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# Health Effects of a Welfare Benefit Cut among Low-Income Retired Migrants*

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**Abstract**

This paper examines the causal health effects of a welfare reform that reduced benefits for low-income retirees with a migration background. The reform created a permanent negative income shock, determined by the number of adult co-residents. Using detailed administrative data and a difference-in-differences design, we analyze the impact of this reform on medication use for stress- and lifestyle-related conditions over a seven-year post-reform period. The welfare cut reduced average personal income by 13 percent. In the longer term, the reform increased medication use for lifestyle-related conditions by 1.8–2.4 percentage points, while we find no significant effects on stress-related conditions. Additional analysis suggests an increase in mortality of 8.1 percent. Our results provide insights into the health disparities between low- and high-income populations.

*JEL-codes:* H55, H75, I38, I12, I14

*Keywords:* Welfare cut, health, low-income retirees, migrants, difference-in-differences

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# 1 Introduction

Developed countries face persistent health disparities both between low- and high-income individuals (e.g. Chetty et al., 2016; Marmot, 2015; WHO, 2019) and between migrant and native populations (e.g. Agyemang et al., 2024; Ikram et al., 2016). A key driver of these disparities is the faster progression of chronic conditions over the life cycle among low-income and migrant groups (Danesh et al., 2024; Jones et al., 2019). Understanding the mechanisms underlying this pattern is essential for designing effective policies. A central policy debate in this context is the role of income and income-support interventions for low-income elderly individuals. While studies on cash transfers and (positive) shocks in pension benefit levels for low-income retirees generally show decreased mortality and increased self-reported (mental) health (e.g. Crossley and Zilio, 2018; Golberstein, 2015; Huang and Zhang, 2021; Malavasi and Ye, 2024; Miglino et al., 2023; Noghanibehambari and Fletcher, 2025; Pak, 2021; Salm, 2011), evidence on their impact on the onset and progression of chronic conditions is limited.

This paper investigates the short- and long-term effects of a welfare benefit cut reform on stress-related and lifestyle-related conditions among low-income retirees with a migration background. We exploit a reform of the Dutch AIO benefit scheme, formally known as the Supplementary Income Provision for the Elderly, which provides income support to retirees with incomplete state pensions and insufficient additional income or wealth. This scheme predominantly serves retirees with a migration background whose incomes are at or near the social minimum. The reform introduced a "cost-sharing" rule that reduced benefit levels based on the number of (non-partner) adult co-residents in the household, reflecting presumed household economies of scale and generating a persistent negative benefit shock.[1] These characteristics make the reform well suited for investigating the income-health gradient among the elderly. The policy change was unanticipated by recipients, clearly separates treatment and control groups, and resulted in a sizable, persistent income

[1] We note that the term "cost-sharing" has a different meaning in our setting than in the context of health economics, where it typically refers to the portion of medical expenditures borne by patients.

reduction for the treated without other modifications to the benefit system. Moreover, because the affected population consists of retirees with limited labor market attachment, the reform largely rules out indirect effects of income on health operating through labor supply responses, which often complicate the identification of direct income effects on health (see e.g. Malavasi and Ye, 2024; Noghanibehambari and Fletcher, 2025, for more detailed discussions). Finally, we track outcomes for up to seven years after the announcement, allowing to examine both the immediate and longer-term effects of the policy change.

For the empirical analysis, we use detailed individual-level administrative data on the welfare reform, along with monthly data on benefit and labor market outcomes. Additionally, we measure total personal income at the yearly level. These income-related measures are used to gain insight into the mechanical and behavioral income effects of the reform. Regarding the health outcomes, we collect quarterly data on medication use for psychological disorders (e.g., antidepressants and anxiolytics), pain (opioids), inflammatory conditions (non-steroidal anti-inflammatory drugs (NSAIDs)), and several lifestyle-related diseases (hypertension, high cholesterol, and diabetes mellitus). Additionally, we use annual data for different health care cost categories, and monthly data on mortality.

We estimate the causal effects of the welfare cut reform using a difference-in-differences design. As previously discussed, the reform introduced a permanent negative income shock for AIO benefit recipients with cost sharers (adult co-residents) in their household, while those without cost sharers remained unaffected. The treatment group consists of benefit recipients living with cost sharers in their household at the pre-treatment month, while benefit recipients without cost sharers serve as the control group. To capture potential anticipation effects, we define the treatment period as starting at the reform's announcement in December 2014, treating the period until its implementation in July 2015 as the anticipation phase. We estimate both dynamic and pooled models, capturing treatment effects over a seven-year period following the announcement of the reform. To support the parallel trends assumption, we show that baseline characteristics are balanced across the treatment and control groups, conduct placebo tests, and demonstrate clear parallel trends in

the four years preceding the announcement of the welfare reform. Additionally, our findings are robust to a range of alternative specifications and sample selections. For mortality, we employ an adapted difference-in-differences model and a proportional hazards model, as our main specification is not suitable for this outcome. Given the unverifiable assumptions of these mortality models, the results should be interpreted cautiously.

Our study provides three key findings. First, the welfare cut results in a significant and long-lasting negative income shock. The reform decreased benefit receipt with 28.8 percentage points (33.6 percent) and the monthly benefit payments with on average 140 Euros (45.8 percent) for a seven-year post-reform period. This negative mechanical effect on benefit payments is only limitedly compensated by increased employment (0.3 percentage points) and earnings (5 Euros per month). As a consequence, the welfare cut reduced the annual total personal income with 1,542 Euros (13 percent) on average for a period of (at least) seven years.

Second, we find significant long-term increases in medication use for lifestyle-related conditions. Between 19 and 28 quarters (4.5–7 years) after the start of the treatment, medication use rises by 2.4 percentage points (+5.3%) for high cholesterol and 1.8 percentage points (+3.1%) for hypertension. The effect on diabetes medication is not robust. We find no significant changes in medication use for stress-related conditions in either the short or long run. The 95% confidence intervals of the pooled estimates rule out effects on the stress-related outcomes smaller than -4.7% and larger than 8.4%. Consistent with these patterns, medication costs increase in the long run. We find no significant effects on costs for GP visits, hospital care, or other services, nor on total health care costs.

Third, our findings indicate an increase in mortality beginning four years after the welfare benefit cut, with an estimated rise of about 1.5 percentage points. The proportional hazards model suggests an 8.1% increase in mortality, though this effect is only marginally significant at the 10%-level and should therefore be interpreted with caution.

The observed effects of the reform on lifestyle-related chronic conditions are likely driven by the permanent negative income shock. By reducing the income of

AIO recipients, who were already near the social minimum, the reform plausibly constrained their ability to maintain a healthy lifestyle, leading to a gradual deterioration in health over time. Increased chronic financial stress may also have contributed, although we do not observe a corresponding rise in medication use for stress-related conditions. While the permanent negative income shock appears to be the dominant mechanism, we cannot fully exclude a role for changes in household composition, as additional analyses indicate that the reform reduced the likelihood of co-residing with cost sharers. Our data do not allow us to disentangle the relative contributions of income and household composition to the observed health effects.

