# Aging and Productive Activity*

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**Abstract**

As people grow older, they usually reduce the scope and volume of their productive market and nonmarket activity ("productive activity"). In this paper, I quantify the importance of chronological and functional aging for these reductions using an event study approach that leverages major declines in handgrip strength in Health and Retirement Study data. The empirical analysis demonstrates that health deterioration reduces productive market and nonmarket activity, with treatment effects differing appreciably across age groups. Health deterioration decreases labor force participation among workers younger than 62 and older than 70 but not around retirement ages where labor force exit is primarily driven by chronological aging. Health deterioration further discourages productive nonmarket activity beyond age 70. Among affected older adults, health deterioration explains about 5–6 percent of the decline in labor force participation and 12–18 percent of the decline in productive nonmarket activity, with the respective contributions reaching 16 and 30 percent in some age groups. The results underscore the importance of health for older adults' economic activity, which extends beyond labor markets to substantial nonmarket contributions.

JEL classification: I10, J14, J22, J26

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# 1 Introduction

As people grow older, they usually reduce the scope and volume of their productive market and nonmarket activity ("productive activity"). For instance, labor force participation in the United States roughly halves between ages 60 and 70 (Health and Retirement Study 2025). While generally understood as the result of aging, this reduction may have multiple causes: some related to chronological aging (e.g., pension eligibility), others related to functional aging (e.g., declining work capacity), and yet others unrelated to aging (e.g., care responsibilities).

Scientific and policy debate usually does not explicitly distinguish between these mechanisms and often focuses on chronological age alone, although chronological and functional aging may vary in pace and influence on productive activity. Indeed, chronological age is a poor proxy for functional age because it fails to capture the diverse ways in which people age, especially when age patterns of health are changing (Kotschy et al. 2025). In an increasingly long-living society, the distinction between chronological and functional aging is therefore essential for understanding the role of aging in productive activity and the design of pension and disability insurance policy.

In this paper, I analyze the extent to which chronological and functional aging influence productive activity among older Americans. The paper addresses two questions. The first question is: how much of the observed decline in productive activity among older adults do chronological and functional aging explain? Taking a broad perspective on time use, I consider not only the effects of aging on productive market activity (i.e., labor supply) but also on productive nonmarket activity (i.e., household production, caregiving, and volunteering), which have received limited attention in the literature. These effects may vary across life stages depending on institutional factors such as pension law. The second question thus is: to what extent do the effects of aging vary depending on whether people are younger, around, or older than retirement ages?

A key challenge is the modeling and measurement of functional aging, defined as the permanent loss of function due to health deterioration. Research estimating the effects of health deterioration on employment commonly assumes that people's health-related "work capacity" or "health capital" can be represented by a low-dimensional, typically scalar, health indicator (see Currie and Madrian 1999; French and Jones 2017, for a literature survey). Disagreement in the literature arises from the measurement of these concepts. Some studies use subjective health measures (e.g., self-assessed health and pain); others rely on objective measures (e.g., diagnosed diseases and chronic conditions); and yet others aggregate subjective and objective measures into a single indicator. However, none of these approaches is free of problems. Subjective measures might not be accurate, interpersonally comparable, or independent of productive activity; objective measures might not adequately represent work capacity or health capital; and combining both types of measures might not guarantee that the different measurement errors cancel out one another (Bound 1991).

In view of these measurement challenges, the literature continues to search for better health indicators, ranging from epigenetic to function-based measures (for a review, see Kotschy et al. 2025). Among those, functional capacity indicators constitute a promising class of health proxies that both comprehensively measure people's physical and mental capacities across various dimensions and broadly correspond to the functional requirements people encounter at work and in daily living. Conceptually, they are grounded in the World Health Organization's framework of healthy aging, which posits that a person's functional ability depends on their physical and mental capacities and the environment in which they live (Beard et al. 2016).

Here I use major declines in handgrip strength ("health shocks") to estimate the effects of functional aging on productive activity in U.S. Health and Retirement Study data. Grip strength is a widely applied indicator to assess muscular strength among people of all ages and reliably predicts current and future health (Tomkinson et al. 2025). While grip strength might at first glance appear as an indicator of physical capacity only, it is in fact a powerful biomarker for overall health: low grip strength has been linked to a wide range of adverse health conditions including premature mortality, comorbidity, frailty, nutritional deprivation, cognitive decline, and depression (Fried et al. 2001; Bohannon 2008, 2019; Norman et al. 2011). Moreover, I will show that it is also indicative of low work capacity measured by work-limiting health problems, pain, arthritis, poor self-assessed health, Social Security Disability Insurance benefit receipt, and dementia.

I apply an event study approach with staggered treatment adoption. Using Waves 8–16 of the Health and Retirement Study (collected through 2006–2022) and its Consumption and Activities Mail Survey, I examine the effects of functional and chronological aging on productive market and nonmarket activity at the extensive and intensive margins. I analyze these effects for pooled and age-specific samples of older adults, focusing on age groups 50–61 (those younger than the early retirement age), 62–70 (those around retirement ages), and 71–80 (those older than the latest age at which delaying Social Security adds more earning years to pension benefits).

I consider the contribution of health shocks to observed declines in productive market and nonmarket activity from two perspectives. First, I estimate the average treatment effect to quantify this contribution at the population level. Second, I estimate the average treatment effect on the treated to quantify this contribution at the treatment group level. The empirical model is fitted with the two-way fixed effects estimator and Borusyak et al.'s (2024) efficient imputation estimator, which is robust to treatment effect heterogeneity.

The empirical analysis demonstrates that health shocks reduce productive market and nonmarket activity, with treatment effects differing appreciably across age groups. In the pooled sample, respondents are on average 7 percentage points less likely to participate in the labor force and 5–6 percentage points less likely to engage in 10 or more weekly hours of productive nonmarket activities after experiencing a health shock. However, these averages mask significant effect heterogeneity

across age groups. Whereas health shocks reduce labor force participation among respondents aged 50–61 and 71–80, the results indicate a precise zero effect for those aged 62–70, with labor force exit around retirement ages being predominantly driven by chronological aging. With regard to productive nonmarket activity, all of the estimated reduction in the pooled sample comes from respondents aged 71 and older, with small and insignificant effects for younger respondents.

At the intensive margin, health shocks affect productive nonmarket activity but not labor supply. After experiencing a health shock, respondents spend on average about 1 hour and 18 minutes less on productive nonmarket activities and perform 0.3 (out of 13) fewer of those activities. In contrast, health shocks do not significantly affect usual weekly work hours, indicating that changes in productive market activity operate primarily through the extensive margin.

How much health shocks contribute to the declines in productive market and nonmarket activity among older adults depends on whether one considers these contributions at the population or treatment group levels. For a given activity and age group, the contribution equals the estimated treatment effect multiplied by the corresponding treatment probability and divided by the unconditional change in that activity. At the population level, health shocks explain on average only 2–3 percent of the decline in productive market and nonmarket activity because of the low treatment probability of about one-tenth among all respondents. In contrast, at the treatment group level, health shocks contribute 5–6 percent of the decline in labor force participation and 12–18 percent of the decline in productive nonmarket activity, with respective contributions reaching 16 and 30 percent in some age groups. Accordingly, functional aging accounts for a moderate to sizable portion of reductions in productive market and nonmarket activities among affected older adults, even though that portion is modest when considered in the context of the overall population.

The empirical findings contribute to the literature on health's role in time use and retirement. A key contribution is the causal analysis of how health deterioration affects productive nonmarket activity. Although older adults produce about seven percent of gross domestic product in the United States through productive nonmarket activities every year (Bloom et al. 2020), the effects of health deterioration on these activities have received little attention. Much of the literature has concentrated on substitution patterns between household production and consumption expenditures or household spending (e.g., Aguiar and Hurst 2005; Been et al. 2020; Zilio et al. 2025). The limited research that has instead investigated health and time use in the United States more systematically tends to be descriptive and has relied on self-reported health measures (Enam et al. 2018; Ferranna et al. 2023; Hamermesh and Myck 2025). In this paper, I estimate the effects of health shocks on productive nonmarket activity and quantify how much those health shocks contribute to declining productive nonmarket activity among older adults. My results demonstrate that health shocks explain at least 12 and up to 30 percent of the decline in productive nonmarket activity among affected older adults. These contributions are about twice as large as those for productive market activity.

The paper also adds to the literature on health's role in labor supply and retirement. Recent research documents that health deterioration reduces employment of workers aged 50–70 by 3–15 percent across various health indicators in the Health and Retirement Study (Blundell et al. 2023), with emergency hospitalizations accounting for employment declines of 12–15 percent among workers aged 50–59 (Dobkin et al. 2018). Health change also affects retirement expectations. Using hypothetical questions about retirement in different health scenarios, Giustinelli and Shapiro (2024) find that people aged 57 and older report 26 percentage points lower probabilities to work four years after onset of a bad health scenario. My estimation results indicate that the extent to which health deterioration affects labor supply varies appreciably with age and that health deterioration does not explain any of the decline in labor force participation around retirement ages. This evidence supports findings from structural approaches emphasizing the importance of Social Security and Medicare for retirement decisions (French 2005; French and Jones 2011), and suggests that employment reductions in response to health change might be smaller than people expect ex ante.

The empirical findings align with research on the employment effects of disability insurance. This literature shows that Social Security Disability Insurance eligibility discourages work (Maestas et al. 2013) and creates incentives to leave the labor force (Autor and Duggan 2003). The treatment effects of health shocks on labor force participation estimated in this paper include behavioral responses along these lines. The literature further documents that few employers accommodate newly disabled workers (Hill et al. 2016) and, more generally, that fixed costs make adjustments in work hours unattractive for firms (Rogerson and Wallenius 2013). My finding that health shocks do not affect labor supply at the intensive margin is consistent with this lack of accommodation and limited adjustments in work hours.

Finally, this paper applies an alternative approach to measure work capacity. A concern about many objective health proxies is that they provide only an incomplete picture of work capacity because they describe diagnosed health conditions but miss information on condition severity (Blundell et al. 2023, p. 285). Grip strength measurements address these concerns, predict a wide range of adverse health conditions, and are comparable across people. Some studies instead use factor analysis to combine multiple health measures into a summary indicator (e.g., Chen et al. 2025; Cutler et al. 2025). While such approaches are valuable for capturing multiple health dimensions simultaneously, they produce composite measures where the relative importance of different health components may vary across studies and datasets, potentially limiting comparability. Despite its advantages, grip strength measurement has seen limited application when assessing health deterioration in economic analysis. One notable exception is work by Decker and Schmitz (2016) that uses grip strength measurements to estimate the effect of health shocks on risk aversion.

# 2 Empirical Framework

I analyze the extent to which health deterioration affects productive market and nonmarket activity with a linear event study framework. I begin by considering a simple conceptual model of the form:

$$Y_{it} = \tau H_{it} + X_{it}'\delta + \varepsilon_{it}, \tag{1}$$

where $Y_{it}$ denotes an indicator of productive activity for individual $i = 1, ..., N$ at time $t = 1, ..., T$; $H_{it}$ denotes a scalar summary measure of health; $X_{it}$ denotes a vector of covariates that may include fixed effects and control variables that may vary across individuals and over time; $\delta$ denotes a parameter vector; and $\varepsilon_{it}$ denotes an error term. While simple, this framework can be derived from a structural model of life-cycle labor supply and health to study the effects of health on employment (Blundell et al. 2023). Outcome $Y_{it}$ may be either binary (extensive margin) or continuous (intensive margin) and may represent productive market activity (e.g., labor force participation) or productive nonmarket activity (e.g., performing more than 10 weekly hours of productive nonmarket activity).

The parameter of interest is $\tau$. It reflects the effects of health changes on productive market and nonmarket activity. These effects can operate through various pathways. For instance, health deterioration might (i) impact market and nonmarket labor-leisure trade-offs; (ii) reduce productivity in a given activity and, related to that, remuneration for market labor; (iii) influence access to employer-sponsored health insurance and disability benefits; (iv) raise medical expenditures and deplete savings; and (v) shape expectations about future health and the capacity to engage in productive activity. Ceteris paribus, $\tau$ captures the total effect of health changes on productive market and nonmarket activity across all mechanisms (rather than the effect of any single mechanism).

However, fitting an empirical model like equation (1) usually does not identify $\tau$. One reason is that the health indicator might be mismeasured and not adequately capture an individual's work capacity (an issue to which I turn in Section 3.2). Estimation of $\tau$ might also be confounded for several other reasons. First, changes in productive market and nonmarket activity (e.g., retirement) might have a reverse causal effect on health. Second, individuals might anticipate gradual health decline and reduce the volume of demanding productive activities and instead spend more time on leisure. Third, both individuals' health trajectories and their decisions to pursue productive activities might be jointly determined by another omitted factor.

To address these identification concerns, the empirical analysis relies on health shocks based on major changes in individuals' health status (e.g., due to accident or illness). The rationale behind this approach is simple: health shocks are uncommon and thus hard to anticipate, they are sizable and thus unlikely to result from reverse causality, and they are arguably random conditional on socioeconomic circumstances and past health. For instance, previous research has used hospitalizations to study their effects on employment, income, medical spending, and financial strain

(García-Gómez et al. 2013; Dobkin et al. 2018).

The empirical model estimates the treatment effect of health shocks on productive market and nonmarket activity. There are two potential outcomes $Y_{it} \in \{Y_{it}(0), Y_{it}(1)\}$: productive activity when an individual has experienced a health shock, i.e., $Y_{it} = Y_{it}(1)$ if $D_{it} = 1$, and productive activity when an individual has not experienced a health shock, i.e., $Y_{it} = Y_{it}(0)$ if $D_{it} = 0$, where $D_{it} \in \{0, 1\}$ denotes the treatment indicator.

The empirical model treats health shocks as an absorbing state, so they can have lasting effects on productive activity. Accordingly, the treatment indicator takes value 1 in every period after an individual has experienced a health shock and 0 before, i.e., $D_{it} = 1$ if $t \geq e_i$ and $D_{it} = 0$ if $t < e_i$, where $e_i$ denotes the period when a health shock takes place. This specification implies a staggered treatment design because individuals may experience health shocks at different times.

