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Health Shocks as Drivers of Persistent Change in Health Behaviors

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Abstract

We study health prevention efforts after acute cardiovascular events using linked Dutch administrative and survey data. In the six years after the event, smoking and alcohol consumption decrease substantially, while preventive medication uptake is near-universal and persistent. Individuals also make forward-looking care and insurance choices. While cardiovascular events impact health, perceived health, and control beliefs, none of these channels explains the behavioral effects, which are similar across socioeconomic and health-risk groups. Our results suggest that the interaction of salient health shocks with broader health-system characteristics, rather than individual-level characteristics or the provision of information through these events, drives successful prevention.

JEL Classification: D15, D83, D91, I12, I18

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I Introduction

Unhealthy lifestyles, such as poor diets, physical inactivity, smoking, and excessive alcohol consumption, are responsible for a substantial share of preventable chronic conditions (Cawley and Ruhm, 2011). The chronic disease burden from unhealthy lifestyles drives rising inequalities in life expectancy and work capacity in high-income countries (Clark et al., 2025; Cutler et al., 2025; Mahler and Yum, 2024; Danesh et al., 2024), resulting in fiscal pressure on health and social security budgets. The decisions individuals make about their own health behaviors therefore have far-reaching consequences for themselves and society at large. Yet, unhealthy lifestyles remain remarkably persistent. Despite the importance of lifestyle for individuals health outcomes and broad policy efforts aimed at increasing health prevention, those engaging in risky health behaviors remain largely unresponsive (Darden, 2017; Oster, 2018).

Experiencing the potential costs of an unhealthy lifestyle in the form of a severe health event could open a window for sustainable behavioral change. Family members observing a spouse's or parent's heart attack, for example, invest substantially more in their own health prevention, irrespective of whether they are themselves at risk or already informed about their own health (Fadlon and Nielsen, 2019; Hoagland, 2025). Clinical guidelines unanimously emphasize the large benefits of lifestyle adjustments even after a first severe health event.[1] Nonetheless, among patients directly experiencing a severe health shock, compliance with such recommendations is not a foregone conclusion.[2] Health perceptions, for example, have been found to often be biased, but also predict lifestyle choices and preventive care use (Arni et al., 2021; Sloan, 2024; Carman and Kooreman, 2014; Spitzer and Shaikh, 2022; Nie et al., 2026). These perceptions can also be remarkably persistent: even when information shocks or interventions alter beliefs about the general link between lifestyle and health risk, they do not necessarily alter beliefs about individuals' own specific risk profile (Ciancio et al., 2024). Health shocks themselves can also fundamentally alter the cost-benefit calculus of prevention. Investing in prevention may be less attractive due to limited remaining life expectancy (Oster et al., 2013), in particular when benefits are uncertain or deferred. Finally, health shocks could also impact other beliefs influencing decision making. This includes beliefs about whether prevention is effective, or more broadly, whether individuals perceive themselves to have control over their lives' outcomes. These types of control beliefs have been shown to influence human capital investments including health prevention (Heckman et al., 2006; Cobb-Clark et al., 2014), while severe health events themselves could induce fatalism and undermine prevention efforts (Ciancio et al., 2025; Kerwin, 2025). Taken together, whether severe health shocks ultimately reinforce or discourage prevention, through which channels they operate, and among whom is thus an empirical question.

In this paper, we study the long-term impact of acute, severe health shocks on health prevention efforts. Subsequently, we consider mechanisms by which health events drive behaviors, including (1) differences in health stock and the perception thereof, (2) individuals' beliefs in their ability to influence their outcomes through their own effort and choices (internal locus of control, subsequently referred to as control beliefs), and (3) evidence for long-term adjustments to health risk perceptions and corresponding forward-looking behaviors. We combine three waves of a large-scale general population health survey

[1] Recommended secondary prevention following a heart attack includes broad lifestyle changes, quitting smoking, reducing alcohol intake, exercise, and weight management, alongside medication uptake and adherence for effective risk reduction (see e.g. Rao et al. (2025) for the US and Piepoli et al. (2016) for Europe).

[2] Evidence from the US suggests that less than half of first-time heart attack patients meet post-event biomarker targets, indicating insufficient secondary prevention efforts (Chiu et al., 2026).

completed by more than one million individuals in the Netherlands with detailed longitudinal administrative data on healthcare use, socioeconomic status, and death records. We exploit the exogenous timing of heart attacks and strokes by comparing the behaviors of individuals experiencing the same event at different points relative to their survey interview, while also adjusting for ex-ante health risk using rich administrative data. This allows us to study health trajectories and behaviors in a 12-year window around the event among a large sample of patients experiencing highly similar cardiovascular events ($N = 9{,}665$).

We first document the effects up to six years after the shock on health prevention efforts: smoking, alcohol consumption, exercise habits, and weight management. These are all significant, modifiable risk factors for cardiovascular morbidity and mortality (Feigin et al., 2017; Roth et al., 2020) requiring costly investments up front, in the case of smoking or alcohol addiction, or sustained over time. We also consider uptake of high-value preventive prescription medication. The health benefits of these medications hinge on long-term adherence (Choudhry et al., 2011; Naderi et al., 2012) with limited evidence on whether the availability and usage of such high-value preventive medication complement or crowd out lifestyle changes (Kaestner et al., 2014; Horn, 2024).

We find that heart attacks and strokes drive broad and sustained investments in health prevention, persisting for at least six years after the shock. Smoking prevalence drops by 9.5 percentage points (40% of the baseline) and alcohol consumption by 1.7 drinks a week (20% of the baseline), and neither effect abates over time. Physical activity and weight management remain unchanged. By contrast, preventive medication use is near-universal and adherence rates remain persistently high even six years after the event: use of at least one cardiovascular medication increases by 45.6 percentage points (84% of the baseline), anti-thrombotic medication by 73.7 percentage points (293%), anti-hypertensive medication by 40.9 percentage points (90%), and statin use by 60.4 percentage points (186%).

To understand the role of individuals' own health stock and its perception, we consider self-assessed health and analyze how it relates to a range of objective indicators of health derived from administrative and survey data (for example, functional limitations, the presence of cardiovascular and non-cardiovascular chronic conditions, and healthcare use). Individuals substantially and immediately downgrade the assessment of their own health after the shock (-0.180 points on a 5-point scale; 26% of the baseline standard deviation). This downward revision contrasts with the observed gradual onset of substantial functional limitations. Controlling for multi-dimensional health differences, whether using objective health measures alone or additionally including self-assessed health itself, does not impact the estimated effects on lifestyle and preventive medication use. This means that while heart attacks and strokes have a direct effect on beliefs about one's health stock, neither differences in health stock nor the perception of one's health stock meaningfully explains observed differences in preventive behaviors following the health shock.

We then consider the role of control beliefs, the degree to which individuals perceive outcomes in their life to be amenable to their own efforts and not primarily driven by external factors. Control beliefs, among other non-cognitive skills, strongly determine human capital accumulation (Heckman et al., 2006). High levels of perceived control, for example, influence job-search effort and success (Caliendo et al., 2015; McGee and McGee, 2016), costly investments in education (Caliendo et al., 2022) as well as health prevention and lifestyle choices (Cobb-Clark et al., 2014; Kesavayuth et al., 2020). An internal locus of control has also been shown to increase resilience to labor market and financial shocks (Schurer, 2017; Kuhnen and Melzer, 2018). However, tentative evidence suggests health shocks could undermine control beliefs (Marsaudon, 2022; Nguyen et al., 2024), potentially impeding health prevention efforts due to fatalism (Ciancio et al., 2025; Kerwin, 2025). We find that health shocks indeed negatively affect control beliefs, with an immediate and permanent decrease of 0.244 standard deviations, translating into a 5.6 percentage point increase (53%) in the share of individuals classified as having low levels of internal locus of control. Despite this substantial negative impact, our results suggest that differences in control beliefs do not explain observed differences in preventive behavior.

Given that changes in health, perceived health stock, and control beliefs do not account for the behavioral response, we finally consider whether individuals act in a forward-looking manner, adjusting their behavior in anticipation of a permanently lower health stock and higher future care needs. Two outcomes are consistent with this. First, individuals persistently increase their GP care consumption after the health shock, likely reflecting increased health information seeking and monitoring. Second, individuals lower their health insurance deductible, reducing their exposure to expected future out-of-pocket costs. Conditioning on health and perceived health explains a large part of these changes (the GP effect falls by 38% and the deductible effect by 31%), but both remain large, suggesting that health shocks shape expectations about future health beyond realized health stock differences and how they are perceived.

Last, we explore whether prevention efforts differ by levels of prior health risk and educational attainment, which may proxy awareness of pre-existing health risk and the ability to process health information.[3] We find only limited evidence of such heterogeneity. While smoking drops by 33% among individuals with a pre-existing high-risk condition and by 47% among those without, alcohol consumption seems to be more persistently impacted for those already having a high-risk condition. Similarly, we observe no differential response in smoking cessation by levels of educational attainment and only suggestive evidence indicating a more persistent decrease in alcohol consumption among those with lower levels of educational attainment. Our results therefore suggest that severe cardiovascular events result in broad behavioral shifts towards health prevention, irrespective of educational differences or pre-existing health risk levels.

All results are robust to alternative specification choices, including the time window used to condition on pre-event characteristics, accounting for income changes in relation to experiencing a health event, or the choice of the survival window we impose on our sample. In addition, we explore the sensitivity of our results to lifestyle-related survival differences and other drivers of non-observability that could bias our results (such as selective survey non-response of individuals with risky lifestyles), showing that the magnitude and pattern of persistent behavioral effects are robust to such dynamics.

The main contribution of this paper is that we identify the effect of a severe and well-defined health event on a wide range of preventive behaviors and potential mechanisms. We can do this because of a unique combination of administrative and survey data containing a broad set of relevant outcomes for a sample of

[3]Individuals at risk of experiencing a cardiovascular event due to lifestyle-related pre-existing chronic conditions might already be selected based on unobservable characteristics making them highly resistant to behavioral adjustments (Oster, 2018). People in lower socioeconomic groups may be less able to change their lifestyle (e.g. Koç and van Kippersluis (2017); James (2024)) and economic decisions (e.g. Handel et al. (2024)) following new information. Descriptive studies in the medical literature for example suggest smaller and less persistent lifestyle adjustments following chronic condition onset (Margolis, 2013; Hernandez et al., 2018).

nearly 10,000 respondents experiencing a similar acute health event whose timing is plausibly exogenous. This combination allows us to complement the precision and long-term follow-up of administrative data (see for example Fadlon and Nielsen 2019; Hoagland 2025; Ahammer et al. 2026; van't Hoff et al. 2025) or consumer choice data (Oster, 2018; Hut and Oster, 2022) and pair it with the depth of outcomes from survey data (see for example Darden 2017; Darden and Gilleskie 2016; Verdun 2025; Horn 2024; Edenbrandt et al. 2025), which in our case includes direct measures of subjective perceptions of health and control beliefs. Thus, this is the first study to jointly capture the broad and long-term behavioral consequences of health events while also being able to (1) explore the role of potential mechanisms through survey-based measures of subjective perceptions of health and control beliefs, (2) do so using a well-defined health event and rich pre-event health information to ensure observed outcomes are driven by the shock itself rather than pre-existing health differences, and (3) account for heterogeneous post-event health profiles.

Our findings on the heterogeneity by health risk and levels of education also contribute to the literature in economics studying the origins of health inequalities over the life course. We find that there are only limited differences in lifestyle and preventive care use following acute health events by education or health risk that would amplify existing health inequalities. This, at least, seems to be the case in the Dutch context, characterized by low barriers to access and high levels of financial protection. We thereby provide causal micro-level evidence on the behavioral response to health changes that suggests heterogeneity in lifestyle changes and broader preventive efforts following acute events is limited and likely plays only a small role in explaining persistent health inequalities. These findings complement recent work, for example Danesh et al. (2024) and Dahl et al. (2024), on the emergence of socioeconomic gradients in life expectancy in Europe and the US. Their findings suggest that these inequalities are predominantly driven by differences in the accumulation of chronic disease burden over the life course rather than by access to and quality of treatment after an (acute) change in health status. Our paper contributes to this debate by confirming that acute cardiovascular events indeed have similar effects across socioeconomic groups with respect to healthcare use, with no evidence for differential levels of lifestyle-driven prevention efforts further entrenching health inequalities.

These findings inform the policy debate on health prevention, and particularly on secondary and tertiary health prevention (i.e. treatment and follow-up after a health shock). We show that severe cardiovascular events create a window of opportunity for behavioral change across the socioeconomic distribution and independent of baseline cardiovascular risk, when intensive healthcare engagement provides structure and support. Furthermore, the persistence of these effects, and their independence from education, prior health information, or perception of health and control beliefs, suggests that converting health shocks into sustained prevention might depend less on individual characteristics than on the quality of healthcare interactions that follow.

II Data

We use the Public Health Monitor 2012, 2016, and 2020 of the Community Health Services, Statistics Netherlands, and the National Institute for Public Health and the Environment (*Gezondheidsmonitor*, henceforth Health Monitor), a large-scale and nationally representative cross-sectional health survey of the general adult population in the Netherlands (aged 18+). The Health Monitor is conducted every four years among non-institutionalized individuals during the last four months of the year. From these, we extract information on physical and mental health, lifestyle, and subjective perceptions of health and locus of control. Pooled over the years, the Health Monitors provide information about more than one million unique respondents.

Health Monitor data are linked to administrative data provided by Statistics Netherlands, covering the full population for the period 1995–2022. These administrative data allow us to build detailed longitudinal panels for 98% of the Health Monitor participants, including Tax Office data on incomes, hospital discharge data, and health insurance claims data. Appendix C contains more information on the full set of administrative datasets we use (C.I), the Health Monitor sampling method (C.II), and detailed variable definitions (C.III), which we summarize below. Summary statistics for our final analysis sample are presented in Section III alongside the sample-construction procedure determining its composition (see Table 1).

Acute Health Events: To select the study sample of Health Monitor respondents who experienced a health event, we identify all individuals admitted for the first time for a heart attack or ischemic stroke between 2010 and 2022 using discharge data on all inpatient hospital admissions to Dutch hospitals between 1995 and 2022.

We ensure that we correctly identify an individual's first-time exposure to an acute cardiovascular event and that different event-time cohorts are comparable over time by applying six exclusion criteria. First, we exclude individuals who experienced a heart attack or stroke in the period 1995–2009 (i.e., prior to our observation window). Second, we exclude those experiencing a previous transient ischemic attack.[4] Third, we exclude those cohabiting with other individuals experiencing a heart attack or stroke at any earlier or later time point to avoid mistakenly attributing to an individual's own event behavioral changes that in fact reflect spillovers from observing such events within the family (Fadlon and Nielsen, 2019). Fourth, we exclude individuals staying at a nursing home prior to their event, as medication consumption during the nursing home stay is unobserved. Likewise, we exclude individuals if their survey participation occurred at any time after a (temporary) nursing home stay in the time period since their health event. Fifth, we exclude individuals whose event occurred more than five years from the mean age-at-onset of heart attacks (mean: 65 years, ages included: 60–70 years) and strokes (mean: 70 years, ages included: 65–75 years). This step mitigates differences in (age-dependent) survival dynamics and ensures that the age-at-onset of individuals across the event-time cohorts we construct is uniform. Finally, we exclude those who die within one year of their date of admission. Heart attack and stroke mortality is concentrated within the first year, and especially the first month following the admission.[5] Like the previous

[4]Transient ischemic attacks are temporary blockages of cerebral blood vessels that cause stroke-like symptoms and signal a high risk of a major cardiovascular event in the near future. These events require immediate medical treatment and the initiation of preventive medication use alongside recommended lifestyle changes (Vinding et al., 2023).

[5]One-year mortality in our sample is 6.6% after a stroke; 3.0% after a heart attack. Appendix Figure D.3 provides a detailed

step, this limits within-sample differences in survival in the post-event period. In robustness checks (Appendix D.III), we consider alternative survival windows and bound how far our results could be affected by differential non-observation across lifestyles, whether through lifestyle-related differences in survival or through other channels such as incapacity or survey non-response.

Healthcare Use, Costs, Chronic Conditions, and Insurance Choice: We measure hospital care use across 19 categories of the International Shortlist for Hospital Morbidity Tabulation (ISHMT). Furthermore, we use data on outpatient medication use at the Anatomical Therapeutic Chemical (ATC) level 3 to capture prescription medication use for cardiovascular prevention and to infer the presence of 21 chronic conditions following Danesh et al. (2024). Finally, we use health insurance claims data to observe health insurance deductible choices, and to capture healthcare use and costs, considering annual hospital care costs, prescription medication costs, and costs from GP consultations, all adjusted for inflation.

Lifestyle, Mental and Physical Health, Perceived Health, and Locus of Control: We use Health Monitor survey data to capture individuals' lifestyle: current smoking status, alcohol consumption, physical activity levels, measured as the number of days per week with 30 or more minutes of at least moderate-intensity exercise, and weight, measured by body mass index (BMI). Our measure of smoking status does not allow us to distinguish complete cessation from substitution with nicotine replacement therapies or other substances not captured in the prescription medication data. Further, we measure physical health using reported functional limitations across seven dimensions, and mental health using the Kessler 10-item depression and anxiety questionnaire (Kessler et al., 2002). Finally, we observe two types of subjective beliefs: (i) self-assessed health on a five-point Likert scale capturing individuals' perception of their own health stock, and (ii) locus of control (2016 and 2020 only), which is the degree to which individuals perceive outcomes in their life as amenable to their own efforts rather than determined by outside circumstances, measured using the Pearlin Mastery Scale (Pearlin and Schooler, 1978).[6]

Demographic and Socioeconomic Information: We take age, sex, and date of death from the mandatory population-wide registry data. Furthermore, we construct age- and sex-specific income rankings and measures of labor market participation based on annual tax records. Data on educational attainment are self-reported and come from the Health Monitor surveys, where we translate the four categories of highest education earned into a binary measure distinguishing those with lower levels of educational attainment (combining no formal education, basic education only, or lower-secondary general and pre-vocational schooling) from those with higher levels (combining upper-secondary general, intermediate vocational, and tertiary education).

breakdown of the 5-year survival dynamics for patients surveyed before their event and those who were not.

[6]This is consistent with the approach taken in the economics literature studying control beliefs and economic behaviors; see, for example, Cobb-Clark et al. (2014); Schurer (2017); Bonsang and Costa-Font (2022); Heckman et al. (2006). For the full set of items, please see Appendix C.III.

III Research Design

III.A Treatment and Control Groups

To identify the effect of health events on preventive behaviors, we exploit the exogenous timing of heart attacks and strokes. We estimate the effect of such acute health events on health-related behaviors by comparing individuals interviewed in the years after their health event (the treatment group) to similar individuals interviewed in the years prior to experiencing the same health event (the control group) using administrative health records to equalize relative *ex-ante* risk at event-time. Our empirical strategy is similar to that of other studies exploiting the exogenous timing of these health events within time windows, in our case a six-year window around the onset of the event, using longitudinal administrative health records to build cohorts of individuals exposed to the same sudden health event at different points in time (see, for example, Chandra and Staiger, 2007; Doyle, 2011; Fadlon and Nielsen, 2019, and most recently Fadlon et al., 2026a,b; van't Hoff et al., 2025).

We assign individuals to an event-time t_i based on the difference between the year of the survey and the year of the health event. For example, a 2012 survey participant with a heart attack in 2014 is assigned to event-time cohort t_{-2}, as they are interviewed two years before the year of their event. Following this approach, we build a dataset of heart attack and stroke patients who are interviewed at different time points relative to their respective health event, ranging from six years before (t_{-6}) to six years after the event (t_6). Individuals surveyed after their event ($t_{\geq 0}$) form the treatment group; those surveyed before ($t_{\leq -1}$) form the control group.

III.B Empirical Estimation

Because the Health Monitor is a cross-sectional survey, each individual is observed at a single point relative to their health event. We do not observe within-person variation and instead compare individuals who experience the same type of health event but are interviewed at different points relative to it, using rich administrative data on pre-event health outcomes to equalize *ex-ante* risk across event-time cohorts. Specifically, we adapt the doubly robust event study design for repeated cross-sections by Callaway and Sant'Anna (2021), comparing treated versus not-yet-treated cohorts. Since each individual i contributes a single observation, we regress outcomes Y_i on event-time dummies $\mathbf{1}(t_i = e)$, a vector of pre-event controls $X^r_{i,0}$, and survey-year fixed effects μ_s:

$$Y_i = \alpha + \sum_{e=-6, e\neq -1}^{6} \delta_e \mathbf{1}(t_i = e) + \beta X^r_{i,0} + \mu_s + \epsilon_i$$

where δ_e captures the difference in outcomes between individuals interviewed e years relative to their health event and the reference cohort interviewed one year before (t_{-1}), and survey-year dummies μ_s capture trends across the 2012, 2016, and 2020 survey waves. To estimate the average treatment effect on the treated (ATT), we run identical regressions but exchange event-time dummies for a single indicator encoding membership in a post-treatment event-time cohort ($t_{\geq 0}$).

Individuals who experience a heart attack or stroke at different calendar times need not share the same *ex-ante* risk profile. For example, even after conditioning on the same event being experienced, two individuals might display vastly different health profiles at the time of their health event with respect to cardiovascular risk factors such as age, sex, socioeconomic status, or the presence of chronic conditions like hypertension or diabetes. To address this, our design ensures that the not-yet-treated control group provides a valid counterfactual for the treated group by weighting observations using a measure of individual-specific relative risk at event-time and directly controlling for a rich set of pre-event health characteristics (Callaway and Sant'Anna, 2021). That is, we calculate propensity scores that are then used as inverse probability weights in all regressions. These scores represent the relative *ex-ante* probability, based on characteristics $X_{i,0}$ just prior to the event, that a given individual experiences their health event in the year in which it actually occurs. $X_{i,0}$ covers pre-event demographics, socioeconomic status, pre-existing medical conditions, healthcare utilization, and healthcare costs incurred right before the time of the health event. Age plays a crucial role in determining absolute risk, and we account for age differences in three steps. First, we restrict the sample to narrow age-at-onset bands around the population mean (60–70 for heart attacks, 65–75 for strokes; see Section II). Second, the propensity score model includes age as a predictor. Third, all outcome regressions control for single-year-of-age dummies, capturing remaining variation in age-at-onset within these bands.

To estimate the propensity scores, we use data on all heart attack and stroke patients in the Netherlands between 2010 and 2022 satisfying our exclusion criteria (see Section II). The regression equation contains a vector of controls $X^r_{i,0}$, a restricted subset of the risk factors used in the propensity score estimation, to account for remaining differences after the inverse probability weighting. These controls cover demographic information (single-year-of-age dummies, sex, living with a partner), socioeconomic status the year before the health event (age- and sex-specific income quintile membership and labor market status), and health information in the year before the health event (chronic conditions, general and cardiovascular-specific hospital care use, and healthcare costs for hospital care, pharmacological care, and GP consultations). Furthermore, we include the type of the health event experienced (heart attack or stroke) and the number of associated inpatient days to account for differences in the intensity of the shock. Appendix D describes our research design in more detail: the construction of our event-time cohorts (D.I); the propensity score estimation procedure, identifying assumptions, and analysis dataset construction (D.II); and the potential influence of survival dynamics (D.III).

We identify 12,900 eligible individuals across the three survey waves after applying the exclusion criteria to Health Monitor respondents experiencing a first-time heart attack or stroke between 2010 and 2022 and restricting to individuals whose event falls within six years of their survey participation. After removing individuals with missing data and applying the further selection criteria, the final analysis sample contains 9,665 individuals, of whom 5,475 (57%) experienced a heart attack and 4,190 (43%) a stroke.[7] Table 1 provides descriptive statistics for the analysis sample at the time of the health event.

[7]Most of these individuals are dropped because of missing data. While we observe 12,900 individuals who participate in the Health Monitor surveys within six years of their respective health event (see Section II), the analysis sample contains 9,665 of these due to five further selection criteria detailed in Appendix D.II: missing propensity score predictors (690); a propensity score outside the common support area (97, or 0.79% of observations for which a propensity score can be estimated); death within one year of admission (361); a health event occurring during the survey collection window, thereby not allowing us to cleanly identify membership in the treatment or control group (396); and incomplete survey outcomes (1,691). A full breakdown of our sample experiencing a health shock by event-time cohort, type of health event, and survey year can be found in Appendix Table D.1.

Table 1: Sample Characteristics at the Time of the Health Event

	All Health Events		Heart Attacks		Strokes	
Health Event Characteristics	Mean	(SD)	Mean	(SD)	Mean	(SD)
Days in Hospital	4.26	(4.52)	4.41	(4.47)	4.07	(4.57)
Heart Attack	0.57	(0.50)				
Stroke	0.43	(0.50)				
Demographic Information						
Age at Onset	67.52	(3.63)	65.64	(3.03)	69.99	(2.77)
Female	0.30	(0.46)	0.25	(0.43)	0.38	(0.48)
Co-habiting Partner	0.78	(0.41)	0.80	(0.40)	0.76	(0.43)
Socioeconomic Status *(in the year before the health event)*						
Employed	0.23	(0.42)	0.34	(0.47)	0.09	(0.28)
Social Benefits	0.03	(0.16)	0.04	(0.19)	0.01	(0.11)
Retired	0.68	(0.47)	0.53	(0.50)	0.88	(0.33)
Disability Benefits	0.04	(0.20)	0.06	(0.23)	0.02	(0.13)
Other Source of Income	0.02	(0.15)	0.03	(0.18)	0.01	(0.09)
Per-capita HH Income (€ 1000s)	2.94	(3.12)	3.20	(2.25)	2.61	(3.95)
1^{st} Income Quintile (lowest)	0.16	(0.37)	0.16	(0.36)	0.17	(0.38)
2^{nd} Income Quintile	0.21	(0.41)	0.21	(0.40)	0.22	(0.41)
3^{rd} Income Quintile	0.21	(0.41)	0.22	(0.41)	0.21	(0.41)
4^{th} Income Quintile	0.21	(0.41)	0.22	(0.41)	0.20	(0.40)
5^{th} Income Quintile (highest)	0.20	(0.40)	0.20	(0.40)	0.19	(0.40)
Health Status *(in the year before the health event)*						
Hospitalizations (CVD-related)	0.05	(0.41)	0.05	(0.42)	0.05	(0.39)
Inpatient Days (CVD-related)	0.12	(1.60)	0.12	(1.69)	0.12	(1.47)
Hospitalizations (Non-CVD)	0.15	(0.55)	0.13	(0.52)	0.16	(0.59)
Inpatient Days (Non-CVD)	0.25	(2.00)	0.22	(1.86)	0.29	(2.17)
Acid-related Disorder	0.29	(0.45)	0.27	(0.44)	0.32	(0.47)
Hypertension	0.48	(0.50)	0.45	(0.50)	0.52	(0.50)
Diabetes	0.13	(0.34)	0.12	(0.32)	0.16	(0.36)
Hyperlipidemia	0.33	(0.47)	0.31	(0.46)	0.35	(0.48)
Depression/Anxiety	0.08	(0.27)	0.07	(0.26)	0.10	(0.29)
Hospital Care Costs (€ 1000s)	2.26	(6.16)	2.16	(6.14)	2.39	(6.18)
Pharmacology Costs (€ 1000s)	0.54	(1.36)	0.51	(1.50)	0.58	(1.15)
GP-visit Costs (€ 1000s)	0.06	(0.07)	0.06	(0.07)	0.07	(0.07)
Observations	9,665		5,475		4,190	

Source: Own calculations based on data from Statistics Netherlands. *Note:* Unweighted means and standard deviations for the analysis sample of 9,665 first-time heart attack and stroke patients (2010–2022) participating in the Health Monitor. See Section II for the exclusion criteria and Appendix D.II for sample construction details.

III.C Identifying Assumptions and Robustness Checks

Our design relies on six identifying assumptions (Callaway and Sant'Anna, 2021). First, treatment status is not reversible, as we study the experience of a heart attack or stroke. Second, to ensure that the treatment is plausibly exogenous and not anticipated, we define the treatment as the timing of the hospital admission conditional on eventually experiencing such an event within the study period. That is, we exclude the never-treated population not experiencing any such event. Third, we consider the plausibility of the parallel trends assumption conditional on pre-event characteristics using the event study coefficients. Fourth, we ensure common support, the comparability of the propensity score distributions across treatment and control groups, event-time cohorts, and survey years, by excluding observations for which the propensity score falls outside the common support area (Appendix D). To achieve this, we only need to remove 0.79% of survey participants for whom a propensity score could be estimated. Fifth, observable demographic, health, and socioeconomic characteristics at event-time across our event-time cohorts are highly balanced, indicating no compositional changes between the 2010 and 2022 admission-year cohorts underlying our sample (Appendix Table D.2). Sixth, the stable unit treatment value assumption is plausible given that we observe limited changes in heart attack and stroke mortality during the observation period and stable treatment patterns for both conditions in that period (Cram et al., 2022). Furthermore, we control for the type of health event and a proxy for severity (associated inpatient days). In robustness checks, we further condition on longer-term survival of up to five years post-event, ensuring homogeneity across event-time cohorts (Section IV). Lastly, spillovers and crowding out of treatment status across individuals, another violation of the stable unit treatment value assumption, are ruled out given our selection of individuals with no prior exposure to severe health events, either through their own earlier events or through within-household occurrences. For a more detailed discussion, see Appendix D.II.

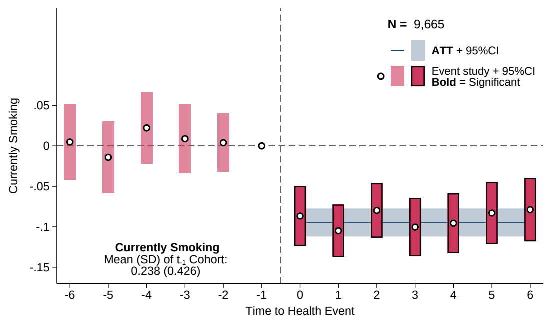
IV Results

IV.A Health Prevention: Lifestyle and Medication Use

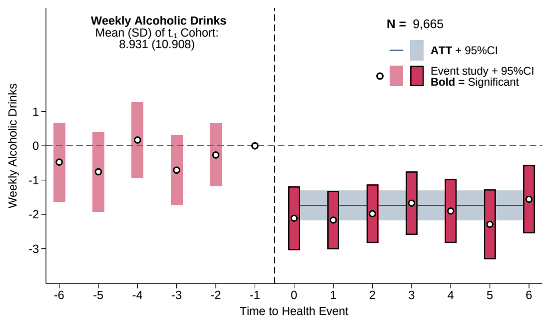
Figure 1 shows the impact of a severe cardiovascular health event on lifestyle: smoking **(a)**, alcohol consumption **(b)**, physical activity **(c)**, and weight management **(d)**. Experiencing a health event results in significant lifestyle differences, decreasing smoking and alcohol consumption, which are the most important dimensions of avoidable risk. The average treatment effect on the treated (ATT) suggests a 9.5 percentage point lower smoking rate following an acute health event, representing a 40% drop given the smoking prevalence of 23.8% among patients interviewed directly before their health event. Alcohol consumption also decreases, by 1.7 weekly drinks compared to a baseline of 8.9 drinks a week, a 20% difference. Both effects persist for at least six years after the event.[8] We find no evidence for changes in physical exercise or weight management; however, event study coefficients indicate that the number of weekly days with physical activity decreases by 0.19 (20% of the baseline) five and six years after experiencing a health event.

Figure 1: The Effect of Acute Health Events on Lifestyle

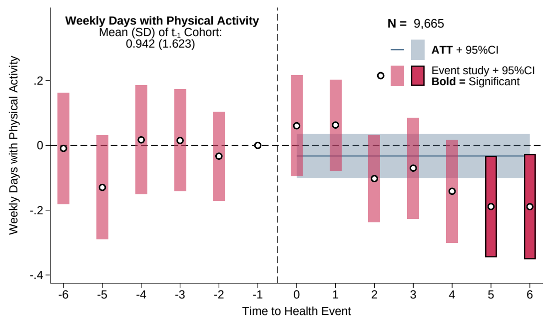
(a) Currently Smoking



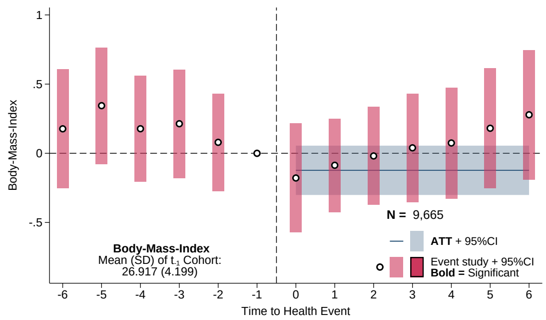
(b) Weekly Alcoholic Drinks



(c) Weekly Days with Physical Activity



(d) Body-Mass-Index



Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the estimated average treatment effect on the treated (**ATT**, solid line) and event study coefficients (dots) alongside their respective 95% confidence intervals (shaded area/bars). All regressions are weighted using inverse probability weights and control for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Column (1) of Appendix Tables A.1 (smoking), A.2 (weekly drinks), A.3 (days physically active), and A.4 (body-mass-index) contains the underlying results.

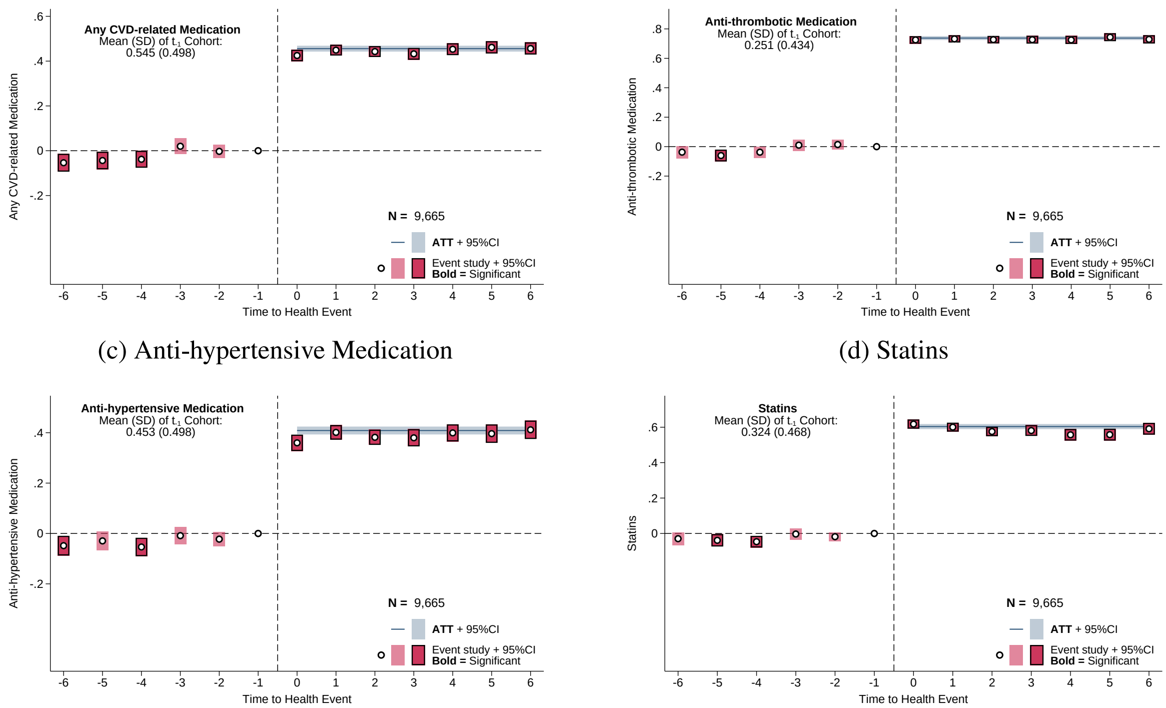
[8]Patients might be forbidden or simply unable to smoke or consume alcohol during their hospitalization; however, this cannot explain these results. Average hospital stays in our sample are 4.26 days (see Table 1), while all regressions also control for the number of inpatient days associated with the health event, accounting for variation in the duration of potential enforced abstinence during hospitalization.

As shown in Figure 2, individuals immediately take up high-value preventive medications with very high levels of adherence. Preventive medication is a key component of risk-management following cardiovascular events with US and European guidelines recommending lifelong medication therapy due to its clinical benefit (Rao et al., 2025; Piepoli et al., 2016). Among those diagnosed, medication uptake and long-term use have been suggested to be insufficient (Naderi et al., 2012; Chiu et al., 2026), while uptake and adherence are often poor both in the presence and absence of monetary barriers like copayments (Choudhry et al., 2011; Chandra et al., 2024). Initial uptake is therefore to a certain degree mechanical, but the level of adherence and persistence over time is high. Average treatment effect estimates indicate a 45.6 percentage point increase (84% of the baseline) in consumption of any cardiovascular medication **(a)**, i.e., anti-thrombotics, anti-hypertensives, or statins, with a 73.7 percentage point increase (294% of the baseline) for anti-thrombotic medications **(b)**, a 40.9 percentage point increase (90% of baseline) for anti-hypertensive medications **(c)**, and a 60.4 percentage point increase (186% of baseline) for statin use **(d)**. These effects are much larger than the impact on lifestyle.