Our findings contribute to the broad literature on income shocks and health in high-income countries (e.g. Adda et al., 2009; Brydon et al., 2024; Gelber et al., 2023; Thomson et al., 2022). We extend this literature by examining both short- and long-run effects of a welfare benefit cut on common stress- and lifestyle-related conditions.[2] We also contribute to the growing literature on the health effects of social policies targeting low-income elderly populations in high-income countries (e.g. Golberstein, 2015; Malavasi and Ye, 2024; Pak, 2021; Salm, 2011).[3] We extend this literature by focusing on retirees with a migration background living at the social minimum and by studying a negative income shock.[4] Taken together, our results add to the understanding of income-related health inequalities (e.g. Chetty et al., 2016; Marmot, 2015; WHO, 2019).

Additionally, our study contributes methodologically by leveraging high-quality

[2]As discussed in Section 2.4, evidence on stress-related outcomes beyond mental health, as well as on lifestyle-related conditions, remains limited, and evidence based on medication use for these outcomes is particularly scarce.

[3]A related strand studies similar policies in low- and middle-income countries (e.g., Barham and Rowberry, 2013; Bernal et al., 2024; Cheng et al., 2018; Cooper et al., 2020; Huang and Zhang, 2021; Jensen and Richter, 2004; Miglino et al., 2023).

[4]Our findings contrast with some studies examining permanent income shock from the U.S. Social Security notch. Moran and Simon (2006) show increases in medication prescriptions following higher pension income, which they attribute to reduced underuse among low-income retirees; however, this mechanism is likely less relevant in the Dutch context, given universal health insurance coverage and the apparent absence of a socioeconomic gradient in treatment for chronic conditions (Ardesch et al., 2025; Danesh et al., 2024). Snyder and Evans (2006) report higher mortality among higher-income individuals, which they link to reduced employment among retirees, but more recent evidence using the same reform and a longer follow-up challenges this, showing that benefit reductions increased mortality instead (Noghanibehambari and Fletcher, 2025). This potential employment channel is unlikely to be relevant in our setting, as only a very small share of our sample remains employed after retirement and we find negligible effects on employment.

administrative data on medication use to estimate treatment effects on specific chronic conditions.[5] This approach addresses a key gap in the literature, which has often examined the link between income and health without exploiting detailed healthcare utilization data (Brydon et al., 2024). Administrative data on medication use offer several advantages: they cover the full population, are more reliable than self-reported measures, provide detailed information across a wide range of conditions, and allow for frequent longitudinal measurement.[6] The latter two features are particularly valuable, as they enable the dynamic estimation of short- and long-term effects on a range of health outcomes. In turn, this provides a more comprehensive understanding of the temporal consequences of income shocks and supports the investigation of heterogeneous mechanisms, such as stress-related versus lifestyle-related pathways.

This paper is structured as follows. Section 2 provides an overview of the Dutch pension system for retirees, details the welfare cut reform and the Dutch health care system, and discusses the expected effects. Section 3 outlines the data and presents descriptive statistics. Section 4 explains the difference-in-differences methodology. Section 5 reports the findings on income- and health-related outcomes. Section 6 discusses explanations for the main findings. Section 7 concludes.

# 2 Institutional context

## 2.1 Pension benefits for low-income retirees

The Dutch AIO benefit scheme, formally known as the Supplementary Income Provision for the Elderly, aims to provide income support to retirees with an incomplete

[5] Some previous studies have used medication data either to study mental health specifically (e.g. Barschkett and Tréguier, 2026; Bastiaans et al., 2024; de Bruijn et al., 2023) or as broad proxies for health without distinguishing between chronic conditions (e.g. Cesarini et al., 2016; Hämäläinen et al., 2025).

[6] Alternative measures of chronic conditions face notable limitations. Self-reported data are subject to measurement error and sample size constraints, as discussed in Evans and Garthwaite (2014). For example, Johnston et al. (2009) find no income–health gradient using self-reported hypertension, but a significant gradient using objective measures. They also show that false negative reporting is income related, suggesting that self-reported data may underestimate health inequalities. Hospital or GP records, while more objective, often suffer from limited availability and coverage; see Danesh et al. (2024) for a further discussion.

state pension (AOW) and insufficient additional income or assets, ensuring their total income reaches the social minimum. A full state pension is granted to individuals who have resided in the Netherlands for the 50 years prior to their date of retirement, with each missing year reducing the state pension amount by 2%-points.[7] Consequently, incomplete state pensions are predominantly found among individuals with a migration background.

Individuals are eligible for the AIO benefit if they have reached the state pension age (set at 65 years and 2 months in 2014), reside in the Netherlands (with a maximum of 13 weeks abroad annually), and have income and assets below the AIO thresholds. In 2014, the income threshold excluded 25% of labor income up to 194 Euros, while the asset limit was set at 5,850 Euros for singles and 11,700 Euros for couples or single parents with children under 18. In practice, since the state pension level exceeds the social minimum, only individuals with no or a partial state pension may qualify for AIO benefits. The AIO benefit level depends on household type (single or cohabiting) and the amount of income received from the state pension, additional pension schemes, or other income sources. As of November 2014, the maximum AIO benefit level was 1,044 Euros for singles and 1,437 Euros for cohabits.

## 2.2 The welfare cut reform

In July 2015, the government introduced the cost-sharing standard ("kostendelersnorm"), which permanently reduced benefit levels based on the number of adult co-residents, excluding partners. This reform aimed to capture presumed economies of scale within households due to adult co-residents, limit benefit accumulation among co-residing adults, and reduce government expenditure.

Before the introduction of the cost-sharing standard, AIO benefit levels were unaffected by the number of adult co-residents, while the presence of a partner was already taken into account. Under the new rules, however, the maximum benefit level decreased with each adult co-resident aged 21 or older, irrespective of their

[7]The social minimum amounts to about 95% of a full state pension; therefore, eligibility generally requires having spent at least around 2.5 years abroad during the accrual period.

income or financial contributions to household expenses. Certain groups, such as students and non-relatives residing on a commercial basis, were excluded from being classified as cost sharers. In practice, most adult co-residents of AIO recipients are their adult children (Statistics Netherlands, 2017). The revised maximum benefit level per person is calculated as:

$$\frac{(40\% + A \times 30\%)}{A} \times B \tag{1}$$

where $A$ represents the number of adult co-residents in the household (including the AIO recipient(s)) and $B$ is the base benefit level (i.e., the maximum benefit for cohabiting recipients). As a result, AIO recipients with more adult co-residents experience larger reductions in their AIO benefit.

