual funds, checking and savings accounts, CDs, government savings bonds, Treasury bills, and bonds and bond funds. Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.

Table 7: Additional robustness checks

	Health shock ($t = 0$)		Log hospital nights ($t = 0$)	
	(1)	(2)	(3)	(4)
Liquid assets	-0.005	-0.027	-0.000	0.010
	(0.003)	(0.020)	(0.010)	(0.047)
Housing assets	-0.004	-0.018	-0.006	-0.097**
	(0.002)	(0.020)	(0.007)	(0.046)
Retirement account balances	0.002	0.014	-0.011**	-0.015
	(0.002)	(0.019)	(0.005)	(0.045)
Other assets	-0.000	-0.017	-0.008	-0.050
	(0.004)	(0.019)	(0.012)	(0.048)
Asset measure	Log ($)	Above median	Log ($)	Above median
N	10,781	10,781	933	933

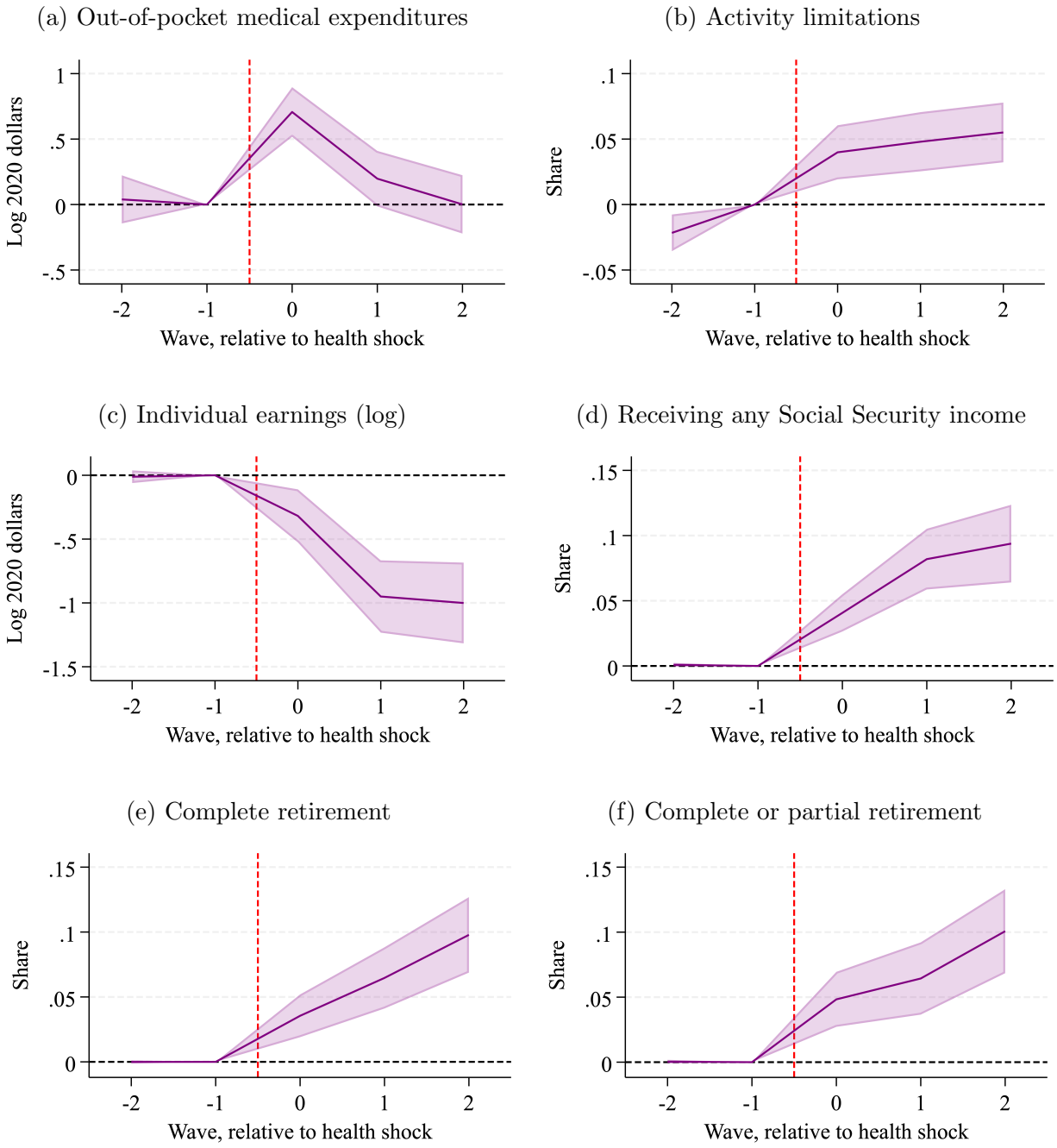
Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using inverse probability of treatment weights, estimated via propensity scores. All models include sub-experiment fixed effects, with standard errors clustered at the household level and reported in parentheses. Assets are averaged over $t - 2$ and $t - 1$. Columns (1)–(2) report linear probability models of treatment status on pre-shock assets, estimated on the full sample of treated and control individuals; pre-shock assets enter in logs in columns (1) and (3) and as above-median indicators in columns (2) and (4). Columns (3)–(4) are estimated among the subset of treated individuals with at least two hospital nights at $t = 0$; the outcome is the log of one plus the count of hospital nights at $t = 0$.