Figure 2: The Effect of Acute Health Events on Prescription Medication Use

(a) Any Cardiovascular-related Medication (b) Anti-thrombotic Medication

(c) Anti-hypertensive Medication (d) Statins



Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the estimated average treatment effect on the treated (**ATT**, solid line) and event study coefficients (dots) alongside their respective 95% confidence intervals (shaded area/bars). All regressions are weighted using inverse probability weights and control for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Column (1) of Appendix Tables A.8 (any cardiovascular medication), A.9 (anti-thrombotics), A.10 (anti-hypertensives), and A.11 (statins) contains the underlying results.

IV.B Mechanisms

To explore potential mechanisms through which health shocks can drive behavioral changes, we first consider the role of differences in health stock and how individuals perceive their own health stock as well as the role of control beliefs. We begin by documenting the impact of health shocks on measures of objective health and self-assessed health, which we interpret as capturing individuals' perceptions of their own health stock. Similarly, we document the impact of health shocks on individuals' control beliefs (locus of control), i.e. the degree to which individuals perceive outcomes as amenable to their own efforts and choices. For both of these candidate mechanisms, we then explore whether variation in perceived health stock and control beliefs can meaningfully explain observed effects on health prevention efforts. These conditioning exercises provide suggestive evidence on whether health stock perceptions and control beliefs are key elements of the causal pathway by which health events drive lifestyle differences across our event-time cohorts. In a final step, we consider two additional measures indicative of individuals' long-term health risk perceptions in the form of forward-looking behaviors: health monitoring efforts via GP care usage, and health insurance coverage choices.

Health Stock Decline and Subjective Health

We first document how health events affect individuals' health stock and its perception. Following an acute cardiovascular event, objective health deteriorates substantially. We observe that 21.7% of individuals report at least one functional limitation before the event, increasing on average by 8.2 percentage points after the event, a 38% increase, with the share reporting such a limitation growing with time since the event (Appendix Figure B.1 panel **(a)**). The more relevant question for our mechanisms is, however, whether individuals' perception of their own health stock moves by more than these objective differences would imply. Figure 3 panel **(a)** reports the effect on self-assessed health after conditioning on non-cardiovascular health at survey time (mental and physical health, non-cardiovascular hospital care use, and chronic conditions). Self-assessed health drops by an average of 0.180 points on a five-point scale (26% of the baseline standard deviation), with an immediate decline of 0.234 attenuating to 0.135 six years post-event.[9] This is more than can be attributed to observable non-cardiovascular health, indicating that individuals genuinely revise their beliefs about their own health downward following the event.[10]

Having established that events lower both health stock and its perception, we ask whether these differences account for the behavioral shifts by adding them to the baseline specification. Figure 3 panels **(b)** and **(c)** show that they do not: neither objective health (triangles, gray) nor the individual's own reassessment of their health stock (diamonds, red) explains a meaningful share of the smoking or alcohol effects. For smoking, the ATT and dynamic estimates are essentially unchanged from baseline; for alcohol the ATT moves only from -1.74 to -1.58 weekly drinks, a difference Wald tests find insignificant (columns (2) to (4) of Appendix Tables A.1 and A.2). The effects on exercise, weight management, and medication use are similarly unaffected, with differences that are statistically but not economically significant. Weekly days of physical activity increase by 0.071 ($p < 0.10$, 7.6% of the pre-event mean) with survey-time health controls against an insignificant -0.025 without them (Appendix Table A.3), and body-mass-index drops by -0.370 ($p < 0.01$, 1.4%) versus an insignificant -0.148 at baseline (Ap-

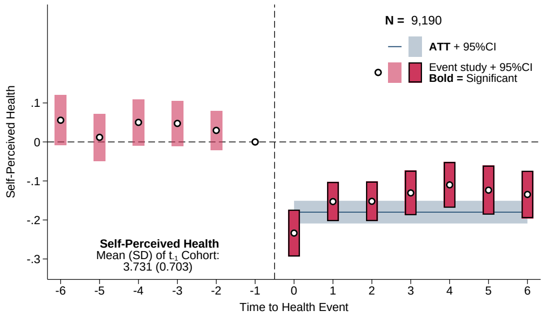
[9] Ordered logit models, which account for the categorical nature of self-assessed health, yield qualitatively equivalent results, with health shocks substantially decreasing the likelihood of reporting the top two categories (*"Very Good"* and *"Good"*) directly after the event and attenuating effects over time (results available upon request).

[10] To understand what drives this reassessment, additionally controlling for cardiovascular risk conditions (diabetes, hypertension, or hyperlipidemia) at survey time, which, because we infer conditions from near-universal post-event medication use absorbs the transition into a diagnosed cardiovascular risk state, shrinks the average effect to -0.099 and renders it insignificant within three years (Appendix Figure B.1 panel **(b)** and Appendix Table A.14).

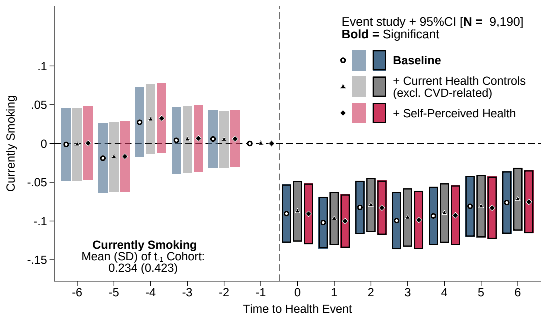
pendix Table A.4); event study estimates indicate both differences are transitory, concentrated in the years directly after the event. For medication use, the change in coefficients is small, between −5.8% (anti-thrombotic) and −10.7% (anti-hypertensive) (columns (2) to (4) of Appendix Tables A.8 to A.11).

Figure 3: Health and Perceived Health and its Role in Explaining Behavioral Effects

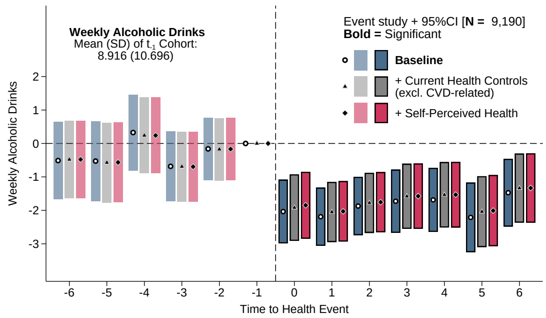
(a) Perceived Health conditional on non-CVD Health



(b) Currently smoking, conditional on Health



(c) Alcohol consumption, conditional on Health



Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the estimated average treatment effect on the treated (**ATT**, solid line) and event study coefficients (dots, triangles, and diamonds) alongside their respective 95% confidence intervals (shaded area/bars). All regressions are weighted using inverse probability weights and control for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Regressions for self-assessed health **(a)** also control for health at survey time (functional limitation dummies, mental health, non-cardiovascular hospital care use and chronic conditions) with Table A.13 column (1) reporting the underlying results. Regressions for smoking **(b)** and alcohol consumption **(c)** compare our baseline estimates using only the basic control variables (dots, blue) against a specification controlling additionally for health at survey time (triangles, gray), and a specification controlling for the basic controls, health at survey time, and self-assessed health (diamonds, red). The underlying results are reported in columns (2) to (4) of Appendix Tables A.1 (smoking) and A.2 (weekly drinks).

Control Beliefs

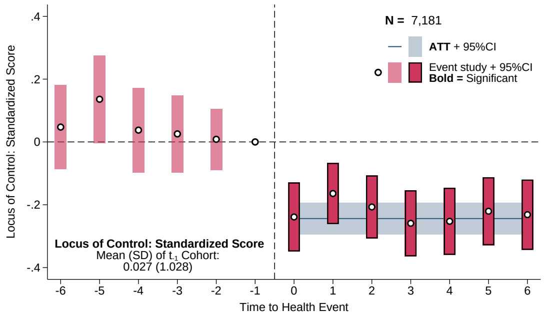
A further mechanism through which the experience of an acute health event could affect behavior is locus of control, the degree to which individuals perceive outcomes in their life as amenable to their own efforts. Experimental studies find that locus of control affects the perceived benefits from costly investments (see e.g. Caliendo et al. (2015)) and that new health information might decrease preventive behavior due to fatalism (Ciancio et al., 2025; Kerwin, 2025). As Figure 4 panel **(a)** shows, locus of control decreases immediately and permanently by 0.244 standard deviations after the health shock.[11] The prevalence of low levels of locus of control (score < 19) increases by 5.6 percentage points, or 53% given the share of

[11] This effect size is considerably larger than previous effects reported by Marsaudon (2022) who reports an average decline of 0.066 standard deviations following an undefined overnight hospitalization, and Nguyen et al. (2024) who report a short-term decline of 0.090 in relation to the self-reported onset of a serious personal injury or illness, lasting for only a year. Our results in contrast are based on a uniform and highly severe health shock, a life-threatening cardiovascular event.

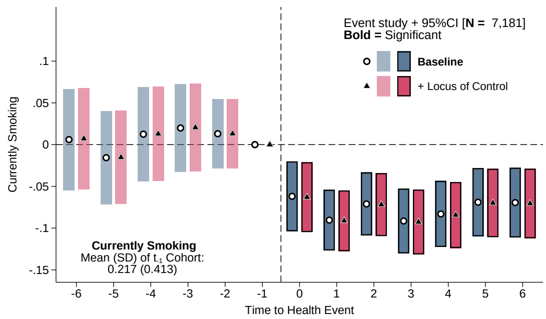
low locus of control among individuals interviewed before their health event (see Appendix Table A.16 for the detailed results).

Figure 4: Control Beliefs and Behavioral Effects conditional on Control Beliefs

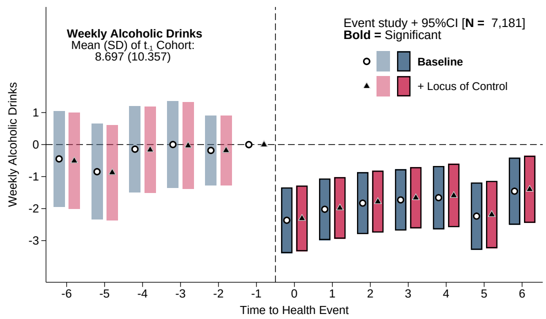
(a) Standardized Locus of Control Score



(b) Currently smoking, conditional on Low Control



(c) Alcohol consumption, conditional on Low Control



Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the estimated average treatment effect on the treated (**ATT**, solid line) and event study coefficients (dots and triangles) alongside their respective 95% confidence intervals (shaded area/bars). All regressions are weighted using inverse probability weights and control for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Regressions for the standardized locus of control score **(a)** control for only the basic control variables with column (1) of Table A.15 reporting the underlying results. Regressions for smoking **(b)** and alcohol consumption **(c)** control for the basic controls (dots, blue) and additionally a dummy for low levels of control beliefs, a locus of control score < 19 (triangles, red). The underlying results are reported in columns (5) and (6) of Appendix Tables A.1 (smoking) and A.2 (weekly drinks).

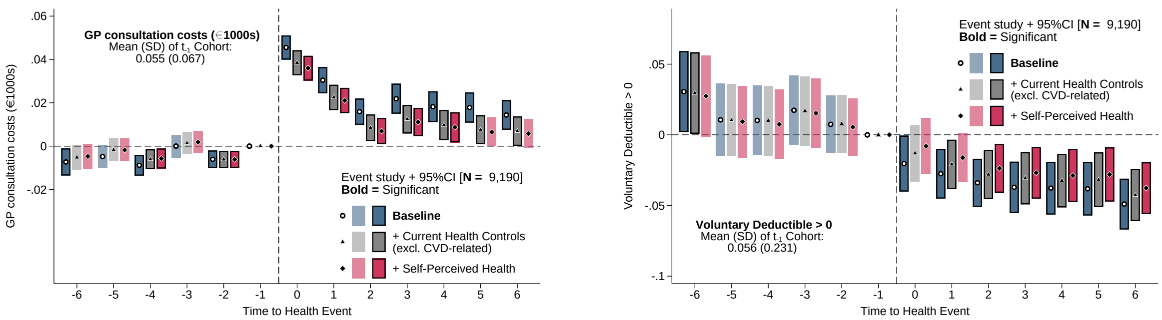
Despite this substantial shift, differences in locus of control add no economically meaningful explanatory power to any of the behavioral outcomes we consider (see Figure 4 panels **(b)** and **(c)** for smoking and alcohol consumption). Detailed results are shown in columns (5) and (6) of Appendix Tables A.1 and A.2 with corresponding results for other outcomes also found in the respective columns in the Appendix Tables reporting the full event study results for a given outcome.

Forward-looking Behavior

Health shocks could also induce individuals to engage in health prevention simply by being more forward-looking, for example due to increased health risk awareness following such a salient event. To explore this, we consider two outcomes for which experiencing a health shock should result in a clear response: a measure of monitoring efforts and health information seeking, and health insurance choices toward increased coverage under expected higher healthcare costs. To capture health monitoring efforts and information seeking, we analyze annual costs for GP consultations. GPs are not involved in the acute treatment of the health shock, which happens in the hospital, but have a role in the follow-up care. Going to the GP ultimately is the patient's decision. To explore individuals' health insurance choices we consider changes in health insurance coverage. In the Netherlands, purchasing health insurance is mandatory and individuals cannot be rejected by insurers. All insurance contracts need to cover a government-mandated basic package of services. Individuals choose a voluntary deductible level between €0 and €500 in €100 increments on top of the mandatory deductible (€385 as of 2022; it rose from €160 in 2010, see Appendix C.III) and receive a discount on the insurance premium in return. The previous year's choice rolls over by default, so a reduction in the voluntary deductible level means that individuals actively increased their coverage level.[12] As having a heart attack or stroke increases the risk of future costs, especially when adhering to preventive medication therapy, forward-looking individuals should lower the voluntary deductible as the expected out-of-pocket costs under the higher deductible outweigh the premium discount. That is, insurance choices may be informed by expected future costs but experiencing such an event does not automatically results in an adjustment to the deductible and thereby these choices are not mechanically related to health.

Figure 5: GP Care and Insurance Choice, conditional on Health

(a) GP consultation costs, conditional on Health (b) Voluntary deductible > 0, conditional on Health



Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the estimated average treatment effect on the treated (**ATT**, solid line) and event study coefficients (dots, triangles, and diamonds) alongside their respective 95% confidence intervals (shaded area/bars). All regressions are weighted using inverse probability weights and control for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Each panel compares our baseline estimates using only the basic control variables (dots, blue) against a specification controlling additionally for health at survey time (triangles, gray), and a specification controlling for the basic controls, health at survey time, and self-assessed health (diamonds, red). The underlying results are reported in columns (2) to (4) of Appendix Tables A.17 (GP consultation costs) and A.18 (deductible choice).

Figure 5 panel **(a)** shows that annual expenditures on general practitioner (GP) consultations increase by €29 (53% of the baseline), equivalent to approximately three regular consultations, suggesting individuals adhere to a regular check-up schedule or seek information and support. The dynamic event study estimates show that the increase is largest in the year directly after the health event (€46 or 84% of the baseline) and decreases to €14 (25% of the baseline) by the sixth year. As depicted in Figure 5 panel **(b)**, the probability of choosing a voluntary deductible drops by 4.2 percentage points (75% of the baseline rate of 5.6%) with event study estimates indicating a gradual decrease starting in the first post-event year.

[12]Primary care, including all GP consultations, is exempted from the deductible. Handel et al. (2024) provides a detailed discussion of the Dutch health insurance system and the health insurance deductible choices. We follow their example by focusing on whether any positive voluntary deductible is chosen as the simplified decision problem individuals face. Health insurance deductibles have to be chosen each year between mid-November and the end of January of the subsequent year and apply for the entire subsequent year. We measure the choice taken in the survey year, effective for the subsequent year.

Conditioning on health and self-assessed health partially accounts for both of these effects. The GP expenditure ATT decreases from € 29 to € 18 when controlling for health at survey time (a 38% reduction), while the deductible ATT attenuates from −4.2 to −2.9 percentage points (a 31% reduction; columns (2) to (4) of Appendix Tables A.17 and A.18). The estimated ATT remains significant, suggesting that health shocks affect GP care use beyond what differences in health can explain, implying that they drive prevention efforts not just via realized differences in health stock and how it is perceived, but also via forward-looking expectations about future health.

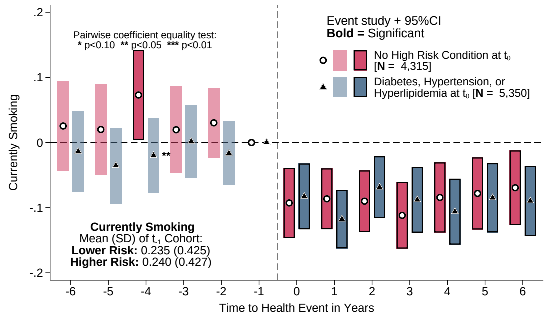
IV.C Heterogeneous Effects

To explore whether the behavioral response to health shocks differs by levels of pre-existing health risk awareness, we split our sample into those already diagnosed with a chronic condition that puts them at increased risk of cardiovascular events, namely hypertension, diabetes, or hyperlipidemia (Roth et al., 2020), and those with none of these conditions. The former group may be better informed about their health conditions and the impact of behavioral change and learns less from the health shock: clinical recommendations for these patient groups to decrease cardiovascular risk emphasize quitting smoking and drinking (next to medication therapy and the related monitoring). In line with this, the group with hypertension, diabetes, or hyperlipidemia has nearly twice as high annual GP consultation costs before the event (€ 72) as those without such a condition (€ 40), translating to roughly three additional visits a year. In addition, downward adjustments in perceived health differ as expected. Those without a pre-existing condition exhibit higher perceived health before the event and revise their self-assessments downward twice as much, by 0.250 (39% of the baseline standard deviation), compared to only 0.126 (18%) among those with such a condition. Furthermore, event study estimates suggest that the impact of an event is shorter-lived among those with an existing high-risk condition (see Appendix Figure B.2).

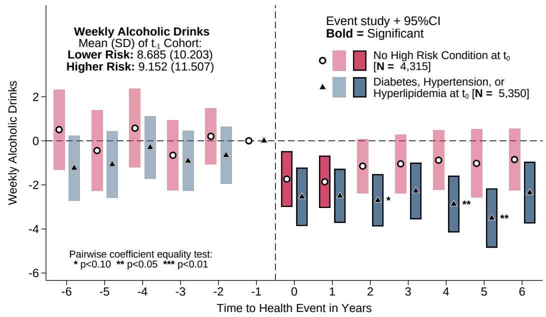
There is very limited evidence that behavioral changes diverge consistently along (known) pre-existing health risk levels. Figure 6 panels **(a)** and **(b)** compare effects among those not having a pre-existing condition (dots, red) against those with a pre-existing condition (triangles, blue). Smoking prevalence decreases more strongly among those without a high-risk condition by 11.1 percentage points (47% of the baseline) compared to an 8.0 percentage point drop (33% of the baseline) among those with such a condition, but this difference is only marginally significant (Wald test p-value < 0.10). The estimated ATT for alcohol consumption is larger among those with a pre-existing condition (−2.07 weekly drinks, 22.6% of the group-specific baseline) than among those without (−1.38 weekly drinks, 15.9%), but this difference is not statistically significant. However, the long-term reduction in alcohol consumption is more persistent among those with a pre-existing condition, as pairwise Wald tests on event study coefficients show.

Figure 6: Lifestyle Effects by Health Risk and Educational Attainment

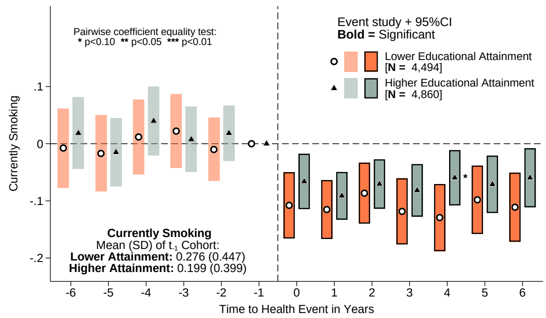
(a) Smoking, by Pre-existing Health Risk



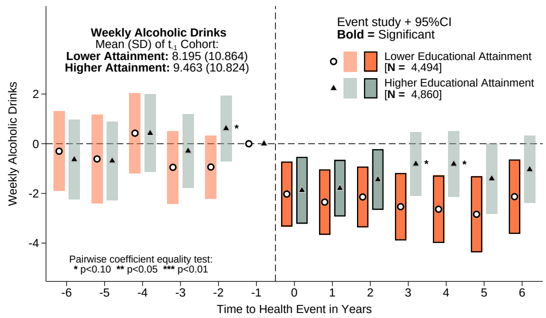
(b) Alcohol Consumption, by Pre-existing Health Risk



(c) Smoking, by Educational Attainment



(d) Alcohol Consumption, by Educational Attainment



Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the estimated event study coefficients (dots and triangles) alongside their respective 95% confidence intervals (bars/shaded area). Stars denote the result of a Wald test of pairwise coefficient equality between the two groups at each event time (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). All regressions are weighted using inverse probability weights and control for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Results by health risk groups: individuals without a high-risk pre-existing condition such as hypertension, diabetes, or hyperlipidemia at event time (dots, red) compared against those with such a condition (triangles, blue). Results by educational attainment: individuals with lower levels of educational attainment (combining no formal education, basic education only, or lower-secondary general and pre-vocational schooling; dots, orange) compared against those with higher educational attainment (combining upper-secondary general, intermediate vocational, and tertiary education; triangles, teal). See Appendix Tables A.1 (smoking) and A.2 (weekly drinks) columns (10) and (11) for the underlying results by health risk, and columns (12) and (13) by educational attainment.

Figure 6 panels **(c)** and **(d)** show that there is no heterogeneity in the impact of a health shock on smoking by education level. Smoking rates fall by 10.7 percentage points (39%) for lower-educated individuals and by 8.4 percentage points (42%) for the higher-educated, and the difference is not statistically significant. Weekly alcohol consumption drops by 1.90 units (23%) among the lower-educated and by 1.48 (16%) for the higher-educated. These differences are not statistically significant, yet the event-study estimates and pairwise coefficient tests show that the reduction of alcohol intake might be more persistent among those with lower educational attainment. Most importantly, across all outcomes we find no evidence suggesting that individuals with lower educational attainment are *less* responsive to cardiovascular events, and hence no evidence that lower levels of post-event health prevention efforts among the lower-educated contribute to health inequalities. Lastly, we find no significant differences in the effect on GP consultation costs but do observe that individuals with higher education drive the increase in insurance coverage (see Appendix Figure B.3), mainly because deductible uptake is generally strongly correlated with socioeconomic status and educational attainment in the Netherlands (Handel et al., 2024).

Robustness Checks

We consider two threats to our identification strategy: specification choices and selective survival. The results from these robustness checks are reported in the last eight columns of each outcome-specific table in Appendix A. First, we consider whether it matters which pre-event health information we use in our regressions: conditioning on observed characteristics at t_{-5} instead of t_{-1} has no systematic impact on the results. Second, we address concerns about selective survival. Health Monitor respondents are healthier and have higher survival rates than the general patient population, which may limit the external validity of our estimates. More critically for internal validity, however, individuals observed at later event-times have by construction survived longer since their event than those observed shortly after. If survival is correlated with health behaviors, either because healthier-behaving individuals before the event are more likely to survive a severe event, or because behavioral change after the event substantially improves survival prospects, this differential survival could bias our estimates. We address this concern in two ways. First, we show that observed survival gaps by lifestyle, or other drivers of selective non-observation by lifestyle, would need to be unrealistically large to explain the magnitude of our effect estimates. Second, we demonstrate that results are unchanged when restricting the sample to individuals who survive at least 3 or 5 years instead of 1 year, which equalizes survival horizons across nearly all event-time cohorts and directly tests whether differential survival drives our findings. For additional details see Appendix D.III.

V Discussion and Conclusion

This paper shows that severe cardiovascular events have a long-lasting impact on health prevention efforts. Smoking and alcohol consumption decrease substantially and permanently, and individuals take up preventive medication with high levels of adherence. These events also decrease individuals' health stock, worsen how they perceive their own health, and weaken their beliefs about whether they are able to influence outcomes in their lives. While these are potential mechanisms by which the experience of a health shock could drive health behaviors, we find that differences in health stock, health perceptions, and control beliefs do not explain differences in prevention efforts. Observed choices regarding GP care and health insurance deductibles are, however, consistent with individuals engaging in forward-looking behavior, reflecting anticipated permanent increases in health risk and future care needs. Lastly, we find no evidence for differential preventive efforts by pre-existing health risk or levels of educational attainment that would suggest that differential engagement in prevention efforts would amplify existing health inequalities.

The persistence of lifestyle choices among high-risk populations, i.e. those with chronic conditions, has been previously explained by the noisiness of health signals (Darden, 2017) and selection on unobservable characteristics determining messaging susceptibility, both making compliance with lifestyle recommendations highly unlikely even with repeated warnings, advice or the onset of a chronic condition (Oster, 2018). Our results show that salient health events such as a severe cardiovascular health shock can overcome this inertia, inducing lasting lifestyle shifts toward increased preventive efforts, even in such high-risk populations. However, this does not appear to be driven by the degree to which these events alter health or the perceptions of one's current health stock. Our results therefore are in line with theoretical models of decision making as event-driven and reliant on salience to overcome behavioral inertia (see, e.g., Bernheim and Rangel, 2004; Bordalo et al., 2022).

Previous work studying the role of personal health risk information in health-related decision making has underlined the importance of non-health related beliefs, such as fatalism, in determining health prevention efforts. Evidence on the behavioral response to HIV risk information in the absence of effective treatment and prevention options, for example, illustrates the substantial degree of fatalism that can undermine prevention efforts by lowering the marginal cost of further risk-taking (Kerwin, 2025), even resulting in increased mortality (Ciancio et al., 2025). We also find evidence of fatalism in response to cardiovascular events as they substantially lower levels of perceived control. Lower levels of control have been shown to drive unhealthy lifestyle choices (Cobb-Clark et al., 2014), and alter costly investment decisions by undermining the perceived benefit of costly effort (Caliendo et al., 2022). Yet, in the context of severe cardiovascular shocks, we document increased care consumption and prevention efforts. This suggests that individuals feel similarly but act differently in an institutional context combining universal access and limited cost sharing with high-quality care (Danesh et al., 2024; OECD, 2025) and a clinical context where efficient secondary and tertiary prevention is possible. Unlike in the US, where copayments have been shown to increase mortality among high-risk individuals by deterring prescription medication use (Chandra et al., 2024), the moderate copayment elements of the Dutch healthcare system seem to not affect preventive care consumption (Ayyagari et al., 2026). Our estimates may therefore represent an upper bound on behavioral adjustments, achieved under favorable conditions in which inertia and countervailing psychological responses can be mitigated.

Our findings also help us understand the origins and persistence of health inequalities over the life course even in high-income countries with well-resourced and accessible healthcare systems. We find little evidence in support of differential health prevention efforts in responses to health shocks by levels of educational attainment. This mirrors results on how individuals react to personalized health risk provided in low-salience contexts (see also Darden 2017; Bhalotra et al. 2020; Oster 2018). Thus, our results provide causal, individual-level evidence consistent with recent work attributing widening life expectancy gaps by education in the United States and Europe to differential accumulation of chronic disease burden over the life course, rather than to differential treatment effectiveness (Danesh et al., 2024; Dahl et al., 2024). We add to this direct evidence that differences in lifestyle and preventive care consumption by education or health-risk levels following acute events are small and unlikely to further increase existing inequalities. This suggests that the broader societal determinants that shape inequalities in chronic health, rather than the differential behavior and treatment across patient groups after severe shocks, are the key policy levers to reduce health inequalities, at least in a system like the Dutch one, in which accessibility of healthcare is already high.

For policy makers seeking to incentivize health prevention efforts, our results are potentially sobering. On the one hand, we find that differences in health stock perceptions do not explain differences in behavior. This implies that improved information provision alone might be insufficient to establish behavioral change without substantial salience in the associated messaging. Inducing salience regarding health risks among the general population is, however, a challenging task for policy makers to achieve. Moreover, ill-tailored but highly salient warnings could result in inefficient allocation of attention (Fadlon and Nielsen, 2019) or outright uptake of low-value care (Hoagland, 2025). At the same time, we find that adjustments in lifestyle and medical prevention efforts, in particular near-universal preventive medication use, are substantial and seem to function as complements rather than substitutes (Kaestner et al., 2014; Horn, 2024). Nonetheless, while the implied lifestyle effects are substantial, they are far from universal. Our results suggest for example that 60% of smokers remain smokers and alcohol use remains at 80% of its level even after severe cardiovascular events. The potential gains from improved prevention efforts therefore likely remain large, and identifying or ruling out sources of this variation helps effectively target public health interventions.

Taken together, our findings suggest that severe cardiovascular events create windows of opportunity for behavioral change across socioeconomic groups, driven by the salience of the shock itself rather than by its impact on health or health perceptions. The persistence of these effects, and their reach across socioeconomic and health groups, points to an important role for interactions with the healthcare system. A particular avenue for future research is therefore the role of system characteristics such as patient-provider interactions, which have been shown to matter not only for chronic disease management and diagnosis (Kristiansen and Sheng, 2025) but also for the use of appropriate care (Boone et al., 2023; Bakx et al., 2026), and which may be equally central to converting acute health shocks into sustained prevention. More broadly, our results imply that secondary and tertiary health prevention (i.e. during treatment and follow-up after the health shock) is a more important policy lever for inducing preventive behavior than information campaigns.

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A Tables

Table A.1: Event-Study Estimates for Smoking – Currently Smoking

		Mechanisms					Subgroups							Robustness Checks							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	0.005	-0.001	-0.001	0.000	0.006	0.007	0.021	0.017	-0.022	0.025	-0.014	-0.008	0.018	0.013	0.006	0.003	-0.002	0.014	0.012	-0.001	0.004
	(0.024)	(0.024)	(0.024)	(0.024)	(0.031)	(0.031)	(0.040)	(0.041)	(0.041)	(0.035)	(0.032)	(0.035)	(0.032)	(0.026)	(0.026)	(0.024)	(0.024)	(0.030)	(0.031)	(0.034)	(0.035)
t_{-5}	-0.014	-0.019	-0.017	-0.017	-0.016	-0.015	0.024	-0.076^b	0.002	0.020	-0.035	-0.017	-0.015	-0.008	-0.013	-0.015	-0.019	-0.013	-0.018	-0.032	-0.038
	(0.023)	(0.023)	(0.023)	(0.023)	(0.029)	(0.029)	(0.039)	(0.037)	(0.040)	(0.035)	(0.030)	(0.034)	(0.030)	(0.025)	(0.025)	(0.023)	(0.023)	(0.023)	(0.023)	(0.033)	(0.034)
t_{-4}	0.022	0.027	0.031	0.033	0.012	0.013	0.029	0.116^c	-0.044	0.073^b	-0.020	0.012	0.040	0.030	0.029	0.023	0.022	0.023	0.015	0.020	0.011
	(0.022)	(0.023)	(0.023)	(0.023)	(0.029)	(0.029)	(0.040)	(0.043)	(0.035)	(0.035)	(0.029)	(0.033)	(0.031)	(0.025)	(0.025)	(0.022)	(0.023)	(0.022)	(0.023)	(0.031)	(0.031)
t_{-3}	0.009	0.004	0.005	0.007	0.020	0.020	0.044	0.015	-0.022	0.020	0.002	0.022	0.008	0.016	0.018	0.009	0.006	0.010	0.006	-0.007	-0.010
	(0.022)	(0.022)	(0.022)	(0.022)	(0.027)	(0.027)	(0.038)	(0.038)	(0.036)	(0.034)	(0.028)	(0.033)	(0.029)	(0.024)	(0.024)	(0.022)	(0.022)	(0.022)	(0.022)	(0.024)	(0.025)
t_{-2}	0.004	0.006	0.005	0.006	0.013	0.013	0.030	0.009	-0.027	0.030	-0.017	-0.010	0.018	0.010	0.009	0.004	0.004	0.007	0.009	-0.011	-0.007
	(0.018)	(0.019)	(0.019)	(0.019)	(0.021)	(0.021)	(0.032)	(0.033)	(0.031)	(0.027)	(0.025)	(0.028)	(0.025)	(0.020)	(0.020)	(0.018)	(0.018)	(0.020)	(0.021)	(0.024)	(0.025)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_0	-0.087^c	-0.090^c	-0.087^c	-0.091^c	-0.062^c	-0.063^c	-0.086^c	-0.087^c	-0.093^c	-0.093^c	-0.083^c	-0.108^c	-0.066^c	-0.062^c	-0.062^c	-0.087^c	-0.086^c	-0.086^c	-0.088^c	-0.117^c	-0.137^c
	(0.019)	(0.019)	(0.020)	(0.020)	(0.021)	(0.021)	(0.032)	(0.033)	(0.033)	(0.028)	(0.026)	(0.030)	(0.025)	(0.021)	(0.021)	(0.019)	(0.019)	(0.019)	(0.019)	(0.023)	(0.023)
t_1	-0.105^c	-0.102^c	-0.097^c	-0.100^c	-0.090^c	-0.091^c	-0.102^c	-0.100^c	-0.113^c	-0.086^c	-0.118^c	-0.115^c	-0.091^c	-0.092^c	-0.088^c	-0.105^c	-0.104^c	-0.104^c	-0.107^c	-0.126^c	-0.125^c
	(0.017)	(0.017)	(0.018)	(0.018)	(0.019)	(0.019)	(0.029)	(0.028)	(0.028)	(0.024)	(0.023)	(0.026)	(0.021)	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)	(0.017)	(0.021)	(0.021)
t_2	-0.080^c	-0.082^c	-0.079^c	-0.083^c	-0.071^c	-0.072^c	-0.092^c	-0.082^c	-0.078^c	-0.090^c	-0.069^c	-0.087^c	-0.071^c	-0.076^c	-0.071^c	-0.080^c	-0.078^c	-0.079^c	-0.082^c	-0.101^c	-0.104^c
	(0.017)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.029)	(0.029)	(0.030)	(0.024)	(0.024)	(0.027)	(0.022)	(0.019)	(0.019)	(0.017)	(0.017)	(0.017)	(0.017)	(0.021)	(0.021)
t_3	-0.100^c	-0.099^c	-0.095^c	-0.099^c	-0.091^c	-0.093^c	-0.089^c	-0.112^c	-0.110^c	-0.112^c	-0.088^c	-0.118^c	-0.082^c	-0.098^c	-0.098^c	-0.101^c	-0.099^c	-0.100^c	-0.100^c	-0.123^c	-0.122^c
	(0.018)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)	(0.033)	(0.031)	(0.031)	(0.026)	(0.026)	(0.029)	(0.023)	(0.022)	(0.022)	(0.018)	(0.019)	(0.018)	(0.019)	(0.023)	(0.023)
t_4	-0.096^c	-0.093^c	-0.090^c	-0.093^c	-0.083^c	-0.084^c	-0.085^c	-0.129^c	-0.084^b	-0.084^c	-0.106^c	-0.129^c	-0.060^b	-0.046^a	-0.039	-0.096^c	-0.093^c	-0.095^c	-0.096^c	-0.119^c	-0.117^c
	(0.019)	(0.019)	(0.020)	(0.020)	(0.020)	(0.020)	(0.032)	(0.031)	(0.034)	(0.028)	(0.026)	(0.030)	(0.025)	(0.025)	(0.025)	(0.019)	(0.019)	(0.019)	(0.019)	(0.023)	(0.023)
t_5	-0.083^c	-0.081^c	-0.081^c	-0.083^c	-0.069^c	-0.070^c	-0.110^c	-0.066^a	-0.079^b	-0.078^c	-0.085^c	-0.098^c	-0.071^c	-0.066^c	-0.064^c	-0.083^c	-0.079^c	-0.083^c	-0.084^c	-0.107^c	-0.104^c
	(0.020)	(0.020)	(0.021)	(0.020)	(0.021)	(0.021)	(0.031)	(0.035)	(0.035)	(0.028)	(0.027)	(0.031)	(0.026)	(0.025)	(0.024)	(0.020)	(0.020)	(0.020)	(0.020)	(0.023)	(0.024)
t_6	-0.079^c	-0.076^c	-0.072^c	-0.075^c	-0.069^c	-0.071^c	-0.075^b	-0.079^b	-0.093^c	-0.069^b	-0.090^c	-0.111^c	-0.060^b	-0.064^c	-0.056^b	-0.079^c	-0.075^c	-0.079^c	-0.080^c	-0.103^c	-0.100^c
	(0.020)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.033)	(0.035)	(0.035)	(0.029)	(0.028)	(0.031)	(0.026)	(0.025)	(0.025)	(0.020)	(0.020)	(0.020)	(0.020)	(0.024)	(0.024)
ATT	-0.095^c	-0.094^c	-0.091^c	-0.095^c	-0.084^c	-0.085^c	-0.114^c	-0.104^c	-0.076^c	-0.111^c	-0.080^c	-0.107^c	-0.084^c	-0.084^c	-0.080^c	-0.095^c	-0.092^c	-0.096^c	-0.096^c	-0.110^c	-0.113^c
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.016)	(0.015)	(0.015)	(0.013)	(0.012)	(0.014)	(0.012)	(0.011)	(0.011)	(0.009)	(0.010)	(0.009)	(0.010)	(0.012)	(0.012)
(% of Mean)	-39.9	-40.2	-38.9	-40.6	-38.7	-39.2	-50.4	-42.1	-31.8	-47.2	-33.3	-38.8	-42.2	-39.1	-37.2	-39.9	-38.7	-40.3	-40.5	-40.4	-42.2
Mean (SD) of	0.238	0.234	0.234	0.234	0.217	0.217	0.226	0.247	0.239	0.235	0.240	0.276	0.199	0.215	0.215	0.238	0.238	0.238	0.237	0.272	0.268
t_{-1} Cohort	(0.426)	(0.423)	(0.423)	(0.423)	(0.413)	(0.413)	(0.419)	(0.432)	(0.427)	(0.425)	(0.427)	(0.447)	(0.399)	(0.411)	(0.411)	(0.426)	(0.426)	(0.426)	(0.426)	(0.445)	(0.443)
Wald F (ATT)	–	ref.	0.583	0.030	ref.	2.176	ref.	0.224	3.124^a	ref.	3.026^a	ref.	1.516	ref.	2.378	ref.	0.346	ref.	0.000	ref.	0.538
Wald F (t_0–t_6, joint)	–	ref.	0.748	0.361	ref.	0.286	ref.	0.632	0.283	ref.	0.791	ref.	0.629	ref.	0.822	ref.	0.254	ref.	1.062	ref.	1.404
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.070	0.072	0.080	0.081	0.064	0.064	0.084	0.102	0.069	0.093	0.065	0.073	0.074	0.074	0.069	0.070	0.071	0.074	0.072	0.078	0.075