This pattern is also reflected in Appendix Figure B.1, which shows the change in AIO benefit payments between June (the final pre-reform month) and July 2015 (the first post-reform month) for both single and cohabiting AIO recipients, by the number of adult co-residents (cost sharers). While AIO recipients without cost sharers did not face a change in their benefit payments, the payments for recipients with one additional co-resident sharply dropped with 367 Euros for singles and 80 Euros (per person) for cohabiting recipients. A larger number of cost sharers further decreases the benefit payments.

The cost-sharing standard was first announced by the Social Insurance Bank (SVB), the agency administering the AIO benefit scheme, in December 2014. Adjusted benefit levels were communicated in June 2015, with reduced payments taking effect in July 2015. To account for potential anticipation effects, we define the treatment period as beginning in December 2014, and consider the interval between the announcement and implementation (December 2014 to July 2015) as the anticipation period.

## 2.3 Dutch health care system

In the Netherlands, basic health insurance is mandatory for all residents. The insurance covers a government-defined benefits package, including general practitioner

(GP) care, hospital services, and prescription medication.[8] Insurers must accept all applicants without price discrimination, ensuring both accessibility and affordability.

For medication costs and several other services, a mandatory annual deductible applies set at €360 in 2014 and €385 since 2016, which patients must pay out-of-pocket before insurance coverage begins.[9] GP visits are exempt from the deductible and thus free at the point of care. Notably, over 80 percent of our sample already had health care costs exceeding the deductible (see Section 3.3), making it unlikely that the target population *reduced* medication use for financial reasons.

## 2.4 Expected effects

We expect a permanent negative effect of the welfare cut on income. As a direct mechanical effect, the reform reduces both the likelihood of benefit eligibility and the total amount received. Although the reform may incentivize increased employment (in addition to receiving pension benefits), this effect is likely limited, as the target population of the benefit program is beyond the statutory retirement age and has very low employment rates. Overall, the direct income loss is expected to outweigh the behavioral response by large, resulting in a permanent negative income shock.

Regarding health outcomes, we expect the reform to increase medication use for relevant chronic conditions through three main channels.[10] First, the welfare cut is likely to increase financial stress, which may accelerate the onset and progression of stress-related conditions and increase the need for related medications, particularly in the short term (see e.g. Cohen et al., 2007; Haushofer and Fehr, 2014). A large body of evidence has documented this pattern for mental health problems,

[8] In addition to basic health insurance, supplementary insurance, which is voluntary and less regulated in terms of benefits, may cover extra costs such as dental care.

[9] Some medications incur co-payments if patients select a more expensive variant than the standard reimbursed option. However, most medications analyzed in this paper are not subject to such charges. Moreover, these co-payments are unlikely to influence whether individuals use medications for specific chronic conditions at all.

[10] A potential concern is that the welfare benefit cut could have offsetting effects on medication use for chronic conditions. While reduced income may worsen health and increase medication demand, it could also constrain the ability to afford medication. However, this latter mechanism is likely limited in our setting. As discussed in Section 2.3, access barriers are low, medications are covered by mandatory basic insurance, and the annual deductible poses a limited constraint, as over 80 percent of our sample already exceeds this threshold.

like depression and anxiety.[11] Increased material hardship and financial stress may also intensify chronic pain, leading to greater reliance on pain-relief medications, including opioids.[12] Finally, financial stress can trigger physiological responses that promote inflammation, which may in turn increase the demand for medications targeting inflammatory conditions.[13]

Second, we predict increased medication use for lifestyle-related conditions in the longer run, specifically hypertension, high cholesterol, and type 2 diabetes mellitus. These conditions are closely tied to health behaviors that are sensitive to income. A reduction in income may increase chronic stress and constrain individuals' ability to maintain a healthy lifestyle, leading to lower-quality diets, reduced physical activity, and poorer sleep (Allcott et al., 2019; de Boer et al., 2023; Grandner et al., 2010; Kari et al., 2015; LaGuardia et al., 2025). These behavioral responses, in turn, heighten the risk and progression of lifestyle-related conditions in the longer term (Park et al., 2023).[14]

[11] Meta-studies and reviews consistently show that negative income shocks worsen mental health outcomes (Brydon et al., 2024; Ridley et al., 2020; Shields-Zeeman and Smit, 2022; Thomson et al., 2022), while a quasi-experimental study among surviving spouses documents increases in mental health medication use (Barschkett and Tréguier, 2026). Among older adults, quasi-experimental studies have found that increases in pension income reduce self-reported depressive symptoms (Golberstein, 2015; Malavasi and Ye, 2024; Pak, 2021), but evidence on related medication use appears to be absent. Correlational evidence from the Netherlands suggests higher use of mental health medication among lower-income individuals at older ages (Danesh et al., 2024).

[12] Correlational studies show that lower-income individuals report higher levels of pain (Schurer et al., 2014; Zajacova et al., 2021) and are more likely to receive opioid prescriptions (Bedene et al., 2019; Nestvold et al., 2024), including in the Netherlands (Danesh et al., 2024). A causal study of Miller et al. (2024) finds no effect of a $1,000 monthly cash transfer on self-reported pain. However, a quasi-experimental study by Hämäläinen et al. (2025) finds that a positive income shock reduces the use of non-opioid painkillers, while Barschkett and Tréguier (2026) provide suggestive evidence of increased opioid use following a reduction in survivor benefits, with no corresponding effect for non-opioid painkillers.

[13] Stress has been linked to elevated inflammation (Steptoe et al., 2007), but evidence on income gradients in the use of anti-inflammatory medication (NSAIDs) is limited and mixed (Bonnesen et al., 2023), and causal evidence seems to be absent.

[14] For hypertension, lower income is consistently associated with higher prevalence (Johnston et al., 2009; Minhas et al., 2023; Nakagomi et al., 2022). One quasi-experimental study suggests that increased pension income can reduce hypertension (Malavasi and Ye, 2024), although evidence remains mixed (Crossley and Zilio, 2018). For high cholesterol, Dutch evidence shows higher use of cholesterol-lowering medication among lower-income individuals (Danesh et al., 2024), though international findings are mixed (Hu et al., 2024; Minhas et al., 2023), and causal evidence is lacking. For diabetes, lower income is consistently associated with higher prevalence (Danesh et al., 2024; Minhas et al., 2023; Park et al., 2023). Causal evidence for a negative effect of income on diabetes remains limited (Malavasi and Ye, 2024; Miller et al., 2024), possibly because existing studies examine relatively short time horizons, whereas type 2 diabetes typically develops over a longer period.

Third, the reform may affect health through changes in household composition. Because the AIO benefit level is contingent on the number of adult co-residents, the reform creates financial incentives to reduce co-residence. Existing evidence suggests that adult co-residents can provide social contact, daily support, and informal care, which may affect health outcomes among older adults (e.g. Dang et al., 2024; Hanum et al., 2024; Kaida and Boyd, 2011; Smits et al., 2010). To the extent that the reform reduces co-residence, it could therefore negatively affect health outcomes by weakening these support networks.