I fit both dynamic and static event study models. The dynamic model takes the form:

$$Y_{it} = \sum_{\substack{l=-a \\ l \neq -1}}^{b-1} \tau_l \mathbb{1}\left[l = t - e_i\right] + \alpha_i + \beta_t + \gamma_{it} + Z_i' \sum_{k=2}^{T} \delta_k \mathbb{1}\left[k = t\right] + \varepsilon_{it}, \tag{2}$$

where $\tau_l$ denotes the treatment effect at time $l$, which is measured relative to the event period $e_i$ ("relative period"); $a$ and $b-1$ denote the lead and lag periods for the estimation of pre-treatment and post-treatment trends; $\alpha_i$ and $\beta_t$ denote individual and period fixed effects; $\gamma_{it}$ denotes age fixed effects; $Z_i$ denotes a vector of time-invariant control variables measured before the health shock and interacted with period indicators (with $t = 1$ as the omitted period); $\delta_k$ denotes the parameter vector reflecting the associations between control and outcome variables; and $\varepsilon_{it}$ denotes an error term. The dynamic event study model's nonparametric structure allows me to flexibly assess the outcome patterns relative to the health shock, with $l = -1$ as the omitted relative period.

The static event study model takes the form:

$$Y_{it} = \tau D_{it} + \alpha_i + \beta_t + \gamma_{it} + Z_i' \sum_{k=2}^{T} \delta_k \mathbb{1}\left[k = t\right] + \varepsilon_{it}, \tag{3}$$

where $\tau$ denotes the treatment effect; $D_{it}$ denotes the treatment indicator; and everything else is defined as in the dynamic model. Its semiparametric structure implies that pre-treatment trends are zero and that the treatment effect is constant across post-treatment periods. I will visually compare the static and dynamic event study models to assess whether the more restrictive static model provides an adequate description of the effects of health deterioration on productive market and nonmarket activity.

To interpret $\tau$ as the causal effect of health deterioration on productive market and nonmarket activity, several identifying assumptions are necessary. First, the difference in outcomes between

treated and untreated individuals would have been constant over time in the absence of health shocks (parallel trends). Second, individuals do not anticipate health shocks and adjust their productive market and nonmarket activity accordingly (no anticipation). Third, assignment of health shocks is independent from potential outcomes conditional on control variables (conditional independence).

These assumptions are fundamentally untestable, so there is reason to be concerned about their validity a priori. For instance, the parallel-trends and no-anticipation assumptions would be violated if health trajectories and productive activity of treated and untreated individuals systematically differed before the onset of health shocks. This could be the case if individuals who eventually experience health shocks adjust their productive market and nonmarket activity before the health shocks occur. Such adjustments might happen because their health trajectories are already worse than those of individuals who do not experience health shocks, or because they foresee the shocks. If that were the case, there should be systematic pre-treatment trends in productive activity. To rule this case out and support the plausibility of the parallel-trends and no-anticipation assumptions, I use the dynamic event study model to test for pre-treatment trends.

To support the conditional-independence assumption, I take several measures. First, I assess whether health change systematically varies with demographic and socioeconomic factors to gauge the extent to which there might be selection into health shocks along those observable margins. Second, I compare the average treatment effect on the treated (including only treated and not-yet-treated individuals) to the average treatment effect (including also never-treated individuals). This comparison is insightful if health shocks are driven by unobserved factors that are the same for treated and not-yet-treated individuals but not for never-treated ones. Third, I include fixed effects and control variables to capture potential selection into health shocks based on observables.

The empirical model includes individual, period, and age fixed effects. Controlling for individual fixed effects, the model accounts for permanent heterogeneity in preferences, health, productivity, statutory retirement ages (which depend on year of birth), gender, and ethnicity. The period and age fixed effects capture time- and age-specific variation in productive activity. This variation may reflect, for instance, preferences for labor supply and time use, income and consumption patterns, experience, and financial incentives due to pension and Medicare eligibility around retirement ages.

The empirical model further includes control variables that are measured when individuals are first observed (initial conditions) and interacted with period indicators. It includes only initial conditions to avoid bias from contemporaneous controls the treatment may affect. All empirical specifications control for initial health, educational attainment, and initial health insurance coverage by a government plan (including Medicare and Medicaid). These controls account for potential heterogeneity in health trajectories, health-related knowledge, employment opportunities, permanent income, and access to government-sponsored health insurance. In addition, the specifications for productive market activity control for initial employer-sponsored health insurance coverage to

capture the link between employment and health insurance. Instead, the specifications for productive nonmarket activity control for initial labor force status to account for heterogeneity in time budgets.

Another important source of heterogeneity is whether individuals have an initial attachment to productive market and nonmarket activities. For instance, it is implausible that a health shock should affect labor force participation of a homemaker without labor market attachment. I account for this heterogeneity by fitting the empirical models separately for individuals with and without labor market attachment, expecting no effects for those without labor market attachment. Similarly, I fit the empirical model separately for individuals with and without attachment to productive nonmarket activity (defined as spending 10 or more weekly hours on such activities), again expecting no effects for individuals without attachment.

I fit the empirical specifications both with the standard two-way fixed effects estimator and with Borusyak et al.'s (2024) efficient imputation estimator. The main advantage of the two-way fixed effects estimator is that it is simple and intuitive. However, it may produce biased results with staggered treatment timing when treatment effects are heterogeneous due to "forbidden comparisons" of treated with earlier-treated observations (Borusyak et al. 2024, p. 3254). In contrast, the efficient imputation estimator produces consistent estimates even if treatment effects are heterogeneous (given that the identifying assumptions are satisfied). I estimate the effects of health deterioration on productive market and nonmarket activity by fitting both estimators, with a preference for the efficient imputation estimator in case the results differ. Standard errors are clustered at the individual level, allowing for arbitrary variance-covariance matrices.

Fitting dynamic event study models requires choosing the event window (denoted by $a$ and $b-1$ in the empirical model) over which one estimates the pre-treatment and post-treatment trends (for a discussion, see Miller 2023, pp. 214–216). This choice trades off a wider, more informative event window with potentially imprecisely estimated distant leads and lags against a narrower event window that provides an examination of pre-treatment and post-treatment trends for a restricted set of periods only. I follow Schmidheiny and Siegloch's (2023) recommendation and use all available observations, binning distant leads and lags into endpoints in the two-way fixed effects estimators. I choose the same endpoints for the efficient imputation estimator. The exact choice depends on data availability for the different samples and includes 2–4 pre-treatment and post-treatment periods in addition to the event period (i.e., $a \in \{-5,-4,-3\}$ and $b \in \{3,4,5\}$).

# 3 Data and Measurement

This section describes the data used in the analysis, explains the measurement of functional capacity and the definition of health shocks, outlines the measurement of productive market and nonmarket activity and the control variables, and reports descriptive statistics.

## 3.1 Data Sources

The analysis uses individual-level data from the Health and Retirement Study (HRS) and the Consumption and Activities Mail Survey (CAMS), which were harmonized by the RAND Corporation and the Global Gateway to Aging Initiative (CAMS 2023; Health and Retirement Study 2025; Wang et al. 2023). The analysis focuses on Waves 8–16, which were fielded every two years during 2006–2022 and for which detailed data on functional capacity were collected during enhanced face-to-face interviews. Both surveys are representative of the community-dwelling U.S. population aged 50 and older after applying post-stratification weights (Bugliari et al. 2025).

## 3.2 Functional Capacity as a Proxy for Health

A tractable analysis of how health deterioration affects productive market and nonmarket activity requires a low-dimensional, typically scalar health proxy. Assuming that such a proxy does exist (an assumption I maintain throughout this paper), research on economic activity typically relies on two approaches to measure a person's health.

The first approach uses subjective measures from surveys in which respondents self-assess health, health-related work limitations, or pain, assuming that those measures provide an overall health assessment reflective of work capacity. However, that assumption might be problematic. Subjective self-assessments need not be accurate, interpersonally comparable, or independent from economic outcomes, raising concerns about measurement error and justification bias (Bound 1991).

The second approach uses objective measures based on limitations in select activities, diagnosed diseases, and biomarkers to alleviate measurement concerns. However, while objective measures guarantee interpersonal comparability and preclude justification bias, they may still suffer from measurement error. Specifically, the observed health dimensions need not reflect work capacity or adequately represent overall health (Bound 1991).

Combination of subjective and objective health measures into a single index does not resolve the concerns about measurement error either as there is no guarantee that errors cancel out one another. Literature has therefore suggested to instrument subjective health measures with objective ones (Stern 1989; Blundell et al. 2023). However, this approach might violate the exclusion restriction. For instance, if a disease diagnosis leads to medical spending and if health insurance is tied to employment (as in the United States), then the objective health measure could causally affect economic activity through a pathway other than work capacity in the first stage. Thus, the question of how to adequately measure health when estimating its effects on economic activity remains.

In view of these measurement challenges, literature has searched for alternative health proxies, of which functional capacity indicators constitute a promising class (see Kotschy et al. 2025, for a literature survey). These indicators both comprehensively measure people's physiological

functioning across various dimensions and broadly correspond to the functional requirements people encounter at work or in their daily lives.

The most prominent approach to measuring functional capacity has been pioneered by the World Health Organization. Its framework of healthy aging defines functional ability as the combination of a person's intrinsic capacity (capturing all physical and mental capacities) and the environment in which that person lives (Beard et al. 2016). In practice, researchers construct intrinsic capacity from cognitive, sensory, vitality, locomotor, and psychological survey items using bifactor analysis (e.g., Beard et al. 2019; Chen et al. 2025). While this approach provides valuable comprehensive health measures, high data requirements and variation in items across surveys potentially limit cross-study comparability and reproducibility of these measures.

Here I approximate functional capacity with maximum grip strength (a core component of intrinsic capacity). This approach is less data-intensive than constructing intrinsic capacity, requires no structural modeling, but still produces a comprehensive health indicator.

Grip strength measurement with a handheld dynamometer is a cheap and widely applied method to assess a person's muscular strength among all ages, predicting current and future health (Tomkinson et al. 2025). Low grip strength levels have been linked with a wide range of adverse health conditions. For instance, low levels of grip strength are associated with a greater likelihood of premature mortality, disability, comorbidity, frailty, nutritional deprivation, hospitalization, cognitive decline, and depression (Fried et al. 2001; Norman et al. 2011; Bohannon 2008, 2019).

Grip strength is also associated with health indicators that are related to work capacity and plausibly influence people's ability to perform productive nonmarket activities. For instance, low levels of grip strength are correlated with a greater probability of health problems that limit work, Social Security Disability Insurance or Social Security Income benefit receipt, poor self-assessed health, arthritis diagnosis, moderate or severe pain, and diagnosis of Alzheimer's disease or a related dementia (Table 1). While these links do not imply that grip strength causally affects each health outcome, its robust predictive power for such a wide range of outcomes makes it a powerful biomarker for aging and health deterioration (Bohannon 2008, 2019).

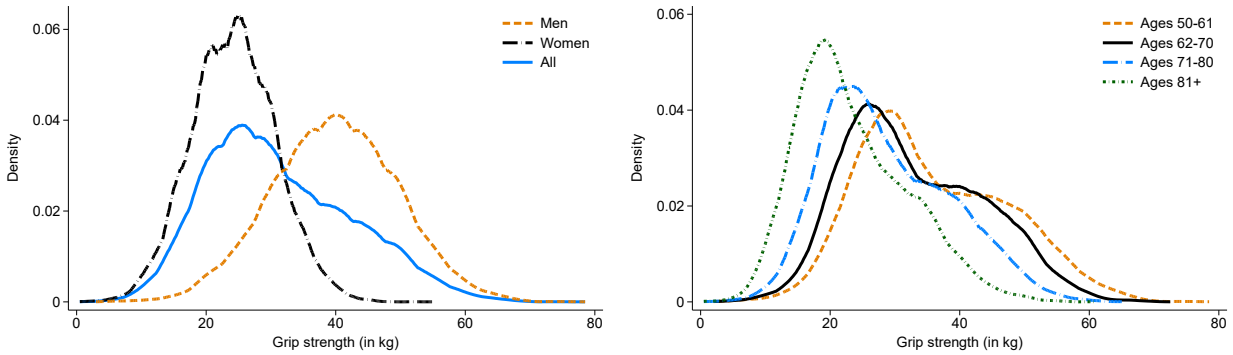
In the Health and Retirement Study, every wave a subset of respondents is selected for enhanced face-to-face interviews during which a nurse takes physical measures. Grip strength has been consistently measured in enhanced interviews for a large number of respondents since Wave 8. Nurses measure respondents' grip strength twice for each hand and record maximum grip strength as the highest value among all measurements (Bugliari et al. 2025). Notably, nurses measure grip strength only every other wave, such that current and previous grip strength measurements lie four years apart. This timing structure is important for the construction of health shocks and the estimation of their effect on economic outcomes, as I detail further below.

The sample includes 44,206 grip strength measurements for 20,041 respondents. Figure 1 shows

Table 1: Association of grip strength with various adverse health conditions

| Dependent variable: | Health problems limit work (1) | Receives SSDI or SSI benefits (2) | Self-assesses health as poor (3) | Has arthritis (4) | Reports moderate or severe pain (5) | Has Alzheimer's disease or a related dementia (6) |
|---|---|---|---|---|---|---|
| Grip strength (in kg) | -0.006*** | -0.001*** | -0.005*** | -0.002*** | -0.001*** | -0.001*** |
| | (0.001) | (0.000) | (0.001) | (0.000) | (0.000) | (0.000) |
| $R^2$ | 0.70 | 0.85 | 0.68 | 0.87 | 0.60 | 0.67 |
| Mean of $Y$ | 0.33 | 0.09 | 0.24 | 0.61 | 0.06 | 0.02 |
| Respondents | 12,104 | 12,809 | 12,890 | 12,902 | 12,133 | 10,285 |
| Observations | 33,208 | 36,811 | 37,028 | 37,066 | 33,130 | 25,985 |

Note: Author's calculations. This table shows the association between grip strength (measured in kilograms) and six health-related outcomes. These include the probabilities of: reporting health problems that limit work (specification 1); receiving Social Security Disability Insurance (SSDI) or Supplemental Security Income (SSI) benefits (specification 2); self-assessing one's own health as poor (specification 3); having arthritis (specification 4); reporting moderate or severe pain (specification 5); and having been diagnosed with Alzheimer's disease or a related dementia (specification 6). Coefficients are obtained from ordinary least squares and refer to respondents aged 50 and older. All specifications include respondent, period (wave), and age fixed effects. Mean of $Y$ reports the sample mean of the respective dependent variable. Standard errors are clustered at the respondent level and reported in parentheses. Asterisks indicate significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.