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

Table A.2: Event-Study Estimates for Alcohol Consumption – Weekly Alcoholic Drinks

		Mechanisms					Subgroups							Robustness Checks							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.475	-0.511	-0.480	-0.478	-0.446	-0.507	-0.910	0.886	-1.078	0.505	-1.236	-0.301	-0.634	-0.606	-0.720	-0.500	-0.050	-0.688	-0.829	-0.679	-0.876
	(0.587)	(0.590)	(0.588)	(0.589)	(0.765)	(0.765)	(0.841)	(1.183)	(0.984)	(0.926)	(0.757)	(0.821)	(0.818)	(0.636)	(0.637)	(0.589)	(0.599)	(0.726)	(0.695)	(0.827)	(0.810)
t_{-5}	-0.759	-0.527	-0.573	-0.565	-0.846	-0.871	-0.362	-1.888^{b}	0.046	-0.442	-1.073	-0.613	-0.689	-0.903	-0.925	-0.790	-0.482	-0.753	-0.609	-0.640	-0.678
	(0.591)	(0.607)	(0.608)	(0.608)	(0.761)	(0.761)	(1.016)	(0.951)	(1.045)	(0.936)	(0.772)	(0.911)	(0.810)	(0.633)	(0.636)	(0.591)	(0.602)	(0.592)	(0.608)	(0.873)	(0.891)
t_{-4}	0.170	0.327	0.239	0.240	-0.143	-0.163	0.487	2.271^{a}	-1.000	0.571	-0.297	0.425	0.423	0.028	0.014	0.164	0.311	0.178	0.268	0.304	0.327
	(0.564)	(0.578)	(0.578)	(0.579)	(0.686)	(0.686)	(0.940)	(1.249)	(0.818)	(0.910)	(0.728)	(0.822)	(0.797)	(0.607)	(0.606)	(0.565)	(0.568)	(0.565)	(0.576)	(0.805)	(0.838)
t_{-3}	-0.710	-0.684	-0.691	-0.695	0.003	-0.031	-0.738	0.443	-1.394	-0.656	-0.914	-0.950	-0.291	-0.824	-0.863	-0.711	-0.567	-0.696	-0.586	-0.797	-0.569
	(0.525)	(0.532)	(0.534)	(0.534)	(0.691)	(0.692)	(0.834)	(0.977)	(0.881)	(0.813)	(0.699)	(0.745)	(0.759)	(0.580)	(0.579)	(0.525)	(0.527)	(0.527)	(0.539)	(0.584)	(0.611)
t_{-2}	-0.267	-0.161	-0.174	-0.167	-0.181	-0.181	-0.124	0.627	-0.973	0.206	-0.659	-0.939	0.616	-0.239	-0.293	-0.248	-0.194	-0.152	-0.081	0.005	0.102
	(0.466)	(0.475)	(0.476)	(0.477)	(0.556)	(0.555)	(0.802)	(0.911)	(0.722)	(0.649)	(0.661)	(0.650)	(0.675)	(0.503)	(0.505)	(0.467)	(0.467)	(0.520)	(0.530)	(0.596)	(0.619)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_0	-2.114^{c}	-2.035^{c}	-1.918^{c}	-1.848^{c}	-2.362^{c}	-2.305^{c}	-2.204^{c}	-1.515^{a}	-2.336^{c}	-1.737^{c}	-2.542^{c}	-2.026^{c}	-1.875^{c}	-2.323^{c}	-2.352^{c}	-2.114^{c}	-2.165^{c}	-2.118^{c}	-1.861^{c}	-2.229^{c}	-2.156^{c}
	(0.474)	(0.486)	(0.508)	(0.511)	(0.523)	(0.524)	(0.775)	(0.894)	(0.780)	(0.646)	(0.677)	(0.668)	(0.687)	(0.519)	(0.519)	(0.474)	(0.474)	(0.474)	(0.485)	(0.589)	(0.599)
t_1	-2.167^{c}	-2.190^{c}	-2.048^{c}	-2.029^{c}	-2.019^{c}	-1.978^{c}	-2.644^{c}	-1.040	-2.730^{c}	-1.861^{c}	-2.501^{c}	-2.351^{c}	-1.792^{c}	-2.017^{c}	-1.947^{c}	-2.167^{c}	-2.269^{c}	-2.182^{c}	-2.029^{c}	-2.431^{c}	-2.242^{c}
	(0.435)	(0.444)	(0.459)	(0.462)	(0.490)	(0.490)	(0.716)	(0.833)	(0.699)	(0.606)	(0.625)	(0.672)	(0.581)	(0.480)	(0.482)	(0.435)	(0.437)	(0.435)	(0.442)	(0.528)	(0.546)
t_2	-1.981^{c}	-1.871^{c}	-1.775^{c}	-1.756^{c}	-1.828^{c}	-1.780^{c}	-2.275^{c}	-0.789	-2.881^{c}	-1.150^{a}	-2.700^{c}	-2.143^{c}	-1.441^{b}	-1.876^{c}	-1.752^{c}	-1.975^{c}	-2.136^{c}	-1.978^{c}	-1.852^{c}	-2.248^{c}	-2.067^{c}
	(0.436)	(0.446)	(0.459)	(0.460)	(0.493)	(0.493)	(0.750)	(0.819)	(0.699)	(0.629)	(0.607)	(0.624)	(0.623)	(0.482)	(0.484)	(0.436)	(0.440)	(0.436)	(0.440)	(0.527)	(0.541)
t_3	-1.673^{c}	-1.727^{c}	-1.580^{c}	-1.575^{c}	-1.726^{c}	-1.660^{c}	-1.891^{b}	-0.887	-1.948^{b}	-1.044	-2.276^{c}	-2.536^{c}	-0.813	-1.000^{a}	-0.963	-1.673^{c}	-1.875^{c}	-1.672^{c}	-1.572^{c}	-2.029^{c}	-1.798^{c}
	(0.472)	(0.483)	(0.497)	(0.499)	(0.488)	(0.488)	(0.766)	(0.850)	(0.827)	(0.681)	(0.655)	(0.693)	(0.653)	(0.602)	(0.602)	(0.472)	(0.475)	(0.471)	(0.473)	(0.569)	(0.582)
t_4	-1.903^{c}	-1.687^{c}	-1.534^{c}	-1.531^{c}	-1.657^{c}	-1.588^{c}	-1.749^{b}	-0.577	-3.092^{c}	-0.882	-2.872^{c}	-2.638^{c}	-0.817	-1.477^{b}	-1.428^{b}	-1.903^{c}	-2.162^{c}	-1.918^{c}	-1.831^{c}	-2.263^{c}	-2.061^{c}
	(0.475)	(0.489)	(0.500)	(0.501)	(0.505)	(0.505)	(0.825)	(0.843)	(0.795)	(0.690)	(0.657)	(0.693)	(0.675)	(0.599)	(0.608)	(0.475)	(0.485)	(0.475)	(0.478)	(0.575)	(0.585)
t_5	-2.290^{c}	-2.210^{c}	-2.038^{c}	-2.008^{c}	-2.234^{c}	-2.184^{c}	-2.407^{c}	-1.246	-2.916^{c}	-1.026	-3.504^{c}	-2.840^{c}	-1.406^{a}	-1.561^{b}	-1.453^{b}	-2.290^{c}	-2.608^{c}	-2.299^{c}	-2.203^{c}	-2.664^{c}	-2.481^{c}
	(0.519)	(0.532)	(0.541)	(0.543)	(0.536)	(0.536)	(0.846)	(0.926)	(0.902)	(0.787)	(0.686)	(0.781)	(0.722)	(0.705)	(0.713)	(0.519)	(0.524)	(0.519)	(0.521)	(0.605)	(0.613)
t_6	-1.560^{c}	-1.472^{c}	-1.336^{b}	-1.332^{b}	-1.455^{c}	-1.396^{c}	-1.391	-1.471^{a}	-1.654^{a}	-0.851	-2.353^{c}	-2.132^{c}	-1.036	-1.440^{b}	-1.271^{b}	-1.560^{c}	-1.921^{c}	-1.563^{c}	-1.464^{c}	-1.936^{c}	-1.750^{c}
	(0.508)	(0.517)	(0.527)	(0.530)	(0.535)	(0.534)	(0.874)	(0.871)	(0.903)	(0.716)	(0.714)	(0.764)	(0.688)	(0.632)	(0.643)	(0.508)	(0.526)	(0.509)	(0.511)	(0.607)	(0.616)
ATT	-1.738^{c}	-1.746^{c}	-1.595^{c}	-1.575^{c}	-1.744^{c}	-1.678^{c}	-1.988^{c}	-1.389^{c}	-1.926^{c}	-1.384^{c}	-2.070^{c}	-1.897^{c}	-1.477^{c}	-1.616^{c}	-1.520^{c}	-1.736^{c}	-2.032^{c}	-1.784^{c}	-1.693^{c}	-2.050^{c}	-1.952^{c}
	(0.223)	(0.229)	(0.251)	(0.255)	(0.258)	(0.258)	(0.378)	(0.418)	(0.366)	(0.325)	(0.310)	(0.331)	(0.314)	(0.276)	(0.279)	(0.223)	(0.247)	(0.237)	(0.241)	(0.296)	(0.305)
(% of Mean)	-19.5	-19.6	-17.9	-17.7	-20.1	-19.3	-22.6	-15.3	-21.6	-15.9	-22.6	-23.1	-15.6	-18.6	-17.5	-19.4	-22.8	-20.0	-19.1	-21.3	-20.5
Mean (SD) of	8.931	8.916	8.916	8.916	8.697	8.697	8.791	9.087	8.917	8.685	9.152	8.195	9.463	8.669	8.669	8.931	8.931	8.931	8.848	9.618	9.514
t_{-1} Cohort	(10.91)	(10.70)	(10.70)	(10.70)	(10.36)	(10.36)	(10.71)	(11.55)	(10.48)	(10.20)	(11.51)	(10.86)	(10.82)	(10.32)	(10.32)	(10.91)	(10.91)	(10.91)	(10.85)	(11.36)	(11.29)
Wald F (ATT)	–	ref.	2.201	2.464	ref.	10.130^{c}	ref.	1.131	0.014	ref.	2.330	ref.	0.850	ref.	2.069	ref.	9.000^{c}	ref.	4.030^{b}	ref.	1.031
Wald F (t_0–t_6, joint)	–	ref.	0.591	0.590	ref.	1.072	ref.	0.609	0.311	ref.	1.267	ref.	0.968	ref.	0.497	ref.	1.755^{a}	ref.	1.803^{a}	ref.	0.553
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.088	0.086	0.091	0.092	0.082	0.084	0.108	0.085	0.110	0.085	0.104	0.102	0.076	0.088	0.082	0.088	0.091	0.089	0.088	0.090	0.088

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

Table A.3: Event-Study Estimates for Physical Activity – Weekly Days with Physical Activity

		Mechanisms					**Subgroups**							**Robustness Checks**							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.009	-0.009	-0.019	-0.035	-0.064	-0.077	0.108	-0.034	-0.100	-0.056	0.036	0.089	-0.168	-0.035	-0.062	-0.005	0.044	0.041	0.047	0.051	0.035
	(0.088)	(0.087)	(0.087)	(0.087)	(0.115)	(0.115)	(0.150)	(0.151)	(0.151)	(0.134)	(0.116)	(0.123)	(0.127)	(0.096)	(0.097)	(0.088)	(0.091)	(0.109)	(0.112)	(0.124)	(0.131)
t_{-5}	-0.130	-0.092	-0.112	-0.118	-0.151	-0.157	-0.187	0.032	-0.224	-0.143	-0.120	-0.086	-0.180	-0.154^{a}	-0.160^{a}	-0.128	-0.087	-0.125	-0.102	-0.132	-0.085
	(0.082)	(0.084)	(0.084)	(0.084)	(0.097)	(0.097)	(0.135)	(0.145)	(0.142)	(0.130)	(0.105)	(0.111)	(0.124)	(0.089)	(0.090)	(0.082)	(0.083)	(0.082)	(0.085)	(0.125)	(0.134)
t_{-4}	0.017	0.048	0.029	0.011	0.116	0.112	0.240	-0.258^{a}	-0.004	-0.038	0.079	-0.002	0.011	-0.010	-0.020	0.018	0.040	0.025	0.022	-0.039	-0.027
	(0.086)	(0.088)	(0.087)	(0.087)	(0.115)	(0.115)	(0.156)	(0.137)	(0.140)	(0.131)	(0.112)	(0.107)	(0.137)	(0.094)	(0.095)	(0.086)	(0.086)	(0.086)	(0.087)	(0.114)	(0.120)
t_{-3}	0.015	0.042	0.030	0.018	-0.035	-0.043	0.085	0.134	-0.126	-0.110	0.102	-0.061	0.042	-0.020	-0.030	0.015	0.031	0.021	0.019	0.004	-0.004
	(0.080)	(0.082)	(0.082)	(0.082)	(0.099)	(0.098)	(0.143)	(0.145)	(0.131)	(0.128)	(0.102)	(0.101)	(0.125)	(0.088)	(0.088)	(0.080)	(0.080)	(0.080)	(0.082)	(0.088)	(0.092)
t_{-2}	-0.033	-0.034	-0.034	-0.049	-0.042	-0.042	-0.062	-0.082	-0.001	-0.045	-0.037	0.003	-0.097	-0.050	-0.052	-0.034	-0.025	-0.011	-0.017	-0.004	-0.022
	(0.070)	(0.071)	(0.071)	(0.070)	(0.082)	(0.082)	(0.116)	(0.119)	(0.121)	(0.109)	(0.090)	(0.093)	(0.105)	(0.076)	(0.076)	(0.070)	(0.070)	(0.077)	(0.078)	(0.086)	(0.091)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	0.060	0.074	0.118	0.165^{b}	0.049	0.062	0.175	0.091	-0.033	0.174	-0.021	-0.024	0.137	0.016	0.015	0.061	0.055	0.060	0.073	0.046	0.069
	(0.079)	(0.081)	(0.083)	(0.083)	(0.090)	(0.090)	(0.135)	(0.142)	(0.135)	(0.128)	(0.100)	(0.100)	(0.122)	(0.087)	(0.087)	(0.079)	(0.079)	(0.079)	(0.081)	(0.097)	(0.103)
t_{1}	0.063	0.071	0.108	0.147^{a}	0.093	0.102	0.023	0.185	-0.016	0.048	0.072	0.028	0.093	0.089	0.100	0.063	0.048	0.065	0.082	0.033	0.043
	(0.072)	(0.073)	(0.075)	(0.075)	(0.083)	(0.083)	(0.121)	(0.126)	(0.122)	(0.107)	(0.096)	(0.093)	(0.110)	(0.081)	(0.082)	(0.072)	(0.072)	(0.072)	(0.073)	(0.083)	(0.087)
t_{2}	-0.102	-0.083	-0.038	-0.001	-0.094	-0.084	-0.004	0.005	-0.264^{b}	-0.061	-0.137	-0.116	-0.066	-0.082	-0.060	-0.101	-0.121^{a}	-0.102	-0.102	-0.135^{a}	-0.138
	(0.069)	(0.071)	(0.072)	(0.072)	(0.079)	(0.079)	(0.128)	(0.118)	(0.112)	(0.106)	(0.089)	(0.086)	(0.107)	(0.078)	(0.078)	(0.069)	(0.069)	(0.069)	(0.069)	(0.081)	(0.085)
t_{3}	-0.070	-0.054	-0.008	0.026	-0.052	-0.037	-0.203	-0.034	0.023	-0.041	-0.093	-0.013	-0.076	-0.082	-0.062	-0.070	-0.098	-0.072	-0.067	-0.113	-0.102
	(0.080)	(0.082)	(0.082)	(0.082)	(0.085)	(0.085)	(0.135)	(0.136)	(0.142)	(0.124)	(0.103)	(0.102)	(0.123)	(0.102)	(0.103)	(0.080)	(0.081)	(0.080)	(0.080)	(0.093)	(0.096)
t_{4}	-0.142^{a}	-0.123	-0.067	-0.037	-0.169^{b}	-0.154^{a}	-0.208	0.052	-0.275^{b}	-0.093	-0.188^{a}	0.064	-0.312^{c}	-0.169	-0.142	-0.141^{a}	-0.178^{b}	-0.143^{a}	-0.137^{a}	-0.183^{a}	-0.189^{a}
	(0.081)	(0.083)	(0.085)	(0.085)	(0.085)	(0.085)	(0.139)	(0.148)	(0.132)	(0.126)	(0.105)	(0.111)	(0.121)	(0.105)	(0.106)	(0.081)	(0.083)	(0.081)	(0.082)	(0.094)	(0.096)
t_{5}	-0.189^{b}	-0.154^{a}	-0.097	-0.066	-0.196^{b}	-0.185^{b}	-0.157	-0.164	-0.252^{a}	-0.126	-0.242^{b}	-0.118	-0.223^{a}	-0.095	-0.073	-0.189^{b}	-0.230^{c}	-0.190^{b}	-0.185^{b}	-0.234^{b}	-0.238^{b}
	(0.080)	(0.083)	(0.084)	(0.084)	(0.086)	(0.086)	(0.141)	(0.137)	(0.140)	(0.119)	(0.107)	(0.106)	(0.123)	(0.107)	(0.108)	(0.080)	(0.082)	(0.080)	(0.080)	(0.093)	(0.095)
t_{6}	-0.189^{b}	-0.159^{a}	-0.112	-0.073	-0.184^{b}	-0.171^{a}	0.040	-0.081	-0.563^{c}	-0.093	-0.261^{b}	-0.211^{b}	-0.143	-0.169	-0.138	-0.189^{b}	-0.228^{c}	-0.192^{b}	-0.187^{b}	-0.235^{b}	-0.241^{b}
	(0.083)	(0.086)	(0.087)	(0.087)	(0.089)	(0.089)	(0.149)	(0.156)	(0.118)	(0.127)	(0.107)	(0.103)	(0.132)	(0.106)	(0.109)	(0.083)	(0.085)	(0.083)	(0.083)	(0.096)	(0.098)
ATT	-0.033	-0.025	0.024	0.071^{a}	-0.038	-0.023	-0.031	0.073	-0.113^{a}	0.036	-0.090^{b}	-0.037	0.010	0.010	0.030	-0.033	-0.057	-0.040	-0.034	-0.060	-0.052
	(0.035)	(0.036)	(0.039)	(0.039)	(0.040)	(0.040)	(0.062)	(0.060)	(0.059)	(0.054)	(0.045)	(0.046)	(0.054)	(0.044)	(0.044)	(0.035)	(0.039)	(0.037)	(0.038)	(0.044)	(0.046)
(% of Mean)	-3.5	-2.7	2.6	7.6	-4.0	-2.4	-3.3	7.5	-12.4	3.5	-10.5	-5.5	0.8	1.1	3.2	-3.5	-6.1	-4.2	-3.6	-6.3	-5.4
Mean (SD) of	0.942	0.939	0.939	0.939	0.949	0.949	0.938	0.976	0.913	1.033	0.861	0.677	1.197	0.940	0.940	0.942	0.942	0.942	0.944	0.952	0.965
t_{-1} Cohort	(1.623)	(1.604)	(1.604)	(1.604)	(1.613)	(1.613)	(1.592)	(1.683)	(1.597)	(1.704)	(1.543)	(1.419)	(1.764)	(1.608)	(1.608)	(1.623)	(1.623)	(1.623)	(1.619)	(1.621)	(1.630)
Wald F (ATT)	–	ref.	8.594^{c}	27.380^{c}	ref.	18.140^{c}	ref.	1.463	0.917	ref.	3.212^{a}	ref.	0.452	ref.	3.370^{a}	ref.	2.378	ref.	0.621	ref.	0.395
Wald F (t_0–t_6, joint)	–	ref.	2.524^{b}	3.797^{c}	ref.	1.500	ref.	0.776	2.882^{c}	ref.	0.428	ref.	1.729^{a}	ref.	0.496	ref.	1.101	ref.	0.951	ref.	1.653
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.053	0.053	0.062	0.068	0.061	0.064	0.065	0.053	0.070	0.062	0.050	0.036	0.046	0.054	0.047	0.053	0.055	0.053	0.052	0.055	0.055

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

Table A.4: Event-Study Estimates for Weight Management – Body-Mass-Index

		Mechanisms					Subgroups							Robustness Checks							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	0.177	0.246	0.319	0.341	0.344	0.357	0.196	-0.262	0.548	-0.064	0.359	-0.073	0.538^a	0.177	0.219	0.188	0.238	0.030	0.125	0.136	0.269
	(0.219)	(0.219)	(0.219)	(0.219)	(0.296)	(0.297)	(0.380)	(0.345)	(0.395)	(0.295)	(0.314)	(0.333)	(0.281)	(0.242)	(0.244)	(0.219)	(0.227)	(0.281)	(0.283)	(0.298)	(0.308)
t_{-5}	0.344	0.427^b	0.471^b	0.481^b	0.554^a	0.559^a	0.697^a	0.068	0.294	0.104	0.553^a	0.526	0.376	0.351	0.440^a	0.351	0.403^a	0.339	0.391^a	0.218	0.274
	(0.215)	(0.217)	(0.217)	(0.216)	(0.293)	(0.293)	(0.381)	(0.372)	(0.361)	(0.320)	(0.290)	(0.329)	(0.275)	(0.238)	(0.242)	(0.215)	(0.219)	(0.216)	(0.221)	(0.290)	(0.298)
t_{-4}	0.177	0.205	0.215	0.236	0.218	0.222	0.460	-0.033	0.104	-0.225	0.497^a	0.088	0.377	0.203	0.224	0.180	0.206	0.175	0.212	0.320	0.268
	(0.195)	(0.199)	(0.199)	(0.199)	(0.258)	(0.258)	(0.377)	(0.352)	(0.306)	(0.277)	(0.270)	(0.303)	(0.253)	(0.220)	(0.223)	(0.195)	(0.197)	(0.196)	(0.200)	(0.251)	(0.265)
t_{-3}	0.214	0.304	0.312	0.321	0.141	0.149	0.421	-0.272	0.356	0.113	0.309	-0.164	0.653^b	0.225	0.317	0.213	0.233	0.213	0.231	0.328	0.361
	(0.200)	(0.204)	(0.203)	(0.203)	(0.255)	(0.255)	(0.367)	(0.331)	(0.335)	(0.286)	(0.275)	(0.297)	(0.274)	(0.222)	(0.226)	(0.200)	(0.201)	(0.201)	(0.204)	(0.216)	(0.220)
t_{-2}	0.079	0.124	0.114	0.138	0.056	0.056	0.019	0.050	0.106	-0.369	0.481^a	-0.212	0.319	0.048	0.095	0.077	0.081	0.049	0.060	0.132	0.135
	(0.180)	(0.183)	(0.183)	(0.183)	(0.217)	(0.217)	(0.307)	(0.281)	(0.316)	(0.239)	(0.262)	(0.289)	(0.222)	(0.199)	(0.202)	(0.180)	(0.180)	(0.190)	(0.193)	(0.208)	(0.214)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_0	-0.178	-0.171	-0.337^a	-0.395^a	-0.249	-0.261	-0.270	-0.360	0.004	-0.227	-0.091	-0.093	-0.206	-0.314	-0.362	-0.179	-0.190	-0.176	-0.147	-0.122	-0.018
	(0.201)	(0.197)	(0.203)	(0.204)	(0.233)	(0.232)	(0.347)	(0.328)	(0.358)	(0.272)	(0.284)	(0.319)	(0.250)	(0.231)	(0.234)	(0.201)	(0.201)	(0.201)	(0.204)	(0.228)	(0.233)
t_1	-0.087	-0.078	-0.239	-0.288	-0.194	-0.203	-0.066	-0.320	0.027	-0.357	0.113	-0.237	0.019	-0.240	-0.236	-0.087	-0.110	-0.084	-0.028	0.085	0.190
	(0.173)	(0.176)	(0.180)	(0.181)	(0.198)	(0.198)	(0.308)	(0.278)	(0.302)	(0.239)	(0.247)	(0.270)	(0.219)	(0.196)	(0.198)	(0.173)	(0.173)	(0.173)	(0.175)	(0.198)	(0.204)
t_2	-0.019	-0.013	-0.136	-0.176	0.008	-0.002	0.142	0.085	-0.328	-0.120	0.063	-0.246	0.152	0.002	0.046	-0.033	-0.062	-0.018	-0.003	0.159	0.255
	(0.181)	(0.184)	(0.185)	(0.185)	(0.212)	(0.212)	(0.341)	(0.279)	(0.314)	(0.259)	(0.252)	(0.284)	(0.225)	(0.209)	(0.210)	(0.180)	(0.182)	(0.181)	(0.181)	(0.202)	(0.206)
t_3	0.039	0.137	-0.018	-0.057	-0.069	-0.083	0.191	0.276	-0.342	-0.002	0.100	-0.058	0.052	-0.057	-0.129	0.040	0.007	0.054	0.076	0.261	0.317
	(0.200)	(0.205)	(0.207)	(0.208)	(0.211)	(0.211)	(0.365)	(0.320)	(0.349)	(0.284)	(0.279)	(0.316)	(0.255)	(0.271)	(0.272)	(0.200)	(0.202)	(0.201)	(0.201)	(0.227)	(0.229)
t_4	0.075	0.096	-0.095	-0.128	0.019	0.004	0.143	0.359	-0.383	-0.120	0.282	-0.104	0.133	-0.326	-0.457^a	0.076	0.034	0.088	0.108	0.284	0.299
	(0.205)	(0.207)	(0.204)	(0.205)	(0.216)	(0.216)	(0.369)	(0.328)	(0.361)	(0.273)	(0.300)	(0.312)	(0.269)	(0.256)	(0.258)	(0.205)	(0.209)	(0.205)	(0.206)	(0.234)	(0.236)
t_5	0.181	0.142	-0.034	-0.081	0.110	0.100	0.237	-0.061	0.254	0.247	0.113	0.281	-0.086	0.116	0.066	0.183	0.133	0.188	0.210	0.397	0.425^a
	(0.221)	(0.225)	(0.226)	(0.225)	(0.232)	(0.232)	(0.385)	(0.342)	(0.420)	(0.296)	(0.327)	(0.337)	(0.277)	(0.295)	(0.293)	(0.221)	(0.227)	(0.222)	(0.222)	(0.247)	(0.249)
t_6	0.278	0.336	0.121	0.072	0.176	0.163	0.969^b	-0.177	-0.068	0.295	0.267	0.129	0.462	0.385	0.337	0.281	0.228	0.292	0.315	0.503^a	0.531^b
	(0.239)	(0.245)	(0.245)	(0.245)	(0.251)	(0.251)	(0.447)	(0.343)	(0.433)	(0.318)	(0.348)	(0.368)	(0.315)	(0.346)	(0.350)	(0.239)	(0.242)	(0.239)	(0.240)	(0.259)	(0.260)
ATT	-0.123	-0.148	-0.313^c	-0.370^c	-0.176^a	-0.190^a	-0.092	-0.020	-0.275^a	0	-0.230^a	-0.081	-0.244^b	-0.224^a	-0.275^b	-0.127	-0.173^a	-0.110	-0.099	-0.029	0.043
	(0.091)	(0.093)	(0.101)	(0.103)	(0.106)	(0.105)	(0.163)	(0.148)	(0.158)	(0.124)	(0.130)	(0.141)	(0.118)	(0.116)	(0.117)	(0.091)	(0.099)	(0.096)	(0.097)	(0.112)	(0.114)
(% of Mean)	-0.5	-0.6	-1.2	-1.4	-0.7	-0.7	-0.3	-0.1	-1.0	0.0	-0.8	-0.3	-0.9	-0.8	-1.0	-0.5	-0.6	-0.4	-0.4	-0.1	0.2
Mean (SD) of	26.92	26.84	26.84	26.84	26.96	26.96	26.91	26.84	27	26.20	27.56	27.45	26.38	27.02	27.02	26.92	26.92	26.92	26.89	26.71	26.67
t_{-1} Cohort	(4.199)	(4.125)	(4.125)	(4.125)	(4.240)	(4.240)	(4.338)	(3.944)	(4.309)	(4.048)	(4.231)	(4.409)	(3.825)	(4.310)	(4.310)	(4.199)	(4.199)	(4.199)	(4.163)	(4.089)	(3.974)
Wald F (ATT)	–	ref.	13.160^c	21.840^c	ref.	1.934	ref.	0.107	0.649	ref.	1.643	ref.	0.784	ref.	2.285	ref.	1.395	ref.	0.306	ref.	3.833^a
Wald F (t_0–t_6, joint)	–	ref.	2.680^c	3.611^c	ref.	0.270	ref.	0.952	1.079	ref.	0.534	ref.	0.665	ref.	1.388	ref.	0.296	ref.	0.535	ref.	1.727^a
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.103	0.102	0.119	0.122	0.102	0.102	0.107	0.114	0.131	0.042	0.096	0.098	0.114	0.113	0.095	0.103	0.103	0.104	0.102	0.099	0.098

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

Table A.5: Event-Study Estimates for Alcohol Consumption – Excessive Drinking (Weekly Drinks ≥ 14 (Male) or ≥ 7 (Female))