# 3 Data

## 3.1 Data sources and sample selection

We use individual-level administrative data from Statistics Netherlands. Specifically, we utilize monthly data from the benefit registry, which provide comprehensive information on the type, level, and amount of AIO benefits. To enrich our analysis, we integrate data from additional sources, including social security records (employment and earnings), the municipal population register (sociodemographics and mortality), the National Health Care Institute (dispensed medicines), and health care insurers (health care costs). By combining these sources, we construct a detailed longitudinal dataset spanning January 2011 to December 2021.[15]

Our initial sample includes all individuals receiving an AIO benefit as of November 30, 2014. We excluded recipients living in institutional households, as the cost-sharing standard did not apply to them. Additionally, we removed a small number of recipients with non-standard or unknown household types.[16] Furthermore, we excluded a limited number of AIO recipients without a migration background.[17] The final sample comprises 42,821 individuals, of whom 6,533 have cost sharers in their

[15]This end date was chosen because, in 2022, the government announced a policy change increasing the age threshold for co-residents to be considered cost sharers from 21 to 27 years.

[16]Non-standard households are private households in which the AIO recipient lives with adults who are neither a partner, parent, nor child (e.g., siblings or other relatives sharing a home).

[17]Individuals are classified as having a migration background if they or at least one of their parents were born abroad. A small group of natives are excluded, as they fall outside the scope of this paper and receive AIO benefits for specific reasons.

household (treatment group) and 36,288 do not (control group).

## 3.2 Data description

We analyze the impact of the welfare cut on income and health using several constructed variables. For income, we examine AIO receipt (binary) and amount, employment status (based on positive earnings), labor income, and total personal annual income including pensions, benefits, and earnings. All continuous measures include zero's and are adjusted for inflation.

For health, we examine binary variables for three categories of stress-related chronic conditions (psychological disorders, pain, and inflammatory conditions) and three categories of lifestyle-related chronic conditions (hypertension, high cholesterol, and diabetes mellitus).[18] To link medication data to chronic conditions, we use the ATC-3 classification framework from Huber et al. (2013), adapted for the Dutch context by Yildiz et al. (2020). This framework focuses on medication categories that are used (almost) exclusively for the treatment of a specific chronic condition. Medications for psychological disorders include antidepressants, anxiolytics, hypnotics, and sedatives. Opioids are classified as pain medication. Inflammatory conditions are identified via non-steroidal anti-inflammatory drugs (NSAIDs), which are commonly used to treat inflammation.[19] Additionally, medications commonly prescribed for high cholesterol (lipid-lowering agents), hypertension (e.g., beta-blockers), and diabetes mellitus (e.g., insulins) are included. A detailed mapping of ATC-3 codes to these chronic conditions is provided in Table A.1 in Appendix A.

Our medication data come from pharmacy dispensation records.[20] The dataset captures only medications that were actually dispensed, thereby excluding prescriptions that were issued but never collected (i.e., primary non-adherence). Addi-

[18] Type 2 diabetes is classified as a lifestyle-related condition, while Type 1 diabetes is not. Since our data do not distinguish between these types, we report their combined prevalence. Given that Type 1 diabetes accounts for only 9.2% of all diabetes cases in the Netherlands (Vanhommerig and Knottnerus, 2024), this combined measure is a reasonable proxy.

[19] Beyond inflammatory conditions, NSAIDs may also be used for pain relief and fever reduction. While Huber et al. (2013) classify NSAIDs under rheumatological conditions, we follow Yildiz et al. (2020) in using the broader category of inflammatory conditions for the Dutch setting.

[20] This excludes medication supplied directly by hospitals or nursing homes, which represent only a small proportion of stress- and lifestyle-related medications.

tionally, rare medications that contribute less than 1% of cases for a given chronic condition are not represented. With precise dates of medication provision available, we conduct the analysis at the quarterly level. This interval corresponds to typical prescription durations of up to three months, and thus provides a more accurate measure of the prevalence of underlying chronic conditions than monthly data.

In addition to medication use, we measure treatment effects on inflation-adjusted annual health care costs covered by basic health insurance across six categories: medication, GP visits, hospital care, mental health care, other health care services, and total health care costs.[21] Costs include expenditures below the deductible (paid out-of-pocket by the insured) but exclude any co-payments. To reduce the influence of outliers, we winsorize the top 0.1% of values within each category and year. Finally, mortality effects are measured using monthly death records.

We constructed a treatment group variable that takes the value of one for individuals with cost sharers in their household as of November 2014, and zero for individuals without cost sharers. Notably, the benefit registry began registering the number of cost sharers from July 2015 (the month of implementation of the reform). To address this issue, we developed a binary proxy variable for cost sharers based on monthly household information, including members' age and student status. Panel (a) of Figure B.2 in Appendix B illustrates that the trends for the proxy cost sharers measure closely mirror those of the administrative cost sharers measure. Furthermore, we calculated an overlap in values for both variables of 97.3 percent for the period between July 2015 and December 2021. Given this high degree of concordance, we conclude that the constructed cost sharing variable provides a reliable proxy for the administrative measure. We note that our data do not reliably identify the number or specific identities of cost sharers.[22] Consequently, we cannot examine the background characteristics of cost sharers or exploit variation in their number to analyze treatment intensity.

[21] Other health care costs, such as nursing home and intensive care for older adults with physical or cognitive impairments, are covered under the Long-Term Care Act (WLZ) and Social Support Act (Wmo) and are not included in basic health insurance. These costs can be substantial for the AIO population, but are not captured in our data.

[22] The benefit registry begun recording the number of cost sharers in July 2015, but it does not capture their identities.

Attrition in our administrative data arises when individuals are no longer registered in the Personal Records Database (BRP). Figure B.3 in Appendix B shows trends in the share of individuals in the treatment and control groups who are not registered in the BRP. In the pre-treatment period, missing observations are limited and likely reflect individuals who have not yet migrated to the Netherlands. In the seven years after the start of the treatment, attrition increases to 29.7 percent in the treatment group and 28.8 percent in the control group, driven primarily by mortality (see Figure B.7), with the remainder attributable to emigration. Importantly, the figure shows similar attrition patterns across groups before and after treatment. In the main analysis, individuals are included until they exit the data. As a robustness check, we restrict the sample to a balanced panel observed over the full 11-year period (see Section 5.3).

## 3.3 Sample characteristics

Panel A of Table 1 presents descriptive statistics for the full sample, treatment group, and control group for the month prior to the announcement of the reform (November 2014). Since the differences between the treatment and control group are small, we highlight the key characteristics of the full sample. Approximately 60% of the sample is female. The average age is 73.5 years, with 64 percent falling between the ages of 65 and 75. About 59% of our sample receives an AIO benefit for singles and the remaining part for couples.