(a) Grip strength by gender

(b) Grip strength by age group

Note: Author's calculations. This figure depicts the distribution of grip strength (in kg) by gender (Panel a) and age group (Panel b). The densities were estimated using 44,195 observations for respondents aged 50 and older (11 observations with a grip strength of more than 80kg are excluded from the figure for visual clarity). Panel (a) includes 24,136 women and 20,059 men. Panel (b) includes 13,919 respondents aged 50–61, 11,551 aged 62–70, 12,139 aged 71–80, and 6,586 aged 81 and older.

Figure 1: Distribution of grip strength by gender and age group

the distribution of grip strength (measured in kilograms) by gender and age group. Average grip strength is 31.3kg (standard deviation 11.3kg), with men having a 65 percent higher grip strength than women (39.9kg vs. 24.2kg). Grip strength is normally distributed for each gender, but the overall distribution is right-skewed as grip strength varies more among men (Figure 1, Panel a).

Older people tend to have lower grip strength than younger ones. As age increases, the grip strength distribution shifts to the left (Figure 1, Panel b). For instance, the average grip strength in age group 71–80 is 7.1kg lower than in age group 50–61 (28.8kg vs. 35.9kg). These age groups correspond to respondents younger than the early retirement age at 62 and those older than 70, the latest age at which delaying Social Security adds more earning years to pension benefits. The

distribution is also less skewed for older age groups as women account for a larger portion of the sample and gender-specific grip strength variances decline with age.

Of the 20,041 respondents with grip strength measurements, 12,345 have at least two (and up to five) consecutive measurements, enabling longitudinal analysis of health changes (23,047 observations in total). Grip strength levels and variances for this subsample are similar to those for the full sample. For these 12,345 respondents, I next construct health shocks based on significant declines in grip strength to identify the effects of health deterioration on productive market and nonmarket activity.

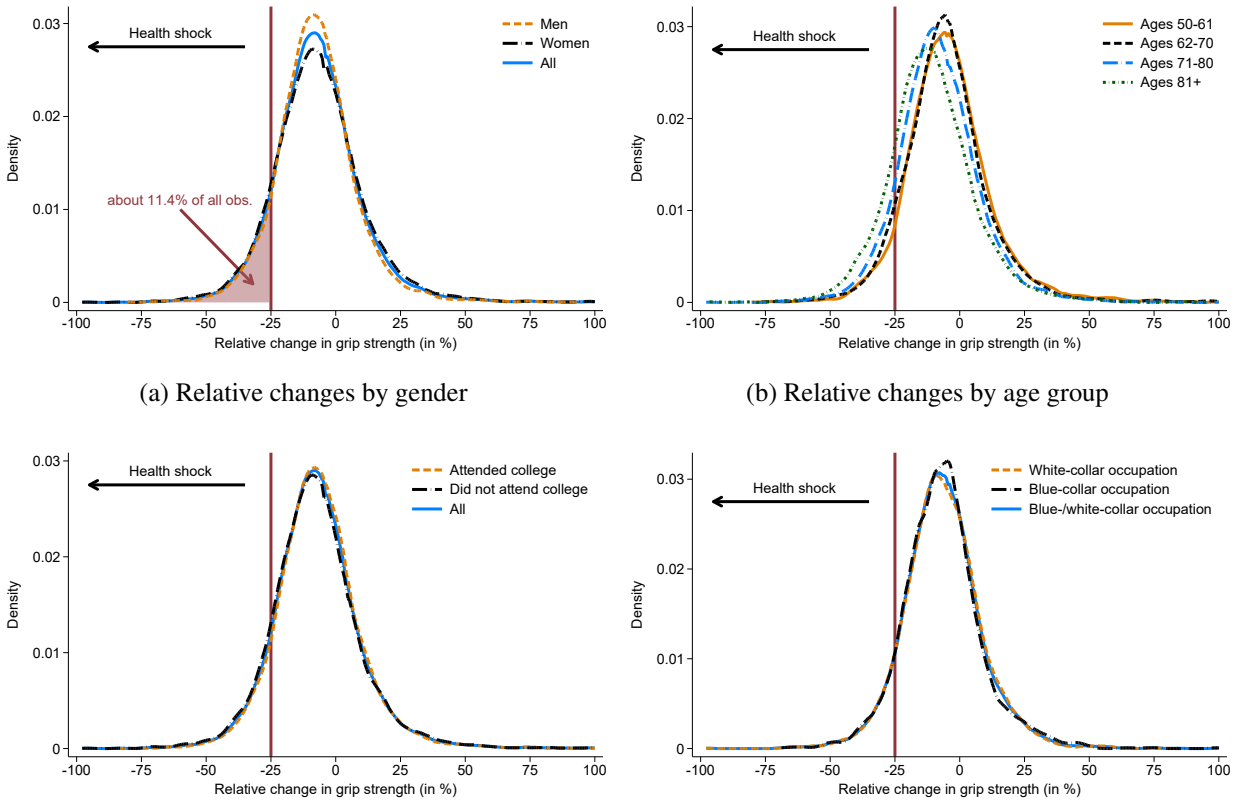
## 3.3 Construction of Health Shocks

To identify how health deterioration affects productive market and nonmarket activity, I require an exogenous health indicator. Gradual changes in grip strength need not fulfill this criterion for several reasons. First, changes in productive activity (e.g., retirement) might have a reverse causal effect on functional capacity. Second, older adults might anticipate gradual declines in their functional capacities and decide to cease or reduce the volume of some activities. Third, health trajectories and the decisions to pursue specific activities might be jointly driven by another omitted factor.

To address these concerns, I construct health shocks based on major changes in grip strength. Importantly, the Health and Retirement Study measures respondents' grip strength only every other survey wave, with measurement timing varying across respondents. I define a health shock as a decline in grip strength of at least 25 percent between two consecutive measurements; that is, between grip strength measurements taken four years apart. This definition follows findings from public health literature showing that such declines are predictive of premature mortality and disability in older people (Ling et al. 2010).

Health shocks of this size are uncommon and hard to anticipate. For adults in their fifties, it takes about 25 years for grip strength to decline by 25 percent, with slightly faster declines among adults aged 80 and older (Tomkinson et al. 2025, Table 2). In view of this slow decline across the population, it is unlikely that changes in productive market and nonmarket activities drive relative declines in grip strength of 25 percent or more. Figure 2 shows the distribution of relative changes in grip strength observed in the Health and Retirement Study. Relative changes in grip strength are normally distributed and broadly similar for men and women (Figure 2, Panel a). According to the definition above, about 11.4 percent of all observations (and 7.5 percent of the observations for respondents younger than 62) qualify as health shocks. As age increases beyond 70, the density curve for relative changes in grip strength shifts to the left, slightly raising the probability of experiencing a health shock (Figure 2, Panel b).

Relative changes in grip strength do not differ systematically across socioeconomic status. The




(a) Relative changes by gender

(b) Relative changes by age group

(c) Relative changes by educational attainment

(d) Relative changes by occupational class

Note: Author's calculations. This figure depicts relative changes in grip strength by gender (Panel a), age group (Panel b), educational attainment (Panel c), and occupational class (Panel d). The depicted density functions refer to all adults aged 50 and older. Health shocks are defined as a decline in grip strength of 25 percent or more between two consecutive measurements. The density functions in Panels (a)–(c) were estimated using 23,004 observations (43 observations with grip strength increases exceeding 100 percent are excluded from the figure for visual clarity). Panel (a) includes 12,552 women and 10,452 men. Panel (b) includes 4,554 respondents aged 50–61, 6,571 aged 62–70, 7,489 aged 71–80, and 4,390 aged 81 and older. Panel (c) includes 11,983 respondents with at least some college education and 11,017 without college education. The density function in Panel (d) was estimated using 5,652 observations with information on occupational status, of which 4,226 are classified as white-collar and 1,426 as blue-collar.

Figure 2: Relative changes in grip strength and coding of health shocks

density curves for relative changes in grip strength are all but identical with respect to educational attainment (Figure 2, Panel c) and occupational class (Figure 2, Panel d). This insensitivity of the health shock indicator to socioeconomic status is reassuring. Educational attainment and occupational class are both potential factors that could concomitantly affect health trajectories and productive activity, or be related to omitted factors that would confound the regression analysis.

The empirical framework treats health shocks as an absorbing state. This assumption is motivated by the observation that average grip strength recovers only marginally after people experience a health shock and remains substantially below pre-shock levels (see Figure A.1 in the appendix). Apart from realistically describing the permanent loss of functional capacity associated with health shocks, the assumption makes it possible to use the available information in the Health and Retirement Study more efficiently. Specifically, it allows me to classify all observations after a

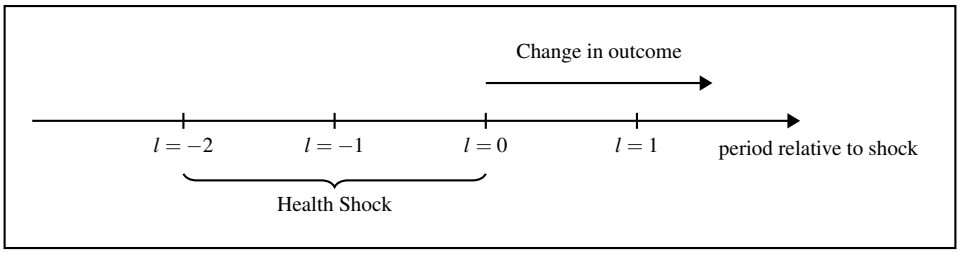
health shock as treated and before a health shock as untreated even if for a specific observation no grip strength measurement is available. This can happen because grip strength is measured only every other wave or because a respondent is in too poor health to take the grip strength measurement.

The measurement of grip strength every other survey wave introduces uncertainty about the exact time health shocks take place. Denote the period in which a respondent's grip strength has declined by 25 percent or more relative to the previous measurement as $e_i$, with all other periods defined relative to $e_i$ such that $l = t - e_i$. This relative-period notation is necessary as respondents may experience health shocks in different survey waves. The set $l \in \{-8, -7, ..., -2, -1, 0, 1, ..., 6\}$ covers all available sample periods for HRS Waves 8–16 (denoted as $t = 1, ..., T$ in the empirical model): it ranges from $l = -8$ for respondents who experience health shocks in the latest waves (with data available since Wave 8) to $l = 6$ for respondents who experience health shocks in the earliest waves (with follow-up data through Wave 16). The timing uncertainty arises because the data structure requires me to define health shocks based on the grip strength measurements in periods $l = -2$ and $l = 0$: a health shock can take place early (between $l = -2$ and $l = -1$), late (between $l = -1$ and $l = 0$), or continuously over the entire range.

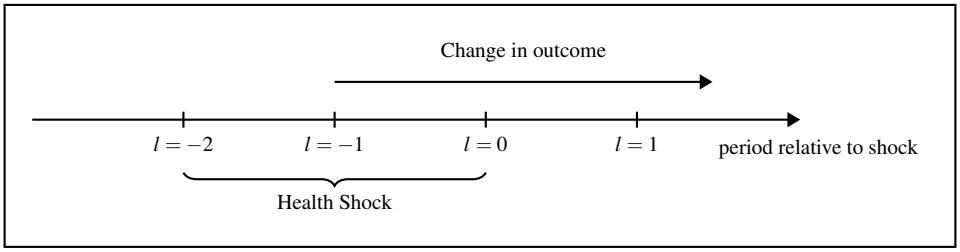
Addressing the timing uncertainty about health shocks requires choosing the relative period in which a health shock may first affect the outcome variable. Figure 3 illustrates this modeling choice. In the textbook case, health shocks would precede respondents' behavioral responses in relative periods $l \in \{0, 1, ...\}$ (Figure 3, Panel a). However, this model structure would miss treatment effects between relative periods $l = -1$ and $l = 0$ for respondents who experience health shocks early (between $l = -2$ and $l = -1$), and incorrectly ascribe them to the pre-treatment trends. To preclude this possibility, I allow for behavioral changes one period earlier, such that health shocks can affect productive activity in $l \in \{-1, 0, 1, ...\}$ (Figure 3, Panel b). Because some respondents experience health shocks late (between $l = -1$ and $l = 0$) or continuously over the range $l = -2$ to $l = 0$, this modeling choice implies that it should take up to two periods for the treatment effects to fully materialize. I inspect the plausibility of this modeling choice with event study plots.

## 3.4 Measurement of Productive Market and Nonmarket Activity

The outcome of interest is whether a respondent engages in productive market and nonmarket activities. These outcomes can be measured both at the extensive and intensive margins. I measure productive market activity by respondents' labor force status at the extensive margin and by usual hours worked last week at the intensive margin. Similarly, I measure productive nonmarket activity by a binary indicator for spending 10 or more hours per week on such activities at the extensive margin and by weekly hours spent on those activities at the intensive margin. In addition, I count the number of productive nonmarket activities a respondent performs.