Table 8: Heterogeneity by liquid assets, alternate health shock definition

	Treat*Post (SE)	Treat*Post*Above median (SE)	N
Activity limitations	0.040**	-0.004	60,410
	(0.017)	(0.020)	
Health limits work	0.130***	-0.039	59,855
	(0.023)	(0.029)	
Depression	0.011	0.025	46,920
	(0.016)	(0.022)	
Doctor visits (log count)	0.435***	-0.029	59,585
	(0.047)	(0.065)	
Medical expenditures (log $)	0.671***	-0.331*	43,650
	(0.153)	(0.192)	
Earnings (log $)	-0.872***	0.664**	60,410
	(0.212)	(0.290)	
Hours worked, if working (# hours)	-59.520	35.457	40,915
	(58.541)	(74.798)	
Working for pay	-0.081***	0.035	60,270
	(0.022)	(0.030)	
Wage, if working ($/hour)	0.606	0.516	32,205
	(1.491)	(1.880)	
Left employer	0.068**	-0.045	59,995
	(0.030)	(0.039)	
Receiving SSDI/SSI	0.033***	0.001	60,410
	(0.009)	(0.014)	
Receiving any SS income	0.089***	-0.061**	60,410
	(0.021)	(0.026)	
Receiving pension income or annuity	0.033	-0.047	60,410
	(0.025)	(0.032)	
Completely retired	0.063***	-0.041*	60,410
	(0.018)	(0.025)	
Completely or partly retired	0.080***	-0.063**	57,380
	(0.023)	(0.031)	
Stopped working earlier than expected	0.022**	0.005	60,410
	(0.011)	(0.017)	
Live to 75+ (% chance)	-5.926***	3.126*	50,230
	(1.422)	(1.856)	