Panel B of Table 1 presents the income situation at baseline. The average monthly AIO benefit is 325 Euros, and only 0.2% of the sample is employed. Total annual personal income is approximately 12,000 Euros, meaning that AIO benefits accounted for roughly one-third of that amount. Given this income, the vast majority of AIO recipients fall below the Dutch poverty line, as shown by Goderis and Muns (2025). Figure B.4 in Appendix B illustrates the distribution of AIO benefit payments in November 2014 for both singles (Panel (a)) and cohabiting individuals (Panel (b)). The distribution is right-skewed, with the majority of recipients receiving AIO benefits between 0 and 300 Euros. These distributions clearly demonstrate the supplemental nature of the AIO benefit.

Panel C of Table 1 highlights substantial health challenges: on average, individuals use medication for 3.1 chronic conditions, drawn from a set of 21 common chronic conditions. At baseline, 8.3% of our sample use medications for psychological disorders, 7.1% for pain, and 10.2% for inflammatory conditions. A substantial share uses medications for lifestyle-related conditions: 48.1% for hypertension, 38.2% for high cholesterol, and 31.4% for diabetes.

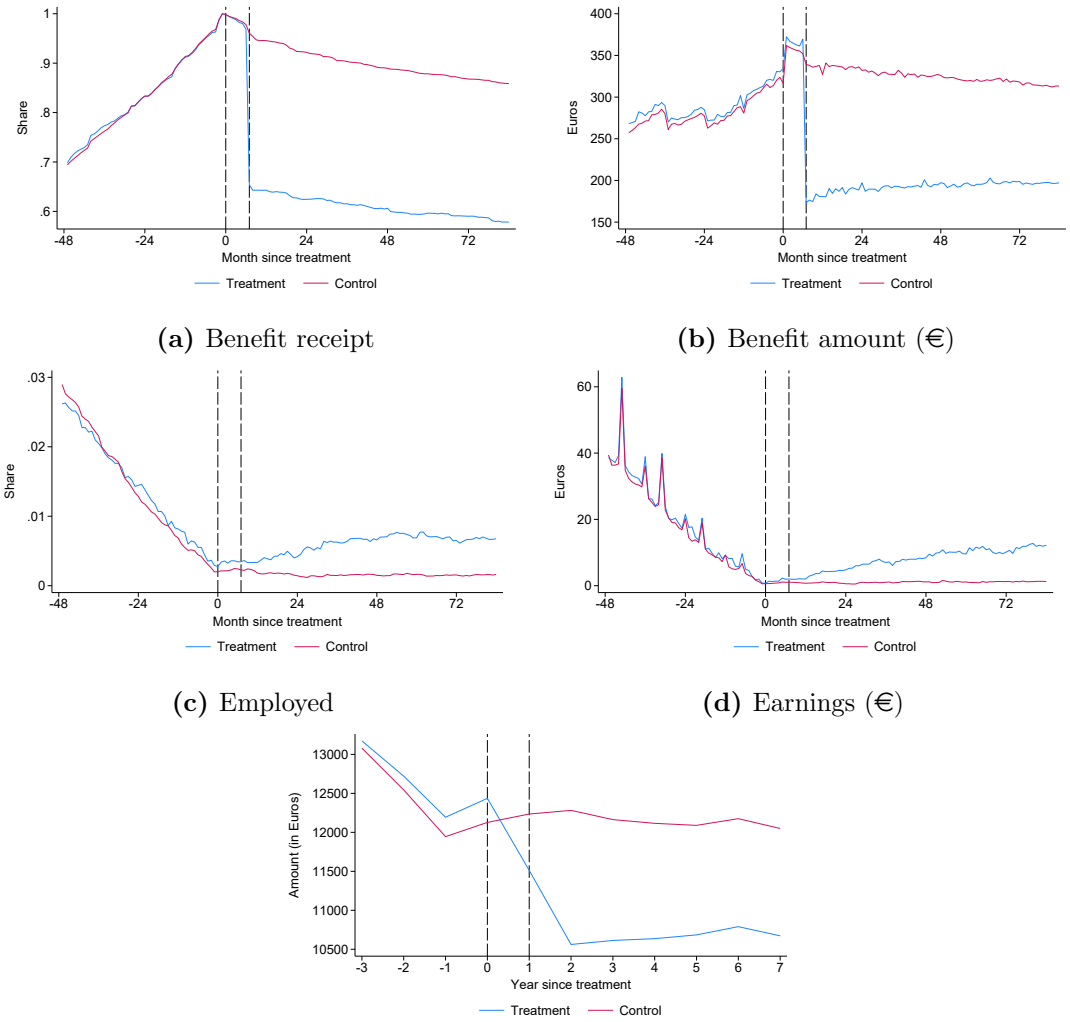
Average annual health care costs are about 4,400 Euros, driven primarily by hospital expenditures (2,640 Euros) and medication costs (814 Euros). Mental health care expenditures are relatively low (93 Euros), reflecting limited use within our sample. Notably, 82.9% of individuals in the sample already have health care costs exceeding the deductible.

Compared to the full population of pension recipients (see Column (5) of Table 1), our sample is in substantially poorer health, as reflected in higher medication use across all chronic conditions, most notably diabetes (31.4% versus 12.1%). Health care expenditures are likewise higher across all cost categories, with total health care costs approximately 900 Euros higher. At the same time, individuals in our sample are younger on average than the broader population of pension recipients (73.5 versus 75.0 years) and have substantially lower annual personal income (11,983 versus 20,949 Euros).

Figures 1 and 2 present pre- and post-treatment trends for the main income-related and health outcomes. Panels (a) and (b) of Figure 1 show a steady increase in benefit receipt and benefit amount during the pre-treatment period, consistent with individuals approaching pension age. In parallel, employment (Panel (c)) and earnings (Panel (d)) exhibit a gradual decline, reflecting labor market exit upon reaching retirement age. Total personal income also trends downward prior to treatment. Following the benefit cut, income remains stable for the control group but declines sharply for the treatment group, indicating a substantial treatment-induced income shock. Figure 2 shows that medication use increases over time for most chronic conditions, consistent with the rising prevalence of chronic conditions over the life cycle.[23] In contrast, the share using medication for inflammatory conditions

[23] Around the treatment period, the upward trend attenuates for both groups, plausibly reflect-