		Mechanisms					Subgroups							Robustness Checks							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.046^{a}	-0.048^{a}	-0.045^{a}	-0.045^{a}	-0.028	-0.030	-0.045	0.013	-0.089^{a}	-0.015	-0.068^{b}	-0.052	-0.038	-0.046	-0.048^{a}	-0.046^{a}	-0.031	-0.063^{b}	-0.064^{b}	-0.065^{a}	-0.064^{a}
	(0.026)	(0.026)	(0.026)	(0.026)	(0.034)	(0.034)	(0.043)	(0.043)	(0.046)	(0.038)	(0.034)	(0.035)	(0.038)	(0.028)	(0.028)	(0.026)	(0.026)	(0.032)	(0.033)	(0.035)	(0.037)
t_{-5}	-0.060^{b}	-0.054^{b}	-0.057^{b}	-0.057^{b}	-0.069^{b}	-0.070^{b}	-0.076^{a}	-0.080^{b}	-0.028	-0.044	-0.073^{b}	-0.064^{a}	-0.061^{a}	-0.061^{b}	-0.062^{b}	-0.061^{b}	-0.050^{a}	-0.060^{b}	-0.054^{b}	-0.052	-0.052
	(0.025)	(0.026)	(0.026)	(0.026)	(0.032)	(0.032)	(0.042)	(0.041)	(0.046)	(0.037)	(0.034)	(0.036)	(0.036)	(0.028)	(0.028)	(0.025)	(0.026)	(0.025)	(0.026)	(0.037)	(0.038)
t_{-4}	-0.014	-0.012	-0.016	-0.016	-0.014	-0.015	-0.005	0.048	-0.053	-0.001	-0.029	0.006	-0.024	-0.016	-0.017	-0.016	-0.011	-0.014	-0.012	-0.022	-0.020
	(0.025)	(0.025)	(0.025)	(0.025)	(0.032)	(0.032)	(0.044)	(0.044)	(0.040)	(0.038)	(0.033)	(0.036)	(0.036)	(0.027)	(0.027)	(0.025)	(0.025)	(0.025)	(0.025)	(0.033)	(0.034)
t_{-3}	-0.041^{a}	-0.040^{a}	-0.041^{a}	-0.041^{a}	0.006	0.005	-0.047	0.019	-0.078^{a}	-0.015	-0.066^{b}	-0.059^{a}	-0.022	-0.040	-0.041	-0.041^{a}	-0.036	-0.041^{a}	-0.036	-0.038	-0.028
	(0.024)	(0.024)	(0.024)	(0.024)	(0.030)	(0.030)	(0.041)	(0.041)	(0.040)	(0.037)	(0.031)	(0.033)	(0.034)	(0.026)	(0.026)	(0.024)	(0.024)	(0.024)	(0.024)	(0.026)	(0.027)
t_{-2}	-0.022	-0.020	-0.020	-0.021	-0.029	-0.029	-0.029	0.027	-0.050	0.026	-0.060^{b}	-0.029	-0.006	-0.019	-0.021	-0.021	-0.019	-0.017	-0.015	-0.006	0.000
	(0.020)	(0.021)	(0.021)	(0.021)	(0.023)	(0.023)	(0.035)	(0.035)	(0.033)	(0.030)	(0.027)	(0.029)	(0.028)	(0.022)	(0.022)	(0.020)	(0.020)	(0.022)	(0.023)	(0.025)	(0.026)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	-0.080^{c}	-0.080^{c}	-0.073^{c}	-0.072^{c}	-0.096^{c}	-0.093^{c}	-0.093^{b}	-0.031	-0.109^{c}	-0.050	-0.106^{c}	-0.062^{b}	-0.083^{c}	-0.091^{c}	-0.094^{c}	-0.080^{c}	-0.082^{c}	-0.081^{c}	-0.069^{c}	-0.071^{c}	-0.061^{b}
	(0.022)	(0.022)	(0.023)	(0.023)	(0.024)	(0.024)	(0.037)	(0.038)	(0.038)	(0.033)	(0.029)	(0.032)	(0.031)	(0.024)	(0.024)	(0.022)	(0.022)	(0.022)	(0.022)	(0.027)	(0.028)
t_{1}	-0.076^{c}	-0.081^{c}	-0.074^{c}	-0.074^{c}	-0.073^{c}	-0.072^{c}	-0.093^{c}	-0.036	-0.096^{c}	-0.057^{b}	-0.095^{c}	-0.086^{c}	-0.065^{b}	-0.071^{c}	-0.068^{c}	-0.076^{c}	-0.080^{c}	-0.077^{c}	-0.069^{c}	-0.077^{c}	-0.065^{c}
	(0.019)	(0.020)	(0.021)	(0.021)	(0.022)	(0.022)	(0.034)	(0.032)	(0.034)	(0.028)	(0.027)	(0.028)	(0.028)	(0.022)	(0.022)	(0.019)	(0.020)	(0.019)	(0.020)	(0.023)	(0.024)
t_{2}	-0.066^{c}	-0.063^{c}	-0.059^{c}	-0.058^{c}	-0.062^{c}	-0.060^{c}	-0.083^{b}	-0.011	-0.104^{c}	-0.008	-0.112^{c}	-0.076^{c}	-0.042	-0.068^{c}	-0.062^{c}	-0.066^{c}	-0.071^{c}	-0.066^{c}	-0.060^{c}	-0.066^{c}	-0.055^{b}
	(0.020)	(0.020)	(0.021)	(0.021)	(0.023)	(0.023)	(0.035)	(0.033)	(0.034)	(0.029)	(0.027)	(0.028)	(0.028)	(0.022)	(0.022)	(0.020)	(0.020)	(0.020)	(0.020)	(0.024)	(0.024)
t_{3}	-0.037^{a}	-0.040^{a}	-0.032	-0.032	-0.037	-0.035	-0.050	-0.015	-0.042	0.011	-0.079^{c}	-0.066^{b}	-0.012	-0.007	-0.005	-0.037^{a}	-0.044^{a}	-0.037^{a}	-0.031	-0.038	-0.024
	(0.022)	(0.023)	(0.024)	(0.024)	(0.024)	(0.024)	(0.039)	(0.037)	(0.040)	(0.034)	(0.030)	(0.033)	(0.031)	(0.029)	(0.029)	(0.022)	(0.023)	(0.022)	(0.023)	(0.026)	(0.027)
t_{4}	-0.075^{c}	-0.068^{c}	-0.061^{c}	-0.061^{c}	-0.066^{c}	-0.063^{c}	-0.069^{a}	-0.014	-0.128^{c}	-0.012	-0.134^{c}	-0.092^{c}	-0.044	-0.055^{b}	-0.055^{a}	-0.075^{c}	-0.084^{c}	-0.075^{c}	-0.070^{c}	-0.075^{c}	-0.061^{b}
	(0.022)	(0.023)	(0.023)	(0.023)	(0.024)	(0.024)	(0.039)	(0.037)	(0.038)	(0.033)	(0.029)	(0.032)	(0.032)	(0.028)	(0.028)	(0.022)	(0.022)	(0.022)	(0.022)	(0.026)	(0.027)
t_{5}	-0.088^{c}	-0.085^{c}	-0.076^{c}	-0.076^{c}	-0.080^{c}	-0.078^{c}	-0.088^{b}	-0.023	-0.139^{c}	-0.043	-0.130^{c}	-0.115^{c}	-0.046	-0.078^{c}	-0.076^{c}	-0.088^{c}	-0.099^{c}	-0.088^{c}	-0.082^{c}	-0.088^{c}	-0.075^{c}
	(0.022)	(0.023)	(0.023)	(0.023)	(0.024)	(0.024)	(0.039)	(0.038)	(0.038)	(0.032)	(0.031)	(0.031)	(0.033)	(0.028)	(0.028)	(0.022)	(0.023)	(0.022)	(0.022)	(0.026)	(0.027)
t_{6}	-0.041^{a}	-0.041^{a}	-0.035	-0.035	-0.040	-0.038	-0.054	-0.035	-0.036	-0.027	-0.060^{a}	-0.060^{a}	-0.023	-0.037	-0.030	-0.041^{a}	-0.052^{b}	-0.040^{a}	-0.035	-0.041	-0.028
	(0.024)	(0.025)	(0.025)	(0.025)	(0.026)	(0.026)	(0.041)	(0.040)	(0.045)	(0.035)	(0.034)	(0.035)	(0.034)	(0.030)	(0.031)	(0.024)	(0.025)	(0.024)	(0.024)	(0.028)	(0.028)
ATT	-0.046^{c}	-0.047^{c}	-0.040^{c}	-0.039^{c}	-0.049^{c}	-0.046^{c}	-0.053^{c}	-0.031^{a}	-0.057^{c}	-0.030^{b}	-0.060^{c}	-0.053^{c}	-0.034^{b}	-0.049^{c}	-0.045^{c}	-0.046^{c}	-0.057^{c}	-0.048^{c}	-0.043^{c}	-0.049^{c}	-0.042^{c}
	(0.010)	(0.010)	(0.012)	(0.012)	(0.012)	(0.012)	(0.018)	(0.017)	(0.017)	(0.015)	(0.014)	(0.015)	(0.015)	(0.012)	(0.012)	(0.010)	(0.011)	(0.011)	(0.011)	(0.013)	(0.014)
(% of Mean)	-14.5	-14.8	-12.6	-12.3	-16.2	-15.2	-15.7	-11.0	-17.3	-10.3	-17.7	-18.1	-10.1	-16.2	-14.9	-14.5	-18.0	-15.1	-13.7	-14.2	-12.5
Mean (SD) of	0.317	0.317	0.317	0.317	0.303	0.303	0.338	0.282	0.330	0.292	0.339	0.293	0.338	0.303	0.303	0.317	0.317	0.317	0.313	0.345	0.336
t_{-1} Cohort	(0.465)	(0.465)	(0.465)	(0.465)	(0.460)	(0.460)	(0.474)	(0.451)	(0.471)	(0.455)	(0.474)	(0.456)	(0.473)	(0.460)	(0.460)	(0.465)	(0.465)	(0.465)	(0.464)	(0.476)	(0.473)
Wald F (ATT)	–	ref.	2.423	2.382	ref.	8.420^{c}	ref.	0.770	0.025	ref.	2.197	ref.	0.851	ref.	1.378	ref.	7.272^{c}	ref.	7.409^{c}	ref.	3.434^{a}
Wald F (t_0–t_6, joint)	–	ref.	0.736	0.757	ref.	0.925	ref.	0.481	0.446	ref.	1.826^{a}	ref.	0.761	ref.	0.705	ref.	1.015	ref.	1.812^{a}	ref.	0.741
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.043	0.042	0.049	0.049	0.041	0.043	0.050	0.051	0.066	0.044	0.058	0.038	0.058	0.045	0.041	0.043	0.046	0.044	0.043	0.047	0.045

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

Table A.6: Event-Study Estimates for Physical Inactivity – Fewer than 3 Days a Week with Physical Activity

		Mechanisms					**Subgroups**							**Robustness Checks**							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.013	-0.015	-0.013	-0.010	-0.018	-0.015	-0.023	-0.009	-0.004	-0.019	-0.007	-0.005	-0.005	-0.010	-0.001	-0.013	-0.020	-0.010	-0.011	-0.013	-0.007
	(0.020)	(0.020)	(0.020)	(0.020)	(0.027)	(0.027)	(0.033)	(0.034)	(0.035)	(0.032)	(0.025)	(0.025)	(0.031)	(0.022)	(0.022)	(0.020)	(0.020)	(0.024)	(0.025)	(0.027)	(0.029)
t_{-5}	0.010	0.001	0.004	0.005	0.021	0.022	0.026	-0.013	0.014	0.003	0.017	0.023	0.003	0.012	0.016	0.010	0.005	0.010	0.004	0.002	-0.006
	(0.019)	(0.019)	(0.019)	(0.019)	(0.023)	(0.023)	(0.030)	(0.034)	(0.033)	(0.031)	(0.024)	(0.023)	(0.030)	(0.021)	(0.021)	(0.019)	(0.019)	(0.019)	(0.020)	(0.028)	(0.030)
t_{-4}	-0.016	-0.022	-0.019	-0.016	-0.043^{a}	-0.043	-0.059^{a}	0.052^{a}	-0.019	0.003	-0.032	-0.000	-0.026	-0.012	-0.009	-0.016	-0.018	-0.017	-0.020	0.002	0.001
	(0.019)	(0.019)	(0.019)	(0.019)	(0.026)	(0.026)	(0.035)	(0.030)	(0.031)	(0.030)	(0.025)	(0.024)	(0.031)	(0.021)	(0.021)	(0.019)	(0.019)	(0.019)	(0.019)	(0.025)	(0.027)
t_{-3}	0.003	-0.002	0.000	0.003	0.010	0.011	-0.042	-0.010	0.044	0.026	-0.011	0.027	-0.006	0.006	0.009	0.003	0.001	0.003	0.001	0.007	0.009
	(0.018)	(0.018)	(0.018)	(0.018)	(0.022)	(0.022)	(0.033)	(0.032)	(0.028)	(0.029)	(0.022)	(0.021)	(0.028)	(0.020)	(0.020)	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)	(0.020)
t_{-2}	-0.002	-0.004	-0.003	-0.000	-0.005	-0.005	0.013	0.001	-0.010	-0.008	0.006	-0.006	0.008	-0.001	0.000	-0.002	-0.003	-0.005	-0.006	-0.005	-0.001
	(0.016)	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)	(0.026)	(0.027)	(0.026)	(0.025)	(0.020)	(0.020)	(0.024)	(0.017)	(0.017)	(0.016)	(0.016)	(0.017)	(0.018)	(0.019)	(0.020)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	-0.009	-0.014	-0.024	-0.034^{a}	-0.016	-0.018	-0.024	-0.026	0.012	-0.033	0.007	0.024	-0.035	-0.008	-0.008	-0.009	-0.009	-0.009	-0.014	-0.005	-0.008
	(0.018)	(0.018)	(0.018)	(0.018)	(0.020)	(0.020)	(0.030)	(0.031)	(0.030)	(0.029)	(0.022)	(0.020)	(0.028)	(0.020)	(0.020)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.023)
t_{1}	-0.014	-0.017	-0.025	-0.034^{b}	-0.021	-0.023	-0.017	-0.030	0.003	-0.010	-0.014	-0.001	-0.024	-0.019	-0.022	-0.014	-0.012	-0.014	-0.018	-0.009	-0.011
	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)	(0.027)	(0.027)	(0.027)	(0.024)	(0.021)	(0.020)	(0.025)	(0.018)	(0.018)	(0.016)	(0.016)	(0.016)	(0.016)	(0.018)	(0.019)
t_{2}	0.019	0.017	0.008	-0.000	0.013	0.011	0.010	0.007	0.038	0.014	0.024	0.026	0.009	0.014	0.008	0.019	0.021	0.019	0.016	0.024	0.024
	(0.015)	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)	(0.027)	(0.026)	(0.026)	(0.024)	(0.020)	(0.019)	(0.024)	(0.017)	(0.017)	(0.015)	(0.015)	(0.015)	(0.015)	(0.018)	(0.019)
t_{3}	0.013	0.010	0.001	-0.007	0.007	0.004	0.040	0.010	-0.006	0.002	0.023	0.005	0.011	0.008	0.002	0.013	0.016	0.013	0.010	0.020	0.018
	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.029)	(0.030)	(0.032)	(0.029)	(0.022)	(0.022)	(0.028)	(0.023)	(0.023)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.021)
t_{4}	0.025	0.019	0.009	0.002	0.026	0.023	0.040	-0.002	0.039	0.022	0.026	-0.009	0.050^{a}	0.015	0.006	0.025	0.029	0.024	0.021	0.031	0.032
	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.030)	(0.031)	(0.030)	(0.027)	(0.023)	(0.023)	(0.027)	(0.023)	(0.023)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.021)
t_{5}	0.045^{c}	0.038^{b}	0.028	0.021	0.043^{b}	0.041^{b}	0.026	0.059^{b}	0.053^{a}	0.054^{b}	0.035	0.029	0.053^{b}	0.022	0.015	0.045^{c}	0.050^{c}	0.044^{c}	0.041^{b}	0.051^{b}	0.052^{b}
	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.031)	(0.029)	(0.030)	(0.026)	(0.023)	(0.021)	(0.027)	(0.023)	(0.023)	(0.017)	(0.018)	(0.017)	(0.017)	(0.020)	(0.021)
t_{6}	0.041^{b}	0.034^{a}	0.025	0.017	0.040^{b}	0.037^{a}	-0.007	0.035	0.106^{c}	0.019	0.058^{c}	0.039^{a}	0.038	0.024	0.012	0.041^{b}	0.046^{b}	0.041^{b}	0.038^{b}	0.048^{b}	0.048^{b}
	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.032)	(0.032)	(0.027)	(0.029)	(0.022)	(0.021)	(0.029)	(0.023)	(0.024)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.021)
ATT	0.013	0.011	0.002	-0.009	0.013	0.010	0.012	-0.003	0.026^{b}	0.006	0.018^{a}	0.011	0.006	0.001	-0.005	0.013	0.014	0.012	0.010	0.013	0.011
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.014)	(0.014)	(0.013)	(0.013)	(0.010)	(0.010)	(0.013)	(0.010)	(0.010)	(0.008)	(0.009)	(0.008)	(0.008)	(0.010)	(0.010)
(% of Mean)	1.5	1.3	0.2	-1.0	1.5	1.2	1.4	-0.4	3.0	0.7	2.1	1.2	0.7	0.1	-0.6	1.5	1.6	1.4	1.2	1.5	1.3
Mean (SD) of	0.857	0.859	0.859	0.859	0.856	0.856	0.862	0.847	0.862	0.836	0.875	0.900	0.814	0.858	0.858	0.857	0.857	0.857	0.858	0.856	0.855
t_{-1} Cohort	(0.350)	(0.349)	(0.349)	(0.349)	(0.351)	(0.351)	(0.346)	(0.360)	(0.345)	(0.370)	(0.331)	(0.300)	(0.389)	(0.349)	(0.349)	(0.350)	(0.350)	(0.350)	(0.349)	(0.351)	(0.353)
Wald F (ATT)	–	ref.	6.926^{c}	24.470^{c}	ref.	16.620^{c}	ref.	0.586	0.532	ref.	0.594	ref.	0.097	ref.	7.676^{c}	ref.	0.081	ref.	0.913	ref.	0.632
Wald F (t_0–t_6, joint)	–	ref.	1.703	3.273^{c}	ref.	1.390	ref.	0.737	1.978^{a}	ref.	0.612	ref.	1.649	ref.	1.242	ref.	0.540	ref.	0.547	ref.	1.360
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.041	0.041	0.048	0.054	0.047	0.049	0.055	0.043	0.051	0.052	0.035	0.030	0.037	0.042	0.037	0.041	0.042	0.040	0.040	0.042	0.043

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

Table A.7: Event-Study Estimates for Weight Management – Overweight (Body-Mass-Index ≥ 25)

		Mechanisms					**Subgroups**							**Robustness Checks**							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	0.009	0.006	0.001	0.001	0.015	0.012	0.034	0.055	-0.042	0.039	-0.019	0.014	0.014	0.008	0.008	0.011	0.010	-0.021	-0.024	-0.002	-0.006
	(0.028)	(0.029)	(0.029)	(0.029)	(0.038)	(0.038)	(0.048)	(0.048)	(0.050)	(0.042)	(0.038)	(0.040)	(0.040)	(0.031)	(0.031)	(0.028)	(0.029)	(0.035)	(0.036)	(0.039)	(0.041)
t_{-5}	0.012	0.003	-0.001	0.000	-0.019	-0.020	0.130^c	-0.063	-0.014	0.011	0.003	0.020	-0.003	0.010	0.010	0.013	0.013	0.008	0.000	0.034	0.026
	(0.027)	(0.028)	(0.028)	(0.028)	(0.036)	(0.036)	(0.046)	(0.046)	(0.047)	(0.042)	(0.037)	(0.041)	(0.038)	(0.030)	(0.030)	(0.027)	(0.028)	(0.028)	(0.028)	(0.039)	(0.041)
t_{-4}	0.041	0.034	0.029	0.031	0.019	0.018	0.047	0.018	0.045	0.045	0.034	0.047	0.020	0.040	0.038	0.042	0.041	0.035	0.036	0.046	0.042
	(0.026)	(0.027)	(0.027)	(0.027)	(0.035)	(0.035)	(0.046)	(0.047)	(0.043)	(0.040)	(0.035)	(0.038)	(0.038)	(0.029)	(0.029)	(0.026)	(0.027)	(0.026)	(0.027)	(0.035)	(0.036)
t_{-3}	-0.036	-0.035	-0.039	-0.039	-0.012	-0.014	-0.016	-0.020	-0.059	0.020	-0.075^b	-0.050	-0.031	-0.040	-0.042	-0.036	-0.035	-0.041	-0.041	-0.035	-0.029
	(0.025)	(0.026)	(0.026)	(0.026)	(0.032)	(0.032)	(0.044)	(0.044)	(0.044)	(0.040)	(0.033)	(0.037)	(0.036)	(0.028)	(0.028)	(0.025)	(0.026)	(0.026)	(0.026)	(0.028)	(0.029)
t_{-2}	0.006	0.006	0.007	0.009	0.003	0.003	0.026	0.040	-0.019	0.022	-0.010	0.000	0.005	0.002	0.002	0.006	0.006	-0.016	-0.017	-0.012	-0.011
	(0.022)	(0.023)	(0.023)	(0.022)	(0.026)	(0.026)	(0.038)	(0.038)	(0.037)	(0.032)	(0.030)	(0.032)	(0.031)	(0.024)	(0.024)	(0.022)	(0.022)	(0.024)	(0.025)	(0.027)	(0.028)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_0	-0.021	-0.023	-0.016	-0.012	-0.049^a	-0.046	-0.052	-0.030	0.016	0.009	-0.047	0.012	-0.050	-0.044	-0.040	-0.021	-0.021	-0.021	-0.029	-0.004	-0.004
	(0.025)	(0.025)	(0.026)	(0.026)	(0.028)	(0.028)	(0.042)	(0.042)	(0.043)	(0.038)	(0.033)	(0.037)	(0.034)	(0.028)	(0.028)	(0.025)	(0.025)	(0.025)	(0.025)	(0.030)	(0.031)
t_1	-0.003	-0.007	-0.000	0.000	-0.012	-0.010	0.021	-0.055	0.037	-0.002	-0.011	0.001	-0.014	-0.006	-0.003	-0.003	-0.003	-0.003	-0.006	0.002	-0.015
	(0.022)	(0.023)	(0.023)	(0.023)	(0.025)	(0.025)	(0.038)	(0.037)	(0.038)	(0.032)	(0.030)	(0.033)	(0.031)	(0.025)	(0.025)	(0.022)	(0.022)	(0.022)	(0.022)	(0.026)	(0.027)
t_2	0.011	0.006	0.014	0.016	-0.019	-0.017	0.051	0.038	-0.044	0.010	0.008	-0.002	0.008	-0.010	-0.005	0.012	0.012	0.011	0.011	0.016	0.010
	(0.022)	(0.023)	(0.023)	(0.023)	(0.025)	(0.025)	(0.039)	(0.037)	(0.039)	(0.033)	(0.030)	(0.033)	(0.031)	(0.025)	(0.025)	(0.022)	(0.022)	(0.022)	(0.022)	(0.026)	(0.027)
t_3	0.016	0.014	0.024	0.026	0.010	0.013	0.074^a	0.013	-0.027	0.036	-0.003	0.041	-0.014	-0.015	-0.011	0.016	0.016	0.019	0.016	0.023	0.009
	(0.025)	(0.026)	(0.027)	(0.027)	(0.027)	(0.027)	(0.044)	(0.042)	(0.045)	(0.038)	(0.034)	(0.038)	(0.035)	(0.033)	(0.033)	(0.025)	(0.026)	(0.025)	(0.025)	(0.029)	(0.030)
t_4	-0.003	-0.007	0.004	0.005	-0.020	-0.017	-0.016	-0.012	0.027	0.004	-0.012	0.025	-0.028	-0.001	0.005	-0.003	-0.003	-0.001	-0.003	0.004	-0.008
	(0.025)	(0.026)	(0.026)	(0.026)	(0.027)	(0.027)	(0.043)	(0.043)	(0.046)	(0.037)	(0.035)	(0.038)	(0.036)	(0.033)	(0.033)	(0.025)	(0.026)	(0.025)	(0.026)	(0.029)	(0.030)
t_5	-0.021	-0.025	-0.014	-0.014	-0.029	-0.026	0.012	-0.028	-0.035	0.005	-0.052	-0.008	-0.029	-0.021	-0.019	-0.021	-0.021	-0.018	-0.020	-0.013	-0.026
	(0.026)	(0.027)	(0.027)	(0.027)	(0.028)	(0.028)	(0.044)	(0.044)	(0.046)	(0.037)	(0.036)	(0.038)	(0.036)	(0.033)	(0.033)	(0.026)	(0.026)	(0.026)	(0.026)	(0.030)	(0.031)
t_6	0.010	0.008	0.019	0.019	-0.005	-0.003	0.049	0.011	-0.014	0.012	0.014	0.033	-0.024	0.001	0.004	0.010	0.011	0.013	0.011	0.018	0.005
	(0.027)	(0.028)	(0.028)	(0.028)	(0.029)	(0.029)	(0.045)	(0.046)	(0.050)	(0.039)	(0.037)	(0.040)	(0.038)	(0.035)	(0.035)	(0.027)	(0.028)	(0.027)	(0.027)	(0.031)	(0.032)
ATT	-0.005	-0.007	0.004	0.004	-0.018	-0.015	-0.008	-0.019	0.009	-0.010	-0.003	0.008	-0.019	-0.016	-0.012	-0.005	-0.005	0.004	0.002	0.013	0.002
	(0.011)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.020)	(0.019)	(0.020)	(0.017)	(0.015)	(0.017)	(0.016)	(0.014)	(0.014)	(0.011)	(0.012)	(0.012)	(0.012)	(0.015)	(0.015)
(% of Mean)	-1.1	-1.5	0.9	0.9	-3.8	-3.2	-1.8	-4.0	1.9	-2.3	-0.6	1.8	-4.0	-3.4	-2.6	-1.1	-1.1	0.9	0.4	2.8	0.4
Mean (SD) of	0.465	0.469	0.469	0.469	0.476	0.476	0.441	0.479	0.473	0.442	0.485	0.454	0.479	0.468	0.468	0.465	0.465	0.465	0.469	0.461	0.475
t_{-1} Cohort	(0.499)	(0.499)	(0.499)	(0.499)	(0.500)	(0.500)	(0.497)	(0.500)	(0.500)	(0.497)	(0.500)	(0.498)	(0.500)	(0.499)	(0.499)	(0.499)	(0.499)	(0.499)	(0.499)	(0.499)	(0.500)
Wald F (ATT)	–	ref.	3.605^a	3.652^a	ref.	6.993^c	ref.	0.151	0.392	ref.	0.084	ref.	1.351	ref.	1.782	ref.	0.015	ref.	0.555	ref.	6.333^b
Wald F (t_0–t_6, joint)	–	ref.	1.416	1.219	ref.	0.802	ref.	0.670	1.921^a	ref.	0.439	ref.	0.614	ref.	0.405	ref.	0.300	ref.	0.691	ref.	1.517
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.026	0.026	0.032	0.034	0.024	0.025	0.041	0.041	0.038	0.028	0.035	0.038	0.031	0.026	0.027	0.026	0.026	0.027	0.027	0.028	0.026

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

Table A.8: Event-Study Estimates for Preventive Medication Use – Any CVD-related Medication

		Mechanisms					**Subgroups**							**Robustness Checks**							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.054^{c}	-0.052^{b}	-0.052^{b}	-0.048^{b}	-0.064^{b}	-0.065^{b}	-0.044	-0.038	-0.073^{b}	0.026	-0.147^{c}	-0.044	-0.073^{c}	-0.053^{b}	-0.056^{c}	-0.054^{c}	-0.053^{b}	-0.025	-0.027	-0.038	-0.052
	(0.020)	(0.021)	(0.020)	(0.020)	(0.029)	(0.029)	(0.033)	(0.035)	(0.036)	(0.022)	(0.021)	(0.030)	(0.028)	(0.021)	(0.021)	(0.020)	(0.021)	(0.026)	(0.026)	(0.030)	(0.031)
t_{-5}	-0.044^{b}	-0.040^{b}	-0.043^{b}	-0.041^{b}	-0.006	-0.006	-0.008	-0.040	-0.077^{b}	0.039^{a}	-0.143^{c}	-0.044	-0.035	-0.042^{b}	-0.041^{b}	-0.043^{b}	-0.042^{b}	-0.043^{b}	-0.042^{b}	-0.084^{c}	-0.090^{c}
	(0.020)	(0.020)	(0.020)	(0.020)	(0.027)	(0.027)	(0.035)	(0.035)	(0.033)	(0.022)	(0.021)	(0.030)	(0.027)	(0.021)	(0.021)	(0.020)	(0.020)	(0.020)	(0.021)	(0.030)	(0.032)
t_{-4}	-0.038^{b}	-0.037^{a}	-0.037^{a}	-0.033^{a}	-0.021	-0.021	-0.004	-0.005	-0.085^{c}	0.021	-0.127^{c}	-0.040	-0.049^{a}	-0.035^{a}	-0.028	-0.038^{a}	-0.037^{a}	-0.037^{a}	-0.034^{a}	-0.048^{a}	-0.059^{b}
	(0.019)	(0.020)	(0.019)	(0.019)	(0.027)	(0.027)	(0.033)	(0.034)	(0.031)	(0.020)	(0.019)	(0.028)	(0.027)	(0.020)	(0.021)	(0.019)	(0.019)	(0.020)	(0.020)	(0.027)	(0.029)
t_{-3}	0.020	0.025	0.023	0.026	0.022	0.022	0.040	0.021	-0.004	0.047^{b}	-0.093^{c}	-0.006	0.044^{a}	0.019	0.028	0.020	0.021	0.023	0.016	0.021	0.008
	(0.018)	(0.018)	(0.018)	(0.018)	(0.024)	(0.024)	(0.030)	(0.033)	(0.028)	(0.021)	(0.014)	(0.026)	(0.025)	(0.019)	(0.020)	(0.018)	(0.018)	(0.018)	(0.019)	(0.021)	(0.022)
t_{-2}	-0.003	0.001	0.001	0.004	0.003	0.003	-0.006	0.011	-0.015	-0.001	-0.034^{c}	0.008	-0.016	-0.000	0.001	-0.002	-0.002	-0.004	-0.012	-0.009	-0.021
	(0.015)	(0.015)	(0.015)	(0.015)	(0.019)	(0.019)	(0.026)	(0.026)	(0.025)	(0.015)	(0.008)	(0.022)	(0.021)	(0.016)	(0.018)	(0.015)	(0.015)	(0.017)	(0.017)	(0.020)	(0.021)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	0.425^{c}	0.431^{c}	0.405^{c}	0.397^{c}	0.440^{c}	0.441^{c}	0.449^{c}	0.486^{c}	0.363^{c}	0.943^{c}	0.006	0.394^{c}	0.452^{c}	0.430^{c}	0.432^{c}	0.425^{c}	0.425^{c}	0.426^{c}	0.431^{c}	0.420^{c}	0.430^{c}
	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.022)	(0.023)	(0.023)	(0.011)	(0.005)	(0.019)	(0.018)	(0.015)	(0.016)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.017)
t_{1}	0.449^{c}	0.453^{c}	0.427^{c}	0.420^{c}	0.459^{c}	0.460^{c}	0.481^{c}	0.502^{c}	0.381^{c}	0.939^{c}	0.011^{b}	0.418^{c}	0.477^{c}	0.454^{c}	0.457^{c}	0.449^{c}	0.448^{c}	0.449^{c}	0.451^{c}	0.443^{c}	0.451^{c}
	(0.012)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.021)	(0.021)	(0.021)	(0.011)	(0.004)	(0.018)	(0.017)	(0.014)	(0.015)	(0.012)	(0.012)	(0.012)	(0.013)	(0.016)	(0.016)
t_{2}	0.443^{c}	0.450^{c}	0.430^{c}	0.423^{c}	0.446^{c}	0.447^{c}	0.488^{c}	0.491^{c}	0.373^{c}	0.941^{c}	0.008	0.410^{c}	0.474^{c}	0.441^{c}	0.458^{c}	0.442^{c}	0.442^{c}	0.443^{c}	0.444^{c}	0.438^{c}	0.445^{c}
	(0.012)	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)	(0.022)	(0.021)	(0.021)	(0.011)	(0.005)	(0.018)	(0.017)	(0.014)	(0.015)	(0.012)	(0.013)	(0.013)	(0.013)	(0.016)	(0.017)
t_{3}	0.433^{c}	0.439^{c}	0.417^{c}	0.412^{c}	0.441^{c}	0.442^{c}	0.448^{c}	0.503^{c}	0.358^{c}	0.935^{c}	0.006	0.394^{c}	0.471^{c}	0.443^{c}	0.438^{c}	0.433^{c}	0.432^{c}	0.433^{c}	0.433^{c}	0.431^{c}	0.437^{c}
	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.023)	(0.023)	(0.023)	(0.013)	(0.005)	(0.020)	(0.019)	(0.017)	(0.019)	(0.013)	(0.014)	(0.013)	(0.014)	(0.017)	(0.017)
t_{4}	0.453^{c}	0.458^{c}	0.439^{c}	0.434^{c}	0.460^{c}	0.461^{c}	0.501^{c}	0.494^{c}	0.380^{c}	0.936^{c}	0.007	0.432^{c}	0.475^{c}	0.459^{c}	0.460^{c}	0.453^{c}	0.452^{c}	0.452^{c}	0.452^{c}	0.448^{c}	0.452^{c}
	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.023)	(0.023)	(0.023)	(0.012)	(0.005)	(0.020)	(0.019)	(0.017)	(0.018)	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)	(0.017)
t_{5}	0.461^{c}	0.465^{c}	0.445^{c}	0.440^{c}	0.462^{c}	0.462^{c}	0.507^{c}	0.497^{c}	0.396^{c}	0.948^{c}	-0.000	0.452^{c}	0.472^{c}	0.473^{c}	0.473^{c}	0.461^{c}	0.460^{c}	0.460^{c}	0.460^{c}	0.456^{c}	0.460^{c}
	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.024)	(0.023)	(0.025)	(0.011)	(0.008)	(0.020)	(0.020)	(0.018)	(0.019)	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)	(0.018)
t_{6}	0.456^{c}	0.465^{c}	0.445^{c}	0.437^{c}	0.461^{c}	0.462^{c}	0.493^{c}	0.521^{c}	0.361^{c}	0.945^{c}	0.008	0.429^{c}	0.484^{c}	0.464^{c}	0.468^{c}	0.456^{c}	0.456^{c}	0.455^{c}	0.456^{c}	0.451^{c}	0.456^{c}
	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.023)	(0.024)	(0.024)	(0.012)	(0.006)	(0.020)	(0.019)	(0.017)	(0.019)	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)	(0.018)
ATT	0.456^{c}	0.459^{c}	0.437^{c}	0.428^{c}	0.456^{c}	0.457^{c}	0.481^{c}	0.500^{c}	0.402^{c}	0.927^{c}	0.072^{c}	0.426^{c}	0.485^{c}	0.454^{c}	0.458^{c}	0.455^{c}	0.450^{c}	0.452^{c}	0.456^{c}	0.446^{c}	0.462^{c}
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.012)	(0.012)	(0.011)	(0.006)	(0.005)	(0.010)	(0.010)	(0.008)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)	(0.009)
(% of Mean)	83.7	85.6	81.5	79.9	85.2	85.4	88.9	101.8	67.0	1972.3	7.3	74.1	93.4	84.2	85.0	83.5	82.6	82.9	84.0	80.4	84.3
Mean (SD) of	0.545	0.536	0.536	0.536	0.535	0.535	0.541	0.491	0.600	0.047	0.991	0.575	0.519	0.539	0.539	0.545	0.545	0.545	0.543	0.555	0.548
t_{-1} Cohort	(0.498)	(0.499)	(0.499)	(0.499)	(0.499)	(0.499)	(0.499)	(0.501)	(0.491)	(0.212)	(0.095)	(0.495)	(0.500)	(0.499)	(0.499)	(0.498)	(0.498)	(0.498)	(0.498)	(0.497)	(0.498)
Wald F (ATT)	–	ref.	56.050^{c}	89.540^{c}	ref.	2.245	ref.	1.340	23.800^{c}	ref.	10000.000^{c}	ref.	18.560^{c}	ref.	0.733	ref.	3.432^{a}	ref.	8.135^{c}	ref.	37.770^{c}
Wald F (t_0–t_6, joint)	–	ref.	9.231^{c}	12.550^{c}	ref.	0.321	ref.	1.514	2.839^{c}	ref.	1028.000^{c}	ref.	1.915^{a}	ref.	1.230	ref.	0.467	ref.	2.133^{b}	ref.	1.691
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.519	0.523	0.529	0.534	0.513	0.513	0.530	0.547	0.514	0.866	0.150	0.497	0.550	0.538	0.520	0.519	0.519	0.508	0.510	0.493	0.500

Source: Own calculations based on data from Statistics Netherlands. *Notes:* *a* indicates $p < 0.10$, *b* indicates $p < 0.05$, and *c* indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group. For medication outcomes, the subgroup split by pre-existing high-risk condition (diabetes, hypertension, or hyperlipidemia) is partly mechanical: high-risk status is identified through prescription medication use, which overlaps with the outcome. The elevated baseline prevalence and attenuated treatment effects in the high-risk subgroup therefore reflect this definitional overlap rather than behavioral heterogeneity, and these columns are reported for completeness.