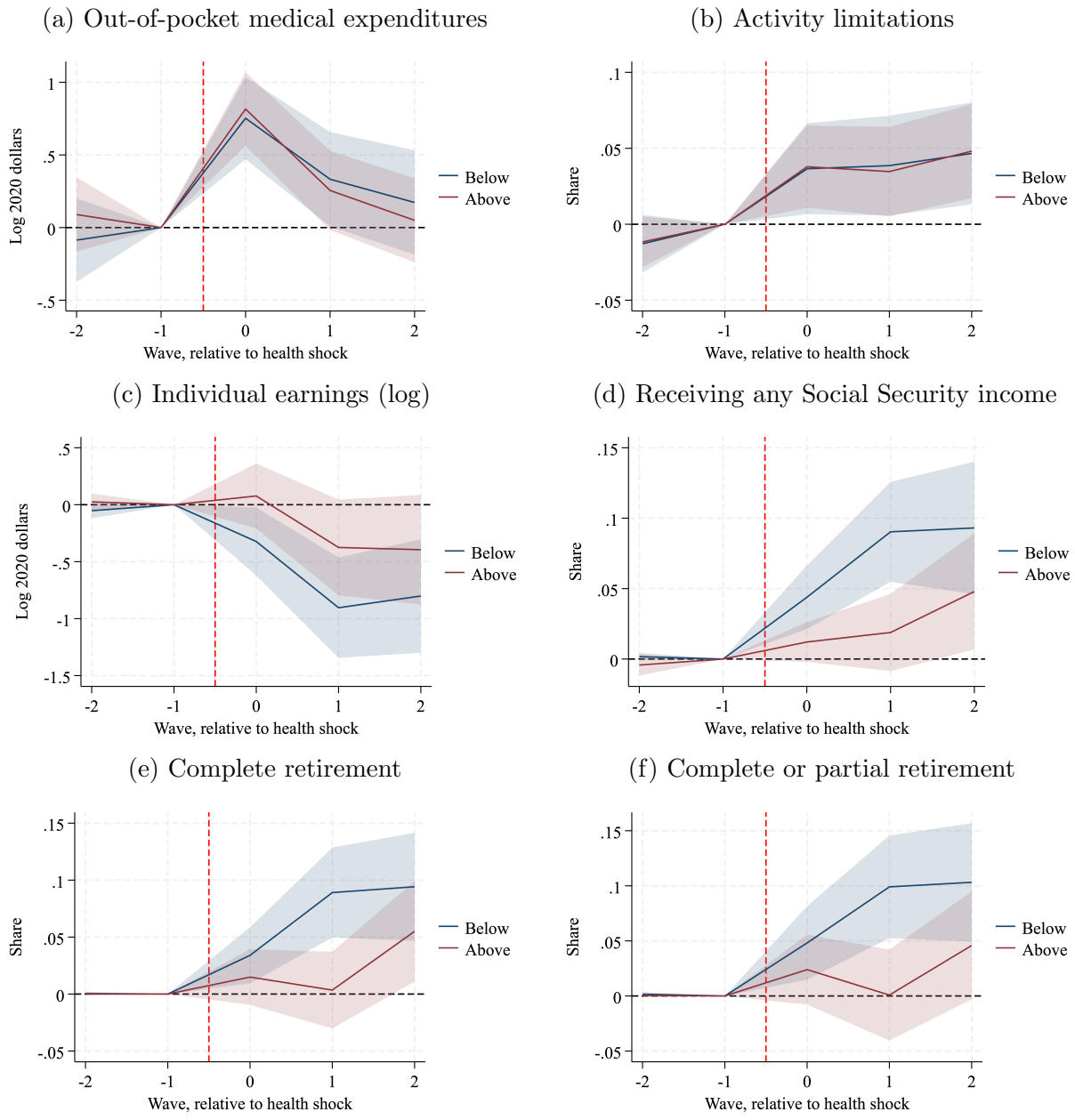
Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using weights constructed from estimated propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for those related to pensions, annuities, IRAs, and total debt. Treat*Post reports the effect of experiencing a health shock in the post-shock period. Treat*Post*Above median reports the additional differential effect for respondents above the sample median of liquid assets. Liquid assets are defined as stocks and mutual funds, checking and savings accounts, CDs, government savings bonds, Treasury bills, and bonds and bond funds. Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.

Figure 1: Baseline effects of a health shock, selected outcomes



Note: Corresponding estimates in Table 3. This figure shows the effects of a health shock on various outcomes. The vertical dashed line, situated between $t-1$ and $t=0$, denotes the interval in which the health shock occurred. The control observations are re-weighted using propensity scores.

Figure 2: Heterogeneity by liquid assets, selected outcomes



Note: Corresponding estimates in Table 6. This figure depicts the effects of a health shock for individuals based on liquid assets. The vertical dashed line, situated between $t-1$ and $t=0$, denotes the interval in which the health shock occurred. The control observations are re-weighted using propensity scores.

Appendix

Table A1: Baseline effects of a health shock, weighting comparison

	Main	Wing	
	Treat*Post (SE)	Treat*Post (SE)	N
Activity limitations	0.058*** (0.009)	0.058*** (0.008)	53,905
Health limits work	0.132*** (0.011)	0.132*** (0.011)	53,430
Depression	0.034*** (0.010)	0.033*** (0.010)	41,650
Doctor visits (log count)	0.356*** (0.024)	0.356*** (0.024)	53,175
Medical expenditures (log $)	0.282*** (0.078)	0.288*** (0.078)	38,740
Earnings (log $)	-0.761*** (0.113)	-0.748*** (0.113)	53,905
Hours worked, if working (# hours)	-48.570** (23.642)	-49.486** (23.455)	36,955
Working for pay	-0.091*** (0.012)	-0.090*** (0.012)	53,785
Wage, if working ($/hour)	-0.298 (0.629)	-0.275 (0.641)	29,060
Left employer	0.069*** (0.014)	0.066*** (0.014)	53,565
Receiving SSDI/SSI	0.057*** (0.006)	0.056*** (0.006)	53,725
Receiving any Social Security income	0.073*** (0.010)	0.073*** (0.010)	53,155
Receiving pension or annuity income	0.021** (0.010)	0.019* (0.010)	53,905
Completely retired	0.067*** (0.010)	0.065*** (0.009)	53,905
Completely or partly retired	0.072*** (0.011)	0.070*** (0.011)	51,205
Stopped working earlier than expected	0.029*** (0.007)	0.028*** (0.007)	53,905
Live to 75+ (% chance)	-1.707** (0.700)	-1.652** (0.699)	44,955