**Figure 1:** Trends in income-related outcomes




**(a)** Benefit receipt

**(b)** Benefit amount (€)

**(c)** Employed

**(d)** Earnings (€)

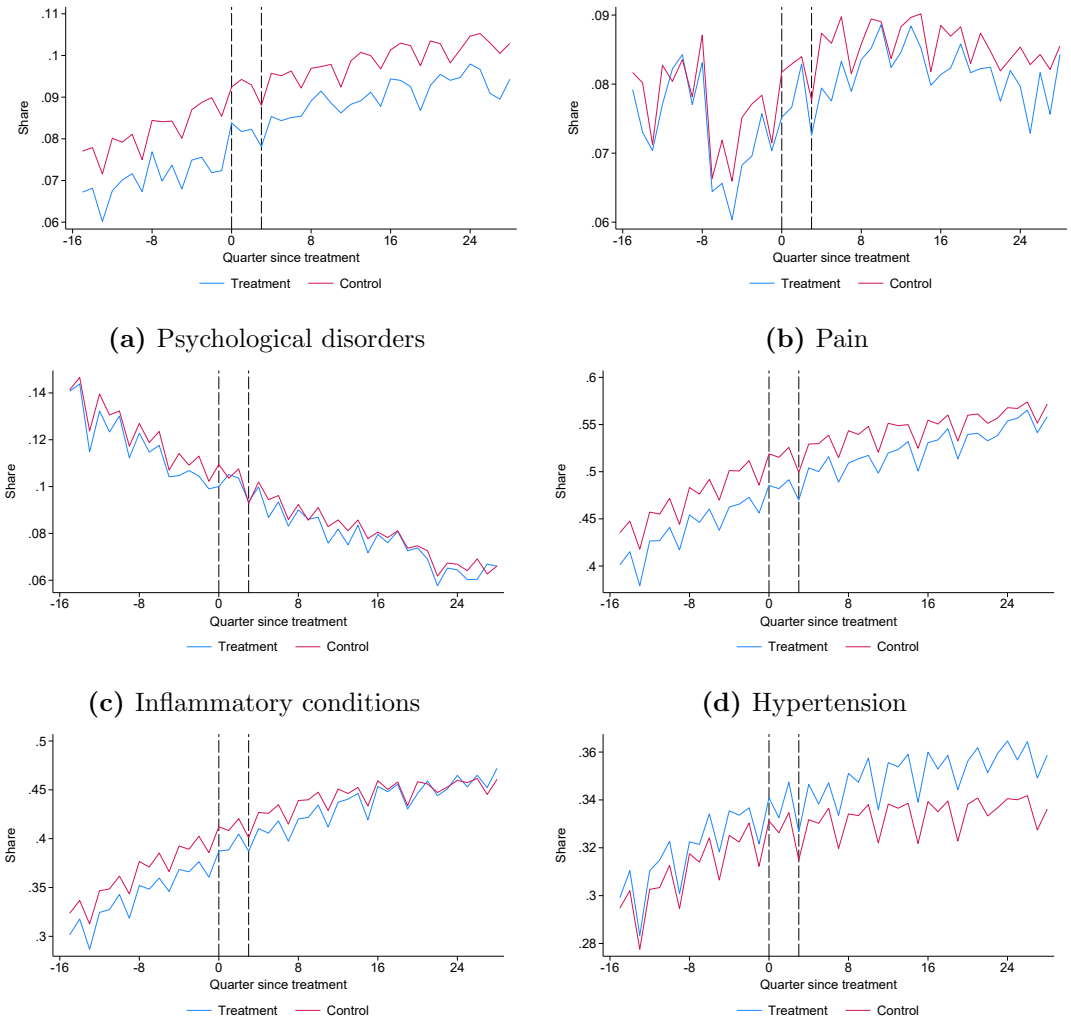
**(e)** Total personal income (€)

*Note:* Trends in income-related outcome variables for treated versus control groups. For Panels (a)–(d), $t = 0$ and $t = 7$ indicate the *month* of announcement and implementation of the welfare cut, respectively. For Panel (e), $t = 0$ and $t = 1$ indicate the *year* of announcement and implementation of the welfare cut, respectively. Number of individuals = 42,821.

declines over time, in line with lower prescription rates at older ages.

ing selective mortality: individuals with high medication use are more likely to die and thus exit the data, mechanically dampening aggregate growth rates. Consistent with this interpretation, trends based on a balanced panel (see Figure B.5) show that the upward trajectory for lifestyle-related conditions persists.

**Figure 2:** Trends in health outcomes




**(a)** Psychological disorders

**(b)** Pain

**(c)** Inflammatory conditions

**(d)** Hypertension

**(e)** High cholesterol

**(f)** Diabetes mellitus

*Note:* Trends in health-related outcome variables for treated versus control groups. $t = 0$ indicates the quarter of first announcement of the welfare cut (Q4 2014). $t = 3$ is the quarter of the introduction of the welfare cut (Q3 2015). Number of individuals = 42,821.

**Table 1:** Descriptive statistics

| | Sample | | | | Benchmark |
|---|---|---|---|---|---|
| | Full | Treated | Not treated | Norm. diff | Pension recipients |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Socio-demographics** | | | | | |
| Female | 0.598 | 0.619 | 0.595 | 0.05 | 0.542 |
| Age (in years) | 73.5 | 73.8 | 73.4 | 0.05 | 75.0 |
| 65–69 years | 0.339 | 0.328 | 0.341 | -0.03 | 0.321 |
| 70–74 years | 0.305 | 0.306 | 0.305 | 0.00 | 0.245 |
| 75–79 years | 0.210 | 0.207 | 0.211 | -0.01 | 0.188 |
| 80–84 years | 0.099 | 0.102 | 0.099 | 0.01 | 0.132 |
| 85+ years | 0.047 | 0.057 | 0.045 | 0.06 | 0.115 |
| Country of origin | | | | | |
| Netherlands | - | - | - | - | 0.817 |
| Western | 0.171 | 0.118 | 0.180 | -0.17 | 0.013 |
| Turkey | 0.140 | 0.170 | 0.135 | 0.10 | 0.010 |
| Morocco | 0.173 | 0.239 | 0.162 | 0.19 | 0.009 |
| Surinam | 0.188 | 0.134 | 0.198 | -0.17 | 0.004 |
| Dutch Antilles/Aruba | 0.045 | 0.038 | 0.046 | -0.04 | 0.137 |
| Other non-Western | 0.280 | 0.297 | 0.277 | 0.04 | 0.009 |
| Unknown | 0.002 | 0.003 | 0.002 | 0.02 | 0.000 |
| Benefit type: Single | 0.589 | 0.577 | 0.591 | -0.03 | - |
| Benefit type: Cohabit | 0.411 | 0.423 | 0.409 | 0.03 | - |
| **Panel B: Income-related outcomes (baseline)** | | | | | |
| Monthly AIO benefit amount (€) | 325 | 330 | 324 | 0.02 | - |
| Employed | 0.002 | 0.003 | 0.002 | 0.02 | 0.030 |
| Monthly earnings (€) | 0.60 | 0.79 | 0.56 | 0.01 | 44.97 |
| Total annual personal income (€) | 11,983 | 12,194 | 11,945 | 0.07 | 20,949 |
| **Panel C: Health outcomes (baseline)** | | | | | |
| Psychological disorder | 0.083 | 0.072 | 0.085 | -0.05 | 0.080 |
| Pain | 0.071 | 0.070 | 0.071 | 0.00 | 0.046 |
| Inflammatory diseases | 0.102 | 0.099 | 0.102 | -0.01 | 0.064 |
| Hypertension | 0.481 | 0.456 | 0.486 | -0.06 | 0.409 |
| High cholesterol | 0.382 | 0.361 | 0.386 | -0.05 | 0.295 |
| Diabetes mellitus | 0.314 | 0.322 | 0.312 | 0.02 | 0.121 |
| # chronic conditions (all) | 3.12 | 3.01 | 3.14 | -0.06 | 2.15 |
| Total health care costs (€) | 4,408 | 4,428 | 4,405 | 0.00 | 3,536 |
| Mental health care costs (€) | 93 | 69 | 98 | -0.03 | 81 |
| GP costs (€) | 217 | 211 | 218 | -0.05 | 179 |
| Medication costs (€) | 814 | 766 | 823 | -0.05 | 511 |
| Hospital costs (€) | 2,640 | 2,742 | 2,621 | 0.01 | 2,153 |
| Other health care costs (€) | 598 | 598 | 598 | -0.00 | 552 |
| Health care costs>deductible | 0.829 | 0.820 | 0.831 | -0.03 | 0.676 |
| N | 42,821 | 6,533 | 36,288 | 42,821 | 3,296,526 |