(a) Model in which outcome changes follow health shocks



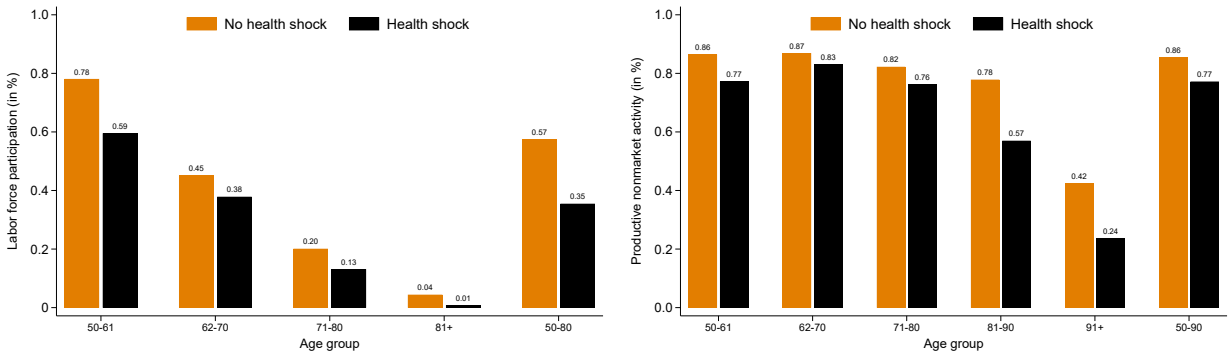

(b) Model in which health shocks and outcome changes partly overlap

Note: This figure illustrates the timing uncertainty about health shocks and model structures to address it. The model in Panel (a) assumes that people change their behavior in response to a health shock only in relative periods $l \in \{0, 1, ...\}$ after the health shock has occurred. In contrast, the model in Panel (b) allows for behavioral changes in $l \in \{-1, 0, 1, ...\}$, accounting for timing uncertainty in health shocks.

Figure 3: Timing uncertainty about health shocks and model structure

I construct measures for productive nonmarket activity from information on time use in the Consumption and Activities Mail Survey. Respondents report how many hours they have spent on 37 different activities in the last week or month (I harmonize responses to weekly time counts). Of those activities, I classify as productive nonmarket activities any unpaid activity that might be replaced by market goods or services if the economic circumstances, market conditions, and personal inclinations permit delegation of the activity to a third party (adapted from Reid 1934, p. 11). In contrast to Reid's definition of household production, my classification also includes activities that create value for non-household members such as caregiving and volunteering.

According to this definition, 13 activities qualify as productive nonmarket activities: (i) house cleaning; (ii) washing clothes; (iii) yard work or gardening; (iv) shopping; (v) preparing meals; (vi) caring for pets; (vii) treating or managing the medical condition of another person; (viii) helping friends, neighbors, or relatives who did not pay for the help; (ix) doing volunteer work for religious, educational, health-related, or other charitable organizations; (x) taking care of finances or investments; (xi) doing home improvements or making home repairs; (xii) working on, maintaining, or cleaning car(s) or vehicle(s); (xiii) paying or managing medical bills (see Section A.2 in the appendix for details on survey items and coding). On average, respondents in the Consumption and Activities Mail Survey spend about 3 hours and 22 minutes on productive nonmarket activities each day. This figure (based on respondents' recollections of their time use) falls in the same range as the 3 hours and 40 minutes (based on detailed time diaries) that Hamermesh (2019, p. 18) documents




(a) Labor force participation

(b) Productive nonmarket activity

Note: Author's calculations. This figure depicts the labor force participation rate by health shock status and age group (Panel a) and the share of people performing at least 10 hours of productive nonmarket activities per week by health shock status and age group (Panel b). Health shocks are defined as a decline in grip strength of 25 percent or more between two consecutive measurements and treated as an absorbing state. The labor force participation rate was estimated using 20,901 observations; the share of people performing at least 10 hours of productive nonmarket activities was estimated using 17,710 observations. Observations were weighted with sampling weights for representativeness.

Figure 4: Productive market and nonmarket activity by health status and age group

for household production in the more comprehensive American Time Use Survey.

Productive market and nonmarket activity declines with age and health status. Figure 4 shows labor force participation and the share of people performing 10 or more weekly hours of productive nonmarket activities by age group and health shock status. Labor force participation falls from close to 80 percent for respondents aged 50–61 to almost zero percent for those aged 81 and older (Figure 4, Panel a). It is systematically lower for respondents who have experienced a health shock than for those who have not. Nevertheless, an appreciable share of respondents remains in the labor force after having experienced health shocks. Like labor force participation, productive nonmarket activity also decreases with age (Figure 4, Panel b). However, the decrease takes place predominantly at later ages. Around legal retirement ages, productive nonmarket activity even slightly increases, consistent with the notion that people can spend more time on those activities after retirement (Aguiar and Hurst 2005). Together, these patterns suggest an appreciable role of health deterioration in productive activity and point to potential effect heterogeneity with respect to age.

## 3.5 Control Variables

The empirical analysis uses control variables to support the conditional-independence assumption and to split the sample with respect to respondents' attachment to productive market and nonmarket activities, with controls varying across specifications. These control variables are time-invariant and measured when a respondent is first observed in the sample, ensuring they precede health shocks. They include initial grip strength (measured in kilograms); educational attainment (binary indicator

for whether respondent has attended college); initial health insurance coverage by a current or previous employer (binary indicator for whether respondent is covered); initial health insurance coverage by a government plan including Medicare and Medicaid (binary indicator for whether respondent is covered); initial labor force status (binary indicator for whether respondent is in labor force); and initial productive nonmarket activity (binary indicator for whether respondent performs 10 or more weekly hours of productive nonmarket activities).

## 3.6 Descriptive Statistics

Table 2 shows descriptive statistics for respondents aged 50–80 by health status. Panel (a) reports results for the full sample (72,783 observations), comparing respondents who have experienced health shocks with those who have not. Panel (b) reports results for a restricted sample (13,541 observations) including only respondents who either have experienced or will eventually experience health shocks, comparing their characteristics before and after the health shock occurs.

The unconditional data show that respondents who have experienced health shocks are 14–20 percentage points less likely to be in the labor force than those who have not. Similarly, respondents who have experienced health shocks work about 2–3 fewer hours per week, are 3 percentage points less likely to engage in productive nonmarket activity, and perform 0.5–0.7 fewer of those activities than respondents who have not experienced health shocks. About 50–60 percent of respondents are in the labor force when they are first observed in the sample and about 85 percent perform nonmarket activities. This information is important because health shocks should only affect the probability of engaging in productive market and nonmarket activities for respondents who have an attachment to those activities. I will use this information in the regression analysis to split the sample into respondents with and without attachment to productive market and nonmarket activities.

Respondents who have experienced health shocks and those who have not differ along several dimensions. First, respondents with health shocks tend to be 3–4 years older than those without health shocks. Second, in the restricted sample, they are less likely to have health insurance coverage through a government plan and more likely to have employer health insurance coverage. Third, respondents with and without health shocks also differ slightly with respect to initial grip strength and educational attainment, though with mixed patterns across the restricted and unrestricted samples. The systematic age differences greatly diminish when grouping respondents in narrower age groups: 50–61, 62–70, 71–80 (see Tables A.1 and A.2).

Table 2: Descriptive statistics for respondents aged 50–80 by health status

| | *Health shock = 0* | | *Health shock = 1* | | | |
|---|---|---|---|---|---|---|
| Variable | Mean | Observations | Mean | Observations | Difference | *p*-value |
| **(a) Treated, not-yet-treated, and untreated respondents** | | | | | | |
| In labor force (in %) | 0.44 | 67,365 | 0.24 | 5,103 | 0.20*** | 0.000 |
| Weekly hours worked | 34.87 | 27,166 | 31.58 | 1,129 | 3.29*** | 0.000 |
| Productive nonmarket activity (in %) | 0.84 | 16,626 | 0.81 | 1,026 | 0.03** | 0.024 |
| Weekly hours of productive nonmarket activity | 24.70 | 16,626 | 23.94 | 1,026 | 0.77 | 0.150 |
| Number of productive nonmarket activities | 7.99 | 16,626 | 7.30 | 1,026 | 0.69*** | 0.000 |
| Share male (in %) | 0.46 | 67,648 | 0.41 | 5,135 | 0.05*** | 0.000 |
| Age (in years) | 66.29 | 67,648 | 70.33 | 5,135 | -4.04*** | 0.000 |
| Initial grip strength (in kg) | 34.92 | 67,648 | 35.39 | 5,135 | -0.47*** | 0.005 |
| Initial government health insurance coverage (in %) | 0.44 | 67,648 | 0.43 | 5,135 | 0.01 | 0.444 |
| Initial employer health insurance coverage (in %) | 0.42 | 67,648 | 0.41 | 5,135 | 0.01 | 0.123 |
| Initial labor force participation (in %) | 0.60 | 67,365 | 0.58 | 5,103 | 0.02*** | 0.002 |
| Initial productive nonmarket activity (in %) | 0.85 | 16,626 | 0.87 | 1,026 | -0.01 | 0.224 |
| Attended college (share in %) | 0.53 | 67,639 | 0.52 | 5,135 | 0.01 | 0.151 |
| **(b) Only treated and not-yet-treated respondents** | | | | | | |
| In labor force (in %) | 0.39 | 8,401 | 0.24 | 5,103 | 0.14*** | 0.000 |
| Weekly hours worked | 33.54 | 2,958 | 31.58 | 1,129 | 1.96*** | 0.000 |
| Productive nonmarket activity (in %) | 0.84 | 2,337 | 0.81 | 1,026 | 0.03** | 0.047 |
| Weekly hours of productive nonmarket activity | 24.42 | 2,337 | 23.94 | 1,026 | 0.48 | 0.426 |
| Number of productive nonmarket activities | 7.81 | 2,337 | 7.30 | 1,026 | 0.51*** | 0.000 |
| Share male (in %) | 0.42 | 8,406 | 0.41 | 5,135 | 0.01 | 0.187 |
| Age (in years) | 67.15 | 8,406 | 70.33 | 5,135 | -3.18*** | 0.000 |
| Initial grip strength (in kg) | 33.45 | 8,406 | 35.39 | 5,135 | -1.94*** | 0.000 |
| Initial government health insurance coverage (in %) | 0.59 | 8,406 | 0.43 | 5,135 | 0.15*** | 0.000 |
| Initial employer health insurance coverage (in %) | 0.37 | 8,406 | 0.41 | 5,135 | -0.05*** | 0.000 |
| Initial labor force participation (in %) | 0.49 | 8,401 | 0.58 | 5,103 | -0.09*** | 0.000 |
| Initial productive nonmarket activity (in %) | 0.86 | 2,337 | 0.87 | 1,026 | -0.01 | 0.675 |
| Attended college (share in %) | 0.49 | 8,406 | 0.52 | 5,135 | -0.03*** | 0.002 |

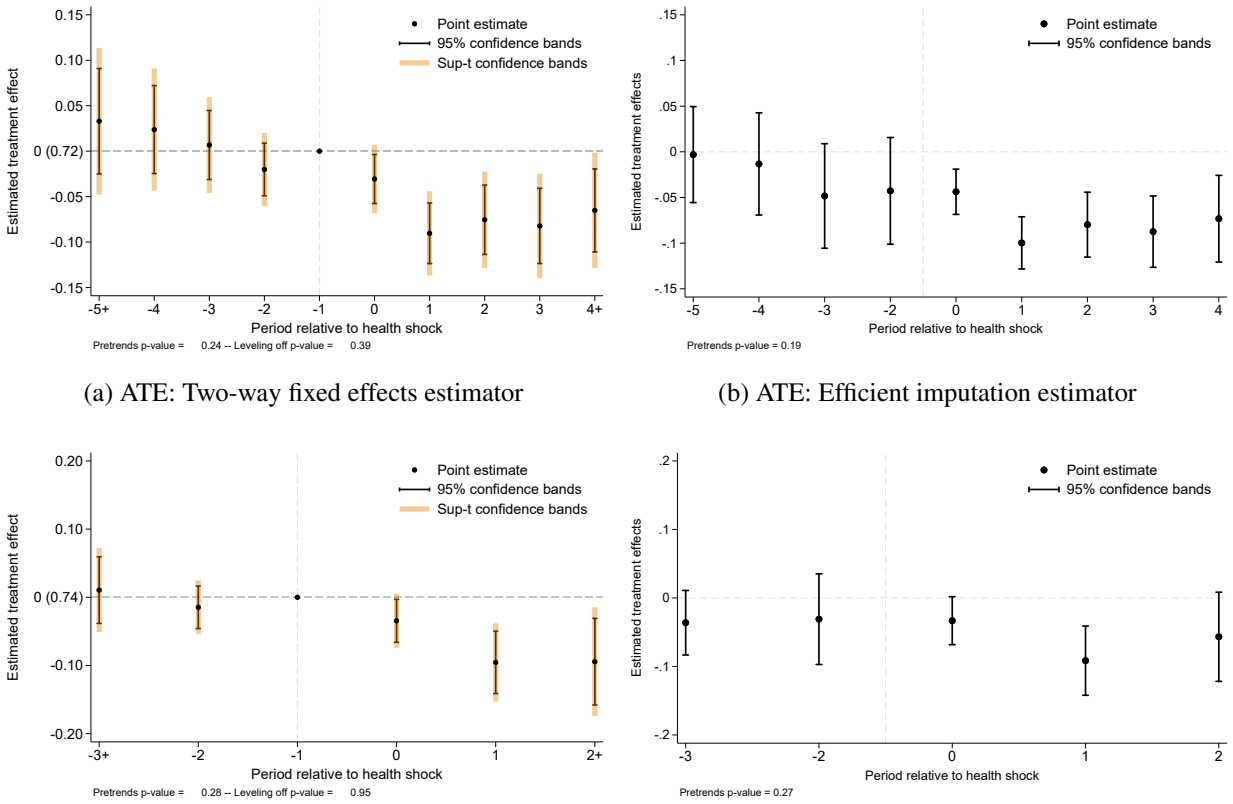
Note: Author's calculations. This table shows descriptive statistics for respondents aged 50–80 by health status. Panel (a) includes all respondents in the sample, comparing those who have experienced health shocks with those who have not. Panel (b) restricts the sample to respondents who either have experienced or will eventually experience health shocks during the study period, comparing their characteristics before and after the health shock occurs. Health shocks are defined as a decline in grip strength of 25 percent or more between two consecutive measurements (see Section 3.3). Values for productive nonmarket activity are derived from a binary indicator that takes value 1 if a respondent spends 10 or more weekly hours on household production, caregiving, and volunteering (see Section 3.4 for a definition and complete activity list).

# 4 Impact of Health Shocks on Productive Activity

This section presents the estimation results for the effects of health shocks on productive market and nonmarket activity. I first examine the extensive-margin effects on labor force participation and productive nonmarket activity and then turn to the intensive-margin effects.