Table A.9: Event-Study Estimates for Preventive Medication Use – Anti-Thrombotic Medication

		Mechanisms					Subgroups							Robustness Checks							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.037^{a}	-0.036^{a}	-0.033	-0.030	-0.058^{b}	-0.059^{b}	-0.002	-0.012	-0.084^{b}	0.019	-0.092^{c}	-0.043	-0.044	-0.024	-0.026	-0.039^{a}	-0.042^{b}	-0.007	-0.002	-0.022	-0.016
	(0.021)	(0.021)	(0.021)	(0.021)	(0.026)	(0.026)	(0.036)	(0.034)	(0.038)	(0.015)	(0.034)	(0.032)	(0.028)	(0.023)	(0.022)	(0.021)	(0.021)	(0.028)	(0.029)	(0.032)	(0.034)
t_{-5}	-0.060^{c}	-0.056^{c}	-0.059^{c}	-0.058^{c}	-0.002	-0.003	-0.039	-0.010	-0.118^{c}	0.019	-0.135^{c}	-0.060^{a}	-0.047^{a}	-0.046^{b}	-0.044^{b}	-0.060^{c}	-0.062^{c}	-0.059^{c}	-0.055^{c}	-0.121^{c}	-0.114^{c}
	(0.021)	(0.021)	(0.021)	(0.021)	(0.028)	(0.029)	(0.034)	(0.034)	(0.036)	(0.014)	(0.033)	(0.032)	(0.028)	(0.022)	(0.022)	(0.021)	(0.021)	(0.021)	(0.021)	(0.028)	(0.029)
t_{-4}	-0.038^{a}	-0.038^{a}	-0.036^{a}	-0.033^{a}	-0.006	-0.007	-0.041	0.027	-0.081^{b}	0.004	-0.081^{b}	-0.033	-0.056^{b}	-0.022	-0.016	-0.037^{a}	-0.038^{a}	-0.036^{a}	-0.034^{a}	-0.070^{c}	-0.092^{c}
	(0.020)	(0.020)	(0.020)	(0.020)	(0.026)	(0.026)	(0.034)	(0.035)	(0.033)	(0.012)	(0.032)	(0.029)	(0.027)	(0.022)	(0.022)	(0.020)	(0.020)	(0.020)	(0.020)	(0.027)	(0.027)
t_{-3}	0.010	0.012	0.010	0.012	0.028	0.028	0.051	0.001	-0.027	0.025^{a}	-0.039	0.022	-0.004	0.024	0.025	0.010	0.009	0.012	0.016	-0.007	-0.007
	(0.020)	(0.020)	(0.020)	(0.020)	(0.025)	(0.025)	(0.034)	(0.033)	(0.034)	(0.014)	(0.030)	(0.030)	(0.027)	(0.021)	(0.021)	(0.020)	(0.020)	(0.020)	(0.020)	(0.022)	(0.023)
t_{-2}	0.014	0.017	0.015	0.017	0.025	0.025	0.028	0.009	0.002	0.015	0.002	0.020	0.003	0.031^{a}	0.033^{a}	0.014	0.014	0.015	0.012	-0.002	-0.010
	(0.017)	(0.017)	(0.017)	(0.017)	(0.020)	(0.020)	(0.028)	(0.028)	(0.029)	(0.010)	(0.027)	(0.025)	(0.023)	(0.018)	(0.019)	(0.017)	(0.017)	(0.019)	(0.019)	(0.022)	(0.022)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	0.725^{c}	0.729^{c}	0.689^{c}	0.681^{c}	0.749^{c}	0.750^{c}	0.748^{c}	0.782^{c}	0.666^{c}	0.974^{c}	0.526^{c}	0.703^{c}	0.747^{c}	0.747^{c}	0.744^{c}	0.725^{c}	0.725^{c}	0.726^{c}	0.738^{c}	0.703^{c}	0.718^{c}
	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)	(0.015)	(0.022)	(0.022)	(0.023)	(0.008)	(0.021)	(0.020)	(0.018)	(0.015)	(0.015)	(0.013)	(0.013)	(0.013)	(0.013)	(0.017)	(0.017)
t_{1}	0.732^{c}	0.737^{c}	0.697^{c}	0.690^{c}	0.753^{c}	0.754^{c}	0.751^{c}	0.785^{c}	0.668^{c}	0.955^{c}	0.539^{c}	0.711^{c}	0.753^{c}	0.754^{c}	0.757^{c}	0.732^{c}	0.732^{c}	0.732^{c}	0.739^{c}	0.709^{c}	0.721^{c}
	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.022)	(0.020)	(0.023)	(0.009)	(0.020)	(0.019)	(0.017)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.017)
t_{2}	0.727^{c}	0.731^{c}	0.701^{c}	0.695^{c}	0.740^{c}	0.741^{c}	0.760^{c}	0.780^{c}	0.658^{c}	0.969^{c}	0.523^{c}	0.701^{c}	0.756^{c}	0.743^{c}	0.753^{c}	0.727^{c}	0.728^{c}	0.727^{c}	0.732^{c}	0.704^{c}	0.715^{c}
	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)	(0.015)	(0.022)	(0.021)	(0.023)	(0.008)	(0.021)	(0.020)	(0.017)	(0.015)	(0.015)	(0.013)	(0.013)	(0.013)	(0.013)	(0.017)	(0.017)
t_{3}	0.727^{c}	0.732^{c}	0.698^{c}	0.692^{c}	0.745^{c}	0.745^{c}	0.739^{c}	0.777^{c}	0.669^{c}	0.959^{c}	0.533^{c}	0.705^{c}	0.753^{c}	0.749^{c}	0.743^{c}	0.727^{c}	0.727^{c}	0.726^{c}	0.731^{c}	0.704^{c}	0.714^{c}
	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.023)	(0.022)	(0.025)	(0.011)	(0.022)	(0.021)	(0.018)	(0.017)	(0.017)	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)	(0.018)
t_{4}	0.725^{c}	0.727^{c}	0.697^{c}	0.691^{c}	0.739^{c}	0.740^{c}	0.761^{c}	0.764^{c}	0.660^{c}	0.944^{c}	0.531^{c}	0.707^{c}	0.750^{c}	0.748^{c}	0.745^{c}	0.725^{c}	0.726^{c}	0.724^{c}	0.729^{c}	0.701^{c}	0.710^{c}
	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.024)	(0.023)	(0.025)	(0.012)	(0.022)	(0.022)	(0.019)	(0.017)	(0.018)	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)	(0.018)
t_{5}	0.744^{c}	0.746^{c}	0.713^{c}	0.708^{c}	0.756^{c}	0.757^{c}	0.775^{c}	0.786^{c}	0.682^{c}	0.970^{c}	0.537^{c}	0.743^{c}	0.748^{c}	0.769^{c}	0.764^{c}	0.744^{c}	0.745^{c}	0.743^{c}	0.747^{c}	0.720^{c}	0.730^{c}
	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.023)	(0.022)	(0.026)	(0.009)	(0.023)	(0.020)	(0.020)	(0.016)	(0.017)	(0.014)	(0.014)	(0.014)	(0.014)	(0.017)	(0.018)
t_{6}	0.729^{c}	0.732^{c}	0.700^{c}	0.694^{c}	0.740^{c}	0.741^{c}	0.743^{c}	0.791^{c}	0.655^{c}	0.947^{c}	0.535^{c}	0.719^{c}	0.747^{c}	0.755^{c}	0.758^{c}	0.729^{c}	0.731^{c}	0.728^{c}	0.731^{c}	0.705^{c}	0.714^{c}
	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.026)	(0.022)	(0.026)	(0.013)	(0.023)	(0.021)	(0.020)	(0.017)	(0.018)	(0.014)	(0.015)	(0.014)	(0.015)	(0.018)	(0.018)
ATT	0.737^{c}	0.740^{c}	0.705^{c}	0.697^{c}	0.741^{c}	0.741^{c}	0.748^{c}	0.778^{c}	0.696^{c}	0.950^{c}	0.567^{c}	0.715^{c}	0.765^{c}	0.744^{c}	0.745^{c}	0.738^{c}	0.735^{c}	0.736^{c}	0.742^{c}	0.722^{c}	0.737^{c}
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.011)	(0.011)	(0.012)	(0.005)	(0.011)	(0.010)	(0.009)	(0.008)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)	(0.009)
(% of Mean)	293.6	300.8	286.6	283.3	316.7	316.7	310.4	383.3	226.7	6785.7	122.5	262.9	329.7	319.3	319.7	294.0	292.8	293.2	302.9	262.5	277.1
Mean (SD) of	0.251	0.246	0.246	0.246	0.234	0.234	0.241	0.203	0.307	0.014	0.463	0.272	0.232	0.233	0.233	0.251	0.251	0.251	0.245	0.275	0.266
t_{-1} Cohort	(0.434)	(0.431)	(0.431)	(0.431)	(0.424)	(0.424)	(0.428)	(0.403)	(0.462)	(0.119)	(0.499)	(0.445)	(0.422)	(0.423)	(0.423)	(0.434)	(0.434)	(0.434)	(0.430)	(0.447)	(0.442)
Wald F (ATT)	–	ref.	122.700^{c}	163.500^{c}	ref.	1.563	ref.	3.632^{a}	9.854^{c}	ref.	1021.000^{c}	ref.	13.500^{c}	ref.	0.034	ref.	0.792	ref.	13.650^{c}	ref.	23.080^{c}
Wald F (t_0–t_6, joint)	–	ref.	18.730^{c}	22.980^{c}	ref.	0.235	ref.	0.890	1.723^{a}	ref.	60.910^{c}	ref.	1.527	ref.	1.472	ref.	0.374	ref.	3.569^{c}	ref.	1.702
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.641	0.642	0.650	0.653	0.662	0.662	0.649	0.691	0.615	0.901	0.459	0.613	0.680	0.631	0.633	0.641	0.642	0.642	0.650	0.641	0.656

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group. For medication outcomes, the subgroup split by pre-existing high-risk condition (diabetes, hypertension, or hyperlipidemia) is partly mechanical: high-risk status is identified through prescription medication use, which overlaps with the outcome. The elevated baseline prevalence and attenuated treatment effects in the high-risk subgroup therefore reflect this definitional overlap rather than behavioral heterogeneity, and these columns are reported for completeness.

Table A.10: Event-Study Estimates for Preventive Medication Use – Anti-Hypertensive Medication

		Mechanisms					Subgroups							Robustness Checks							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.048^{b}	-0.045^{b}	-0.043^{b}	-0.040^{b}	-0.064^{b}	-0.065^{b}	-0.036	-0.011	-0.083^{b}	0.012	-0.113^{c}	-0.029	-0.077^{c}	-0.049^{b}	-0.047^{b}	-0.047^{b}	-0.039^{a}	-0.033	-0.039	-0.026	-0.042
	(0.020)	(0.020)	(0.020)	(0.020)	(0.028)	(0.028)	(0.032)	(0.034)	(0.036)	(0.020)	(0.029)	(0.030)	(0.027)	(0.021)	(0.020)	(0.020)	(0.020)	(0.026)	(0.026)	(0.029)	(0.030)
t_{-5}	-0.029	-0.029	-0.030	-0.028	-0.013	-0.014	-0.025	-0.018	-0.037	0.037^{a}	-0.096^{c}	-0.055^{a}	-0.004	-0.030	-0.025	-0.029	-0.022	-0.029	-0.030	-0.048^{a}	-0.060^{b}
	(0.019)	(0.019)	(0.019)	(0.019)	(0.026)	(0.026)	(0.032)	(0.033)	(0.032)	(0.020)	(0.026)	(0.028)	(0.026)	(0.020)	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)	(0.028)	(0.030)
t_{-4}	-0.054^{c}	-0.049^{c}	-0.049^{c}	-0.045^{b}	-0.031	-0.031	-0.051	-0.028	-0.067^{b}	0.025	-0.128^{c}	-0.043	-0.058^{b}	-0.054^{c}	-0.040^{b}	-0.053^{c}	-0.049^{c}	-0.053^{c}	-0.054^{c}	-0.069^{c}	-0.087^{c}
	(0.019)	(0.019)	(0.019)	(0.019)	(0.025)	(0.025)	(0.032)	(0.033)	(0.030)	(0.020)	(0.026)	(0.027)	(0.026)	(0.020)	(0.021)	(0.019)	(0.019)	(0.019)	(0.019)	(0.026)	(0.027)
t_{-3}	-0.009	-0.004	-0.007	-0.004	-0.017	-0.018	-0.035	-0.024	0.010	0.002	-0.077^{c}	-0.012	-0.005	-0.010	0.001	-0.009	-0.005	-0.007	-0.013	-0.006	-0.018
	(0.018)	(0.018)	(0.018)	(0.018)	(0.023)	(0.023)	(0.029)	(0.031)	(0.029)	(0.020)	(0.023)	(0.026)	(0.024)	(0.018)	(0.020)	(0.018)	(0.018)	(0.018)	(0.018)	(0.020)	(0.021)
t_{-2}	-0.022	-0.017	-0.018	-0.015	-0.012	-0.012	-0.025	-0.024	-0.021	-0.009	-0.048^{b}	-0.011	-0.034^{a}	-0.021	-0.019	-0.022	-0.021	-0.022	-0.029^{a}	-0.022	-0.030
	(0.014)	(0.015)	(0.015)	(0.015)	(0.018)	(0.018)	(0.023)	(0.026)	(0.024)	(0.014)	(0.020)	(0.021)	(0.020)	(0.015)	(0.017)	(0.014)	(0.014)	(0.016)	(0.016)	(0.019)	(0.020)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	0.360^{c}	0.366^{c}	0.322^{c}	0.313^{c}	0.367^{c}	0.368^{c}	0.373^{c}	0.432^{c}	0.301^{c}	0.688^{c}	0.100^{c}	0.370^{c}	0.353^{c}	0.358^{c}	0.361^{c}	0.360^{c}	0.359^{c}	0.360^{c}	0.363^{c}	0.354^{c}	0.362^{c}
	(0.017)	(0.017)	(0.018)	(0.018)	(0.019)	(0.019)	(0.029)	(0.028)	(0.029)	(0.025)	(0.018)	(0.024)	(0.024)	(0.019)	(0.020)	(0.017)	(0.017)	(0.017)	(0.017)	(0.020)	(0.021)
t_{1}	0.401^{c}	0.406^{c}	0.367^{c}	0.359^{c}	0.422^{c}	0.422^{c}	0.401^{c}	0.446^{c}	0.372^{c}	0.726^{c}	0.125^{c}	0.383^{c}	0.420^{c}	0.418^{c}	0.415^{c}	0.401^{c}	0.398^{c}	0.400^{c}	0.404^{c}	0.398^{c}	0.406^{c}
	(0.015)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.025)	(0.025)	(0.026)	(0.020)	(0.017)	(0.022)	(0.021)	(0.016)	(0.018)	(0.015)	(0.015)	(0.015)	(0.015)	(0.018)	(0.018)
t_{2}	0.382^{c}	0.387^{c}	0.357^{c}	0.350^{c}	0.377^{c}	0.377^{c}	0.429^{c}	0.414^{c}	0.332^{c}	0.729^{c}	0.090^{c}	0.387^{c}	0.388^{c}	0.380^{c}	0.394^{c}	0.382^{c}	0.378^{c}	0.381^{c}	0.382^{c}	0.379^{c}	0.388^{c}
	(0.015)	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)	(0.027)	(0.026)	(0.027)	(0.021)	(0.018)	(0.023)	(0.021)	(0.017)	(0.019)	(0.015)	(0.016)	(0.015)	(0.016)	(0.018)	(0.019)
t_{3}	0.380^{c}	0.383^{c}	0.350^{c}	0.344^{c}	0.384^{c}	0.385^{c}	0.369^{c}	0.463^{c}	0.315^{c}	0.715^{c}	0.104^{c}	0.358^{c}	0.404^{c}	0.389^{c}	0.379^{c}	0.380^{c}	0.374^{c}	0.379^{c}	0.378^{c}	0.378^{c}	0.386^{c}
	(0.017)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.029)	(0.029)	(0.032)	(0.027)	(0.019)	(0.026)	(0.024)	(0.023)	(0.024)	(0.017)	(0.018)	(0.017)	(0.018)	(0.020)	(0.021)
t_{4}	0.399^{c}	0.406^{c}	0.375^{c}	0.370^{c}	0.406^{c}	0.407^{c}	0.430^{c}	0.425^{c}	0.364^{c}	0.703^{c}	0.126^{c}	0.392^{c}	0.406^{c}	0.405^{c}	0.401^{c}	0.399^{c}	0.392^{c}	0.397^{c}	0.397^{c}	0.395^{c}	0.401^{c}
	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.030)	(0.030)	(0.030)	(0.026)	(0.019)	(0.026)	(0.025)	(0.023)	(0.025)	(0.017)	(0.018)	(0.017)	(0.017)	(0.020)	(0.021)
t_{5}	0.396^{c}	0.404^{c}	0.372^{c}	0.366^{c}	0.395^{c}	0.396^{c}	0.430^{c}	0.442^{c}	0.342^{c}	0.706^{c}	0.111^{c}	0.410^{c}	0.398^{c}	0.414^{c}	0.407^{c}	0.396^{c}	0.388^{c}	0.394^{c}	0.394^{c}	0.391^{c}	0.396^{c}
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)	(0.031)	(0.031)	(0.034)	(0.026)	(0.022)	(0.027)	(0.027)	(0.024)	(0.025)	(0.019)	(0.019)	(0.019)	(0.019)	(0.021)	(0.022)
t_{6}	0.412^{c}	0.423^{c}	0.388^{c}	0.381^{c}	0.410^{c}	0.410^{c}	0.451^{c}	0.476^{c}	0.318^{c}	0.743^{c}	0.112^{c}	0.413^{c}	0.416^{c}	0.434^{c}	0.434^{c}	0.412^{c}	0.404^{c}	0.409^{c}	0.409^{c}	0.407^{c}	0.413^{c}
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)	(0.030)	(0.031)	(0.035)	(0.025)	(0.021)	(0.027)	(0.027)	(0.023)	(0.025)	(0.019)	(0.019)	(0.019)	(0.019)	(0.022)	(0.022)
ATT	0.409^{c}	0.412^{c}	0.377^{c}	0.368^{c}	0.409^{c}	0.410^{c}	0.431^{c}	0.453^{c}	0.361^{c}	0.712^{c}	0.167^{c}	0.403^{c}	0.420^{c}	0.411^{c}	0.410^{c}	0.408^{c}	0.398^{c}	0.404^{c}	0.409^{c}	0.401^{c}	0.417^{c}
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.014)	(0.014)	(0.014)	(0.011)	(0.010)	(0.012)	(0.011)	(0.010)	(0.010)	(0.008)	(0.009)	(0.008)	(0.009)	(0.010)	(0.011)
(% of Mean)	90.3	92.8	84.9	82.9	92.7	93.0	96.4	110.8	72.1	1924.3	20.2	86.7	95.0	92.4	92.1	90.1	87.9	89.2	90.5	86.8	92.1
Mean (SD) of	0.453	0.444	0.444	0.444	0.441	0.441	0.447	0.409	0.501	0.037	0.826	0.465	0.442	0.445	0.445	0.453	0.453	0.453	0.452	0.462	0.453
t_{-1} Cohort	(0.498)	(0.497)	(0.497)	(0.497)	(0.497)	(0.497)	(0.498)	(0.492)	(0.501)	(0.188)	(0.379)	(0.499)	(0.497)	(0.497)	(0.497)	(0.498)	(0.498)	(0.498)	(0.498)	(0.499)	(0.498)
Wald F (ATT)	–	ref.	70.640^{c}	99.860^{c}	ref.	1.361	ref.	1.336	13.090^{c}	ref.	1384.000^{c}	ref.	1.182	ref.	0.081	ref.	8.304^{c}	ref.	8.286^{c}	ref.	28.490^{c}
Wald F (t_0–t_6, joint)	–	ref.	11.220^{c}	14.250^{c}	ref.	0.200	ref.	1.477	1.850^{a}	ref.	134.600^{c}	ref.	0.736	ref.	0.964	ref.	1.119	ref.	0.811	ref.	0.694
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.455	0.460	0.469	0.472	0.430	0.430	0.465	0.466	0.465	0.576	0.244	0.455	0.472	0.487	0.461	0.455	0.456	0.441	0.441	0.415	0.418

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group. For medication outcomes, the subgroup split by pre-existing high-risk condition (diabetes, hypertension, or hyperlipidemia) is partly mechanical: high-risk status is identified through prescription medication use, which overlaps with the outcome. The elevated baseline prevalence and attenuated treatment effects in the high-risk subgroup therefore reflect this definitional overlap rather than behavioral heterogeneity, and these columns are reported for completeness.

Table A.11: Event-Study Estimates for Preventive Medication Use – Statins

	Mechanisms					Subgroups							Robustness Checks								
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.029	-0.031^a	-0.030^a	-0.028	-0.020	-0.021	-0.048	-0.022	-0.016	0.031^b	-0.092^c	-0.032	-0.034	-0.029	-0.048^b	-0.030	-0.023	-0.022	-0.018	-0.034	-0.029
	(0.018)	(0.018)	(0.018)	(0.018)	(0.024)	(0.024)	(0.030)	(0.032)	(0.032)	(0.013)	(0.028)	(0.027)	(0.024)	(0.019)	(0.019)	(0.018)	(0.019)	(0.025)	(0.026)	(0.028)	(0.030)
t_{-5}	-0.039^b	-0.032^a	-0.035^a	-0.034^a	-0.038	-0.038	-0.010	-0.042	-0.054	0.021^a	-0.100^c	-0.028	-0.040^a	-0.038^b	-0.042^b	-0.038^b	-0.032^a	-0.037^b	-0.040^b	-0.030	-0.036
	(0.018)	(0.018)	(0.018)	(0.018)	(0.025)	(0.025)	(0.030)	(0.028)	(0.033)	(0.011)	(0.027)	(0.028)	(0.023)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)	(0.026)	(0.027)
t_{-4}	-0.046^c	-0.042^b	-0.043^c	-0.040^b	-0.042^a	-0.043^a	-0.033	-0.007	-0.083^c	0.023^a	-0.118^c	-0.036	-0.063^c	-0.044^b	-0.046^b	-0.046^c	-0.042^b	-0.046^c	-0.045^b	-0.048^b	-0.057^b
	(0.017)	(0.017)	(0.017)	(0.017)	(0.023)	(0.023)	(0.029)	(0.031)	(0.027)	(0.012)	(0.025)	(0.025)	(0.023)	(0.018)	(0.019)	(0.017)	(0.017)	(0.017)	(0.017)	(0.024)	(0.025)
t_{-3}	-0.003	-0.002	-0.003	-0.001	0.009	0.008	0.007	-0.024	0.000	0.017^a	-0.068^c	-0.013	0.005	-0.004	0.005	-0.003	-0.001	-0.000	-0.006	0.009	-0.004
	(0.017)	(0.017)	(0.017)	(0.017)	(0.022)	(0.022)	(0.029)	(0.027)	(0.028)	(0.011)	(0.023)	(0.025)	(0.023)	(0.017)	(0.019)	(0.017)	(0.017)	(0.017)	(0.017)	(0.019)	(0.020)
t_{-2}	-0.018	-0.015	-0.016	-0.014	-0.018	-0.018	0.008	-0.004	-0.043^b	0.009^a	-0.056^c	-0.016	-0.020	-0.016	-0.018	-0.018	-0.017	-0.014	-0.011	-0.008	-0.008
	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)	(0.022)	(0.023)	(0.021)	(0.005)	(0.019)	(0.019)	(0.018)	(0.013)	(0.016)	(0.013)	(0.013)	(0.015)	(0.015)	(0.017)	(0.018)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_0	0.618^c	0.625^c	0.590^c	0.582^c	0.627^c	0.629^c	0.601^c	0.673^c	0.596^c	0.951^c	0.352^c	0.579^c	0.653^c	0.617^c	0.613^c	0.618^c	0.617^c	0.619^c	0.626^c	0.620^c	0.628^c
	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.023)	(0.023)	(0.023)	(0.013)	(0.018)	(0.020)	(0.018)	(0.015)	(0.018)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.017)
t_1	0.600^c	0.609^c	0.578^c	0.572^c	0.598^c	0.599^c	0.594^c	0.648^c	0.569^c	0.908^c	0.333^c	0.589^c	0.609^c	0.595^c	0.605^c	0.600^c	0.597^c	0.600^c	0.605^c	0.605^c	0.611^c
	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)	(0.015)	(0.021)	(0.022)	(0.023)	(0.013)	(0.018)	(0.019)	(0.018)	(0.015)	(0.017)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)
t_2	0.576^c	0.579^c	0.555^c	0.549^c	0.565^c	0.566^c	0.563^c	0.635^c	0.542^c	0.881^c	0.313^c	0.528^c	0.618^c	0.572^c	0.595^c	0.575^c	0.572^c	0.575^c	0.578^c	0.581^c	0.587^c
	(0.013)	(0.014)	(0.014)	(0.014)	(0.016)	(0.016)	(0.024)	(0.022)	(0.024)	(0.015)	(0.018)	(0.020)	(0.018)	(0.015)	(0.018)	(0.013)	(0.014)	(0.014)	(0.014)	(0.016)	(0.017)
t_3	0.581^c	0.587^c	0.563^c	0.557^c	0.580^c	0.582^c	0.554^c	0.649^c	0.549^c	0.914^c	0.304^c	0.545^c	0.617^c	0.590^c	0.588^c	0.581^c	0.576^c	0.581^c	0.582^c	0.589^c	0.596^c
	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.025)	(0.025)	(0.028)	(0.017)	(0.021)	(0.023)	(0.021)	(0.020)	(0.022)	(0.015)	(0.016)	(0.016)	(0.016)	(0.018)	(0.019)
t_4	0.557^c	0.561^c	0.539^c	0.534^c	0.556^c	0.558^c	0.565^c	0.592^c	0.526^c	0.859^c	0.289^c	0.536^c	0.582^c	0.566^c	0.570^c	0.557^c	0.551^c	0.556^c	0.557^c	0.561^c	0.566^c
	(0.017)	(0.017)	(0.017)	(0.017)	(0.018)	(0.018)	(0.028)	(0.028)	(0.030)	(0.020)	(0.022)	(0.025)	(0.023)	(0.022)	(0.024)	(0.017)	(0.017)	(0.017)	(0.017)	(0.019)	(0.020)
t_5	0.558^c	0.570^c	0.547^c	0.542^c	0.553^c	0.554^c	0.525^c	0.596^c	0.562^c	0.867^c	0.273^c	0.546^c	0.568^c	0.562^c	0.567^c	0.558^c	0.551^c	0.557^c	0.558^c	0.564^c	0.567^c
	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)	(0.031)	(0.028)	(0.030)	(0.019)	(0.024)	(0.025)	(0.024)	(0.022)	(0.024)	(0.017)	(0.018)	(0.017)	(0.017)	(0.020)	(0.020)
t_6	0.591^c	0.599^c	0.573^c	0.566^c	0.588^c	0.589^c	0.584^c	0.637^c	0.557^c	0.904^c	0.310^c	0.551^c	0.624^c	0.579^c	0.577^c	0.591^c	0.584^c	0.590^c	0.590^c	0.595^c	0.598^c
	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)	(0.018)	(0.027)	(0.029)	(0.033)	(0.018)	(0.024)	(0.026)	(0.023)	(0.023)	(0.025)	(0.017)	(0.018)	(0.017)	(0.017)	(0.019)	(0.020)
ATT	0.604^c	0.609^c	0.581^c	0.573^c	0.595^c	0.597^c	0.580^c	0.649^c	0.589^c	0.887^c	0.375^c	0.573^c	0.632^c	0.602^c	0.612^c	0.604^c	0.598^c	0.601^c	0.605^c	0.601^c	0.611^c
	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)	(0.009)	(0.012)	(0.012)	(0.013)	(0.007)	(0.011)	(0.011)	(0.010)	(0.009)	(0.009)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.010)
(% of Mean)	186.4	193.3	184.4	181.9	185.9	186.6	162.9	232.6	175.8		61.1	157.9	218.7	186.4	189.5	186.4	184.6	185.5	187.9	183.2	188.6
Mean (SD) of t_{-1} Cohort	0.324	0.315	0.315	0.315	0.320	0.320	0.356	0.279	0.335	0	0.614	0.363	0.289	0.323	0.323	0.324	0.324	0.324	0.322	0.328	0.324
	(0.468)	(0.465)	(0.465)	(0.465)	(0.467)	(0.467)	(0.479)	(0.449)	(0.473)	(0)	(0.487)	(0.481)	(0.454)	(0.468)	(0.468)	(0.468)	(0.468)	(0.468)	(0.467)	(0.470)	(0.469)
Wald F (ATT)	–	ref.	50.280^c	75.930^c	ref.	3.642^a	ref.	16.180^c	0.245	ref.	1592.000^c	ref.	15.670^c	ref.	3.919^b	ref.	3.090^a	ref.	5.121^b	ref.	9.821^c
Wald F (t_0–t_6, joint)	–	ref.	8.624^c	11.570^c	ref.	0.463	ref.	1.502	0.496	ref.	205.500^c	ref.	2.504^b	ref.	1.668	ref.	0.775	ref.	1.663	ref.	0.875
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.560	0.566	0.574	0.576	0.535	0.535	0.555	0.588	0.562	0.782	0.433	0.540	0.590	0.585	0.576	0.560	0.561	0.546	0.547	0.519	0.520

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group. For medication outcomes, the subgroup split by pre-existing high-risk condition (diabetes, hypertension, or hyperlipidemia) is partly mechanical: high-risk status is identified through prescription medication use, which overlaps with the outcome. The elevated baseline prevalence and attenuated treatment effects in the high-risk subgroup therefore reflect this definitional overlap rather than behavioral heterogeneity, and these columns are reported for completeness.

Table A.12: Event-Study Estimates for No Functional Limitation being Reported

		Subgroups							Robustness Checks							
		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Time to Health Event																
t_{-6}	0.030	-0.000	0.112^{c}	-0.012	0.063^{b}	0.002	0.018	0.047^{a}	0.046^{a}	0.041^{a}	0.028	0.044^{a}	0.016	0.005	-0.001	-0.029
	(0.022)	(0.036)	(0.036)	(0.042)	(0.030)	(0.032)	(0.036)	(0.027)	(0.024)	(0.024)	(0.022)	(0.023)	(0.030)	(0.030)	(0.033)	(0.034)
t_{-5}	0.045^{b}	0.009	0.065^{a}	0.043	0.068^{b}	0.024	0.024	0.051^{b}	0.062^{c}	0.055^{b}	0.045^{b}	0.052^{b}	0.044^{b}	0.033	0.009	-0.009
	(0.021)	(0.034)	(0.035)	(0.036)	(0.026)	(0.030)	(0.034)	(0.024)	(0.023)	(0.023)	(0.021)	(0.021)	(0.021)	(0.021)	(0.029)	(0.030)
t_{-4}	0.037^{a}	0.044	0.088^{b}	-0.004	0.066^{b}	0.009	0.046	0.037	0.054^{b}	0.044^{a}	0.037^{a}	0.041^{b}	0.037^{a}	0.027	0.004	-0.030
	(0.021)	(0.034)	(0.035)	(0.035)	(0.028)	(0.030)	(0.033)	(0.026)	(0.023)	(0.023)	(0.021)	(0.021)	(0.021)	(0.021)	(0.028)	(0.030)
t_{-3}	0.030	0.015	0.060^{a}	0.021	0.037	0.023	0.050	0.017	0.046^{b}	0.029	0.030	0.034^{a}	0.030	0.021	0.033	0.009
	(0.020)	(0.033)	(0.036)	(0.035)	(0.027)	(0.028)	(0.032)	(0.025)	(0.023)	(0.023)	(0.020)	(0.020)	(0.020)	(0.020)	(0.022)	(0.022)
t_{-2}	0.016	0.038	0.036	-0.015	0.036	-0.001	0.033	0.011	0.030	0.023	0.016	0.018	0.014	0.011	0.017	0.008
	(0.017)	(0.028)	(0.030)	(0.029)	(0.023)	(0.025)	(0.027)	(0.021)	(0.019)	(0.019)	(0.017)	(0.017)	(0.019)	(0.019)	(0.021)	(0.022)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	-0.037^{a}	-0.022	-0.040	-0.045	-0.015	-0.059^{b}	-0.063^{a}	-0.004	-0.036	-0.035	-0.037^{a}	-0.038^{a}	-0.037^{a}	-0.038^{a}	-0.025	-0.035
	(0.020)	(0.032)	(0.034)	(0.035)	(0.027)	(0.028)	(0.033)	(0.024)	(0.023)	(0.022)	(0.020)	(0.020)	(0.020)	(0.020)	(0.024)	(0.024)
t_{1}	-0.053^{c}	-0.074^{b}	-0.003	-0.079^{b}	-0.020	-0.084^{c}	-0.054^{a}	-0.035	-0.042^{b}	-0.045^{b}	-0.053^{c}	-0.055^{c}	-0.052^{c}	-0.054^{c}	-0.049^{b}	-0.067^{c}
	(0.018)	(0.030)	(0.028)	(0.032)	(0.023)	(0.025)	(0.028)	(0.022)	(0.020)	(0.020)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.021)
t_{2}	-0.060^{c}	-0.032	-0.029	-0.108^{c}	-0.023	-0.090^{c}	-0.079^{c}	-0.021	-0.049^{b}	-0.046^{b}	-0.059^{c}	-0.063^{c}	-0.059^{c}	-0.063^{c}	-0.056^{c}	-0.064^{c}
	(0.018)	(0.030)	(0.029)	(0.032)	(0.024)	(0.026)	(0.029)	(0.022)	(0.020)	(0.020)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.021)
t_{3}	-0.077^{c}	-0.045	-0.050	-0.131^{c}	-0.036	-0.111^{c}	-0.073^{b}	-0.066^{c}	-0.071^{c}	-0.067^{b}	-0.077^{c}	-0.083^{c}	-0.076^{c}	-0.083^{c}	-0.071^{c}	-0.089^{c}
	(0.021)	(0.033)	(0.034)	(0.038)	(0.028)	(0.029)	(0.033)	(0.026)	(0.027)	(0.027)	(0.021)	(0.021)	(0.021)	(0.021)	(0.024)	(0.024)
t_{4}	-0.095^{c}	-0.160^{c}	-0.052	-0.071^{a}	-0.071^{b}	-0.123^{c}	-0.112^{c}	-0.062^{b}	-0.070^{c}	-0.062^{b}	-0.095^{c}	-0.102^{c}	-0.094^{c}	-0.100^{c}	-0.088^{c}	-0.106^{c}
	(0.021)	(0.035)	(0.034)	(0.036)	(0.028)	(0.030)	(0.033)	(0.026)	(0.026)	(0.027)	(0.021)	(0.021)	(0.021)	(0.021)	(0.024)	(0.024)
t_{5}	-0.077^{c}	-0.077^{b}	-0.070^{a}	-0.086^{b}	-0.057^{b}	-0.095^{c}	-0.104^{c}	-0.035	-0.097^{c}	-0.084^{c}	-0.077^{c}	-0.086^{c}	-0.075^{c}	-0.082^{c}	-0.070^{c}	-0.092^{c}
	(0.021)	(0.035)	(0.035)	(0.039)	(0.029)	(0.030)	(0.034)	(0.027)	(0.028)	(0.028)	(0.021)	(0.022)	(0.021)	(0.021)	(0.024)	(0.025)
t_{6}	-0.081^{c}	-0.066^{b}	-0.051	-0.134^{c}	-0.047	-0.111^{c}	-0.043	-0.083^{c}	-0.076^{c}	-0.062^{b}	-0.081^{c}	-0.092^{c}	-0.079^{c}	-0.085^{c}	-0.075^{c}	-0.097^{c}
	(0.022)	(0.034)	(0.037)	(0.043)	(0.030)	(0.032)	(0.034)	(0.028)	(0.029)	(0.029)	(0.022)	(0.023)	(0.022)	(0.022)	(0.025)	(0.025)
ATT	-0.082^{c}	-0.080^{c}	-0.078^{c}	-0.089^{c}	-0.066^{c}	-0.097^{c}	-0.098^{c}	-0.055^{c}	-0.078^{c}	-0.070^{c}	-0.082^{c}	-0.086^{c}	-0.080^{c}	-0.079^{c}	-0.068^{c}	-0.069^{c}
	(0.009)	(0.015)	(0.016)	(0.016)	(0.012)	(0.013)	(0.015)	(0.012)	(0.011)	(0.011)	(0.009)	(0.010)	(0.010)	(0.010)	(0.012)	(0.012)
(% of Mean)	-10.5	-10.1	-10.1	-11.4	-8.0	-13.0	-13.7	-6.5	-10.0	-9.0	-10.5	-11.0	-10.2	-10.0	-8.8	-8.6
Mean (SD) of	0.783	0.794	0.774	0.780	0.822	0.747	0.713	0.846	0.778	0.778	0.783	0.783	0.783	0.792	0.771	0.798
t_{-1} Cohort	(0.413)	(0.405)	(0.419)	(0.415)	(0.383)	(0.435)	(0.453)	(0.361)	(0.416)	(0.416)	(0.413)	(0.413)	(0.413)	(0.406)	(0.420)	(0.402)
Wald F (ATT)	–	ref.	0.011	0.170	ref.	2.941^{a}	ref.	5.179^{b}	ref.	4.678^{b}	ref.	0.810	ref.	0.289	ref.	0.072
Wald F (t_0–t_6, joint)	–	ref.	1.570	2.148^{b}	ref.	0.833	ref.	1.379	ref.	0.802	ref.	1.447	ref.	1.226	ref.	3.414^{c}
Observations	9,665	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.143	0.164	0.153	0.157	0.112	0.153	0.131	0.139	0.136	0.142	0.143	0.148	0.143	0.143	0.148	0.144

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Subgroup analyses (columns 2–8) partition the baseline sample (sample differences reflect missing data). Robustness checks (columns 9–16) re-estimate the baseline within each subsample for comparability. Compare columns within each results group.