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table compares baseline treatment effect estimates from the main specification (Table 3) to those obtained after applying cohort-specific sample weights following Wing et al. (2024). All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. All outcomes are at the individual level, except for receiving pension or annuity income. Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations under the main specification.

Table A2: Heterogeneity by gender

	Treat*Post (SE)	Treat*Post*Female (SE)	N
Activity limitations	0.090***	-0.034	53,905
	(0.022)	(0.025)	
Health limits work	0.180***	-0.055	53,430
	(0.033)	(0.036)	
Depression	-0.023	0.080***	41,650
	(0.025)	(0.028)	
Doctor visits (log count)	0.463***	-0.133*	53,175
	(0.061)	(0.069)	
Medical expenditures (log $)	0.418**	-0.155	38,740
	(0.179)	(0.203)	
Earnings (log $)	-1.038***	0.351	53,905
	(0.275)	(0.308)	
Hours worked, if working (# hours)	-30.670	-26.674	36,955
	(54.976)	(62.495)	
Working for pay	-0.133***	0.051	53,785
	(0.028)	(0.032)	
Wage, if working ($/hour)	0.209	-0.084	29,060
	(0.941)	(1.286)	
Left employer	0.123***	-0.067*	53,565
	(0.032)	(0.037)	
Receiving SSDI/SSI	0.063***	-0.001	53,905
	(0.016)	(0.018)	
Receiving any Social Security income	0.107***	-0.044	53,905
	(0.025)	(0.028)	
Receiving pension or annuity income	0.015	0.000	53,905
	(0.019)	(0.023)	
Completely retired	0.091***	-0.033	53,905
	(0.020)	(0.023)	
Completely or partly retired	0.079***	-0.009	51,205
	(0.025)	(0.029)	
Stopped working earlier than expected	0.044**	-0.019	53,905
	(0.018)	(0.020)	
Live to 75+ (% chance)	-0.577	-1.096	44,955
	(1.510)	(1.766)	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using weights constructed from estimated propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for receiving pension or annuity income. Treat*Post reports the effect of experiencing a health shock in the post-shock period. Treat*Post*Female reports the additional differential effect for women. Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.

Table A3: Heterogeneity by age group

	Treat*Post (SE)	Treat*Post*Young (SE)	N
Activity limitations	0.063***	-0.010	53,905
	(0.014)	(0.017)	
Health limits work	0.145***	-0.019	53,430
	(0.019)	(0.023)	
Depression	0.046***	-0.017	41,650
	(0.014)	(0.019)	
Doctor visits (log count)	0.366***	-0.024	53,175
	(0.044)	(0.053)	
Medical expenditures (log $)	0.490***	-0.300*	38,740
	(0.133)	(0.164)	
Earnings (log $)	-0.894***	0.227	53,905
	(0.197)	(0.235)	
Hours worked, if working (# hours)	-77.833*	37.570	36,955
	(47.149)	(53.433)	
Working for pay	-0.109***	0.027	53,785
	(0.022)	(0.026)	
Wage, if working ($/hour)	0.357	-0.916	29,060
	(1.106)	(1.342)	
Left employer	0.078***	-0.017	53,565
	(0.024)	(0.029)	
Receiving SSDI/SSI	0.078***	-0.033**	53,905
	(0.012)	(0.014)	
Receiving any Social Security income	0.087***	-0.020	53,905
	(0.018)	(0.021)	
Receiving pension or annuity income	0.035*	-0.024	53,905
	(0.020)	(0.023)	
Completely retired	0.086***	-0.029	53,905
	(0.020)	(0.022)	
Completely or partly retired	0.074***	0.001	51,205
	(0.021)	(0.025)	
Stopped working earlier than expected	0.021	0.008	53,905
	(0.014)	(0.016)	
Live to 75+ (% chance)	-3.384***	2.553*	44,955
	(1.166)	(1.441)	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using weights constructed from estimated propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for receiving pension or annuity income. Treat*Post reports the effect of experiencing a health shock in the post-shock period. Treat*Post*Young reports the additional differential effect for respondents younger than 59 years old. Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.