## 4.1 Impact of Health Shocks on Labor Force Participation

Figure 5 presents dynamic event study estimates for the effects of health shocks on labor force participation. The estimated coefficients reflect the impact of health shocks on the probability of being in the labor force. Panels (a) and (b) report average treatment effects (including treated, not-yet-treated, never-treated respondents) for the two-way fixed effects and efficient imputation estimators. Panels (c) and (d) report average treatment effects on the treated (excluding never-treated respondents). I restrict the sample to respondents aged 50–80 with initial labor market attachment




(a) ATE: Two-way fixed effects estimator

(b) ATE: Efficient imputation estimator

(c) ATT: Two-way fixed effects estimator

(d) ATT: Efficient imputation estimator

Note: Author's calculations. This figure depicts the effect of declines in grip strength of 25% or more on the probability of participating in the labor force for respondents aged 50–80. Coefficients are estimated with a dynamic event study model (see equation 2) and obtained from the ordinary least squares two-way fixed effects estimator in Panels (a) and (c) and Borusyak et al.'s (2024) efficient imputation estimator in Panels (b) and (d). Panels (a) and (b) report estimates of the average treatment effect (ATE) using observations from treated, not-yet-treated, and untreated respondents. Panels (c) and (d) report estimates of the average treatment effect on the treated (ATT) using only observations from treated and not-yet-treated respondents. The estimated treatment effects refer to respondents who reported participating in the labor force when they were first observed. All specifications include respondent, period (wave), and age fixed effects and control for initial grip strength, educational attainment, initial employer-sponsored health insurance coverage, and initial government health insurance coverage (all interacted with period dummies). Event windows were chosen based on data availability (see Section 2). Coefficients were estimated using 37,159 observations from 5,349 respondents (Panel a), 54,006 observations from 9,959 respondents (Panel b), 6,816 observations from 1,018 respondents (Panel c), and 5,362 observations from 1,061 respondents (Panel d). Confidence intervals are based on standard errors clustered at the respondent level. Montiel Olea and Plagborg-Møller's (2019) 95-percent sup-t confidence bands are reported for the two-way fixed effects estimator to account for joint uncertainty of the dynamic effects.

Figure 5: Dynamic effects of health shocks on labor force participation

and subsequently report results separately for age groups 50–61, 62–70, and 71–80. Labor force participation beyond age 80 approaches zero percent.

Health shocks significantly reduce labor force participation. After experiencing a health shock, respondents are about 5–10 percentage points less likely to participate in the labor force. Treatment effects reach their maximum after two periods and then plateau. This pattern aligns with the timing uncertainty about health shocks that arises because grip strength is measured only every other wave in the Health and Retirement Study: treatment effects for respondents who experience their health shocks early (between $l = -2$ and $l = -1$) should emerge between $l = -1$ and $l = 0$, whereas they

should emerge between $l = 0$ and $l = 1$ for respondents who experience their health shocks late (between $l = -1$ and $l = 0$).

The estimation results support the plausibility of the identifying assumptions. The estimated coefficients are of similar size for both estimators and do not differ appreciably between the full sample (average treatment effect) and the restricted sample excluding never-treated respondents (average treatment effect on the treated). The close match between the two samples suggests it is unlikely that unobserved factors differing between eventually treated and never-treated respondents drive selection into health shocks. Moreover, the lead coefficients are individually and jointly insignificant, indicating no systematic pre-treatment trends.

To assess the extent to which health deterioration explains the observed decline in labor force participation over the age range 50–80 and for specific age groups, I fit the more restrictive static model. Stability of the treatment effects after period $l = 1$ suggests that the static model provides a reasonable approximation of the overall treatment effect. A visual comparison of the event study plots for the dynamic and static models confirms this conjecture (see Figure A.2).

Table 3 presents the static event study estimates for the effects of health shocks on labor force participation. Panel (a) reports estimates for average treatment effects, Panel (b) for average treatment effects on the treated. The table reports results first for all respondents aged 50–80 with initial labor market attachment and then separately for the age groups 50–61, 62–70, and 71–80.

The estimated effects of health shocks on labor force participation for the static model are quantitatively similar to those for the dynamic model. On average, health shocks reduce labor force participation by about 7 percentage points. The coefficients are slightly less precise for the restricted sample (excluding respondents who never experience a health shock) but otherwise broadly similar.

The extent to which health shocks affect labor force participation differs appreciably between respondents around retirement ages and other age groups. Specifically, the results for respondents aged 50–61 and 71–80 produce coefficients that are similar to the average effect for all respondents aged 50–80. If anything, the coefficients are slightly larger when focusing only on respondents aged 71–80 who eventually experience health shocks. Accordingly, the regression coefficients do not differ much between workers who are too young to retire (the early retirement age is 62) and those who work beyond age 70 (the latest age at which delaying Social Security adds more earning years to pension benefits). However, when fitting the empirical model for respondents aged 62–70, I find a precise zero effect of health shocks on labor force participation. Therefore, health deterioration does not explain any of the observed decline in labor force participation around retirement ages.

To quantify the role of health deterioration in changes in labor force participation, I calculate what fraction of those changes is explained by health shocks. For each age group, I multiply the estimated treatment effects with the age-specific treatment probabilities, divide this quantity by the unconditional change in labor force participation, and then multiply it by 100 percent.

Table 3: Health shocks and labor force participation

| Dependent variable:<br>Labor force participation | Ages<br>50–80<br>(1) | Ages<br>50–61<br>(2) | Ages<br>62–70<br>(3) | Ages<br>71–80<br>(4) |
|---|---|---|---|---|
| **(a) Treated, not-yet-treated, and untreated respondents (ATE)** | | | | |
| *Two-way fixed effects estimator* | | | | |
| Decline in grip strength of 25% or more | -0.07*** | -0.07*** | 0.00 | -0.08*** |
| | (0.01) | (0.02) | (0.02) | (0.03) |
| Change in labor force participation (in pp.) | -69.0 | -18.9 | -32.6 | -17.5 |
| Fraction of change explained (in %) | 1.8 | 2.8 | 0.0 | 3.5 |
| Respondents | 6,627 | 4,328 | 4,434 | 2,464 |
| Observations | 43,390 | 16,889 | 15,410 | 9,247 |
| *Efficient imputation estimator* | | | | |
| Decline in grip strength of 25% or more | -0.07*** | -0.07*** | -0.00 | -0.08*** |
| | (0.01) | (0.02) | (0.02) | (0.03) |
| Change in labor force participation (in pp.) | -69.0 | -18.9 | -32.6 | -17.5 |
| Fraction of change explained (in %) | 1.8 | 2.8 | 0.0 | 3.5 |
| Respondents | 9,959 | 7,453 | 6,044 | 3,161 |
| Observations | 54,393 | 24,919 | 17,548 | 9,885 |
| **(b) Only treated and not-yet-treated respondents (ATT)** | | | | |
| *Two-way fixed effects estimator* | | | | |
| Decline in grip strength of 25% or more | -0.05*** | -0.05* | -0.01 | -0.07** |
| | (0.02) | (0.03) | (0.03) | (0.03) |
| Change in labor force participation (in pp.) | -72.1 | -20.8 | -29.4 | -21.9 |
| Fraction of change explained (in %) | 4.6 | 11.5 | -0.1 | 6.8 |
| Respondents | 1,053 | 548 | 734 | 536 |
| Observations | 7,070 | 2,080 | 2,630 | 2,054 |
| *Efficient imputation estimator* | | | | |
| Decline in grip strength of 25% or more | -0.06** | -0.07* | -0.02 | -0.14*** |
| | (0.02) | (0.04) | (0.04) | (0.04) |
| Change in labor force participation (in pp.) | -72.1 | -20.8 | -29.4 | -21.9 |
| Fraction of change explained (in %) | 5.8 | 16.4 | -0.1 | 13.1 |
| Respondents | 1,061 | 605 | 508 | 290 |
| Observations | 5,910 | 2,015 | 1,587 | 1,106 |

Note: Author's calculations. This table shows the effect of declines in grip strength of 25% or more on a person's probability of participating in the labor force. Coefficients are obtained from the ordinary least squares two-way fixed effects estimator (top of Panels a and b) and Borusyak et al.'s (2024) efficient imputation estimator (bottom of Panels a and b). Panel (a) reports estimates of the average treatment effect (ATE) using observations from treated, not-yet-treated, and untreated respondents. Panel (b) reports estimates of the average treatment effect on the treated (ATT) using only observations from treated and not-yet-treated respondents. The estimated treatment effects refer to respondents who reported participating in the labor force (i.e., being employed/unemployed) in the first period in which they are observed. All specifications include respondent, period (wave), and age fixed effects and control for initial grip strength, initial educational attainment (college vs. non-college educated), initial employer health insurance coverage, and initial government health insurance coverage (all interacted with period dummies). Changes in labor force participation are reported in percentage points (pp.) and reflect the unconditional change (i.e., decline) in labor force participation rates between ages 50–80, 50–61, 61–70, and 70–80. To calculate what fraction of the change in labor force participation is explained by health shocks, the estimated treatment effect is multiplied by the treatment probability (measured by changes in the post-treatment indicator between ages 50–80, 50–61, 61–70, and 70–80), then divided by the observed change in labor force participation, and multiplied by 100%. Standard errors are clustered at the respondent level and reported in parentheses. Asterisks indicate significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Health shocks explain a small to moderate fraction of the decline in labor force participation, with the magnitude depending on whether one considers all respondents or only those who eventually experience health shocks. When considering the estimated effects at population level, health shocks explain about 2 percent of the decline in labor force participation, with the values ranging between 0 percent around retirement ages and up to 4 percent for respondents aged 71–80. When instead considering the estimated effects only for respondents who eventually experience health shocks,

they explain 5–6 percent of the decline in labor force participation, with values ranging between 0 percent around retirement ages and up to 16 percent for respondents aged 50–61. The difference between these two samples emerges because only 10 percent of respondents aged 50–80 experience health shocks. The low treatment probability pushes down the fraction of changes in labor force participation that health shocks explain when their effects are considered at the population level.

## 4.2 Impact of Health Shocks on Productive Nonmarket Activity

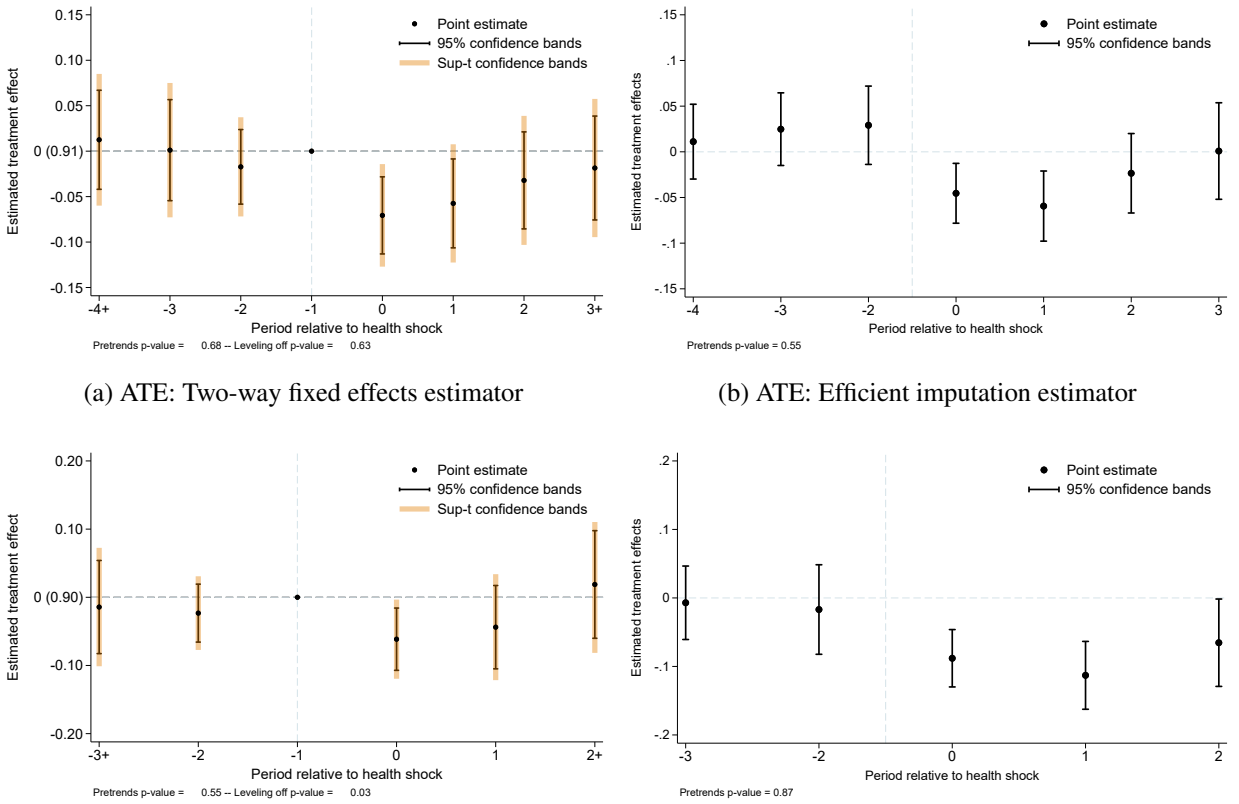
Figure 6 presents dynamic event study estimates for the effects of health shocks on productive nonmarket activity. The estimated coefficients reflect the impact of health shocks on the probability of performing 10 or more weekly hours of productive nonmarket activities. Panels (a) and (b) report average treatment effects, Panels (c) and (d) report average treatment effects on the treated. I restrict the sample to respondents aged 50–90 who perform at least 10 hours of productive nonmarket activities when first observed and subsequently report results separately for age groups 50–61, 62–70, 71–80, and 81–90. While respondents perform productive nonmarket activities beyond age 90, sample size and selection become concerns.

Health shocks reduce the probability of performing 10 or more weekly hours of productive nonmarket activity by 5–11 percentage. The estimated treatment effects slightly decline over time, with more stable patterns for the efficient imputation estimator and especially for the restricted sample in which all respondents eventually experience health shocks. The lead coefficients show no signs of systematic pre-treatment trends, supporting the plausibility of the identifying assumptions.