Table A.13: Event-Study Estimates for Self-Perceived Health

		Subgroups							**Robustness Checks**							
		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Time to Health Event																
t_{-6}	0.056^{a}	0.008	0.033	0.120^{b}	0.032	0.085^{a}	0.046	0.068	0.066^{a}	0.059	0.055^{a}	0.066^{b}	0.029	0.016	0.028	0.036
	(0.033)	(0.053)	(0.054)	(0.061)	(0.047)	(0.045)	(0.047)	(0.046)	(0.036)	(0.036)	(0.033)	(0.033)	(0.040)	(0.040)	(0.043)	(0.044)
t_{-5}	0.012	-0.092^{a}	0.034	0.080	0.042	-0.007	0.005	0.048	0.027	0.015	0.013	0.019	0.013	0.014	-0.008	-0.009
	(0.031)	(0.053)	(0.051)	(0.054)	(0.045)	(0.042)	(0.046)	(0.042)	(0.034)	(0.034)	(0.031)	(0.031)	(0.031)	(0.031)	(0.045)	(0.046)
t_{-4}	0.050^{a}	-0.013	0.013	0.131^{c}	0.090^{b}	0.032	0.004	0.099^{b}	0.059^{a}	0.045	0.051^{a}	0.052^{a}	0.053^{a}	0.052^{a}	0.039	0.047
	(0.030)	(0.052)	(0.056)	(0.049)	(0.045)	(0.040)	(0.044)	(0.043)	(0.033)	(0.034)	(0.030)	(0.030)	(0.030)	(0.030)	(0.040)	(0.042)
t_{-3}	0.048	-0.010	0.045	0.107^{b}	0.083^{a}	0.043	-0.017	0.091^{b}	0.063^{a}	0.044	0.048	0.052^{a}	0.050^{a}	0.044	0.029	0.018
	(0.029)	(0.050)	(0.047)	(0.052)	(0.044)	(0.039)	(0.044)	(0.040)	(0.032)	(0.033)	(0.029)	(0.030)	(0.029)	(0.030)	(0.032)	(0.032)
t_{-2}	0.030	-0.026	0.014	0.091^{b}	0.051	0.011	-0.004	0.052	0.045	0.037	0.029	0.029	0.039	0.044	0.017	0.019
	(0.026)	(0.044)	(0.043)	(0.043)	(0.037)	(0.035)	(0.038)	(0.035)	(0.028)	(0.028)	(0.026)	(0.026)	(0.028)	(0.028)	(0.031)	(0.032)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	-0.234^{c}	-0.275^{c}	-0.301^{c}	-0.143^{c}	-0.255^{c}	-0.213^{c}	-0.279^{c}	-0.209^{c}	-0.214^{c}	-0.230^{c}	-0.234^{c}	-0.236^{c}	-0.236^{c}	-0.239^{c}	-0.255^{c}	-0.256^{c}
	(0.031)	(0.051)	(0.051)	(0.055)	(0.044)	(0.042)	(0.046)	(0.042)	(0.035)	(0.035)	(0.031)	(0.031)	(0.031)	(0.031)	(0.036)	(0.037)
t_{1}	-0.153^{c}	-0.272^{c}	-0.195^{c}	-0.021	-0.218^{c}	-0.099^{c}	-0.127^{c}	-0.188^{c}	-0.119^{c}	-0.125^{c}	-0.153^{c}	-0.156^{c}	-0.154^{c}	-0.161^{c}	-0.186^{c}	-0.192^{c}
	(0.026)	(0.045)	(0.042)	(0.045)	(0.036)	(0.036)	(0.037)	(0.037)	(0.029)	(0.029)	(0.026)	(0.026)	(0.026)	(0.026)	(0.030)	(0.030)
t_{2}	-0.152^{c}	-0.201^{c}	-0.162^{c}	-0.105^{b}	-0.222^{c}	-0.093^{b}	-0.152^{c}	-0.165^{c}	-0.139^{c}	-0.141^{c}	-0.151^{c}	-0.155^{c}	-0.153^{c}	-0.163^{c}	-0.185^{c}	-0.197^{c}
	(0.026)	(0.046)	(0.041)	(0.047)	(0.036)	(0.037)	(0.039)	(0.036)	(0.029)	(0.029)	(0.026)	(0.026)	(0.026)	(0.026)	(0.030)	(0.030)
t_{3}	-0.131^{c}	-0.168^{c}	-0.201^{c}	-0.063	-0.225^{c}	-0.057	-0.123^{c}	-0.162^{c}	-0.123^{c}	-0.117^{c}	-0.132^{c}	-0.135^{c}	-0.132^{c}	-0.142^{c}	-0.170^{c}	-0.180^{c}
	(0.029)	(0.048)	(0.050)	(0.052)	(0.044)	(0.039)	(0.045)	(0.038)	(0.039)	(0.039)	(0.029)	(0.030)	(0.029)	(0.029)	(0.033)	(0.034)
t_{4}	-0.110^{c}	-0.176^{c}	-0.121^{b}	-0.069	-0.154^{c}	-0.063	-0.140^{c}	-0.098^{b}	-0.083^{b}	-0.070^{a}	-0.111^{c}	-0.115^{c}	-0.112^{c}	-0.123^{c}	-0.149^{c}	-0.163^{c}
	(0.030)	(0.051)	(0.049)	(0.054)	(0.043)	(0.041)	(0.043)	(0.042)	(0.038)	(0.038)	(0.030)	(0.030)	(0.030)	(0.030)	(0.034)	(0.034)
t_{5}	-0.124^{c}	-0.193^{c}	-0.148^{c}	-0.047	-0.165^{c}	-0.082^{a}	-0.103^{b}	-0.135^{c}	-0.082^{a}	-0.072^{a}	-0.124^{c}	-0.131^{c}	-0.126^{c}	-0.136^{c}	-0.163^{c}	-0.177^{c}
	(0.032)	(0.054)	(0.052)	(0.059)	(0.044)	(0.046)	(0.047)	(0.044)	(0.043)	(0.043)	(0.032)	(0.033)	(0.032)	(0.032)	(0.036)	(0.036)
t_{6}	-0.135^{c}	-0.208^{c}	-0.181^{c}	-0.051	-0.192^{c}	-0.088^{a}	-0.137^{c}	-0.146^{c}	-0.111^{c}	-0.092^{b}	-0.136^{c}	-0.145^{c}	-0.136^{c}	-0.147^{c}	-0.175^{c}	-0.192^{c}
	(0.031)	(0.051)	(0.049)	(0.062)	(0.043)	(0.045)	(0.048)	(0.041)	(0.040)	(0.039)	(0.031)	(0.032)	(0.031)	(0.031)	(0.035)	(0.035)
ATT	-0.180^{c}	-0.204^{c}	-0.206^{c}	-0.146^{c}	-0.250^{c}	-0.126^{c}	-0.157^{c}	-0.211^{c}	-0.169^{c}	-0.166^{c}	-0.180^{c}	-0.187^{c}	-0.182^{c}	-0.189^{c}	-0.208^{c}	-0.215^{c}
	(0.015)	(0.025)	(0.024)	(0.027)	(0.021)	(0.020)	(0.022)	(0.021)	(0.019)	(0.019)	(0.015)	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)
(% of SD)	-25.6	-28.9	-31.0	-20.1	-38.8	-17.7	-21.6	-31.1	-24.0	-23.5	-25.6	-26.6	-25.9	-27.2	-29.7	-32.2
Mean (SD) of	3.731	3.817	3.752	3.625	3.917	3.556	3.651	3.805	3.733	3.733	3.731	3.731	3.731	3.744	3.705	3.742
t_{-1} Cohort	(0.703)	(0.706)	(0.665)	(0.725)	(0.645)	(0.711)	(0.726)	(0.678)	(0.705)	(0.705)	(0.703)	(0.703)	(0.703)	(0.694)	(0.701)	(0.668)
Wald F (ATT)	–	ref.	0.003	2.558	ref.	17.580^{c}	ref.	3.241^{a}	ref.	0.299	ref.	1.656	ref.	4.698^{b}	ref.	1.438
Wald F (t_0–t_6, joint)	–	ref.	0.623	2.357^{b}	ref.	1.671	ref.	0.981	ref.	1.939^{a}	ref.	0.804	ref.	1.185	ref.	1.435
Observations	9,190	3,079	3,139	2,972	4,132	5,058	4,233	4,708	7,352	7,352	9,181	9,181	8,697	8,486	7,565	7,152
Adj. R^2	0.419	0.423	0.414	0.444	0.377	0.413	0.438	0.407	0.419	0.418	0.419	0.420	0.420	0.414	0.422	0.406

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Subgroup analyses (columns 2–8) partition the baseline sample (sample differences reflect missing data). Robustness checks (columns 9–16) re-estimate the baseline within each subsample for comparability. Compare columns within each results group.

Table A.14: Event-Study Estimates for Self-Perceived Health by Health Controls in Regression

	Regression Specifications					
	Weighting, Demographics, and Health Event Characteristics	+ Health at Event-Time (t_{-1})	+ Physical and Mental Health	+ Chronic Conditions (excl. CVD-related)	+ Hospitalizations (excl. CVD-related)	+ Diabetes, Hypertension, or Hyperlipidemia
	(1)	(2)	(3)	(4)	(5)	(6)
Time to Health Event						
t_{-6}	0.054	0.081^b	0.064^a	0.058^a	0.056^a	0.044
	(0.041)	(0.038)	(0.033)	(0.033)	(0.033)	(0.033)
t_{-5}	0.010	0.044	0.018	0.010	0.012	0.002
	(0.039)	(0.034)	(0.031)	(0.031)	(0.031)	(0.031)
t_{-4}	0.077^b	0.080^b	0.061^b	0.051^a	0.050^a	0.042
	(0.038)	(0.034)	(0.030)	(0.030)	(0.030)	(0.030)
t_{-3}	0.060	0.076^b	0.048	0.047	0.048	0.045
	(0.037)	(0.032)	(0.030)	(0.029)	(0.029)	(0.029)
t_{-2}	0.005	0.028	0.027	0.030	0.030	0.028
	(0.032)	(0.028)	(0.026)	(0.026)	(0.026)	(0.025)
t_{-1}	–	–	–	–	–	–
t_0	-0.383^c	-0.342^c	-0.292^c	-0.224^c	-0.234^c	-0.180^c
	(0.036)	(0.032)	(0.030)	(0.031)	(0.031)	(0.033)
t_1	-0.266^c	-0.260^c	-0.214^c	-0.154^c	-0.153^c	-0.058^b
	(0.030)	(0.028)	(0.025)	(0.026)	(0.026)	(0.029)
t_2	-0.261^c	-0.251^c	-0.205^c	-0.153^c	-0.152^c	-0.058^b
	(0.031)	(0.028)	(0.026)	(0.026)	(0.026)	(0.029)
t_3	-0.268^c	-0.256^c	-0.191^c	-0.132^c	-0.131^c	-0.037
	(0.035)	(0.033)	(0.029)	(0.029)	(0.029)	(0.032)
t_4	-0.243^c	-0.247^c	-0.161^c	-0.112^c	-0.110^c	-0.014
	(0.036)	(0.033)	(0.030)	(0.030)	(0.030)	(0.033)
t_5	-0.269^c	-0.259^c	-0.187^c	-0.124^c	-0.124^c	-0.026
	(0.039)	(0.036)	(0.032)	(0.032)	(0.032)	(0.035)
t_6	-0.249^c	-0.251^c	-0.189^c	-0.134^c	-0.135^c	-0.034
	(0.038)	(0.035)	(0.031)	(0.031)	(0.031)	(0.034)
ATT	-0.305^c	-0.308^c	-0.241^c	-0.179^c	-0.180^c	-0.099^c
	(0.017)	(0.015)	(0.014)	(0.015)	(0.015)	(0.019)
(% of SD)	-43.4	-43.8	-34.3	-25.5	-25.6	-14.1
Mean (SD) of t_{-1} Cohort	3.731	3.731	3.731	3.731	3.731	3.731
	(0.703)	(0.703)	(0.703)	(0.703)	(0.703)	(0.703)
Observations	9,190	9,190	9,190	9,190	9,190	9,190
Adj. R^2	0.111	0.260	0.402	0.416	0.419	0.423

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column headers indicate the controls included in the regression specification. Demographic controls contain information on age at health event onset (single-year-of-age), sex, and marital status at event time. Health characteristics at event time contain information on hospital care use, chronic conditions, and healthcare costs. Physical and mental health at survey time is measured using dummies encoding at least great difficulty with each of seven functional limitations, and a dummy encoding elevated risk of depression or anxiety based on the Kessler 10-item questionnaire. Chronic conditions at survey time are measured based on prescription medication, excluding dummies encoding conditions associated with high cardiovascular risk: diabetes, hypertension, or hyperlipidemia. Hospital care use is encoded using disease-group-specific dummies encoding any admission in the survey year for a given disease group, excluding cardiovascular admissions, and the total number of inpatient days in that year.

Table A.15: Event-Study Estimates for Locus of Control: Standardized Score

		Subgroups							Robustness Checks							
		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Time to Health Event																
t_{-6}	0.047	0.104	-0.074	0.073	0.020	0.081	0.035	0.029	0.046	0.049	0.047	0.102	0.020	-0.000		
	(0.068)	(0.105)	(0.111)	(0.128)	(0.099)	(0.093)	(0.102)	(0.091)	(0.069)	(0.067)	(0.069)	(0.069)	(0.179)	(0.183)		
t_{-5}	0.136^a	0.151	0.007	0.237^a	0.075	0.156	0.124	0.173^a	0.134^a	0.123^a	0.136^a	0.162^b	0.141^b	0.128^a		
	(0.071)	(0.117)	(0.126)	(0.123)	(0.105)	(0.096)	(0.108)	(0.098)	(0.072)	(0.072)	(0.071)	(0.072)	(0.071)	(0.073)		
t_{-4}	0.038	0.022	0.043	0.073	0.052	0.014	0.005	0.055	0.024	-0.002	0.034	0.043	0.046	0.039	-0.038	-0.130
	(0.069)	(0.130)	(0.113)	(0.111)	(0.103)	(0.092)	(0.104)	(0.094)	(0.069)	(0.069)	(0.069)	(0.069)	(0.069)	(0.070)	(0.188)	(0.200)
t_{-3}	0.026	-0.030	0.094	0.015	0.169^a	-0.080	-0.121	0.157^a	0.025	-0.017	0.026	0.043	0.032	0.025	0.043	0.017
	(0.062)	(0.106)	(0.101)	(0.110)	(0.091)	(0.085)	(0.094)	(0.084)	(0.063)	(0.062)	(0.062)	(0.062)	(0.063)	(0.064)	(0.069)	(0.072)
t_{-2}	0.008	-0.022	-0.034	0.059	0.063	-0.039	-0.067	0.099	0.011	-0.011	0.012	0.018	0.047	0.049	0.060	0.054
	(0.050)	(0.087)	(0.086)	(0.083)	(0.073)	(0.068)	(0.078)	(0.065)	(0.050)	(0.050)	(0.050)	(0.050)	(0.058)	(0.059)	(0.067)	(0.070)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_0	-0.239^c	-0.234^b	-0.238^c	-0.229^b	-0.181^b	-0.299^c	-0.407^c	-0.075	-0.247^c	-0.239^c	-0.239^c	-0.246^c	-0.238^c	-0.239^c	-0.236^c	-0.250^c
	(0.056)	(0.096)	(0.092)	(0.097)	(0.086)	(0.074)	(0.084)	(0.074)	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)	(0.057)	(0.072)	(0.076)
t_1	-0.164^c	-0.125	-0.119	-0.211^b	-0.091	-0.229^c	-0.215^c	-0.074	-0.163^c	-0.163^c	-0.164^c	-0.174^c	-0.164^c	-0.170^c	-0.164^c	-0.185^c
	(0.050)	(0.087)	(0.080)	(0.088)	(0.070)	(0.070)	(0.076)	(0.066)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.062)	(0.063)
t_2	-0.207^c	-0.203^b	-0.204^b	-0.213^b	-0.031	-0.347^c	-0.247^c	-0.140^b	-0.203^c	-0.180^c	-0.207^c	-0.223^c	-0.206^c	-0.208^c	-0.204^c	-0.193^c
	(0.051)	(0.089)	(0.082)	(0.091)	(0.072)	(0.072)	(0.079)	(0.067)	(0.051)	(0.052)	(0.051)	(0.052)	(0.051)	(0.052)	(0.063)	(0.064)
t_3	-0.259^c	-0.257^c	-0.140	-0.360^c	-0.149^a	-0.353^c	-0.348^c	-0.163^b	-0.268^c	-0.248^c	-0.259^c	-0.279^c	-0.260^c	-0.272^c	-0.259^c	-0.280^c
	(0.054)	(0.092)	(0.086)	(0.098)	(0.076)	(0.075)	(0.082)	(0.072)	(0.066)	(0.066)	(0.054)	(0.054)	(0.054)	(0.054)	(0.065)	(0.066)
t_4	-0.253^c	-0.299^c	-0.246^c	-0.193^a	-0.146^a	-0.359^c	-0.275^c	-0.189^b	-0.234^c	-0.199^c	-0.253^c	-0.278^c	-0.256^c	-0.266^c	-0.259^c	-0.282^c
	(0.054)	(0.091)	(0.088)	(0.099)	(0.078)	(0.076)	(0.082)	(0.075)	(0.067)	(0.068)	(0.054)	(0.056)	(0.055)	(0.055)	(0.066)	(0.067)
t_5	-0.221^c	-0.279^c	-0.190^b	-0.174^a	-0.115	-0.327^c	-0.244^c	-0.164^b	-0.201^c	-0.153^b	-0.221^c	-0.254^c	-0.222^c	-0.232^c	-0.222^c	-0.246^c
	(0.055)	(0.098)	(0.092)	(0.096)	(0.077)	(0.078)	(0.086)	(0.073)	(0.069)	(0.069)	(0.055)	(0.057)	(0.055)	(0.055)	(0.067)	(0.067)
t_6	-0.232^c	-0.232^b	-0.258^c	-0.184^a	-0.180^b	-0.278^c	-0.275^c	-0.138^a	-0.222^c	-0.164^b	-0.232^c	-0.270^c	-0.233^c	-0.245^c	-0.235^c	-0.261^c
	(0.057)	(0.095)	(0.096)	(0.103)	(0.079)	(0.082)	(0.086)	(0.077)	(0.070)	(0.071)	(0.057)	(0.059)	(0.057)	(0.057)	(0.068)	(0.069)
ATT	-0.244^c	-0.238^c	-0.192^c	-0.274^c	-0.166^c	-0.310^c	-0.261^c	-0.199^c	-0.228^c	-0.198^c	-0.245^c	-0.265^c	-0.254^c	-0.258^c	-0.249^c	-0.252^c
	(0.026)	(0.045)	(0.044)	(0.044)	(0.037)	(0.036)	(0.040)	(0.035)	(0.029)	(0.029)	(0.026)	(0.028)	(0.028)	(0.029)	(0.036)	(0.038)
Mean (SD) of	0.027	0.043	0.117	-0.067	0.068	-0.011	-0.121	0.139	0.029	0.029	0.027	0.027	0.027	0.040	0.013	0.057
t_{-1} Cohort	(1.028)	(1.042)	(0.927)	(1.094)	(1.009)	(1.046)	(1.047)	(0.986)	(1.030)	(1.030)	(1.028)	(1.028)	(1.028)	(1.025)	(1.044)	(1.024)
Wald F (ATT)	–	ref.	0.539	0.324	ref.	7.709^c	ref.	1.397	ref.	10.320^c	ref.	3.379^a	ref.	0.512	ref.	0.055
Wald F (t_0–t_6, joint)	–	ref.	0.287	0.624	ref.	1.650	ref.	1.411	ref.	1.504	ref.	1.445	ref.	1.152	ref.	1.504
Observations	7,181	2,399	2,446	2,336	3,249	3,932	3,182	3,790	6,087	6,087	7,175	7,175	6,692	6,548	5,559	5,300
Adj. R^2	0.153	0.171	0.156	0.174	0.114	0.177	0.133	0.147	0.151	0.149	0.152	0.160	0.155	0.155	0.160	0.159

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Subgroup analyses (columns 2–8) partition the baseline sample (sample differences reflect missing data). Robustness checks (columns 9–16) re-estimate the baseline within each subsample for comparability. Compare columns within each results group.

Table A.16: Event-Study Estimates for Locus of Control: Low Locus of Control (Score < 19)

		Subgroups							Robustness Checks							
		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Time to Health Event																
t_{-6}	-0.051^b	-0.060^b	-0.020	-0.064^a	-0.048^a	-0.057^b	-0.069^b	-0.021	-0.054^c	-0.049^c	-0.052^c	-0.066^c	-0.048	-0.037		
	(0.020)	(0.029)	(0.037)	(0.036)	(0.028)	(0.029)	(0.030)	(0.027)	(0.020)	(0.019)	(0.020)	(0.020)	(0.059)	(0.060)		
t_{-5}	-0.021	-0.053^a	-0.016	0.002	-0.048^a	-0.000	-0.017	-0.022	-0.026	-0.018	-0.021	-0.026	-0.022	-0.022		
	(0.021)	(0.032)	(0.034)	(0.039)	(0.026)	(0.030)	(0.036)	(0.024)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)		
t_{-4}	-0.017	0.004	-0.018	-0.029	-0.018	-0.015	-0.011	-0.012	-0.019	-0.013	-0.016	-0.018	-0.019	-0.018	-0.003	0.010
	(0.020)	(0.039)	(0.032)	(0.032)	(0.030)	(0.027)	(0.034)	(0.025)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.057)	(0.062)
t_{-3}	-0.028	-0.039	-0.034	-0.011	-0.065^c	0.000	0.007	-0.040^a	-0.031	-0.020	-0.028	-0.032^a	-0.029	-0.028	-0.028	-0.018
	(0.019)	(0.031)	(0.028)	(0.034)	(0.022)	(0.027)	(0.033)	(0.021)	(0.019)	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.021)	(0.021)
t_{-2}	-0.000	-0.003	0.022	-0.016	-0.012	0.008	0.030	-0.032^a	0.001	0.007	-0.001	-0.002	-0.004	-0.003	-0.001	0.002
	(0.015)	(0.026)	(0.028)	(0.025)	(0.021)	(0.022)	(0.026)	(0.018)	(0.015)	(0.015)	(0.015)	(0.015)	(0.018)	(0.018)	(0.021)	(0.022)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_0	0.047^b	0.027	0.052^a	0.057^a	0.035	0.062^b	0.095^c	0.004	0.050^c	0.048^c	0.048^b	0.050^c	0.047^b	0.048^c	0.049^b	0.044^a
	(0.019)	(0.030)	(0.030)	(0.033)	(0.027)	(0.025)	(0.032)	(0.021)	(0.019)	(0.018)	(0.019)	(0.018)	(0.019)	(0.019)	(0.024)	(0.025)
t_1	0.034^b	0.010	0.030	0.051^a	0.013	0.048^b	0.049^a	0.016	0.034^b	0.036^b	0.034^b	0.037^b	0.035^b	0.035^b	0.039^a	0.039^a
	(0.016)	(0.027)	(0.025)	(0.029)	(0.022)	(0.023)	(0.026)	(0.020)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.020)	(0.020)
t_2	0.039^b	0.026	0.046^a	0.044	-0.007	0.075^c	0.050^a	0.021	0.038^b	0.034^b	0.040^b	0.044^c	0.039^b	0.039^b	0.042^b	0.034^a
	(0.016)	(0.028)	(0.025)	(0.029)	(0.021)	(0.024)	(0.026)	(0.020)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.020)	(0.020)
t_3	0.056^c	0.025	0.038	0.089^c	0.028	0.075^c	0.091^c	0.022	0.071^c	0.064^c	0.055^c	0.061^c	0.055^c	0.057^c	0.059^c	0.061^c
	(0.018)	(0.030)	(0.028)	(0.034)	(0.024)	(0.025)	(0.030)	(0.022)	(0.023)	(0.023)	(0.018)	(0.018)	(0.018)	(0.018)	(0.022)	(0.022)
t_4	0.058^c	0.067^b	0.070^b	0.028	0.044^a	0.071^c	0.070^b	0.045^b	0.082^c	0.072^c	0.058^c	0.064^c	0.059^c	0.061^c	0.064^c	0.068^c
	(0.018)	(0.031)	(0.028)	(0.031)	(0.024)	(0.026)	(0.029)	(0.022)	(0.023)	(0.023)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.021)
t_5	0.042^b	0.060^a	0.053^a	0.013	0.019	0.062^b	0.070^b	0.010	0.050^b	0.036	0.041^b	0.050^c	0.042^b	0.044^b	0.045^b	0.049^b
	(0.018)	(0.032)	(0.029)	(0.032)	(0.024)	(0.026)	(0.030)	(0.021)	(0.022)	(0.023)	(0.018)	(0.018)	(0.018)	(0.018)	(0.022)	(0.022)
t_6	0.049^c	0.033	0.083^c	0.031	0.010	0.082^c	0.051^a	0.042^a	0.043^a	0.028	0.049^c	0.059^c	0.049^c	0.052^c	0.054^b	0.058^b
	(0.019)	(0.029)	(0.031)	(0.035)	(0.023)	(0.028)	(0.030)	(0.024)	(0.022)	(0.022)	(0.019)	(0.019)	(0.019)	(0.019)	(0.022)	(0.023)
ATT	0.056^c	0.048^c	0.053^c	0.059^c	0.038^c	0.070^c	0.065^c	0.040^c	0.055^c	0.047^c	0.056^c	0.061^c	0.056^c	0.056^c	0.057^c	0.053^c
	(0.008)	(0.014)	(0.014)	(0.014)	(0.011)	(0.012)	(0.014)	(0.010)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.012)	(0.012)
(% of Mean)	53.3	43.2	65.4	48.0	40.0	60.9	50.0	47.1	52.4	44.8	53.3	58.1	53.3	54.4	51.8	51.0
Mean (SD) of	0.105	0.111	0.081	0.123	0.095	0.115	0.130	0.085	0.105	0.105	0.105	0.105	0.105	0.103	0.110	0.104
t_{-1} Cohort	(0.307)	(0.314)	(0.273)	(0.329)	(0.294)	(0.319)	(0.337)	(0.279)	(0.307)	(0.307)	(0.307)	(0.307)	(0.307)	(0.305)	(0.314)	(0.306)
Wald F (ATT)	–	ref.	0.061	0.331	ref.	3.620^a	ref.	2.064	ref.	7.664^c	ref.	1.764	ref.	0.066	ref.	1.505
Wald F (t_0–t_6, joint)	–	ref.	0.310	1.226	ref.	1.201	ref.	1.244	ref.	1.381	ref.	0.994	ref.	0.550	ref.	1.258
Observations	7,181	2,399	2,446	2,336	3,249	3,932	3,182	3,790	6,087	6,087	7,175	7,175	6,692	6,548	5,559	5,300
Adj. R^2	0.097	0.131	0.105	0.119	0.071	0.111	0.100	0.096	0.098	0.101	0.097	0.103	0.101	0.100	0.101	0.100

Source: Own calculations based on data from Statistics Netherlands. *Notes:* *a* indicates $p < 0.10$, *b* indicates $p < 0.05$, and *c* indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Subgroup analyses (columns 2–8) partition the baseline sample (sample differences reflect missing data). Robustness checks (columns 9–16) re-estimate the baseline within each subsample for comparability. Compare columns within each results group.

Table A.17: Event-Study Estimates for GP Visits – GP Consultation Costs (€ 1000s)

		Mechanisms					Subgroups							Robustness Checks							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	-0.008^b	-0.007^b	-0.005^a	-0.005	-0.011^c	-0.010^b	-0.001	-0.003	-0.017^b	-0.008^a	-0.008^a	-0.005	-0.006	-0.010^c	-0.004	-0.008^b	-0.008^b	-0.004	-0.004	-0.004	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.004)	(0.005)	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)
t_{-5}	-0.004	-0.005^a	-0.002	-0.002	-0.005	-0.005	0.005	-0.005	-0.012^b	-0.006	-0.004	-0.005	-0.003	-0.007^b	-0.001	-0.004	-0.005^a	-0.004	-0.003	-0.004	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
t_{-4}	-0.009^c	-0.009^c	-0.006^b	-0.006^b	-0.008^b	-0.008^b	-0.001	-0.008^b	-0.016^c	-0.005	-0.013^c	-0.007^b	-0.008^b	-0.012^c	-0.006	-0.009^c	-0.009^c	-0.009^c	-0.008^c	-0.011^c	-0.011^c
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
t_{-3}	-0.000	0.000	0.001	0.002	-0.003	-0.002	0.012^c	-0.004	-0.010^b	-0.003	-0.001	-0.004	0.006	-0.003	0.002	-0.000	-0.001	0.000	0.001	-0.002	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
t_{-2}	-0.006^c	-0.006^c	-0.006^c	-0.006^c	-0.006^b	-0.006^b	-0.002	-0.004	-0.013^c	-0.004	-0.009^c	-0.005	-0.005^a	-0.007^c	-0.005^a	-0.007^c	-0.007^c	-0.004^a	-0.004^a	-0.005^b	-0.005^b
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_0	0.046^c	0.046^c	0.038^c	0.036^c	0.047^c	0.045^c	0.052^c	0.048^c	0.037^c	0.048^c	0.044^c	0.050^c	0.043^c	0.046^c	0.045^c	0.046^c	0.046^c	0.046^c	0.046^c	0.044^c	0.043^c
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
t_1	0.030^c	0.030^c	0.023^c	0.021^c	0.029^c	0.028^c	0.030^c	0.027^c	0.031^c	0.033^c	0.026^c	0.031^c	0.029^c	0.030^c	0.027^c	0.030^c	0.030^c	0.030^c	0.028^c	0.027^c	0.025^c
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
t_2	0.018^c	0.016^c	0.009^c	0.007^b	0.021^c	0.020^c	0.018^c	0.016^c	0.018^c	0.020^c	0.016^c	0.018^c	0.017^c	0.021^c	0.014^c	0.018^c	0.018^c	0.017^c	0.017^c	0.015^c	0.014^c
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.003)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
t_3	0.021^c	0.022^c	0.012^c	0.011^c	0.022^c	0.021^c	0.021^c	0.017^c	0.024^c	0.030^c	0.013^c	0.021^c	0.024^c	0.022^c	0.018^c	0.021^c	0.021^c	0.020^c	0.022^c	0.017^c	0.018^c
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.005)	(0.006)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
t_4	0.019^c	0.018^c	0.010^c	0.009^b	0.021^c	0.020^c	0.021^c	0.026^c	0.011^a	0.024^c	0.016^c	0.020^c	0.018^c	0.022^c	0.011^a	0.019^c	0.020^c	0.019^c	0.020^c	0.016^c	0.016^c
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
t_5	0.019^c	0.018^c	0.008^b	0.007^a	0.021^c	0.020^c	0.021^c	0.019^c	0.014^b	0.024^c	0.014^c	0.016^c	0.022^c	0.019^c	0.011^b	0.019^c	0.019^c	0.018^c	0.020^c	0.015^c	0.016^c
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
t_6	0.014^c	0.014^c	0.007^b	0.006^a	0.015^c	0.014^c	0.024^c	0.007	0.012^a	0.020^c	0.009^a	0.015^c	0.016^c	0.019^c	-0.003	0.014^c	0.014^c	0.013^c	0.015^c	0.010^c	0.012^c
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.007)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
ATT	0.029^c	0.029^c	0.020^c	0.018^c	0.030^c	0.029^c	0.027^c	0.027^c	0.033^c	0.032^c	0.028^c	0.030^c	0.028^c	0.033^c	0.026^c	0.029^c	0.031^c	0.028^c	0.028^c	0.027^c	0.026^c
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
(% of Mean)	50.9	52.7	36.4	32.7	52.6	50.9	58.7	55.1	44.0	80.0	38.9	48.4	57.1	55.9	44.1	50.9	54.4	49.1	50.0	48.2	47.3
Mean (SD) of	0.057	0.055	0.055	0.055	0.057	0.057	0.046	0.049	0.075	0.040	0.072	0.062	0.049	0.059	0.059	0.057	0.057	0.057	0.056	0.056	0.055
t_{-1} Cohort	(0.071)	(0.067)	(0.067)	(0.067)	(0.069)	(0.069)	(0.053)	(0.051)	(0.094)	(0.058)	(0.077)	(0.065)	(0.058)	(0.075)	(0.075)	(0.071)	(0.071)	(0.071)	(0.065)	(0.064)	(0.058)
Wald F (ATT)	–	ref.	100.900^c	132.400^c	ref.	22.330^c	ref.	0.058	2.771^a	ref.	2.407	ref.	0.768	ref.	38.550^c	ref.	3.936^b	ref.	1.031	ref.	5.991^b
Wald F (t_0–t_6, joint)	–	ref.	11.570^c	14.280^c	ref.	1.728^a	ref.	0.848	1.180	ref.	1.326	ref.	0.401	ref.	9.405^c	ref.	0.451	ref.	1.401	ref.	1.303
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.293	0.286	0.352	0.359	0.279	0.290	0.286	0.239	0.348	0.268	0.288	0.316	0.256	0.296	0.229	0.294	0.294	0.295	0.286	0.266	0.263

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

Table A.18: Event-Study Estimates for Insurance Choice – Voluntary Deductible > 0

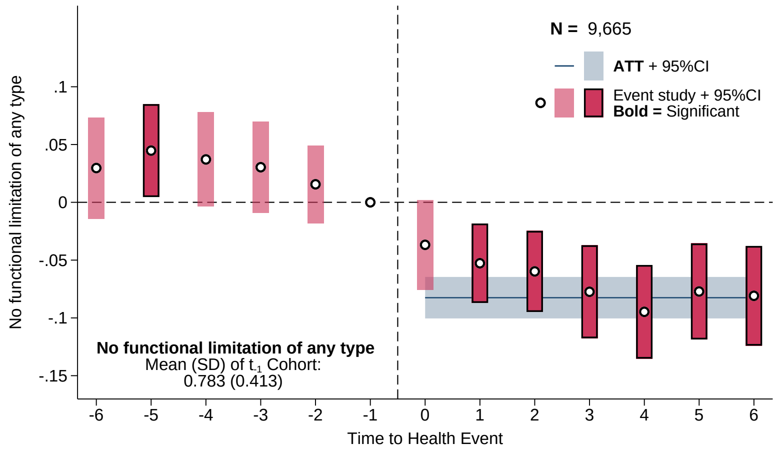
		Mechanisms					**Subgroups**							**Robustness Checks**							
		Objective (Obj) & Self-Assessed Health (SAH)			Locus of Control (LoC)		Propensity Score Terciles			Pre-Existing High-Risk Condition		Educational Attainment		Pre-Event Health Controls		Current Income (Inc)		3-Year Survival Window		5-Year Survival Window	
		w/o	+ Obj	+ SAH	w/o	+ LoC	Bottom	Middle	Top	None	> 1	Lower	Higher	t_{-1}	t_{-5}	w/o	+ Inc	All	Surviving	All	Surviving
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Time to Health Event																					
t_{-6}	0.031^{b}	0.031^{b}	0.030^{b}	0.027^{a}	0.014	0.014	0.047^{a}	0.020	0.025	0.029	0.036^{b}	0.034^{b}	0.028	0.036^{b}	0.035^{b}	0.030^{b}	0.027^{a}	0.052^{c}	0.049^{b}	0.056^{c}	0.057^{c}
	(0.014)	(0.015)	(0.015)	(0.015)	(0.019)	(0.019)	(0.026)	(0.023)	(0.024)	(0.026)	(0.015)	(0.017)	(0.023)	(0.015)	(0.015)	(0.014)	(0.014)	(0.019)	(0.019)	(0.021)	(0.022)
t_{-5}	0.018	0.011	0.010	0.009	0.005	0.005	-0.001	0.024	0.026	0.033	0.009	0.009	0.029	0.024^{a}	0.022	0.019	0.015	0.017	0.015	0.038^{a}	0.041^{a}
	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)	(0.021)	(0.022)	(0.022)	(0.026)	(0.011)	(0.014)	(0.022)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.019)	(0.021)
t_{-4}	0.013	0.010	0.010	0.008	-0.001	-0.001	0.007	0.011	0.016	0.010	0.020	0.021	0.008	0.016	0.015	0.013	0.010	0.013	0.013	0.037^{b}	0.042^{b}
	(0.012)	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)	(0.023)	(0.022)	(0.020)	(0.023)	(0.012)	(0.016)	(0.020)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.013)	(0.018)	(0.019)
t_{-3}	0.017	0.017	0.017	0.015	0.032^{a}	0.032^{a}	0.018	0.031	0.002	0.017	0.023^{a}	0.029^{a}	0.004	0.021	0.020	0.017	0.016	0.017	0.018	0.020	0.022
	(0.012)	(0.012)	(0.012)	(0.012)	(0.016)	(0.016)	(0.022)	(0.022)	(0.019)	(0.023)	(0.012)	(0.016)	(0.018)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.014)
t_{-2}	0.008	0.007	0.008	0.006	0.013	0.013	0.003	0.013	0.009	-0.014	0.027^{c}	0.017	-0.004	0.011	0.011	0.008	0.008	-0.000	0.003	0.002	0.008
	(0.010)	(0.010)	(0.010)	(0.010)	(0.012)	(0.012)	(0.018)	(0.017)	(0.016)	(0.018)	(0.010)	(0.012)	(0.016)	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	(0.013)
t_{-1}	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
t_{0}	-0.018^{a}	-0.020^{b}	-0.013	-0.008	-0.017	-0.017	-0.012	-0.022	-0.017	-0.039^{b}	0.001	-0.011	-0.026	-0.016	-0.015	-0.018^{a}	-0.017^{a}	-0.018^{a}	-0.017^{a}	-0.020^{a}	-0.022^{a}
	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.019)	(0.016)	(0.016)	(0.019)	(0.009)	(0.010)	(0.016)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)
t_{1}	-0.026^{c}	-0.028^{c}	-0.021^{b}	-0.016^{a}	-0.025^{b}	-0.025^{b}	-0.039^{b}	-0.022	-0.017	-0.055^{c}	-0.003	-0.014	-0.036^{b}	-0.022^{b}	-0.021^{b}	-0.026^{c}	-0.024^{c}	-0.026^{c}	-0.026^{c}	-0.022^{b}	-0.023^{b}
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.015)	(0.014)	(0.014)	(0.016)	(0.008)	(0.009)	(0.014)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)
t_{2}	-0.031^{c}	-0.034^{c}	-0.028^{c}	-0.024^{c}	-0.029^{c}	-0.029^{c}	-0.034^{b}	-0.020	-0.038^{c}	-0.062^{c}	-0.006	-0.020^{b}	-0.041^{c}	-0.028^{c}	-0.026^{c}	-0.031^{c}	-0.028^{c}	-0.031^{c}	-0.030^{c}	-0.026^{c}	-0.025^{b}
	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.016)	(0.014)	(0.012)	(0.015)	(0.008)	(0.008)	(0.014)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)
t_{3}	-0.036^{c}	-0.037^{c}	-0.031^{c}	-0.027^{c}	-0.036^{c}	-0.036^{c}	-0.046^{c}	-0.024	-0.039^{c}	-0.063^{c}	-0.012	-0.019^{b}	-0.054^{c}	-0.049^{c}	-0.045^{c}	-0.036^{c}	-0.032^{c}	-0.035^{c}	-0.034^{c}	-0.031^{c}	-0.030^{c}
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.016)	(0.017)	(0.013)	(0.017)	(0.008)	(0.009)	(0.014)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)
t_{4}	-0.035^{c}	-0.038^{c}	-0.032^{c}	-0.029^{c}	-0.035^{c}	-0.035^{c}	-0.039^{b}	-0.027	-0.041^{c}	-0.063^{c}	-0.008	-0.015	-0.050^{c}	-0.048^{c}	-0.044^{c}	-0.035^{c}	-0.031^{c}	-0.034^{c}	-0.033^{c}	-0.030^{c}	-0.029^{c}
	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.018)	(0.017)	(0.013)	(0.017)	(0.009)	(0.011)	(0.015)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)
t_{5}	-0.037^{c}	-0.038^{c}	-0.032^{c}	-0.028^{c}	-0.037^{c}	-0.036^{c}	-0.041^{b}	-0.028^{a}	-0.043^{c}	-0.068^{c}	-0.009	-0.018^{a}	-0.052^{c}	-0.047^{c}	-0.044^{c}	-0.037^{c}	-0.033^{c}	-0.037^{c}	-0.035^{c}	-0.032^{c}	-0.031^{c}
	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.018)	(0.016)	(0.013)	(0.016)	(0.009)	(0.010)	(0.015)	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)
t_{6}	-0.046^{c}	-0.049^{c}	-0.043^{c}	-0.038^{c}	-0.046^{c}	-0.046^{c}	-0.062^{c}	-0.036^{b}	-0.043^{c}	-0.079^{c}	-0.019^{b}	-0.023^{b}	-0.067^{c}	-0.051^{c}	-0.046^{c}	-0.046^{c}	-0.042^{c}	-0.045^{c}	-0.044^{c}	-0.041^{c}	-0.040^{c}
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.015)	(0.016)	(0.014)	(0.016)	(0.008)	(0.010)	(0.014)	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)
ATT	-0.041^{c}	-0.042^{c}	-0.035^{c}	-0.029^{c}	-0.040^{c}	-0.040^{c}	-0.044^{c}	-0.037^{c}	-0.040^{c}	-0.064^{c}	-0.023^{c}	-0.032^{c}	-0.049^{c}	-0.040^{c}	-0.037^{c}	-0.041^{c}	-0.035^{c}	-0.038^{c}	-0.038^{c}	-0.039^{c}	-0.042^{c}
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.009)	(0.008)	(0.007)	(0.008)	(0.005)	(0.006)	(0.008)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
(% of Mean)	-77.4	-75.0	-62.5	-51.8	-74.1	-74.1	-71.0	-74.0	-83.3	-71.1	-115.0	-128.0	-62.8	-78.4	-72.5	-77.4	-66.0	-71.7	-71.7	-76.5	-80.8
Mean (SD) of	0.053	0.056	0.056	0.056	0.054	0.054	0.062	0.050	0.048	0.090	0.020	0.025	0.078	0.051	0.051	0.053	0.053	0.053	0.053	0.051	0.052
t_{-1} Cohort	(0.224)	(0.231)	(0.231)	(0.231)	(0.226)	(0.226)	(0.241)	(0.218)	(0.214)	(0.286)	(0.141)	(0.158)	(0.268)	(0.220)	(0.220)	(0.224)	(0.224)	(0.224)	(0.224)	(0.220)	(0.222)
Wald F (ATT)	–	ref.	11.250^{c}	28.460^{c}	ref.	0.049	ref.	0.361	0.135	ref.	17.620^{c}	ref.	3.213^{a}	ref.	4.593^{b}	ref.	7.403^{c}	ref.	0.051	ref.	3.981^{b}
Wald F (t_0–t_6, joint)	–	ref.	1.958^{a}	3.890^{c}	ref.	0.003	ref.	0.500	0.739	ref.	2.121^{b}	ref.	1.203	ref.	1.025	ref.	0.880	ref.	0.548	ref.	0.756
Observations	9,665	9,190	9,190	9,190	7,181	7,181	3,232	3,295	3,138	4,315	5,350	4,494	4,860	7,698	7,698	9,656	9,656	9,160	8,935	7,975	7,526
Adj. R^2	0.036	0.037	0.040	0.047	0.035	0.035	0.046	0.046	0.044	0.046	0.027	0.033	0.042	0.040	0.040	0.036	0.039	0.037	0.036	0.038	0.038

Source: Own calculations based on data from Statistics Netherlands. *Notes:* a indicates $p < 0.10$, b indicates $p < 0.05$, and c indicates $p < 0.01$. Depicted are the results of our regressions estimating the dynamic (event study) and static (average treatment effect on the treated, **ATT**) effect of a health event. All regressions are weighted using inverse probability weights. Column (1) shows the baseline specification controlling for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Other columns vary from this baseline as indicated by column headers. Mechanism analyses (columns 2–6) and robustness checks (columns 14–21) re-estimate the baseline within each subsample for comparability. Subgroup analyses (columns 7–13) partition the baseline sample (sample differences reflect missing data). Compare columns within each results group.