*Note:* Descriptive statistics of AIO benefit recipients and all pension recipients in the month prior to treatment (November 2014). Medication use for chronic conditions is measured in the preceding quarter. Total personal income, number of chronic conditions, and health care costs are measured in the preceding year. The number of chronic chronic conditions is measured using the ATC-3 medication classification, covering 21 common chronic diseases (Yildiz et al., 2020).

# 4 Empirical strategy

## 4.1 The difference-in-differences model: specification

We employ a difference-in-differences (DiD) design to investigate the effects of the welfare benefit cut. Specifically, we compare the changes in outcomes over time for the treatment group (AIO recipients *with* cost sharers in their household pre-reform) to changes in outcomes for the control group (AIO recipients *without* cost sharers in their household pre-reform). Since the control group was not directly affected by the reform, it serves as a counterfactual for estimating what would have occurred in the absence of the policy change.[24]

We estimate two main specifications. The first is a simple dynamic event-study model, specified as follows:

$$y_{it} = \beta \cdot Treatment_i + \sum_{t \neq -1} \delta_t \cdot (Period_t \times Treatment_i) + \theta_t + \epsilon_{it} \tag{2}$$

where $y_{it}$ denotes the outcome for individual $i$ at time $t$. The time variable $t$ is measured in months for AIO benefit and labor market outcomes, in quarters for medication use, and in years for total personal income and health care costs (see also Section 3.2).

The variable $Treatment_i$ is an indicator equal to one if individual $i$ is part of the treatment group, i.e., had cost sharers in their household at $t = -1$. The specification includes time period fixed effects ($\theta_t$). Standard errors are robust and clustered at the household level. Given that unconditional parallel trends appear plausible (see Section 4.2), we do not include background characteristics as covariates in our main specification, following Baker et al. (2025) and Roth et al. (2023).

The coefficients of interest, $\delta_t$, capture the average treatment effect on the treated (ATT) for each time period $t$, with $t = -1$ as baseline. This dynamic specication allows us to examine effect heterogeneity over time, including potential anticipation

[24] Technically, the cost-sharing standard also applied to the control group, but they were not directly affected, as they had no cost sharers in their household. While the status of having cost sharers may change over time, such changes are limited for the control group (see Panel (b) of Figure B.2) and do not appear to be driven by the introduction of the cost-sharing standard (see Panel (a) of Figure I.1). We discuss the implications of these changes in Section 6.

effects between the reform announcement (December 2014) and its implementation (July 2015).

The second specification is a pooled DiD model, estimated as:

$$y_{it} = \beta \cdot Treatment_i + \gamma \cdot (Post_t \times Treatment_i) + \theta_t + \epsilon_{it} \tag{3}$$

where $Post_t$ is a dummy variable that indicates the period after the announcement of the welfare reform. The coefficient of interest, $\gamma$, captures the pooled average treatment effect on the treated of the welfare reform on the outcome variable. The estimation includes a pre-treatment period of 8 quarters (or 24 months) to account for, among other factors, seasonal fluctuations in medication use (see Figure 2). We use this specification to estimate overall treatment effects (quarters 0–28) as well as effects for specific post-treatment intervals: the anticipation period (quarters 0–2), quarters 3–10, quarters 11–18, and quarters 19–28.

Our main model is not suitable for estimating effects on mortality. Instead, we examine this outcome using a linear probability model and a proportional hazard model in a cohort-based difference-in-differences framework. We discuss the specification of both models in Appendix C. Since the assumptions underlying these models cannot be fully verified, we interpret the results on mortality with caution.

## 4.2 Validity of the difference-in-differences model

This section outlines the rationale and supporting evidence for the DiD assumptions.

*No anticipation.* The no anticipation assumption requires that individuals do not change their behavior or outcomes to the expected implementation of the welfare reform prior to its actual introduction. This ensures that pre-treatment trends are unaffected by forward-looking responses. As discussed in Section 2.2, we address this concern by defining the treatment period as starting in the month the reform was first announced (December 2014), thereby excluding any potential anticipatory effects from the pre-treatment period.

*Parallel trends.* The key identifying assumption of the DiD approach is that, in the absence of treatment, outcomes in the treatment and control groups would have

followed parallal trends. Although this assumption cannot be directly tested, several empirical checks can support its plausibility. First, a common falsification test is assessing balance in relevant pre-determined socio-demographic characteristics (Baker et al., 2025). Substantial imbalances may undermine the parallel trends assumption if these characteristics are correlated with untreated potential outcomes. Following standard practice, we report absolute normalized differences in means in Column (7) of Table 1. All observed socio-demographic characteristics and all pre-treatment outcomes show absolute normalized difference values well below the conventional threshold of 0.25 (Baker et al., 2025; Imbens and Rubin, 2015), supporting the plausibility of (unconditional) parallel trends.