Like for labor force participation, I fit static event study regressions to assess the extent to which health deterioration explains the observed in productive nonmarket activity. The static model provides a reasonable approximation of the overall treatment effect, as a visual comparison of the dynamic and static event study results shows (see Figure A.3).

Table 4 presents the static event study estimates for the effects of health shocks on productive nonmarket activity. Panel (a) reports estimates for average treatment effects, Panel (b) for average treatment effects on the treated. The table reports results first for all respondents aged 50–90 who perform 10 or more weekly hours of productive nonmarket activities when first observed and then separately for the age groups 50–61, 62–70, 71–80, and 81–90.

The estimated effects of health shocks on productive nonmarket activity differ appreciably between younger and older respondents. On average, health shocks reduce the probability of performing 10 or more weekly hours of productive nonmarket activities by 3–9 percentage points, with higher values when only considering respondents who eventually experience health shocks. Notably, the results indicate that this average effect is entirely driven by respondents aged 71 and older, with small and insignificant coefficients for younger respondents. The estimated coefficients




(a) ATE: Two-way fixed effects estimator

(b) ATE: Efficient imputation estimator

(c) ATT: Two-way fixed effects estimator

(d) ATT: Efficient imputation estimator

Note: Author's calculations. This figure depicts the effect of declines in grip strength of 25% or more on the probability of performing 10 or more weekly hours of productive nonmarket activities for respondents aged 50–90. Coefficients are estimated with a dynamic event study model (see equation 2) and obtained from the ordinary least squares two-way fixed effects estimator in Panels (a) and (c) and Borusyak et al.'s (2024) efficient imputation estimator in Panels (b) and (d). Panels (a) and (b) report estimates of the average treatment effect (ATE) using observations from treated, not-yet-treated, and untreated respondents. Panels (c) and (d) report estimates of the average treatment effect on the treated (ATT) using only observations from treated and not-yet-treated respondents. The estimated treatment effects refer to respondents who reported 10 or more weekly hours of productive nonmarket activity when they were first observed. All specifications include respondent, period (wave), and age fixed effects and control for initial grip strength, educational attainment, initial labor force participation, and initial government health insurance coverage (all interacted with period dummies). Event windows were chosen based on data availability (see Section 2). Coefficients were estimated using 10,647 observations from 2,363 respondents (Panel a), 19,528 observations from 5,178 respondents (Panel b), 2,678 observations from 559 respondents (Panel c), and 2,815 observations from 710 respondents (Panel d). Confidence intervals are based on standard errors clustered at the respondent level. Montiel Olea and Plagborg-Møller's (2019) 95-percent sup-t confidence bands are reported for the two-way fixed effects estimator to account for joint uncertainty of the dynamic effects.

Figure 6: Dynamic effects of health shocks on productive nonmarket activity

are broadly similar across age groups 71–80 and 81–90; however, they are more imprecisely estimated for the older age group due to smaller sample size and tend to be insignificant.

Health shocks explain an appreciable fraction of the decline in productive nonmarket activity, with the magnitude varying depending on whether one considers all respondents or only those who eventually experience health shocks. When considering the estimated effects at population level, health shocks explain on average 2–3 percent of the decline in productive nonmarket activity and up to 7 percent for respondents aged 71–80. When instead considering the estimated effects only for

Table 4: Health shocks and productive nonmarket activity

| Dependent variable:<br>Productive nonmarket activity | Ages<br>50–90<br>(1) | Ages<br>50–61<br>(2) | Ages<br>62–70<br>(3) | Ages<br>71–80<br>(4) | Ages<br>81–90<br>(5) |
|---|---|---|---|---|---|
| **(a) Treated, not-yet-treated, and untreated respondents (ATE)** | | | | | |
| *Two-way fixed effects estimator* | | | | | |
| Decline in grip strength of 25% or more | -0.05*** | -0.00 | 0.03 | -0.09*** | -0.07 |
| | (0.01) | (0.03) | (0.03) | (0.03) | (0.05) |
| $R^2$ | 0.40 | 0.42 | 0.51 | 0.52 | 0.57 |
| Change in productive nonmarket activity (in pp.) | -42.9 | -5.5 | 0.8 | -10.1 | -28.2 |
| Fraction of change explained (in %) | 3.1 | 0.2 | 7.3 | 6.9 | 2.3 |
| Respondents | 3,609 | 1,323 | 1,524 | 1,496 | 624 |
| Observations | 16,785 | 4,130 | 4,416 | 4,784 | 1,794 |
| *Efficient imputation estimator* | | | | | |
| Decline in grip strength of 25% or more | -0.03*** | 0.00 | 0.02 | -0.07** | -0.06 |
| | (0.01) | (0.03) | (0.04) | (0.03) | (0.05) |
| Change in productive nonmarket activity (in pp.) | -42.9 | -5.5 | 0.8 | -10.1 | -28.2 |
| Fraction of change explained (in %) | 2.1 | -0.3 | 6.5 | 5.1 | 1.8 |
| Respondents | 5,177 | 2,423 | 2,437 | 2,180 | 998 |
| Observations | 19,629 | 5,768 | 5,390 | 5,470 | 2,151 |
| **(b) Only treated and not-yet-treated respondents (ATT)** | | | | | |
| *Two-way fixed effects estimator* | | | | | |
| Decline in grip strength of 25% or more | -0.06*** | -0.03 | 0.05 | -0.11*** | -0.08 |
| | (0.02) | (0.05) | (0.04) | (0.03) | (0.07) |
| $R^2$ | 0.44 | 0.53 | 0.51 | 0.54 | 0.62 |
| Change in productive nonmarket activity (in pp.) | -41.6 | -5.2 | 3.5 | -9.4 | -30.5 |
| Fraction of change explained (in %) | 12.7 | 33.7 | -2.8 | 25.0 | 4.0 |
| Respondents | 724 | 184 | 281 | 353 | 167 |
| Observations | 3,472 | 556 | 830 | 1,171 | 482 |
| *Efficient imputation estimator* | | | | | |
| Decline in grip strength of 25% or more | -0.09*** | 0.01 | 0.10 | -0.13*** | -0.36*** |
| | (0.02) | (0.07) | (0.06) | (0.04) | (0.11) |
| Change in productive nonmarket activity (in pp.) | -41.6 | -5.2 | 3.5 | -9.4 | -30.5 |
| Fraction of change explained (in %) | 18.2 | -5.7 | -5.7 | 29.8 | 17.4 |
| Respondents | 710 | 227 | 287 | 312 | 93 |
| Observations | 3,048 | 564 | 648 | 886 | 271 |

Note: Author's calculations. This table shows the effect of declines in grip strength of 25% or more on a person's probability of performing more than 10 hours of productive nonmarket activity per week. Coefficients are obtained from the ordinary least squares two-way fixed effects estimator (top of Panels a and b) and Borusyak et al.'s (2024) efficient imputation estimator (bottom of Panels a and b). Panel (a) reports estimates of the average treatment effect (ATE) using observations from treated, not-yet-treated, and untreated respondents. Panel (b) reports estimates of the average treatment effect on the treated (ATT) using only observations from treated and not-yet-treated respondents. The estimated treatment effects refer to respondents who reported performing more than 10 hours of productive nonmarket activity in the first period in which they are observed. All specifications include respondent, period (wave), and age fixed effects and control for initial grip strength, initial educational attainment (college vs. non-college educated), initial labor force participation, and initial government health insurance coverage (all interacted with period dummies). Changes in productive nonmarket activity are reported in percentage points (pp.) and reflect the unconditional change in productive nonmarket activity between ages 50–90, 50–61, 61–70, 70–80, and 80–90. To calculate what fraction of the change in productive nonmarket activity is explained by health shocks, the estimated treatment effect is multiplied by the treatment probability (measured by changes in the post-treatment indicator between ages 50–90, 50–61, 61–70, 70–80, and 80–90), then divided by the observed change in productive nonmarket activity, and multiplied by 100%. Standard errors are clustered at the respondent level and reported in parentheses. Asterisks indicate significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

respondents who eventually experience health shocks, they explain on average 12–18 percent of the decline in productive nonmarket activity and up to 30 percent for respondents aged 71–80. As for labor force participation, the stark difference between the two samples emerges because only a small share of respondents experience health shocks.

Table 5: Health shocks and productive market and nonmarket activity at the intensive margin

| Dependent variable: | Two-way fixed effects estimator | | | Efficient imputation estimator | | |
|---|---|---|---|---|---|---|
| | Usual weekly work hours | Weekly hours of productive nonmarket activities | Number of productive nonmarket activities performed | Usual weekly work hours | Weekly hours of productive nonmarket activities | Number of productive nonmarket activities performed |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **(a) Treated, not-yet-treated, and untreated respondents (ATE)** | | | | | | |
| Decline in grip strength of 25% or more | 0.10 | -1.28** | -0.34*** | 0.12 | -1.46** | -0.32*** |
| | (0.46) | (0.52) | (0.08) | (0.49) | (0.64) | (0.09) |
| Change in dependent variable | -17.6 | -14.2 | -4.5 | -17.6 | -14.2 | -4.5 |
| Fraction of change explained (in %) | -0.1 | 2.4 | 2.1 | -0.1 | 2.7 | 1.9 |
| Respondents | 5,967 | 4,244 | 4,244 | 10,300 | 6,167 | 6,167 |
| Observations | 27,164 | 19,692 | 19,692 | 36,589 | 23,156 | 23,156 |
| **(b) Only treated and not-yet-treated respondents (ATT)** | | | | | | |
| Decline in grip strength of 25% or more | 0.50 | -1.33* | -0.33*** | 0.90 | -2.89*** | -0.60*** |
| | (0.56) | (0.68) | (0.10) | (0.81) | (0.93) | (0.14) |
| Change in dependent variable | -11.8 | -3.3 | -2.7 | -11.8 | -3.3 | -2.7 |
| Fraction of change explained (in %) | -3.0 | 34.5 | 10.4 | -5.5 | 74.7 | 19.2 |
| Respondents | 905 | 837 | 837 | 1,080 | 829 | 829 |
| Observations | 3,845 | 3,937 | 3,937 | 3,605 | 3,533 | 3,533 |

Note: Author's calculations. This table shows the effect of declines in grip strength of 25% or more on usual weekly work hours in specifications (1) and (4); weekly hours of productive nonmarket activities in specifications (2) and (5); and the number of productive nonmarket activities performed in specifications (3) and (6). Coefficients are obtained from the ordinary least squares two-way fixed effects estimator in specifications (1) to (3) and Borusyak et al.'s (2024) efficient imputation estimator in specifications (4) to (6). Panel (a) reports estimates of the average treatment effect (ATE) using observations from treated, not-yet-treated, and untreated respondents. Panel (b) reports estimates of the average treatment effect on the treated (ATT) using only observations from treated and not-yet-treated respondents. The estimated treatment effects refer to respondents aged 50–80 in specifications (1) and (4) and to respondents aged 50–90 in specifications (2), (3), (5), and (6). All specifications include respondent, period (wave), and age fixed effects and control for initial grip strength, educational attainment (college vs. non-college educated), and initial government health insurance coverage (all interacted with period dummies). Specifications (1) and (4) additionally control for initial employer health insurance coverage (interacted with period dummies), whereas all other specifications additionally control for initial labor force participation (interacted with period dummies). Changes in the dependent variables reflect the unconditional increases or decreases in the corresponding variables. To calculate what fraction of the change in the dependent variable is explained by health shocks, the estimated treatment effect is multiplied by the treatment probability, then divided by the observed change in the dependent variable, and multiplied by 100%. Standard errors are clustered at the respondent level and reported in parentheses. Asterisks indicate significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Notably, health shocks explain up to twice as much of the observed decline in productive nonmarket activity as for labor force participation. Accordingly, health deterioration plays a more important role in productive nonmarket activity than in productive market activity at the extensive margin. I next examine the extent to which health shocks influence productive market and nonmarket activity at the intensive margin.

## 4.3 Impact of Health Shocks at the Intensive Margin

Table 5 presents static event study estimates for the effects of health shocks on usual weekly work hours, weekly hours of productive nonmarket activities, and number of productive nonmarket activities performed. Panel (a) reports estimates for average treatment effects, Panel (b) for average treatment effects on the treated. Estimates refer to respondents aged 50–80 for weekly work hours and to respondents aged 50–90 for productive nonmarket activities.

The consideration of both hours and number of productive nonmarket activities is important because individuals may respond to health shocks by reallocating time along two separate margins. First, they may shift hours from productive nonmarket activity to less demanding leisure activities (e.g., reading and watching TV) and self-care activities (e.g., sleeping, washing oneself). Second, they may shift hours from more demanding to less demanding productive nonmarket activities without changing overall time allocation to leisure and self-care.

Health shocks significantly reduce both weekly hours and the number of productive nonmarket activities but not usual weekly hours of work. On average, respondents spend about 1 hour and 18 minutes less on productive nonmarket activities after experiencing health shocks, with slightly larger estimates for the efficient imputation estimator. This estimate implies a 38-percent reduction relative to the 3 hours and 22 minutes that the average respondent in the Consumption and Activities Mail survey spends on productive nonmarket activities. In addition, respondents perform about 0.3 fewer productive nonmarket activities after experiencing health shocks. This estimate implies a 4-percent reduction relative to the 7.5 (out of 13) productive nonmarket activities that the average respondent performs. In contrast to the decline in productive nonmarket activity, usual weekly hours are not significantly affected by health shocks, indicating that adjustments in productive market activity in response to health shocks predominantly operate through labor force participation.

Health shocks explain a sizable fraction of the observed decline in weekly hours and number of productive nonmarket activities for respondents who experience health shocks. When considering effects on the treated, they account for 35–75 percent of the decline in weekly hours spent on productive nonmarket activities and for 10–20 percent of the decline in number of activities performed. These quantities suggest that extensive-margin and intensive-margin behavioral responses to health shocks are about equally important, with a greater adjustment in hours than in activities. When instead considering the effects at population level, health shocks again explain a much smaller fraction of 2–3 percent of the decline in productive nonmarket activity because only a small share of respondents experience health shocks.