Table A4: Heterogeneity by education

	Treat*Post (SE)	Treat*Post*College (SE)	N
Activity limitations	0.044***	0.000	53,905
	(0.010)	(0.018)	
Health limits work	0.137***	-0.070***	53,430
	(0.015)	(0.023)	
Depression	0.030**	-0.007	41,650
	(0.013)	(0.019)	
Doctor visits (log count)	0.363***	-0.072	53,175
	(0.034)	(0.055)	
Medical expenditures (log $)	0.388***	-0.106	38,740
	(0.110)	(0.164)	
Earnings (log $)	-0.767***	0.471*	53,905
	(0.162)	(0.267)	
Hours worked, if working (# hours)	-15.745	-87.094*	36,955
	(29.706)	(51.388)	
Working for pay	-0.078***	0.021	53,785
	(0.016)	(0.027)	
Wage, if working ($/hour)	-0.900	1.153	29,060
	(0.776)	(2.110)	
Left employer	0.058***	-0.034	53,565
	(0.020)	(0.034)	
Receiving SSDI/SSI	0.051***	-0.023*	53,905
	(0.007)	(0.012)	
Receiving any Social Security income	0.071***	-0.022	53,905
	(0.012)	(0.020)	
Receiving pension or annuity income	0.018	-0.003	53,905
	(0.014)	(0.026)	
Completely retired	0.057***	-0.011	53,905
	(0.014)	(0.023)	
Completely or partly retired	0.062***	-0.014	51,205
	(0.016)	(0.028)	
Stopped working earlier than expected	0.023***	-0.012	53,905
	(0.009)	(0.014)	
Live to 75+ (% chance)	-2.167**	0.693	44,955
	(0.964)	(1.524)	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using weights constructed from estimated propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for receiving pension or annuity income. Treat*Post reports the effect of experiencing a health shock in the post-shock period. Treat*Post*College reports the additional differential effect for college-educated respondents. Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.

Table A5: Heterogeneity by liquid assets, matching solely on pre-treatment income

	Treat*Post (SE)	Treat*Post*Above median (SE)	N
Activity limitations	0.059***	-0.012	53,905
	(0.015)	(0.019)	
Health limits work	0.129***	-0.037	53,430
	(0.020)	(0.024)	
Depression	0.035**	-0.019	41,650
	(0.017)	(0.021)	
Doctor visits (log count)	0.325***	-0.029	53,175
	(0.041)	(0.053)	
Medical expenditures (log $)	0.395**	-0.059	38,740
	(0.157)	(0.184)	
Earnings (log $)	-0.893***	0.532**	53,905
	(0.206)	(0.261)	
Hours worked, if working (# hours)	-74.307	35.094	36,955
	(60.156)	(68.173)	
Working for pay	-0.102***	0.057**	53,785
	(0.020)	(0.026)	
Wage, if working ($/hour)	0.071	-0.058	29,060
	(1.223)	(1.672)	
Left employer	0.059**	-0.016	53,565
	(0.026)	(0.033)	
Receiving SSDI/SSI	0.051***	-0.022**	53,905
	(0.009)	(0.011)	
Receiving any Social Security income	0.083***	-0.047**	53,905
	(0.015)	(0.019)	
Receiving pension or annuity income	0.025	-0.010	53,905
	(0.020)	(0.026)	
Completely retired	0.070***	-0.038*	53,905
	(0.016)	(0.021)	
Completely or partly retired	0.096***	-0.063**	51,205
	(0.021)	(0.027)	
Stopped working earlier than expected	0.038***	-0.029**	53,905
	(0.012)	(0.015)	
Live to 75+ (% chance)	-1.600	-0.070	44,955
	(1.325)	(1.603)	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All models are weighted using weights constructed from estimated propensity scores. All models include individual and interview-wave fixed effects, with standard errors clustered at the household level and reported in parentheses. Unless otherwise noted, outcomes are binary. All outcomes are at the individual level, except for receiving pension or annuity income. Treat*Post reports the effect of experiencing a health shock in the post-shock period. Treat*Post*Above median reports the additional differential effect for respondents above the sample median of liquid assets. Liquid assets are defined as stocks and mutual funds, checking and savings accounts, CDs, government savings bonds, Treasury bills, and bonds and bond funds. Dollar figures are in 2020 dollars. N refers to the total number of respondent-wave observations.