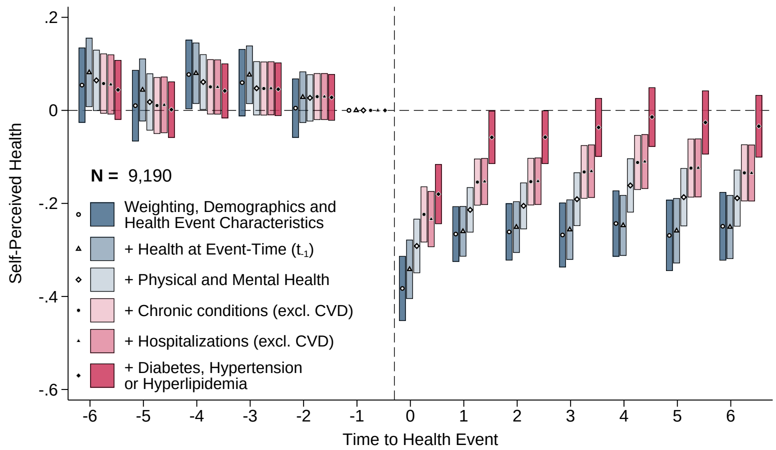
B Supplementary Figures

Figure B.1: The Effect on the Absence of any Functional Limitation and Self-Perceived Health

(a) Absence of any Functional Limitation



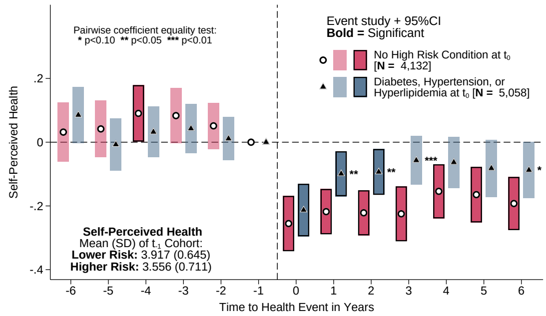
(b) The Effect on Self-Perceived Health by included Health Controls



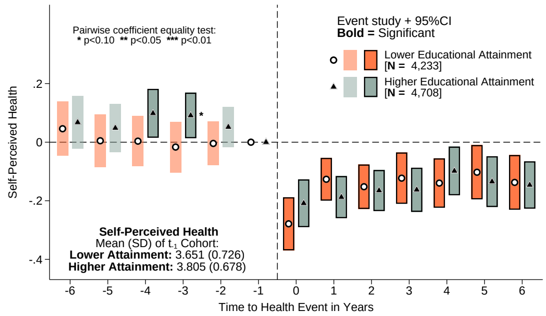
Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the event study coefficients (dots, triangles, and diamonds) and their 95% confidence intervals (bars). All regressions are weighted using inverse probability weights. The regression for the absence of a functional limitation **(a)** controls for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. See column (1) of Table A.12 for the underlying results. The regressions for self-perceived health **(b)** successively add health-related control variables, comparing a weighted regression containing only survey-year dummies, pre-event demographic (age, sex, and marital status) and socioeconomic characteristics (labor market participation, income, and income rank), and information on the health event experienced (health event type and inpatient days) against specifications controlling for health characteristics at event time (chronic conditions, hospital care use, and healthcare costs), physical and mental health at survey time (functional limitations and depression/anxiety risk), chronic conditions and hospitalizations at survey time excluding those related to cardiovascular health, and lastly whether at survey time an individual uses medications for diabetes, hypertension, or hyperlipidemia. See Table A.14 for the underlying results.

Figure B.2: The Effect of Acute Health Events on Self-Perceived Health by Health Risk and Education

(a) Effects on Self-Perceived Health, by Pre-Existing Health Risk



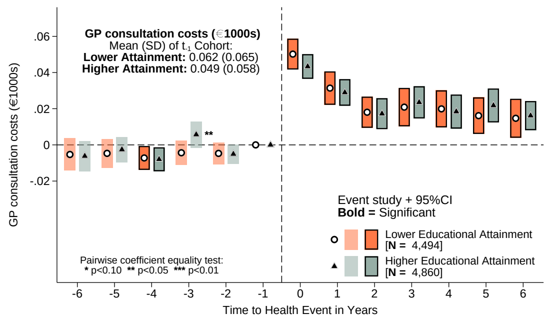
(b) Effects on Self-Perceived Health, by Educational Attainment



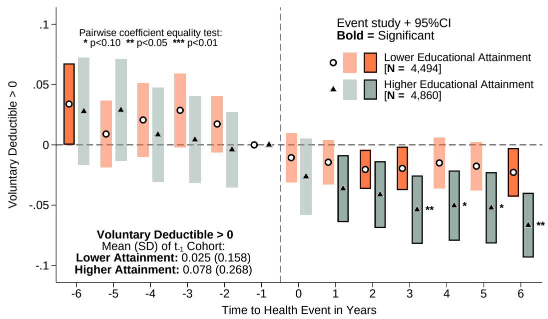
Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the estimated event study coefficients (dots and triangles) alongside their respective 95% confidence intervals (bars/shaded area). Stars denote the result of a Wald test of pairwise coefficient equality between the two groups at each event time (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). All regressions are weighted using inverse probability weights and control for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Results by health risk groups: individuals without a high-risk pre-existing condition such as hypertension, diabetes, or hyperlipidemia at event time (dots, red) compared against those with such a condition (triangles, blue). Results by educational attainment: individuals with lower levels of educational attainment (combining no formal education, basic education only, or lower-secondary general and pre-vocational schooling; dots, orange) compared against those with higher educational attainment (combining upper-secondary general, intermediate vocational, and tertiary education; triangles, teal). See Table A.13 columns (5) and (6) for the underlying results by health risk, and columns (7) and (8) by educational attainment.

Figure B.3: Event-Study Estimates for GP Consultation Costs and Insurance Choice by Education

(a) GP Consultation Costs (€ 1000s), by Educational Attainment



(b) Voluntary Deductible > 0, by Educational Attainment



Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the estimated event study coefficients (dots and triangles) alongside their respective 95% confidence intervals (bars/shaded area). Stars denote the result of a Wald test of pairwise coefficient equality between the two groups at each event time (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). All regressions are weighted using inverse probability weights and control for pre-event demographic (age, sex, and marital status), health (chronic conditions, hospital care use, and healthcare costs), and socioeconomic characteristics (labor market participation, income, and income rank), information on the health event experienced (health event type and inpatient days), and survey-year dummies. Results by educational attainment: individuals with lower levels of educational attainment (combining no formal education, basic education only, or lower-secondary general and pre-vocational schooling; dots, orange) compared against those with higher educational attainment (combining upper-secondary general, intermediate vocational, and tertiary education; triangles, teal). See columns (12) and (13) in Table A.17 for the underlying GP consultation results by educational attainment, and Table A.18 for the underlying deductible choice results by educational attainment.

C Dataset Details

C.I Data Sources

We use data provided by Statistics Netherlands (CBS, *Centraal Bureau voor de Statistiek*) covering a range of individual- and household-level information. The data can be accessed via a remote desktop connection and is stored in individual, topic-specific datasets. Within these datasets, individuals are identified based on a persistent unique pseudonymized identification number allowing for the linkage of records across datasets and time. Table C.1 outlines all datasets used.

Table C.1: Dataset Contents, Coverage, and Source

	Data from the CBS Microdata Catalog (CBS dataset name in *italics*)	
Demographic Information		**Years**
Population Registry	The population registry (*gbapersoonktab*) contains all individuals legally registered within the Netherlands since 1995 and their date of birth and official sex. It is updated on an annual basis and forms the basis of all administrative records. Registration within municipalities is legally required.	1995–2024
Death Registry	The death registry (*gbaoverlijdentab*) contains all individuals deceased within the Netherlands since 1995 and we use it to identify the official date of death. It is updated on an annual basis and covers deaths that occurred between January 1^{st} 1995 and February 14^{th} 2025 (2025 release file).	1995–2025
Household Registry	The household registry (*gbahuishoudensbus*) contains all households registered in the Netherlands since 1995, their individual members and relationship to each other.	1995–2024
Socioeconomic Status		
Household Income	Annual household income (*ihi* and *inhatab*) based on declared incomes for tax-households.	2006–2024
Individual Income	Annual individual income (*ipi* and *inpatab*) based on declared income to the Tax Office and the main source of this income. These records can be linked to household-level records via the core household member.	2006–2024
Health and Healthcare Use		
Health Monitor	The Health Monitors (*gemon*) are conducted every four years since 2012, covering a wide range of self-reported information on health status and health-related behaviors among other repeated survey modules.	2012, 2016, 2020
Hospital Care Use	Annual data on all admissions to hospitals in the Netherlands (*lmrbasistab* and *lbzbasistab*) covering the date of admission, main diagnosis, main treatment received, and date and destination of discharge.	1995–2022
Medication Use	Annual data on individual-level prescription medication use by 4-digit Anatomical Therapeutic Chemical code (*medicijntab*). From 2020 onward covering also annual dosages dispensed.	2006–2022
Nursing Care Use	Annual data on usage of inpatient nursing home care (*zorgmvtab* and *gebwlztab*) covering all admission spells to a nursing home and their duration.	2006–2022
Healthcare Costs	Annual data on healthcare costs incurred by cost-categories (*zvwzorgkostentab*) based on health insurance records.	2009–2022
	Data from other Sources	
Health Insurance Choice		
Health Insurance Deductibles	Annual data on the voluntary deductible (*eigen risico*) level chosen. Data were provided by the Ministry of Health, Welfare and Sport.	2011–2021

Source: Statistics Netherlands (CBS) microdata catalog.

C.II Health Monitor Survey Methodology and Linkage to Administrative Records

The Health Monitors provide a representative sample of the adult Dutch population; CBS selects the individuals who are approached for participation. Excluded from participation are only those in an institutionalized setting, such as permanent nursing home residents, and those aged below 18 on 1 January of the survey year. Every four years since 2012, municipalities collect responses over the four-month period from 1 September to 31 December. Individuals receive a paper-based survey and instructions for online participation. Responses are overwhelmingly paper-based (58.3% in 2012, falling to 41.2% in 2020) or online (41.5% in 2012, rising to 58.4% in 2020), with in-person or phone-based interviews making up only 0.2% (2012/2020) to 0.8% (2016) of responses. The 2012 Health Monitor invited approximately 700,000 individuals and obtained 387,195 responses (ca. 55%); this rose to 1.15 million invited in 2016 with 457,153 respondents (ca. 40%), and 1.39 million in 2020 with 539,902 respondents (ca. 39%).

The 2012, 2016, and 2020 Health Monitors have a combined 1,384,250 observations across 1,291,014 unique respondents. We include only each individual's first-time participation and further exclude 23,182 individuals participating in a version of the Health Monitor that was fielded directly by CBS and for which the collection window is unknown. After applying these exclusion criteria, the sample of Health Monitor participants identifiable within the population registry data and linkable to other administrative records contains 1,267,832 individuals, or 98% of the original sample of unique individuals across the three Health Monitors, before conditioning on the availability of any information from the survey itself or administrative records.

C.III Definitions

Acute Health Event Definition

We identify patients experiencing an ischemic heart attack or stroke based on three-digit diagnosis codes from the 9th (ICD-9) and 10th (ICD-10) revisions of the International Classification of Diseases, using the main diagnosis assigned to hospital admissions. For the years 1995 to 2012, we use ICD-9 codes 410 (heart attacks) and 434/436 (strokes). From 2013 onwards, we use ICD-10 codes I21/I22 (heart attacks) and I63/I64 (strokes). This follows both the established economics literature (Chandra and Staiger, 2007; Fadlon and Nielsen, 2019) and CBS's own approach when calculating aggregate longitudinal trends in case rates and outcomes for ischemic heart attack and stroke patients, ensuring consistency across years.

To identify the first time such an event occurs at the individual level, we use the earliest admission date observed between 1995 and 2022. We do not require patients to have experienced only a single heart attack or stroke, nor only one type of event; subsequent events may occur. For a small number of admission episodes, the diagnosis codes are not those recorded by the hospital provider but are instead imputed from patient characteristics and physician specialty. Following CBS recommendations, we do not use imputed episodes for longitudinal analyses and remove all individuals who ever experienced any hospital admission with an imputed diagnosis code. This affects 6.71% of all unique first-time heart attack and stroke patients admitted between 2010 and 2022, the focus of our analysis.

Variables Created Based on Administrative and Survey Data

Below we describe all variables we create from the longitudinally available administrative data to supplement the Health Monitors, as well as any variables derived from Health Monitor data where changes were implemented, such as the recoding of variables or categories.

Hospital Care Use: We create dummies encoding any admission in a given year within 17 of the 19 main chapters of the International Shortlist for Hospital Morbidity Tabulation (ISHMT), an international classification system of hospital care use (see, for example, Wong et al., 2011 for detail on its applicability in the Dutch context). We exclude pregnancy-related and perinatal care given the age of our sample of heart attack and stroke patients (age-at-onset ranges from 60 to 75 after applying our sample restrictions). In addition, we create measures of the total number of hospital visits in a given year and the total length of stay (all inpatient days) across these visits, split into cardiovascular-health-related (ISHMT grouping 9: *Diseases of the Circulatory System*) and all other admissions.

Healthcare Costs: We use detailed healthcare cost data by spending category to create three measures of domain-specific healthcare costs in a given year. These are costs associated with pharmacological care, hospital care, and general practitioner consultations. All are adjusted for inflation using official annual consumer price indices published by CBS.

Medication Use: Based on data on prescription medication use, classified by Anatomical Therapeutic Chemical (ATC) codes, we identify use of medications for cardiovascular prevention, namely anti-thrombotic agents (ATC: B01A), anti-hypertensive agents (ATC: C01/C02, C07–C09, and C04A), and statins (ATC: C10), as well as an indicator for whether at least one of these was used in a given year.

Chronic Conditions: We follow Danesh et al. (2024) in inferring the presence of 21 distinct chronic conditions based on patterns in prescription medication use. Their mapping of ATC codes onto chronic conditions is a refined version of the mapping proposed by Huber et al. (2013), adapted for the Dutch context. The conditions we identify are acid-related disorders, bone-related diseases (e.g., osteoporosis), cancer, cardiovascular diseases (including hypertension), dementia, diabetes, epilepsy, glaucoma, gout, HIV, hyperlipidemia, intestinal (inflammatory) diseases, anemia, migraines, pain-related disorders, Parkinson's disease, psychological disorders (depression and anxiety), psychoses, respiratory illnesses, rheumatological conditions, and thyroid disorders.

Deductible Choice: All insured individuals in the Dutch health insurance system choose their voluntary deductible level each year, which decreases their health insurance premium but increases their copayment toward healthcare costs if care is consumed. The voluntary deductible is added to the universal copayment component, which ranged from € 160 (2010) to € 385 (2022). The voluntary deductible can be chosen in € 100 increments from € 0 to € 500. Following Handel et al. (2024), we focus on whether individuals choose any non-zero deductible as a measure of voluntary exposure to healthcare costs and create a dummy for having a voluntary deductible > 0 in a given year.

Income, Income Rank, and Labor Market Status: Based on tax records and observed household compositions, we calculate the pre-tax per-capita income within households and translate this into age- and sex-specific income quintiles, defined within 5-year age bands, based on the entire income distribution for a given age-sex group in a given year. Measures of monthly individual income are calculated from annual pre-tax income levels, adjusted for inflation with official annual consumer price indices published by CBS. Using the main source of individual income as listed in the tax records, we create dummies for labor market participation, distinguishing employment, social benefit receipt (such as unemployment benefits), pensions, disability benefits, or any other source of income (for example, dividends and capital gains) as the main income source.

Highest Education Level: Health Monitor participants indicate their highest level of education as either low (*geen opleiding* and *lager opleiding*), lower-medium (*middelbaar algemeen voortgezet onderwijs* and *lager beroepsonderwijs*), upper-medium (*hoger algemeen voortgezet onderwijs*, *voorbereidend wetenschappelijk onderwijs*, and *middelbaar beroepsonderwijs*), or high (*hoger beroepsonderwijs* and *wetenschappelijk onderwijs*). We collapse these into two categories: lower educational attainment (combining low and lower-medium) and higher educational attainment (combining upper-medium and high).

Functional Limitations: Health Monitor participants indicate whether their health impacts their ability to perform each of seven day-to-day activities: i) *"being able to follow a conversation of three or more persons"*; ii) *"being able to have a conversation with one person"*; iii) *"being able to read the small print in newspapers"*; iv) *"being able to recognize someone's face at a distance of four meters or more"*; v) *"being able to carry 5 kg for 10 meters"*; vi) *"being able to reach the ground"*; and vii) *"being able to walk 400 meters without stopping"*. For each activity respondents indicate whether they are able to do these activities *"without difficulty"*, *"with some difficulty"*, *"with great difficulty"*, or *"not at all"*. We create a dummy for each activity encoding whether a given respondent has at least great difficulty performing it.

Mental Health: We translate responses to the 10-item Kessler mental health questionnaire (Kessler et al., 2002) into a binary measure indicating an elevated risk of depression or anxiety requiring further screening. Questionnaire items ask individuals to indicate their feelings in the past month, covering feelings of depression, anxiety, restlessness, tiredness, nervousness, and lack of energy. Individuals indicate the frequency of these feelings from *"None of the time"*, assigned a score of 1, to *"All of the time"*, assigned a score of 5. We create the total score ranging from 10 (all responses are *"None of the time"*) to 50 (all responses are *"All of the time"*) and translate it into a binary indicator for those individuals scoring 16 or above, indicating an elevated risk of depression or anxiety and requiring further medical examination by a mental health practitioner.

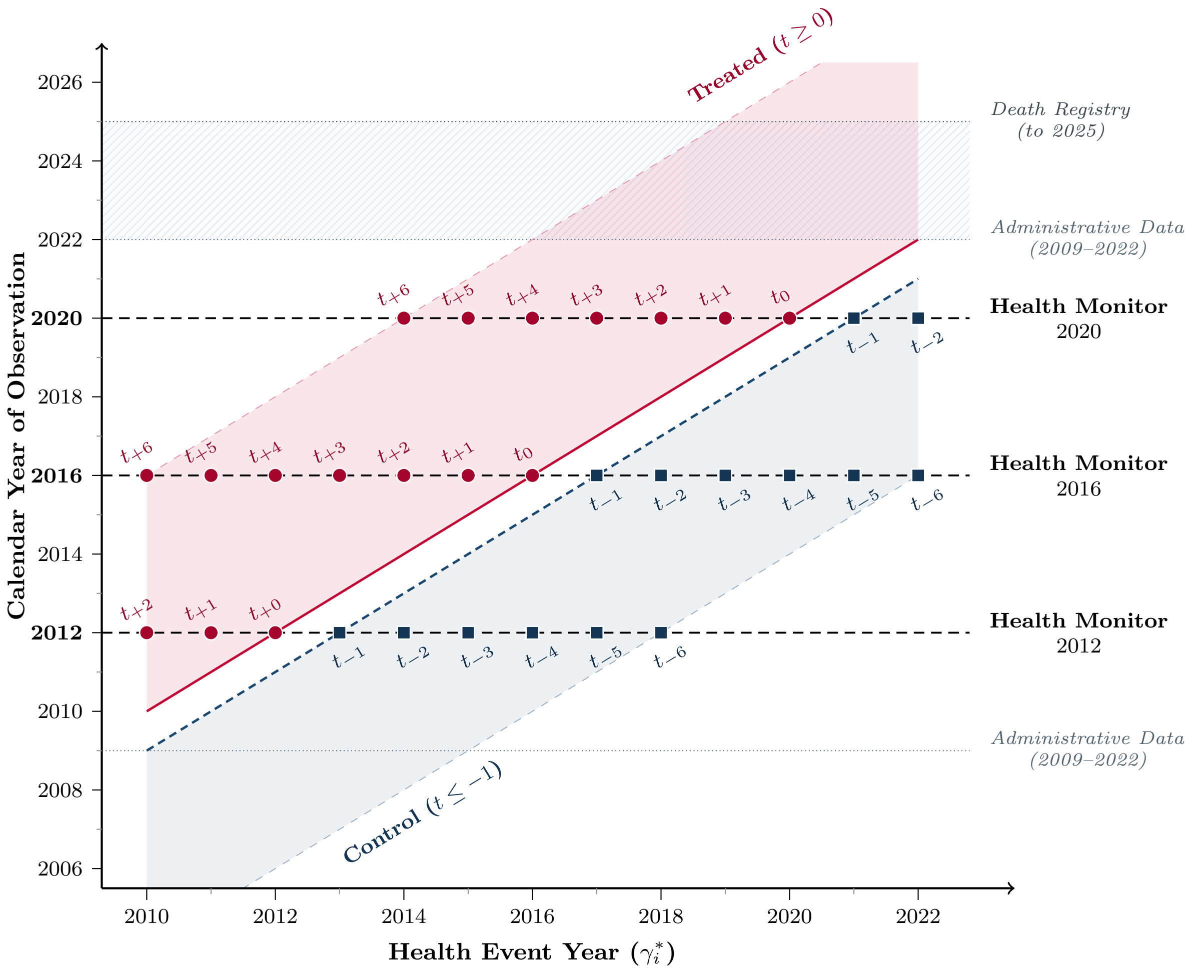
Locus of Control: We translate a 7-item questionnaire version of the Pearlin Mastery Scale (Pearlin and Schooler, 1978) into continuous and binary measures of locus of control. The seven items in the questionnaire are split into five questions on external control, i) *"I have little control over the things that happen to me"*; ii) *"Some of my problems I cannot possibly solve"*; iii) *"There is little I can do to change important things in my life"*; iv) *"I often feel helpless when dealing with life's problems"*; v) *"Sometimes I feel that I am a plaything of life"*; and two questions on internal control, vi) *"What happens to me in the future depends for the most part on myself"*; and vii) *"I can do almost anything if I set my mind to it"*. Each is answered on a 5-point scale ranging from *"Completely Agree"* to *"Completely Disagree"*. Items vi) and vii) are reverse-coded before calculating a total score ranging from 7 to 35, with higher scores indicating a strong internal locus of control (an individual has strong beliefs in their own ability to influence outcomes in their life) and lower scores indicating a strong external locus of control. We create a standardized score and a dummy for scores below 19, indicating low perceived control and an external locus of control.

D Methodological Details

D.I Time Structure

Our analysis sample divides individuals into treatment and control groups based on when they are surveyed relative to their health event. Event-time cohorts are determined by the calendar year in which individuals experience their first-time heart attack or stroke, γ_i^*, and the year of Health Monitor participation s_i (2012, 2016, or 2020). We define event time as $t_i = s_i - \gamma_i^*$. Figure D.1 illustrates this. Individuals surveyed after their event ($t_{\geq 0}$) form the treatment group; those surveyed before ($t_{\leq -1}$) form the control group. An individual whose health event is in 2014 ($\gamma_i^* = 2014$) participating in the 2012 Health Monitor ($s_i = 2012$) enters our control group as a member of event-time cohort t_{-2} (as survey year, 2012, and admission year, 2014, are two years apart). Had this individual participated in the 2016 or 2020 Health Monitors, they would have been assigned membership in the treatment group and event-time cohorts t_2 and t_6 respectively.

Figure D.1: Time Structure – Event-Time Cohorts by Survey and Event Years



Source: Own illustration. *Note:* Each marker represents an event-time cohort of first-time heart attack or stroke patients participating in one of three Health Monitor surveys (2012, 2016, or 2020), positioned by survey year (vertical axis) and health-event year γ_i^* (horizontal axis), with event time $t_i = s_i - \gamma_i^*$. Red circles above the solid line denote treated cohorts ($t \geq 0$, interviewed in or after the event year); blue squares below the dashed line denote control cohorts ($t \leq -1$, interviewed before the event). The shaded bands mark the estimation window $t \in [-6, +6]$ (red for treated, blue for control). Dotted horizontal lines delimit coverage of the longitudinal administrative data (2009–2022); the hatched band above (2022–2025) indicates the additional mortality follow-up available from the death registry.

While we can observe the date of the first admission, we cannot observe the date of survey response. To ensure that our event-time cohorts are nonetheless correctly divided into those interviewed before their respective health event and those interviewed thereafter, we exclude individuals if their first-time health event occurred within the four-month Health Monitor data collection window covering 1 September to 31 December of 2012, 2016, and 2020. We therefore remove individuals for whom the relative time to the health event and membership in the treatment ($t_{\geq 0}$) or control ($t_{\leq -1}$) group would be ambiguous.

We can construct event-time cohorts covering the full range determined by observable health events (1995–2022) and the survey years (2012, 2016, and 2020). However, we restrict our analysis sample to event-time cohorts t_{-6} to t_6 and to first-time admissions between 2010 and 2022, for several reasons. First, this allows us to observe longitudinal health information for multiple years before each event, ensuring we identify first-time (rather than recurrent) events. Second, our methodological approach conditions on differences in health at the time of the event. As our healthcare cost data starts in 2009, the 2010 cohort of patients is the first for which we can observe healthcare costs in the year before the event. Third, given the survey years of 2012, 2016, and 2020, and the 2010 to 2022 admission-year range, the 2016 survey contributes the most observations in our sample with a possible event-time cohort range of t_{-6} (survey participation in 2016, admission in 2022) to t_6 (survey participation in 2016, admission in 2010). We could observe event-time cohorts outside this range from t_{-10} (survey participation in 2012, event in 2022) to t_{10} (survey participation in 2020, event in 2010), but we impose this common event-time range so that each cohort in our sample is sourced from at least two adjacent Health Monitors.

D.II Estimation Procedure, Identifying Assumptions, and Sample Construction

We compare the behaviors of heart attack and stroke patients providing survey responses at different time points relative to their respective acute health event. Setups such as this, with time-varying treatment adoption (health event occurrence), are prone to producing biased dynamic treatment effect estimates when using standard event study designs that do not account for selection into treatment-timing groups (Goodman-Bacon, 2021). To address this, we rely on a "doubly robust" design (Callaway and Sant'Anna, 2021) to estimate weighted event study regressions. The "doubly robust" event study design extends the estimator by Sant'Anna and Zhao (2020), combining outcomes-regression (Heckman et al., 1997) and inverse-probability-weighting approaches to Difference-in-Differences designs (Abadie, 2005), to the case of time-varying treatment adoption and longitudinal or repeated cross-sectional data. The estimation procedure by Callaway and Sant'Anna (2021) consists of two steps. First, the estimation of generalized propensity score models capturing the likelihood of membership in a specific treatment-timing group, in our context the relative timing of health events. Second, an outcomes regression model extrapolating the counterfactual outcome evolution based on pre-treatment characteristics. It is labeled as "doubly robust" as it requires only one of the two — the propensity score model or the outcomes regression — to be correctly specified.

In our design, which compares patients interviewed at different time points relative to their respective acute health event, both steps have an intuitive interpretation. While we exploit the exogeneity in the timing of heart attacks and strokes (see similar designs such as Chandra and Staiger, 2007; Doyle, 2011; Fadlon and Nielsen, 2019), not all individuals are equally likely to experience an acute health event at the time they experience it. In other words, we want to avoid comparing individuals who were at high relative risk of a health event due to pre-existing risk factors with those at very low risk at the time the event occurred. The generalized propensity score model in our context therefore aims to capture these *differences in relative ex-ante (health) risk* based on observable characteristics at event time. As we compare individuals interviewed at the same time but experiencing a health event at different time points, we want to ensure that we observe a similar distribution of relative risk across the different treatment times (admission years) and between the treatment (interviewed after the event) and control groups (interviewed before the event).

We estimate the generalized propensity score models based on data on the entire population of first-time heart attack or stroke patients between 2010 and 2022 ($N = 96{,}021$) that underlies our subsample of patients who respond to the Health Monitor. We therefore include only individuals whose health event occurs at a certain age (60 to 70 for heart attacks and 65 to 75 for strokes), who survive for at least one year after the health event, and who do not experience a prior transient ischemic attack, do not live with an individual experiencing a heart attack or stroke, and have no nursing home admission prior to the event. We use the entire population of patients satisfying these criteria, as opposed to only the subgroup of Health Monitor participants, to ensure that our propensity score model is calibrated based on the actual relationship between observable characteristics and health event timing as observed in the true underlying patient population.

In practical terms, we estimate the individual-level likelihood of experiencing a heart attack or stroke in some base year γ^b, conditional on a set of observable characteristics X_i at this point in time and not yet having experienced the health event but eventually doing so:

$$Pr(\gamma_i^* = \gamma^b | X_{i,\gamma^b}, \gamma_i^* \geq \gamma^b) = \Lambda(\beta X_{i,\gamma^b})$$

where Λ is the logistic function and X_i contains a set of health and demographic characteristics. The underlying sample is always those individuals who by the time of the base year γ^b have not yet experienced their health event. To account for potential differences in the patient population and health risk profiles over time, we use three different base years, one for each of the three Health Monitor samples, thereby creating three distinct prediction models. For each Health Monitor, we choose the base year corresponding to event-time cohort t_2, the latest event-time cohort observable in all three surveys, i.e., $\gamma^b = 2010$ for 2012, $\gamma^b = 2014$ for 2016, and $\gamma^b = 2018$ for the 2020 Health Monitor.

The set of predictors X_i covers demographic, socioeconomic, and health characteristics. Demographic characteristics include single-year-of-age dummies, being female, and cohabiting with a partner. Socioeconomic status is measured using one-year-lagged income-quintile membership based on per-capita household income and dummies for the individual labor force participation categories. Health characteristics cover one- and two-year-lagged dummies for 21 distinct chronic conditions, the use of disease-specific hospital care across 17 categories, the total number of hospital visits and total length of stay across these visits, and one-year-lagged healthcare costs for hospital care, pharmacological care, and GP consultations. See the Data Appendix C.III for detailed definitions of all predictors.

Our prediction models yield three sets of estimated base-year-specific coefficients $\hat{\beta}^{\gamma^b}$. We use these coefficients to estimate propensity scores at the time of an individual's health event for our Health Monitor sample based on their observed characteristics at event time $X_{i,0}$, yielding an individual-specific estimated propensity score:

$$\hat{w}_i = \Lambda(\hat{\beta}^{\gamma^b} X_{i,0})$$

Propensity scores $\hat{w}_i$ thereby capture the likelihood of having experienced a health event (being treated) at the actual event-time rather than at a later time, at which point the individual would belong to the *not-yet-treated* control group. These scores therefore represent a measure of *relative risk* of belonging to an earlier health-event cohort, not *absolute risk* of having any event among the entire population, nor of survival conditional on having an event, although unsurprisingly there are overlapping predictive factors.[13] The specific set of coefficients applied to a given Health Monitor participant is chosen based on the respective base year chosen for their survey cohort. To check whether our results are robust to the choice of the base years, we have run all analyses using a fixed base year (independent of the survey-year cohort) or survey-year-specific alternative base years, with no meaningful impact on our results.[14]

[13]Five-year survival among the bottom decile of propensity scores in our Health Monitor sample is 80.10% against 85.64% among the top decile without any survival conditioning, and 87.92% (bottom decile) versus 90.36% (top decile) when conditioning on one-year survival, indicating that propensity scores also capture survival chances to some degree.

[14]Results are available upon request.

In the second step, following Callaway and Sant'Anna (2021), we use the estimated propensity scores $\hat{w}_i$ as inverse probability weights in event study regressions of the following form:

$$Y_i = \alpha + \sum_{e=-6, e\neq -1}^{6} \delta_e \mathbf{1}(t_i = e) + \beta X_{i,0}^r + \mu_s + \epsilon_i$$

These event study regressions contain covariates capturing differences in demographic, socioeconomic, and health characteristics at event time ($X_{i,0}^r$). These covariates are a subset of all predictors used in the estimation of propensity scores (X_i), covering demographic information (single-year-of-age dummies, sex, living with a partner), information on socioeconomic status the year before the health event (age- and sex-specific income quintile membership and labor market status), and health information in the year before the health event (chronic conditions, general and cardiovascular-specific hospital care use, and healthcare costs for hospital care, pharmacological care, and GP consultations). As our sample of Health Monitor participants is substantially smaller than the sample of all underlying heart attack and stroke patients used in the estimation of the generalized propensity score models, we opt for a more parsimonious set of health-related predictors. We replace dummies encoding disease-specific hospital care use with a single dummy for any cardiovascular-related hospital stays, the total number of hospital admissions and inpatient days, and a single lag for all chronic conditions. In addition to these, all regressions include survey-year dummies μ_s, the number of inpatient days associated with the individual's acute health event, and a dummy encoding whether the health event was a heart attack or stroke. To estimate the average treatment effect on the treated (ATT), we run identical regressions exchanging event-time dummies for a single indicator encoding post-treatment event-time cohort membership ($t_{\geq 0}$).