A natural question is where respondents reallocate their time. Additional results in the appendix show that respondents slightly increase the time they spend on self-care, with no change in the number of activities performed (Table A.3). However, these results should be viewed as suggestive in that the corresponding coefficients are imprecisely estimated and not always statistically significant. With regard to leisure, the results indicate no systematic changes in response to health shocks.

# 5 Conclusion

This paper quantifies the importance of health deterioration for productive market and nonmarket activity among older adults. Using an event study approach that leverages major declines in

grip strength measurements in the Health and Retirement Study, I demonstrate that health shocks reduce productive market and nonmarket activity. However, the estimated treatment effects differ appreciably across age groups. Health shocks decrease labor force participation among workers younger than 62 and older than 70 but not around retirement ages. Moreover, health shocks discourage productive nonmarket activity beyond age 70.

How much health deterioration contributes to the observed decline in productive market and nonmarket activity among older adults depends on the perspective one takes. Health shocks explain only a modest fraction of changes in productive market and nonmarket activity at the population level because they are rare. When instead considering the treatment group level, health shocks account for up to 16 percent of the decline in productive market activity and up to 30 percent of the decline in productive nonmarket activity. Together these estimates indicate that health deterioration contributes about twice as much to the decline in productive nonmarket activity as to the decline in productive market activity. This finding underscores the importance of health for older adults' productive activity, which extends beyond labor markets to substantial nonmarket contributions.

From a policy perspective, the empirical results indicate that concerns about declines in productive market activity due to health deterioration pertain perhaps to one-tenth of the population. Accordingly, improvements in longevity and healthy aging could expand economic activity further into the older ages for most people, potentially offsetting adverse economic consequences of population aging at the aggregate level (Kotschy and Bloom 2023). However, the empirical results also underscore the sizable negative effect of experiencing health shocks on productive market activity, which weighs economically especially heavy for workers who are too young to retire and ineligible for Medicare. Notably, health shocks often lead to labor force exit rather than adjustment in work hours. Improving accommodations for disabilities in the workplace could thus promote economic activity of workers with work-limiting health problems and lower the social cost of health shocks.

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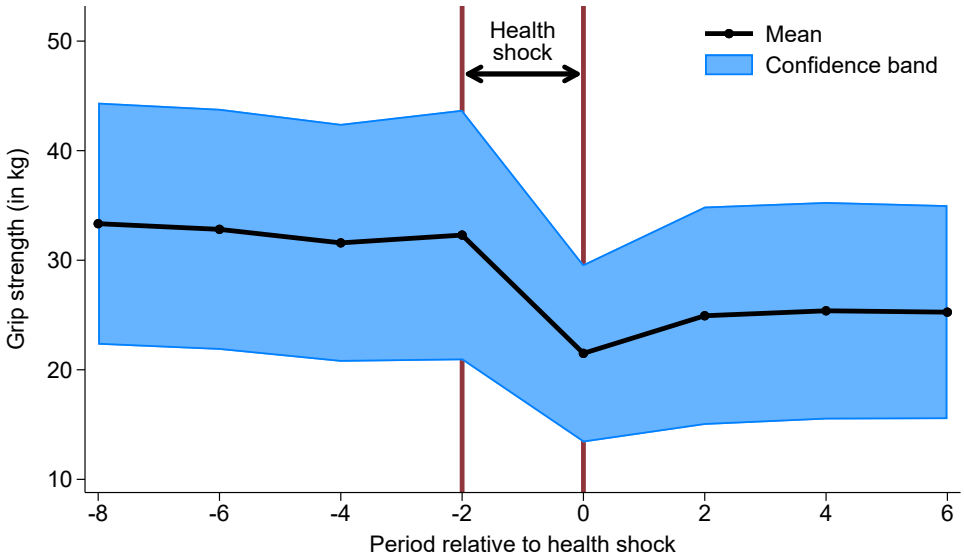
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# Supplemental Appendix

## A.1 Additional Figures and Tables




Note: Author's calculations. This figure depicts average grip strength before, during, and after experiencing a health shock. Health shocks are defined as a decline in grip strength of 25 percent or more between two consecutive measurements. Periods $l = t - e_i$ are defined relative to the event period $e_i$ when grip strength has declined by 25 percent or more. Average grip strength was estimated using 9,074 observations, with 127 in relative period $l = -8$, 448 in $l = -6$, 1,294 in $l = -4$, 2,535 in $l = -2$, 2,535 in $l = 0$, 1,451 in $l = 2$, 568 in $l = 4$, and 116 in $l = 6$. The confidence band reflects average grip strength plus or minus one standard deviation of grip strength in the corresponding period.

Figure A.1: Average grip strength levels before, during, and after experiencing a health shock

Table A.1: Descriptive statistics by age and health status: Full sample

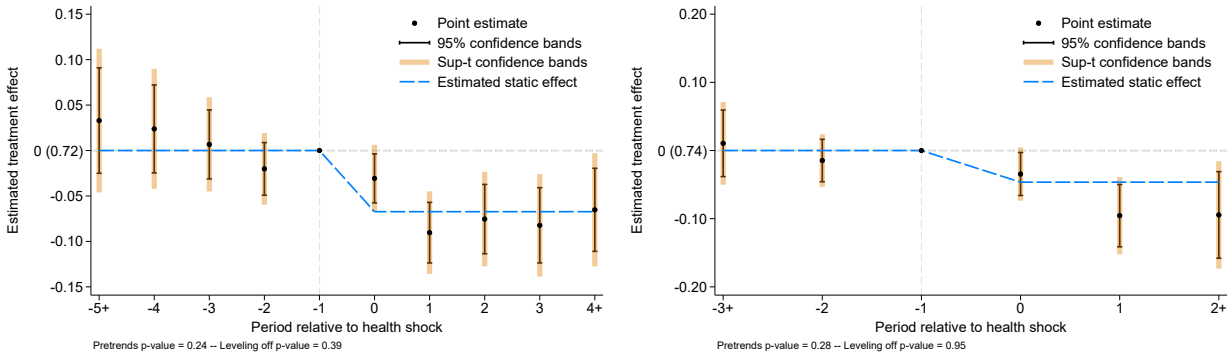
| | *Health shock = 0* | | *Health shock = 1* | | | |
|---|---|---|---|---|---|---|
| Variable | Mean | Observations | Mean | Observations | Difference | *p*-value |
| **(a) Respondents aged 50–61** | | | | | | |
| In labor force (in %) | 0.73 | 21,330 | 0.56 | 601 | 0.17*** | 0.000 |
| Weekly hours worked | 39.32 | 14,253 | 37.80 | 289 | 1.52** | 0.033 |
| Productive nonmarket activity (in %) | 0.85 | 5,193 | 0.85 | 110 | -0.01 | 0.834 |
| Weekly hours of productive nonmarket activity | 24.46 | 5,193 | 25.82 | 110 | -1.36 | 0.381 |
| Number of productive nonmarket activities | 8.32 | 5,193 | 7.88 | 110 | 0.44* | 0.052 |
| Share male (in %) | 0.46 | 21,409 | 0.40 | 601 | 0.06*** | 0.003 |
| Age (in years) | 56.98 | 21,409 | 59.00 | 601 | -2.02*** | 0.000 |
| Initial grip strength (in kg) | 36.81 | 21,409 | 37.92 | 601 | -1.11** | 0.023 |
| Initial government health insurance coverage (in %) | 0.17 | 21,409 | 0.23 | 601 | -0.06*** | 0.000 |
| Initial employer health insurance coverage (in %) | 0.51 | 21,409 | 0.43 | 601 | 0.07*** | 0.000 |
| Initial labor force participation (in %) | 0.79 | 21,330 | 0.73 | 601 | 0.06*** | 0.001 |
| Initial productive nonmarket activity (in %) | 0.86 | 5,193 | 0.89 | 110 | -0.03 | 0.385 |
| Attended college (share in %) | 0.59 | 21,409 | 0.56 | 601 | 0.03* | 0.091 |
| **(b) Respondents aged 62–70** | | | | | | |
| In labor force (in %) | 0.42 | 23,161 | 0.32 | 1,851 | 0.10*** | 0.000 |
| Weekly hours worked | 32.58 | 8,981 | 31.65 | 544 | 0.93 | 0.158 |
| Productive nonmarket activity (in %) | 0.84 | 5,707 | 0.82 | 383 | 0.02 | 0.241 |
| Weekly hours of productive nonmarket activity | 25.37 | 5,707 | 24.75 | 383 | 0.63 | 0.487 |
| Number of productive nonmarket activities | 8.07 | 5,707 | 7.57 | 383 | 0.50*** | 0.000 |
| Share male (in %) | 0.47 | 23,239 | 0.43 | 1,857 | 0.04*** | 0.001 |
| Age (in years) | 66.01 | 23,239 | 66.19 | 1,857 | -0.18*** | 0.003 |
| Initial grip strength (in kg) | 35.58 | 23,239 | 37.28 | 1,857 | -1.70*** | 0.000 |
| Initial government health insurance coverage (in %) | 0.34 | 23,239 | 0.21 | 1,857 | 0.12*** | 0.000 |
| Initial employer health insurance coverage (in %) | 0.46 | 23,239 | 0.47 | 1,857 | -0.00 | 0.768 |
| Initial labor force participation (in %) | 0.65 | 23,161 | 0.70 | 1,851 | -0.05*** | 0.000 |
| Initial productive nonmarket activity (in %) | 0.85 | 5,707 | 0.83 | 383 | 0.02 | 0.358 |
| Attended college (share in %) | 0.55 | 23,235 | 0.56 | 1,857 | -0.01 | 0.366 |
| **(c) Respondents aged 71–80** | | | | | | |
| In labor force (in %) | 0.19 | 22,874 | 0.12 | 2,651 | 0.07*** | 0.000 |
| Weekly hours worked | 23.96 | 3,932 | 25.38 | 296 | -1.41 | 0.115 |
| Productive nonmarket activity (in %) | 0.81 | 5,726 | 0.79 | 533 | 0.03 | 0.139 |
| Weekly hours of productive nonmarket activity | 24.26 | 5,726 | 22.96 | 533 | 1.30* | 0.083 |
| Number of productive nonmarket activities | 7.61 | 5,726 | 6.98 | 533 | 0.63*** | 0.000 |
| Share male (in %) | 0.45 | 23,000 | 0.40 | 2,677 | 0.05*** | 0.000 |
| Age (in years) | 75.24 | 23,000 | 75.75 | 2,677 | -0.51*** | 0.000 |
| Initial grip strength (in kg) | 32.49 | 23,000 | 33.51 | 2,677 | -1.02*** | 0.000 |
| Initial government health insurance coverage (in %) | 0.79 | 23,000 | 0.63 | 2,677 | 0.16*** | 0.000 |
| Initial employer health insurance coverage (in %) | 0.31 | 23,000 | 0.37 | 2,677 | -0.06*** | 0.000 |
| Initial labor force participation (in %) | 0.38 | 22,874 | 0.46 | 2,651 | -0.08*** | 0.000 |
| Initial productive nonmarket activity (in %) | 0.85 | 5,726 | 0.89 | 533 | -0.04** | 0.015 |
| Attended college (share in %) | 0.45 | 22,995 | 0.48 | 2,677 | -0.03*** | 0.004 |

Note: Author's calculations. This table shows descriptive statistics for the full sample, comparing respondents who have experienced health shocks with those who have not. Results are reported for respondents aged 50–61 (Panel a), aged 62–70 (Panel b), and aged 71–80 (Panel c). Health shocks are defined as a decline in grip strength of 25 percent or more between two consecutive grip strength measurements (see Section 3.3). Values for productive nonmarket activity are derived from a binary indicator that takes value 1 if a respondent spends 10 or more weekly hours on activities such as cleaning, cooking, gardening, and volunteering (see Section 3.4 for a definition and complete activity list).