Identifying Assumptions

The "doubly robust" estimation approach by Callaway and Sant'Anna (2021) applied to repeated cross-sectional data requires six assumptions to hold. For each of these, we outline below its specific implications in our context and its plausibility.

Irreversibility of Treatment: A treated unit remains treated until the end of the observation period. In the context of this study, once an individual has experienced a health event, this experience is not forgotten. This assumption is very likely to be satisfied given the salience of the health events we consider, both of which are life-threatening medical emergencies which for our entire sample were associated with being treated in a hospital.

Limited Anticipation of Treatment: A treated unit cannot anticipate the exact onset of treatment. Our entire analysis sample only contains individuals who all eventually experience similar health events in the form of ischemic heart attacks and strokes. The exact timing of these events is inherently unpredictable, and it is this exogeneity in the timing of events, not whether an individual experienced the event at all, that we exploit. In addition, we include only those individuals who have not been previously exposed to such an event or similar health shocks, either directly or by proxy through an occurrence within their family.

Conditional Parallel Trends: Treated and control units (in our case not-yet-treated) would have followed the same outcome trajectory in the absence of treatment once conditioned on the pre-treatment characteristics. In our context this assumption implies that individuals in the different observed event-time groups would have followed a similar trajectory with respect to their health-related behaviors if the health event had not occurred. While this assumption is inherently untestable, as it would require the observation of the true counterfactual, its plausibility can be examined, as one can directly compare pre-trends between the treated and control groups.

Overlap and Common Support: For any combination of observed covariates, there is a non-zero probability of being treated or untreated. In addition, there is a large overlap in the distribution of estimated propensity scores between treated and untreated units. In the context of our study, this means we need an overlap between characteristics before the health event and the estimated propensity scores across the different event-time cohorts that define our treatment and control groups. As outlined in detail below, we observe a highly comparable set of pre-event characteristics (see Table D.2) and propensity score distributions (see Figure D.2) across all event-time cohorts and between the treatment and control groups. Furthermore, we exclude only 0.79% of our sample for whom propensity scores can be estimated, due to a score being outside the area of common support.

Stable Unit Treatment Value Assumption: All units receive the same treatment dose and treatments cannot spill over; that is, one individual's treatment does not affect another individual's outcomes. Spillovers are ruled out in our context as we consider only households in which a single individual experiences a health event. Further, the considered health events can occur independently of each other, so that one individual's treatment status cannot crowd out that of others.

Concerns around the stability of the treatment dose, the intensity of heart attacks or strokes across different admission years, are theoretically warranted. For example, the impact of a health event could change over time if medical technology or follow-up guidelines for physicians improve the management of post-event health trajectories. In this case, individuals exposed to a health event in different years might also be impacted differently. While theoretically possible, such variation is unlikely to matter in this context. Both heart attacks and strokes have seen significant decreases in mortality and the number of inpatient days between 1995 and 2022, but decreases during the period of 2010 to 2022 that we focus on are considerably smaller. Mortality decreased from 8.18% (2010) to 6.58% (2022) for all heart attack and stroke patients (heart attack mortality decreased from 6.28% to 3.52%, stroke mortality from 11.47% to 9.70%). The associated inpatient days also decreased only slightly, from 5.68 days to 4.07 days (for heart attacks the change was from 5.13 to 3.91 and for strokes from 6.61 to 4.24). All our regressions account for differences in inpatient days, thereby already controlling for some of these dynamics, while we also have, by construction, a high overlap in event-time cohorts and admission years across the Health Monitors (see Table D.1). In addition, we directly explored how differential survival dynamics across treatment and control groups would potentially impact our results, finding that differences would have to be unrealistically large given the magnitude of the effects we identify (see Section D.III).

Absence of Compositional Changes: This assumption applies when cross-sectional data are used and ensures the comparability of samples of treated units over time. In our context, it implies that admission/ event-time cohorts show no significant drift in their pre-event characteristics that could render observed effect estimates attributable to compositional changes. We make this assumption plausible by constraining our sample to a group of comparable individuals, as we condition on a baseline survival corridor of one year (which is expanded in the robustness checks) and a 5-year age band around the observed mean age-at-onset of the health event within that period. Conditional on age and minimum post-event survival, we observe no strong differences in key characteristics before the onset of the health event that could explain our results (see Table D.2).

Analysis Sample Construction

The total number of patients satisfying the general inclusion criteria (no previous exposure and age-at-onset within the 5-year bands around the patient population mean) whose health event occurs between 2010 and 2022 and who are observable in the Health Monitor surveys is 14,998 (7,882 heart attack and 7,116 stroke patients). After imposing the event-time range of t_{-6} to t_6, this drops to 12,900 (6,733 heart attack and 6,167 stroke patients). For a further 690 individuals no propensity score can be estimated, due to missing information on any of the considered predictors. This decreases the potential sample size to 12,210, or 94.65% (6,701 heart attack and 5,509 stroke patients), before conditioning on common support, survival, and the availability of all outcomes.

A core assumption necessary for the approach by Callaway and Sant'Anna (2021) is the presence of common support and covariate overlap. In our context, this means that, across the different event-time cohorts, we want to observe similar distributions of both the propensity scores and the pre-health-event characteristics. This ensures that for each individual in our treatment group ($t_{\geq 0}$), we can identify a comparable individual in the control group ($t_{\leq -1}$) with a similar propensity score and covariate combination. To make this assumption more credible, we follow common practice in the application of matching estimators by imposing a minimum-maximum restriction (see, for example, Caliendo and Kopeinig, 2008; Stuart et al., 2013). This means we exclude individuals in the treatment group if their estimated propensity score is larger than the observed maximum in the control group and those in the control group if their estimated propensity score is smaller than the minimum observed in the treatment group. Doing so results in 97 individuals, or 0.79% of the 12,210 for whom the propensity score can be estimated, being excluded from the analysis sample.

We further exclude 361 individuals who die within one year of their admission and 396 individuals whose health event falls within the Health Monitor survey period, for whom we therefore cannot ascertain whether they were interviewed before or after their health event. Lastly, 1,691 individuals are dropped as not all outcomes are observable. The resulting baseline sample contains 9,665 unique individuals, of whom 5,475 experienced a heart attack and 4,190 experienced a stroke. Table D.1 provides a breakdown of the sample by event-time cohort and Health Monitor year.

Table D.1: Event-Time Cohorts by Health Monitor and Types of Health Events

Panel A: All Health Events

	Time to Health Event in Years													
	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	**All**
Survey Year														
2012	247	250	283	286	295	226	146	234	210					2,177
2016	245	276	308	370	431	421	317	457	414	292	294	269	227	4,321
2020					330	388	228	392	402	382	359	358	328	3,167
Pooled	492	526	591	656	1,056	1,035	691	1,083	1,026	674	653	627	555	9,665

Panel B: Heart Attacks

	Time to Health Event in Years													
	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	**All**
Survey Year														
2012	111	98	145	160	153	127	100	134	139					1,167
2016	118	126	131	173	183	219	168	287	258	197	211	181	168	2,420
2020					165	202	111	233	221	246	226	248	236	1,888
Pooled	229	224	276	333	501	548	379	654	618	443	437	429	404	5,475

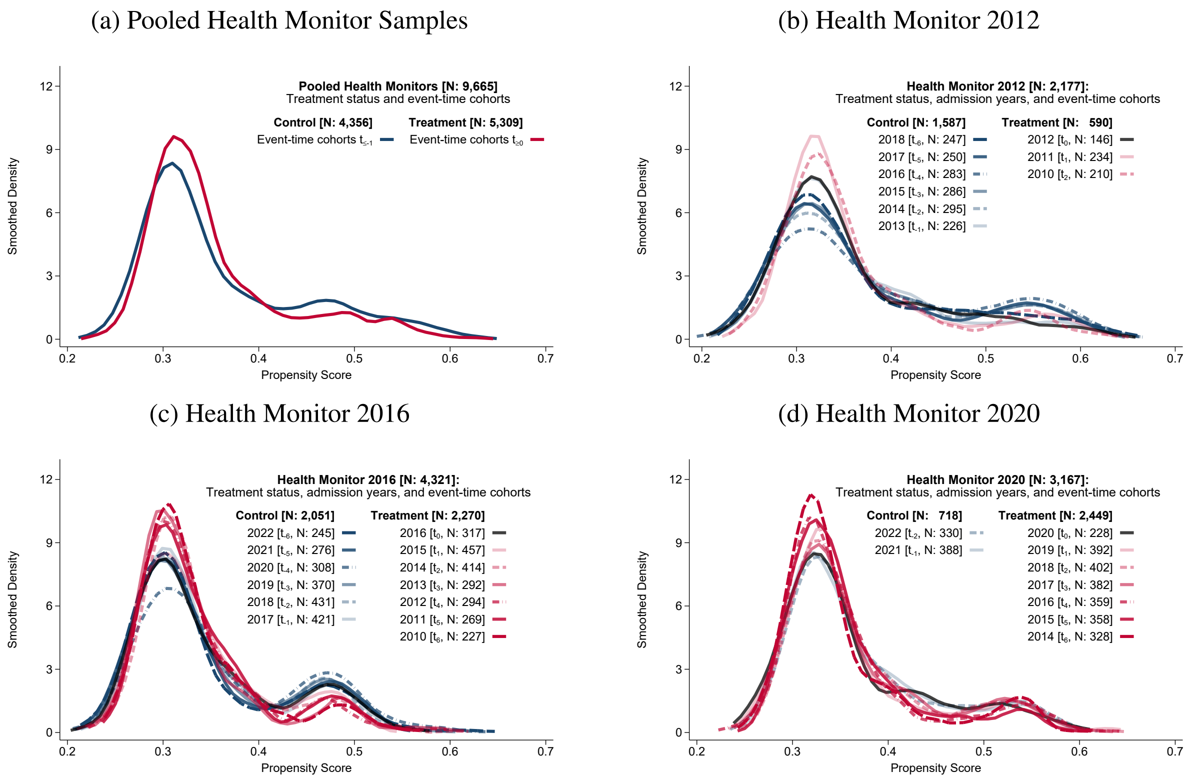
Panel C: Strokes

	Time to Health Event in Years													
	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	**All**
Survey Year														
2012	136	152	138	126	142	99	46	100	71					1,010
2016	127	150	177	197	248	202	149	170	156	95	83	88	59	1,901
2020					165	186	117	159	181	136	133	110	92	1,279
Pooled	263	302	315	323	555	487	312	429	408	231	216	198	151	4,190

Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted is the number of unique individuals in our analysis sample of first-time heart attack and stroke patients between 2010 and 2022 participating in the Health Monitor for whom propensity scores can be estimated, whose event does not occur during the survey collection period, who survive for at least one year after the event, and who provide information on all outcomes considered.

Figure D.2 depicts the propensity score distributions for the pooled Health Monitor sample, split into treatment and control groups (panel **(a)**), and separately for each Health Monitor by event-time cohort (panels **(b)** to **(d)**). The propensity score distributions are highly comparable both when comparing treatment and control groups in general and when splitting the sample by survey year and event-time cohort.

Figure D.2: Propensity Score Distributions



Source: Own illustration based on data from Statistics Netherlands. *Note:* Depicted are the propensity score distributions based on estimated propensity scores using observed demographic, socioeconomic, and health characteristics at event time for our analysis sample of first-time heart attack and stroke patients between 2010 and 2022 participating in the Health Monitor. Panel **(a)** depicts the pooled sample ($N = 9{,}665$) split into control (blue) and treatment (red) groups. The individual Health Monitor survey years split by event-time cohorts are depicted in panel **(b)**, 2012 Health Monitor ($N = 2{,}177$), panel **(c)**, 2016 Health Monitor ($N = 4{,}321$), and panel **(d)**, 2020 Health Monitor ($N = 3{,}167$), with blue-shaded lines depicting control group cohorts ($t_{\leq -1}$) and gray/red-shaded lines depicting treatment group cohorts ($t_{\geq 0}$).

Table 1 in the main body provides an overview of the pre-health-event characteristics that we condition on and the sample characteristics at the time of the health event for our analysis sample across all health events and by type of health event. Table D.2 reports the unweighted sample means across the same pre-event characteristics but split by event-time groups. Following the recommendation of Stuart et al. (2013), we compute a measure of standardized differences between event-time cohorts. This is done by comparing event-time cohort t_{-1} (the reference cohort in all event study regressions) against all other cohorts and using the weighted means and standard deviations based on the estimated propensity scores. Bold-print and a $*$ indicate whether for a given covariate the absolute standardized difference exceeds the recommended 0.25 threshold. We observe very little imbalance, entirely limited to the age-at-onset for event-time cohorts t_3 to t_6.

Table D.2: Unweighted Pre-Event Characteristics by Event-Time Cohort

	Time to Health Event in Years												
	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6
Health Event Characteristics													
Days in Hospital	3.76	4.07	3.84	4.12	4.15	4.25	4.04	4.51	4.55	4.41	4.20	4.43	4.74
Demographic Information													
Age at Onset	68.37	68.67	68.59	68.31	68.24	67.88	67.61	67.24	67.26	**66.75***	**66.50***	**66.26***	**66.11***
Female	0.34	0.35	0.29	0.33	0.33	0.32	0.29	0.29	0.31	0.27	0.25	0.28	0.27
Co-habiting Partner	0.77	0.77	0.79	0.77	0.78	0.75	0.79	0.79	0.79	0.80	0.79	0.81	0.79
Socioeconomic Status *(in the year before the health event)*													
Employed	0.26	0.21	0.20	0.19	0.19	0.22	0.23	0.24	0.21	0.26	0.27	0.25	0.27
Social Benefits	0.04	0.02	0.02	0.03	0.02	0.02	0.03	0.02	0.02	0.03	0.04	0.04	0.03
Retired	0.66	0.72	0.72	0.73	0.73	0.70	0.69	0.67	0.70	0.63	0.62	0.62	0.63
Disability Benefits	0.03	0.03	0.04	0.04	0.04	0.04	0.03	0.05	0.04	0.05	0.04	0.04	0.05
Other Source of Income	0.01	0.02	0.02	0.02	0.01	0.02	0.02	0.02	0.02	0.02	0.02	0.04	0.03
Per-capita HH Income (€ 1000s)	3.02	3.10	2.92	2.80	2.96	2.98	2.89	3.00	2.93	2.70	2.93	3.01	3.01
1^{st} Income Quintile (lowest)	0.16	0.15	0.17	0.18	0.16	0.16	0.18	0.16	0.17	0.18	0.16	0.16	0.14
2^{nd} Income Quintile	0.23	0.19	0.24	0.21	0.22	0.21	0.20	0.21	0.20	0.21	0.21	0.21	0.22
3^{rd} Income Quintile	0.21	0.22	0.23	0.23	0.20	0.22	0.22	0.21	0.20	0.20	0.22	0.24	0.21
4^{th} Income Quintile	0.21	0.22	0.18	0.20	0.22	0.22	0.20	0.22	0.22	0.21	0.22	0.19	0.23
5^{th} Income Quintile (highest)	0.19	0.22	0.20	0.18	0.20	0.19	0.20	0.20	0.21	0.21	0.19	0.21	0.20
Health Status *(in the year before the health event)*													
Hospitalizations (CVD-related)	0.09	0.04	0.05	0.04	0.05	0.03	0.08	0.04	0.04	0.05	0.06	0.04	0.06
Inpatient Days (CVD-related)	0.29	0.09	0.11	0.08	0.10	0.08	0.21	0.15	0.08	0.12	0.14	0.09	0.14
Hospitalizations (Non-CVD)	0.15	0.13	0.14	0.15	0.16	0.12	0.17	0.14	0.17	0.12	0.14	0.15	0.17
Inpatient Days (Non-CVD)	0.29	0.10	0.35	0.22	0.24	0.19	0.28	0.31	0.26	0.18	0.29	0.18	0.35
Acid-related Disorder	0.33	0.33	0.29	0.26	0.32	0.29	0.30	0.30	0.27	0.27	0.30	0.28	0.25
Hypertension	0.51	0.51	0.51	0.54	0.49	0.46	0.51	0.46	0.47	0.50	0.45	0.43	0.46
Diabetes	0.16	0.16	0.13	0.15	0.13	0.13	0.17	0.11	0.13	0.13	0.11	0.14	0.09
Hyperlipidemia	0.32	0.33	0.32	0.40	0.33	0.32	0.36	0.31	0.32	0.34	0.30	0.30	0.28
Depression/Anxiety	0.11	0.09	0.08	0.07	0.10	0.07	0.10	0.08	0.08	0.09	0.07	0.07	0.08
Hospital Care Costs (€ 1000s)	2.60	2.11	2.63	2.10	2.51	2.22	2.75	2.41	2.15	1.78	1.89	1.95	2.19
Pharmacology Costs (€ 1000s)	0.52	0.57	0.66	0.54	0.59	0.49	0.60	0.48	0.52	0.48	0.54	0.58	0.56
GP-visit Costs (€ 1000s)	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.05	0.06	0.06
Observations	492	526	591	656	1,056	1,035	691	1,083	1,026	674	653	627	555

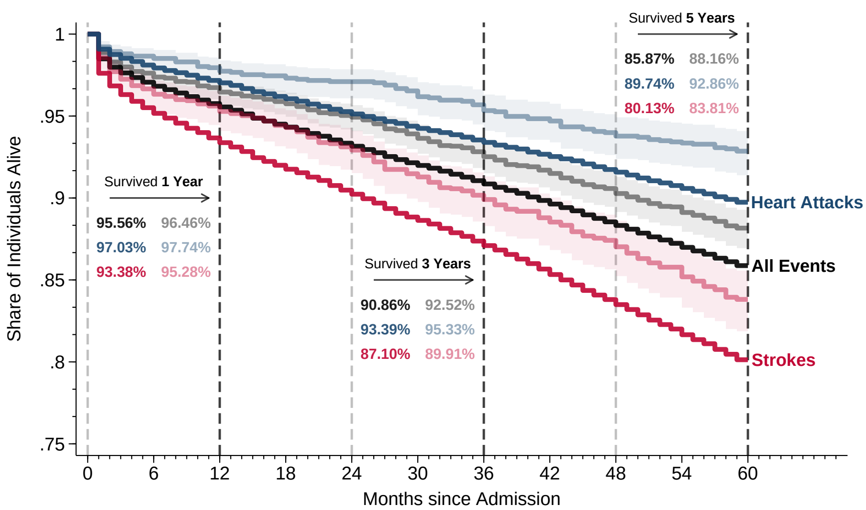
Source: Own calculations based on data from Statistics Netherlands. *Note:* Depicted are the unweighted means rounded to the second decimal for variables included in all event-study regressions for our analysis sample of first-time heart attack and stroke patients between 2010 and 2022 participating in the Health Monitor. **Bold-printed** values with a * indicate a standardized difference > 0.25, therefore exceeding the recommended threshold and indicating covariate imbalance (Stuart et al., 2013). Standardized differences are computed by comparing a given event-time cohort against the reference cohort t_{-1}. They are calculated using the formula $\frac{\bar{X}_{t_{-1}} - \bar{X}_{t_e}}{\sqrt{(s^2_{t_{-1}} + s^2_{t_e})/2}}$ where $\bar{X}$ is the sample mean and s the sample standard deviation when weighting observations using the inverse-probability weights derived from estimated propensity scores.

D.III Survival Dynamics and Sensitivity to Selective Non-Observation

External Validity: Health Monitor Selection

As our analysis dataset is constructed from a pool of Health Monitor participants, the sample of heart attack and stroke patients among them might not be representative of the underlying population of patients. To explore whether this is the case, we plot in Figure D.3 unconditional five-year Kaplan-Meier survival curves for the non-surveyed and surveyed populations of heart attack and stroke patients. The underlying population for these curves consists of first-time admissions between 1 January 2010 and 13 February 2020 for whom survival is observable for five years after the event, without any prior exposure to an acute cardiovascular event directly or by proxy, not residing in a nursing home before their event, and aged between 60 and 70 (heart attacks) or 65 and 75 (strokes) at the time of admission. Individuals not participating in any Health Monitor ($N = 68{,}097$) are depicted in full color intensity with all events colored in black, heart attacks ($N = 40{,}681$) in blue, and strokes ($N = 27{,}416$) in red. Survival curves for patients participating in the Health Monitor before their event (All events $N = 2{,}940$; heart attacks $N = 1{,}414$; strokes $N = 1{,}526$) are depicted in the same color but at lower intensity with their respective 95% confidence bands.

Figure D.3: Kaplan-Meier Survival Curves by Health Event Type and Survey Participation



Source: Own calculations based on data from Statistics Netherlands. *Note:* Displayed are estimated survival probabilities based on a Kaplan-Meier survival function. The underlying population consists of all first-time heart attack and stroke patients between 1 January 2010 and 13 February 2020. We exclude patients who previously experienced a transient ischemic attack, those cohabiting with another individual previously or subsequently experiencing a severe cardiovascular event, those living in a nursing home before their admission, and those not aged between 60 and 70 (heart attacks) or 65 and 75 (strokes) at the time of their event. Survival curves are calculated separately for individuals not participating in the Health Monitor (all events (black), heart attacks (blue), and strokes (red)), and participants in the Health Monitor (lower intensity color), with shaded areas indicating the 95% confidence interval.

There are significant differences in survival between surveyed and non-surveyed populations. Among non-surveyed patients, 95.56% survive for one year, compared to 96.46% among pre-event survey participants, translating into a 25.42% higher mortality rate among the non-surveyed population. This gap closes over time, narrowing to 22.19% after three years and 19.34% after five years. For heart attacks, the relative mortality gaps are 31.42% (one-year), 41.54% (three-year), and 43.70% (five-year), and for strokes 40.25% (one-year), 27.85% (three-year), and 22.73% (five-year). Therefore, the sample of patients in the Health Monitor is likely drawn from the healthier part of the underlying patient population.

The positive selection of Health Monitor participants relative to the underlying patient population affects the external validity of our estimates, as our findings might not fully generalize to the broader patient population. However, while we do observe significant differences in unconditional mortality trajectories, our analyses condition on a rich set of pre-event health information which accounts for relevant observable factors likely impacting survival. Furthermore, we also condition on a minimum post-event survival period of one year, which should capture additional unobservable determinants of survival. When comparing five-year survival trajectories among all patients who survived to the initial one-year threshold, the patient and survey populations have near-identical trajectories. Of those never surveyed but surviving for at least one year, 89.87% survive the full five years ($N = 65{,}072$); among those surveyed before their event, 91.40% survive the full five years ($N = 2{,}836$); and among those interviewed after their event but within one year of admission, conditional on survival to one year, 89.70% survive the full five years ($N = 494$).[15] As we are interested in studying behavioral change following the experience of an acute health event, initial survival is a necessary precondition for any change to have occurred. Therefore, we are confident that the Health Monitor survey sample we construct is representative of the underlying patient population of interest.

Internal Validity: Sensitivity to Differential Non-Observation and Survival Conditioning

Our repeated cross-section allows us to observe each individual only once. Due to this, the more relevant question regarding (survival) selection relates to its impact on the internal validity of our estimates via differential non-observation, meaning that the likelihood of being observed across event-time cohorts *within* our analysis sample is correlated with our outcomes of interest. For example, smoking could impact long-term survival prospects in addition to other factors we account for, such as health at event time. If *pre-event* smoking lowers survival, we would mechanically observe fewer individuals who had been smokers before the event in our post-event survey cohorts (treatment group), inflating the estimated ATT in absolute magnitude irrespective of whether smoking prevalence actually decreased. Similarly, if *post-event* smoking affects survival, this would bias our results, as survey respondents in later event-time cohorts are increasingly drawn from the non-smoking population, whether they never smoked or stopped smoking, also inflating the ATT and dynamic treatment effect estimates. Differential non-observation could also stem from factors not related to survival. Pre- or post-event smoking status could raise levels of incapacity and thereby reduce the ability to respond, raise the likelihood of being institutionalized and thereby of not being survey-eligible (i.e., nursing home entry[16]), or reduce the willingness to respond to a health survey due to stigma associated with continued engagement in high-risk behaviors. To address these concerns, we consider two complementary approaches: a sensitivity analysis quantifying plausible bias from differential non-observation, and a re-estimation conditioning on alternative survival windows.[17]

Sensitivity to Differential Non-Observation: To assess whether differential non-observation could meaningfully explain our results, the magnitude of the necessary aggregate mechanical shift is more important than the exact channel by which it occurs. Therefore, rather than separately modeling lifestyle-dependent mortality, incapacity, or survey non-response, we focus on total differential non-observation represented by a single quantity ρ to conduct a bounding exercise illustrating the sensitivity of our results to varying assumptions about its magnitude. As lifestyle-dependent survival might be the most potent of these channels, and as we observe survival outcomes, we use survival-related non-observation as a benchmark for a very conservative assumption on the degree of total and survival-only differential non-observation.[18]

[15] Due to disclosure restrictions by Statistics Netherlands we cannot present the equivalent to Figure D.3 for these populations as small cell counts on month-to-month changes risk identification.

[16] Throughout we condition on not having been admitted to a nursing home before the event. Post-event nursing home entry remains a possible channel of differential non-observation, captured by ρ.

[17] For smoking we have a direct proxy measure for potential non-response as the Health Monitor also asks whether individuals have ever smoked in the past. We find no effect of a heart attack or stroke on the share of never-smokers in our sample, further supporting the view that differential non-response cannot explain our results. Full results of this are available upon request.

[18] As individuals need to consent to their linkage across Health Monitors and CBS microdata, we cannot directly explore non-response as by definition we cannot identify individuals invited to the Health Monitor but not responding or not consenting to microdata linkage.

Define $\rho = R_1/R_0$ as the relative probability of being observed in the post-event sample for individuals engaging in a behavior (R_1), such as smoking, versus those not engaging in it (R_0). When $\rho = 1$, there is no differential non-observation and our recovered ATT is unbiased and requires no adjustment. When $\rho < 1$, individuals with the unhealthy behavior are less likely to be observed, inflating the apparent treatment effect toward a lower prevalence of risky behaviors. When $\rho > 1$, the opposite occurs, and individuals engaging in unhealthy behaviors would be more likely to be observed, biasing us against finding evidence for increased preventive behavior. Given the observed post-event prevalence $P_{\text{obs}} = P_{\text{pre}} + \widehat{\text{ATT}}$, where both P_{pre} and $\widehat{\text{ATT}}$ are measured within the sample observable for a full potential survival period of five years, so that P_{obs} is the prevalence actually observed in that sample, the true post-event prevalence under an assumed ρ is recovered as:

$$P_{\text{true}}(\rho) = \frac{P_{\text{obs}}}{P_{\text{obs}} + \rho \cdot (1 - P_{\text{obs}})} \tag{1}$$

and the adjusted treatment effect is $\widehat{\text{ATT}}_{\text{adj}}(\rho) = P_{\text{true}}(\rho) - P_{\text{pre}}$. The breakdown point ρ^*, at which the adjusted ATT equals zero, is:[19]

$$\rho^* = \frac{P_{\text{obs}} \cdot (1 - P_{\text{pre}})}{P_{\text{pre}} \cdot (1 - P_{\text{obs}})} \tag{2}$$

We use observed five-year survival ratios S_1/S_0 by lifestyle as reference values for ρ, representing the special case in which mortality is the *only* source of differential non-observation and in which *observable mortality differences* across behaviors are *entirely causal*. If non-mortality channels (incapacitation, survey non-response) reinforce mortality selection, actual ρ would be lower than the survival ratio; the distance from the survival ratio to the breakdown point ρ^* therefore indicates the margin by which additional differential non-observation would need to exceed observed mortality differentials to overturn the estimated effects. This benchmark is conservative with respect to the role of selective survival: it treats observable survival differences as entirely causal, whereas our regressions already condition on pre-event health characteristics that are likely to account for much of the survival variation.

Besides smoking status, our lifestyle outcomes are measured using continuous variables. We therefore translate alcohol consumption, physical activity, and weight management into binary versions, coded so that the coefficients share a consistent interpretational direction, to calculate differences between low- and high-risk lifestyles. These are excessive drinking (weekly drinks ≥ 14 for males or ≥ 7 for females), physical inactivity (< 3 days a week with 30 minutes of moderate/intense physical activity), and overweight (BMI ≥ 25). Tables A.5 to A.7 report all results for these outcomes, replicating our main lifestyle findings.

Figure D.4 presents the results. Panel **(a)** plots for each behavior the five-year survival ratios separately for the pre-event sample using filled dots ($N = 3{,}029$) and the post-event sample using hollow dots ($N = 504$)[20], alongside adjusted ATT estimates and ρ^*, the point at which differential observation ratios would be sufficient for the adjusted ATT to equal zero, shown as diamonds. For smoking, five-year survival rates are 87.3% among non-smokers versus 82.2% among smokers in the pre-event sample (ratio: 0.942) and 91.7% versus 76.1% in the post-event sample (ratio: 0.830). With a pre-event smoking prevalence $P_{\text{pre}} = 0.269$ in this sample and the smoking effect estimated in the same five-year-observable sample, $\widehat{\text{ATT}} = -0.110$, the observed post-event prevalence is $P_{\text{obs}} = P_{\text{pre}} + \widehat{\text{ATT}} = 0.159$. Even under the more extreme post-event survival ratio, the adjusted ATT remains large at -0.083 with $\rho^* = 0.52$. In other

[19] By Bayes' theorem, individuals engaging in the behavior are observed with probability R_1 and those not engaging with probability R_0. The observed prevalence for a true prevalence P_{true} satisfies $P_{\text{obs}} = \rho P_{\text{true}} / \left(\rho P_{\text{true}} + (1 - P_{\text{true}})\right)$ where $\rho = R_1/R_0$. Multiplying through by the denominator and rearranging yields $P_{\text{obs}}(1 - P_{\text{true}}) = \rho\, P_{\text{true}}(1 - P_{\text{obs}})$, which solves directly to Equation (1). The breakdown point Equation (2) follows by setting $\widehat{\text{ATT}}_{\text{adj}}(\rho^*) = 0$ and solving for ρ^*.

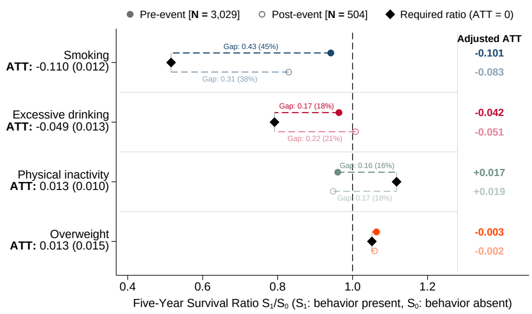
[20] The samples in the bounding exercise ($N_{\text{pre}} = 3{,}029$; $N_{\text{post}} = 504$) differ slightly from the Kaplan-Meier comparison ($N_{\text{pre}} = 2{,}836$; $N_{\text{post}} = 494$) as the Kaplan-Meier survival function uses exact admission dates to determine the five-year observability window while the bounding exercise determines eligibility at the admission month-year level, producing marginally different inclusion at the boundary.

words, to fully explain the smoking effect through differential non-observation alone, smokers would need to appear in the post-event sample at roughly half the rate of non-smokers, equivalent to 48% of smokers being differentially missing. Or, if the post-event survival difference is taken as entirely causal, smokers would need to be observed at 63% of the rate of non-smokers ($0.52/0.83 \approx 0.63$), or 37% less likely to respond, *after* taking into account very conservative survival effects. For excessive drinking, $\rho^* = 0.79$, requiring 21% of excessive drinkers to be differentially missing. For physical inactivity and overweight, where effects are small and insignificant, observed survival ratios are close to ρ^*.

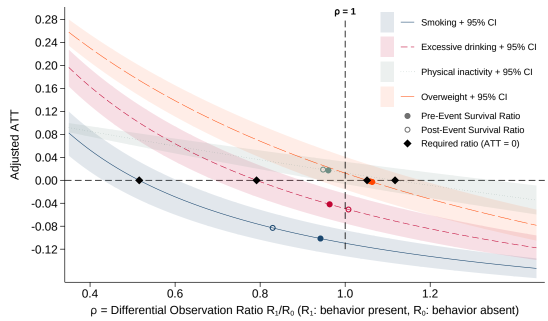
Panel **(b)** extends this to a general bounding exercise, tracing $\widehat{\text{ATT}}_{\text{adj}}(\rho)$ for each behavior. Shaded bands indicate 95% confidence intervals $\widehat{\text{ATT}}_{\text{adj}}(\rho) \pm 1.96 \times \text{SE}$. The observed survival ratios are marked on the curves as empirical reference points using filled (pre-event) and hollow (post-event) dots, representing ρ under the assumption that mortality is the only channel and that observable survival differences are entirely causal, a very high bar given that our actual effect estimates already take into account pre-event health differences. The curves show that the effects on smoking and excessive alcohol consumption remain large and statistically significant well beyond the range of observed survival differentials, and beyond even substantial departures from those ratios that would incorporate non-mortality channels.

Figure D.4: Sensitivity of Treatment Effects to Selective Non-Observation

(a) Survival Differences and Adjusted ATTs



(b) ATT Sensitivity to Differential Non-Observation



Source: Own calculations based on data from Statistics Netherlands. *Note:* Panel **(a)** depicts observed five-year survival ratios S_1/S_0 comparing individuals engaging in each behavior to those not, separately for the pre-event sample (filled dots, $N = 3{,}029$) and post-event sample (hollow dots, $N = 504$), alongside the differential observation ratio ρ^* required to fully explain the estimated average treatment effect on the treated (ATT) through differential non-observation alone (diamonds). Panel **(b)** traces $\widehat{\text{ATT}}_{\text{adj}}(\rho)$, the estimated ATT as a function of ρ, the relative probability of being observed in the post-event sample for individuals with the behavior relative to those without, combining all channels of differential non-observation. Observed survival ratios from panel (a) are marked as empirical reference points representing ρ under mortality selection only (filled dots: pre-event; hollow dots: post-event). Shaded bands indicate 95% confidence intervals $\widehat{\text{ATT}}_{\text{adj}}(\rho) \pm 1.96 \times \text{SE}$. The adjusted ATTs and ρ^* are computed using the ATT estimated within this same five-year-observable sample (e.g. -0.110 for smoking), consistent with the sample in which P_{pre} and the survival ratios are measured; this within-sample estimate differs from the baseline ATT reported in the main text (e.g. -0.095 for smoking) owing to differences in sample composition (see the survival-window discussion below).

Survival Window Conditioning: Our sensitivity analysis above relies on observed survival differences without adjusting for latent differences in health that are already controlled for in our regressions. As a complementary test, we directly examine how imposing longer survival windows affects our regression results. We estimate all regressions conditioning on three- and five-year survival, comparing results within the same sample under one-year baseline conditioning versus the longer survival requirement. Not all individuals have an observable three- or five-year survival window. The within-sample comparison therefore ensures that any differences are attributable to survival-related attrition, not differences in the composition of admission-year cohorts with differential observable survival horizons.

If mortality driven by *pre-event* or *post-event* lifestyle (or lifestyle change) were an important contributor to our results, conditioning on longer-term survival would decrease prevalence among those interviewed before the event while effect estimates would gradually increase, indicating positive selection for individuals engaging in low-risk lifestyles over time. We observe neither pattern. For smoking, the share of smokers among the t_{-1} event-time cohort is indistinguishable between the one-year and five-year survival conditioning within both the three-year and five-year observable samples (Table A.1 columns (18) to (21)). Likewise, our results are virtually indistinguishable across the imposed survival windows, as estimated ATT and dynamic treatment effects are numerically close and show a similar, stable pattern over time. Wald equivalence tests indicate that the small numerical differences are not statistically significant, or, where they are, that they remain small in magnitude. The pattern is shared across all lifestyle outcomes. Importantly, while absolute ATT estimates for the change in smoking prevalence in the five-year observable sample are at times numerically larger than in the baseline sample (e.g., -0.095 for smoking in our baseline analysis vs. -0.110 in the five-year observable sample when conditioning on one-year survival and -0.113 when conditioning on five-year survival), this reflects differences in sample composition rather than survival-driven attenuation. The baseline smoking rate in the five-year sample is correspondingly higher, as individuals are sourced from event-time cohorts with earlier events in calendar time and are therefore affected by general trends toward lower levels of smoking conditional on age. That said, relative effects are similar at $\approx 40\%$ of the pre-event prevalence across all imposed survival windows.