Table A.2: Descriptive statistics by age and health status: Restricted sample

| | *Health shock = 0* | | *Health shock = 1* | | | |
|---|---|---|---|---|---|---|
| Variable | Mean | Observations | Mean | Observations | Difference | *p*-value |
| **(a) Respondents aged 50–61** | | | | | | |
| In labor force (in %) | 0.68 | 2,339 | 0.56 | 601 | 0.12*** | 0.000 |
| Weekly hours worked | 38.47 | 1,422 | 37.80 | 289 | 0.68 | 0.387 |
| Productive nonmarket activity (in %) | 0.85 | 612 | 0.85 | 110 | -0.00 | 0.930 |
| Weekly hours of productive nonmarket activity | 24.39 | 612 | 25.82 | 110 | -1.42 | 0.398 |
| Number of productive nonmarket activities | 8.20 | 612 | 7.88 | 110 | 0.31 | 0.198 |
| Share male (in %) | 0.43 | 2,341 | 0.40 | 601 | 0.02 | 0.313 |
| Age (in years) | 56.81 | 2,341 | 59.00 | 601 | -2.19*** | 0.000 |
| Initial grip strength (in kg) | 36.76 | 2,341 | 37.92 | 601 | -1.15** | 0.050 |
| Initial government health insurance coverage (in %) | 0.21 | 2,341 | 0.23 | 601 | -0.02 | 0.212 |
| Initial employer health insurance coverage (in %) | 0.47 | 2,341 | 0.43 | 601 | 0.04 | 0.102 |
| Initial labor force participation (in %) | 0.73 | 2,339 | 0.73 | 601 | -0.00 | 0.825 |
| Initial productive nonmarket activity (in %) | 0.86 | 612 | 0.89 | 110 | -0.03 | 0.424 |
| Attended college (share in %) | 0.56 | 2,341 | 0.56 | 601 | 0.00 | 0.895 |
| **(b) Respondents aged 62–70** | | | | | | |
| In labor force (in %) | 0.39 | 2,738 | 0.32 | 1,851 | 0.07*** | 0.000 |
| Weekly hours worked | 31.94 | 989 | 31.65 | 544 | 0.29 | 0.708 |
| Productive nonmarket activity (in %) | 0.84 | 778 | 0.82 | 383 | 0.02 | 0.468 |
| Weekly hours of productive nonmarket activity | 24.79 | 778 | 24.75 | 383 | 0.05 | 0.965 |
| Number of productive nonmarket activities | 7.80 | 778 | 7.57 | 383 | 0.22 | 0.170 |
| Share male (in %) | 0.44 | 2,739 | 0.43 | 1,857 | 0.01 | 0.624 |
| Age (in years) | 66.29 | 2,739 | 66.19 | 1,857 | 0.10 | 0.208 |
| Initial grip strength (in kg) | 34.18 | 2,739 | 37.28 | 1,857 | -3.09*** | 0.000 |
| Initial government health insurance coverage (in %) | 0.50 | 2,739 | 0.21 | 1,857 | 0.28*** | 0.000 |
| Initial employer health insurance coverage (in %) | 0.41 | 2,739 | 0.47 | 1,857 | -0.05*** | 0.000 |
| Initial labor force participation (in %) | 0.53 | 2,738 | 0.70 | 1,851 | -0.17*** | 0.000 |
| Initial productive nonmarket activity (in %) | 0.85 | 778 | 0.83 | 383 | 0.02 | 0.397 |
| Attended college (share in %) | 0.51 | 2,739 | 0.56 | 1,857 | -0.05*** | 0.001 |
| **(c) Respondents aged 71–80** | | | | | | |
| In labor force (in %) | 0.18 | 3,324 | 0.12 | 2,651 | 0.06*** | 0.000 |
| Weekly hours worked | 23.61 | 547 | 25.38 | 296 | -1.77* | 0.091 |
| Productive nonmarket activity (in %) | 0.82 | 947 | 0.79 | 533 | 0.04* | 0.093 |
| Weekly hours of productive nonmarket activity | 24.13 | 947 | 22.96 | 533 | 1.17 | 0.177 |
| Number of productive nonmarket activities | 7.57 | 947 | 6.98 | 533 | 0.59*** | 0.000 |
| Share male (in %) | 0.40 | 3,326 | 0.40 | 2,677 | 0.01 | 0.566 |
| Age (in years) | 75.14 | 3,326 | 75.75 | 2,677 | -0.61*** | 0.000 |
| Initial grip strength (in kg) | 30.52 | 3,326 | 33.51 | 2,677 | -2.99*** | 0.000 |
| Initial government health insurance coverage (in %) | 0.93 | 3,326 | 0.63 | 2,677 | 0.30*** | 0.000 |
| Initial employer health insurance coverage (in %) | 0.25 | 3,326 | 0.37 | 2,677 | -0.12*** | 0.000 |
| Initial labor force participation (in %) | 0.29 | 3,324 | 0.46 | 2,651 | -0.17*** | 0.000 |
| Initial productive nonmarket activity (in %) | 0.87 | 947 | 0.89 | 533 | -0.02 | 0.304 |
| Attended college (share in %) | 0.43 | 3,326 | 0.48 | 2,677 | -0.05*** | 0.000 |

Note: Author's calculations. This table shows descriptive statistics for a restricted sample including only respondents who either have experienced or will eventually experience health shocks, comparing their characteristics before and after the health shock occurs. Results are reported for respondents aged 50–61 (Panel a), aged 62–70 (Panel b), and aged 71–80 (Panel c). Health shocks are defined as a decline in grip strength of 25 percent or more between two consecutive grip strength measurements (see Section 3.3). Values for productive nonmarket activity are derived from a binary indicator that takes value 1 if a respondent spends 10 or more weekly hours on activities such as cleaning, cooking, gardening, and volunteering (see Section 3.4 for a definition and complete activity list).

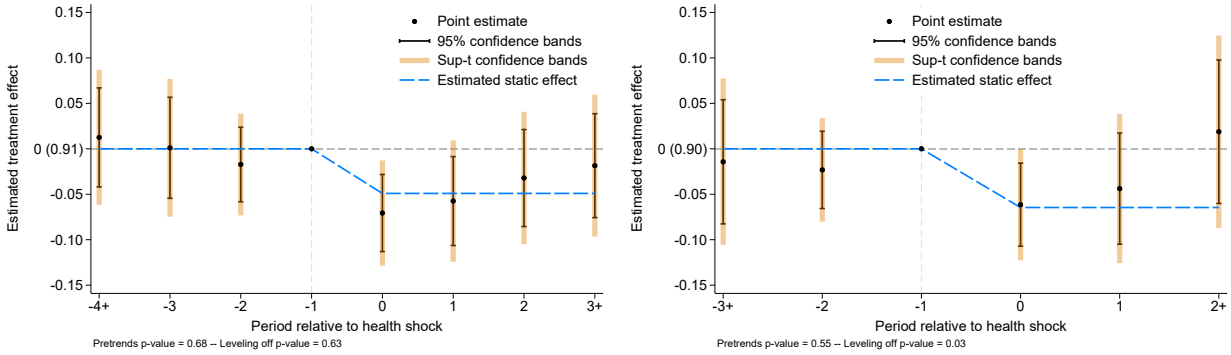



(a) Average treatment effect

(b) Average treatment effect on the treated

Note: Author's calculations. This figure depicts the effect of declines in grip strength of 25% or more on the probability of participating in the labor force for respondents aged 50–80. Coefficients are estimated with dynamic and static event study models (see equations 2 and 3) and obtained from the ordinary least squares two-way fixed effects estimator. Panel (a) reports estimates of the average treatment effect (ATE) using observations from treated, not-yet-treated, and untreated respondents. Panel (b) reports estimates of the average treatment effect on the treated (ATT) using only observations from treated and not-yet-treated respondents. The estimated treatment effects refer to respondents who reported participating in the labor force when they were first observed. All specifications include respondent, period (wave), and age fixed effects and control for initial grip strength, educational attainment, initial employer-sponsored health insurance coverage, and initial government health insurance coverage (all interacted with period dummies). Event windows were chosen based on data availability (see Section 2). Confidence intervals are based on standard errors clustered at the respondent level. Montiel Olea and Plagborg-Møller's (2019) 95-percent sup-t confidence bands are reported for the two-way fixed effects estimator to account for joint uncertainty of the dynamic effects.

Figure A.2: Effects of health shocks on labor force participation: Dynamic versus static model




(a) Average treatment effect

(b) Average treatment effect on the treated

Note: Author's calculations. This figure depicts the effect of declines in grip strength of 25% or more on the probability of performing 10 or more weekly hours of productive nonmarket activity for respondents aged 50–90. Coefficients are estimated with dynamic and static event study models (see equations 2 and 3) and obtained from the ordinary least squares two-way fixed effects estimator. Panel (a) reports estimates of the average treatment effect (ATE) using observations from treated, not-yet-treated, and untreated respondents. Panel (b) reports estimates of the average treatment effect on the treated (ATT) using only observations from treated and not-yet-treated respondents. The estimated treatment effects refer to respondents who performed 10 or more weekly hours of productive nonmarket activities when they were first observed. All specifications include respondent, period (wave), and age fixed effects and control for initial grip strength, educational attainment, initial labor force participation, and initial government health insurance coverage (all interacted with period dummies). Event windows were chosen based on data availability (see Section 2). Confidence intervals are based on standard errors clustered at the respondent level. Montiel Olea and Plagborg-Møller's (2019) 95-percent sup-t confidence bands are reported for the two-way fixed effects estimator to account for joint uncertainty of the dynamic effects.

Figure A.3: Effects of health shocks on productive nonmarket activity: Dynamic versus static model

Table A.3: Health shocks and leisure and self-care

| Dependent variable: | Two-way fixed effects estimator | | | | Efficient imputation estimator | | | |
|---|---|---|---|---|---|---|---|---|
| | Weekly hours of leisure (1) | Number of leisure activities (2) | Weekly hours self-care (3) | Number of self-care activities (4) | Weekly hours of of leisure (5) | Number of leisure activities (6) | Weekly hours of self-care (7) | Number of self-care activities (8) |
| **(a) Treated, not-yet-treated, and untreated respondents (ATE)** | | | | | | | | |
| Decline in grip strength of 25% or more | 0.37 | -0.07 | 0.33 | 0.02 | 0.86 | -0.00 | 0.36 | 0.03 |
| | (1.03) | (0.07) | (0.39) | (0.02) | (1.15) | (0.08) | (0.43) | (0.02) |
| Change in dependent variable | 2.3 | -1.3 | 10.6 | 0.1 | 2.3 | -1.3 | 10.6 | 0.1 |
| Fraction of change explained (in %) | 4.2 | 1.3 | 0.8 | 4.5 | 10.0 | 0.1 | 0.9 | 7.2 |
| Cluster | 4,239 | 4,239 | 3,913 | 3,913 | 6,244 | 6,244 | 5,809 | 5,809 |
| Observations | 19,247 | 19,247 | 18,246 | 18,246 | 22,816 | 22,816 | 21,521 | 21,521 |
| **(b) Only treated and not-yet-treated respondents (ATT)** | | | | | | | | |
| Decline in grip strength of 25% or more | -0.33 | -0.11 | 0.81 | -0.01 | -0.59 | -0.22* | 1.62** | -0.03 |
| | (1.40) | (0.10) | (0.57) | (0.03) | (1.91) | (0.12) | (0.77) | (0.04) |
| Change in dependent variable | 18.3 | -1.5 | 21.8 | 0.8 | 18.3 | -1.5 | 21.8 | 0.8 |
| Fraction of change explained (in %) | -1.6 | 6.6 | 3.2 | -1.2 | -2.8 | 13.0 | 6.4 | -2.9 |
| Cluster | 826 | 826 | 773 | 773 | 834 | 834 | 785 | 785 |
| Observations | 3,885 | 3,885 | 3,597 | 3,597 | 3,498 | 3,498 | 3,273 | 3,273 |

Note: Author's calculations. This table shows the effect of declines in grip strength of 25% or more on weekly hours of leisure in specifications (1) and (5); the number of leisure activities in specifications (2) and (6); weekly hours of self-care in specifications (3) and (7); and the number of self-care activities in specifications (4) and (8). Coefficients are obtained from the ordinary least squares two-way fixed effects estimator in specifications (1) to (4) and Borusyak et al.'s (2024) efficient imputation estimator in specifications (5) to (8). Panel (a) reports estimates of the average treatment effect (ATE) using observations from treated, not-yet-treated, and untreated respondents. Panel (b) reports estimates of the average treatment effect on the treated (ATT) using only observations from treated and not-yet-treated respondents. The estimated treatment effects refer to respondents aged 50–90. All specifications include respondent, period (wave), and age fixed effects and control for initial grip strength, educational attainment (college vs. non-college educated), initial labor force participation, and initial government health insurance coverage (all interacted with period dummies). Changes in the dependent variables reflect the unconditional change in the corresponding variables. To calculate what fraction of the change in the dependent variable is explained by health shocks, the estimated treatment effect is multiplied by the treatment probability, divided by the observed change in the dependent variable, and multiplied by 100%. Standard errors are clustered at the respondent level. Asterisks indicate significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

## A.2 Data Appendix

### Data Sources

The analysis is based on the following publicly available data sources. Note that accessing these data resources requires permission by the data provider (see https://hrsdata.isr.umich.edu/).

- RAND HRS Longitudinal file: `randhrs1992\_2022v1.dta`, https://hrsdata.isr.umich.edu/data-products/rand-hrs-longitudinal-file-2022.
- Gateway to Global Aging Harmonized HRS file: `H\_HRS\_d.dta`, https://hrsdata.isr.umich.edu/data-products/gateway-harmonized-hrs.
- HRS Consumption and Activities Mail Survey (CAMS) files: `CAMS01\_R.dct`, `CAMS01\_R.da`, `CAMS03\_R.dct`, `CAMS03\_R.da`, ..., `CAMS21\_R.dct`, `CAMS21\_R.da`. https://hrsdata.isr.umich.edu/data-products/public-survey-data.

### Construction of Variables

The construction of aggregate time use measures requires that each variable entering the aggregate measure is observed. Any missing value results in a missing value for the aggregate measure. In some cases, time spent on an activity was reported in hours per month rather than in hours per week. In these cases, the corresponding values were multiplied by 7 and divided by 30 to transform the measure into hours per week (see data_prep_hrs.do).

- **Productive Nonmarket Activity.** This variable is constructed from the 13 following activities: (i) house cleaning; (ii) washing, ironing, or mending clothes; (iii) yard work or gardening; (iv) shopping or running errands; (v) preparing meals and cleaning up afterwards; (vi) caring for pets; (vii) treating or managing the medical condition of another person; (viii) helping friends, neighbors, or relatives who did not live with you and did not pay you for the help; (ix) doing volunteer work for religious, educational, health-related, or other charitable organizations; (x) taking care of finances or investments, such as banking, paying bills, balancing the checkbook, doing taxes, etc.; (xi) doing home improvements, including painting, redecorating, or making home repairs; (xii) working on, maintaining, or cleaning your car(s) or vehicle(s); (xiii) paying or managing medical bills, including dealing with insurance claims.
- **Leisure.** This variable is constructed from the following 17 activities: (i) watching programs or movies/videos on TV, computers, etc.; (ii) reading newspapers or magazines; (iii) reading books; (iv) listening to music; (v) walking; (vi) participating in sports or other exercise activities; (vii) visiting in-person with friends, neighbors, or relatives; (viii) communicating by telephone, letters, e-mail, Facebook, Skype, or other media with friends, neighbors, or relatives; (ix) praying or meditating; (x) physically showing affection for others through hugging, kissing, etc.; (xi) attending religious services; (xii) attending meetings of clubs or religious groups; (xiii) playing cards or games, or solving puzzles; (xiv) attending concerts, movies or lectures, or visiting museums; (xv) singing or playing a musical instrument; (xvi) doing arts and crafts projects, including knitting, embroidery, or painting; (xvii) dining or eating outside the home (not related to business or work).

- **Self-Care.** This variable is constructed from the following 5 activities: (i) sleeping and napping (including at night); (ii) personal grooming and hygiene, such as bathing and dressing; (iii) treating or managing an existing medical condition of your own; (iv) time spend eating meals at home last week; (v) time spend seeing doctors, nurses, therapists, or other healthcare providers about your own health, or going to the pharmacy for your own medications.