Second, we examine trends in outcomes during the pre-treatment period. Figures 1 and 2 display outcome trajectories for both groups before and after the reform. The figures show that income- and health-related outcomes evolved similarly in the four years preceding the announcement of the welfare cut. In line with this, Figure 3 and Figure 4 show no meaningful or consistent DiD effects for any outcome across the pre-treatment period.[25] Similarly, pre-treatment trends in health care costs are parallel across treatment and control groups (see Figure B.6), and no statistically significant effects are observed prior to the welfare cut (see Figure E.1). To formally test the parallel pre-treatment trend, we estimated DiD coefficients for each outcome and each time period using Equation (2) and calculated the averages for 8-quarter and 14-quarter pre-treatment periods. Table D.1 in Appendix D presents the results of this test. Consistent with the findings in Figures 1 and 2, we observe coefficients close to zero and statistically insignificant for all health-related outcomes and almost all income-related outcomes across both time periods.[26] To further emphasize the similarity in trends, the average coefficients for all health outcomes across both time periods ranged from -0.003 to 0.002, essentially zero.

Third, we conduct a placebo test to assess the validity of the parallel trends

[25] All pre-treatment DiD effects are insignificant with the exception of a statistically significant effect on total personal yearly income in year $t = -3$. However, this effect is limited – both in absolute terms (156 Euros) and relative terms (approximately 1.3 percent) – and hence not indicative of a pre-trend. Given the number of tests conducted, this isolated result could plausibly be the result of chance.

[26] Again, the only exception is total personal income, for which we find a statistically significant effect. However, the magnitude of this effect (114 Euros annually; <1 percent) is small.

assumption for the longer-run treatment effects. We construct a placebo cohort consisting of AIO recipients as of November 2007, seven years prior to the actual policy reform. Placebo 'treatment' and 'control' groups are defined based on the presence of cost sharers in the household at that time. Applying the same sample restrictions as in the main analysis, we estimate dynamic placebo effects using Equation (2), allowing us to examine outcome trajectories up to seven years after the placebo 'treatment'. As shown in Figure D.1, we observe no systematic placebo effects for five of the six outcomes, reinforcing the plausibility of the parallel trends assumption. The only exception is an upward trend in diabetes prevalence, warranting caution in interpreting results for this outcome. We return to this issue in Section 5.

*Stationarity.* Our DiD-design relies on a form of stationarity, meaning that the sample composition remains stable over time (Baker et al., 2025). As noted in Section 3.1, the sample is subject to attrition both before treatment (e.g., individuals not yet residing in the Netherlands) and after treatment (due to death or emigration). If attrition patterns differ between treatment and control groups, this may lead to differential compositional changes that bias the estimated treatment effects on medication outcomes. To assess this risk, we compare normalized differences in baseline covariates between treatment and control groups at three time points: the pre-treatment month (November 2014), the first observed period (January 2011), and the last observed period (December 2021). We then calculate the absolute change in these normalized differences between the baseline and each of the two comparison periods. The results, shown in Table D.2, indicate that these changes are minimal, with all absolute values remaining below 0.04. This suggests only a limited differential compositional change over time.

We perform several robustness checks to assess the stability of the treatment effects across alternative model specifications and sample restrictions. First, we estimate a two-way fixed effects (TWFE) model by incorporating individual fixed effects into Equation (2). Second, although baseline covariate imbalances between treatment and control groups are minimal, even small differences could potentially bias long-term estimates. To address this, we implement a doubly robust difference-in-

differences (DiD) approach, following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021), as recommended by Baker et al. (2025). This method combines inverse probability weighting with regression adjustment to account for observed baseline differences. Third, we restrict the sample to a balanced panel, including only individuals observed over the full 11-year period. Overall, our results are highly robust (see Section 5.3).

# 5 Results

## 5.1 Effects on income-related outcomes

The welfare cut has a substantial and long-lasting negative impact on AIO benefit receipt and payments. Panels (a) and (b) of Figure 3 and Table 2 show the corresponding monthly and pooled difference-in-differences effects, respectively. At implementation, AIO benefit receipt drops sharply by approximately 30 percentage points, and monthly payments fell by 170 Euros (48%). These immediate declines are mechanical effects of the introduction of the cost-sharing standard, which reduced maximum benefit levels and (thus) eligibility for some of the recipients with adult co-residents. These effects persist: seven years later, AIO benefit receipt is still 28 percentage points lower, and monthly payments remain 123 Euro lower. Over the full post-treatment period, the pooled treatment effects are a 28.8 percentage point reduction in benefit receipt (a 33.6% decrease compared to the sample mean) and a 140 Euro reduction in monthly benefit payments (a 45.8% decrease) (see Column (6) of Table 2).

The negative shock to AIO benefits has been only slightly compensated by increases in earnings. As shown in Panels (c) and (d) of Figure 3 and Table 2, the welfare reform leads to economically small gains in employment and earnings – raising employment by 0.3 percentage points and monthly earnings by 5.40 Euro over the full post-treatment period. Consequently, these modest earnings effects do little to offset the substantial reductions in AIO benefits.

Overall, the welfare cut reform led to a substantial and persistent reduction in income. Panel (e) of Figure 3 presents the yearly treatment effects on total

**Table 2:** Pooled estimates of the effects of the welfare cut on income-related outcomes

| | Anticipation period | Post-treatment period | | | Anticipation & post-treatment period | Full post-treatment period |
|---|---|---|---|---|---|---|
| | Month 0–6 | Month 7–30 | Month 31–54 | Month 55–84 | Month 0–84 | Month 7–84 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: Benefit receipt** | | | | | | |
| Coefficient | -0.001 | -0.298*** | -0.286*** | -0.280*** | -0.261*** | -0.288*** |
| (s.e.) | (0.003) | (0.007) | (0.008) | (0.008) | (0.007) | (0.007) |
| Sample mean | 0.989 | 0.888 | 0.853 | 0.829 | 0.869 | 0.856 |
| **Panel B: Benefit payment** | | | | | | |
| Coefficient | 4 | -155*** | -139*** | -127*** | -127*** | -140*** |
| (s.e.) | (3) | (3) | (3) | (3) | (3) | (3) |
| Sample mean | 353 | 312 | 306 | 300 | 310 | 306 |
| **Panel C: Employed** | | | | | | |
| Coefficient | -0.000 | 0.001 | 0.004** | 0.004** | 0.003** | 0.003*** |
| (s.e.) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Sample mean | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 |
| **Panel D: Earnings** | | | | | | |
| Coefficient | -0.58 | 1.89 | 5.76*** | 8.52*** | 4.83** | 5.40** |
| (s.e.) | (1.39) | (1.62) | (2.10) | (2.69) | (1.87) | (1.96) |
| Sample mean | 0.92 | 1.33 | 2.16 | 2.65 | 1.94 | 2.05 |
| N observations | 1,318,789 | 1,987,054 | 1,910,856 | 2,012,876 | 4,161,343 | 3,865,298 |

*Note:* The table shows pooled difference-in-differences (DiD) treatment effects on income-related outcomes for the corresponding treatment period. The pooled DiD estimates are obtained using Equation (3), with months $t = -1$ to $t = -24$ serving as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals = 42,821; number of clusters = 36,601. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

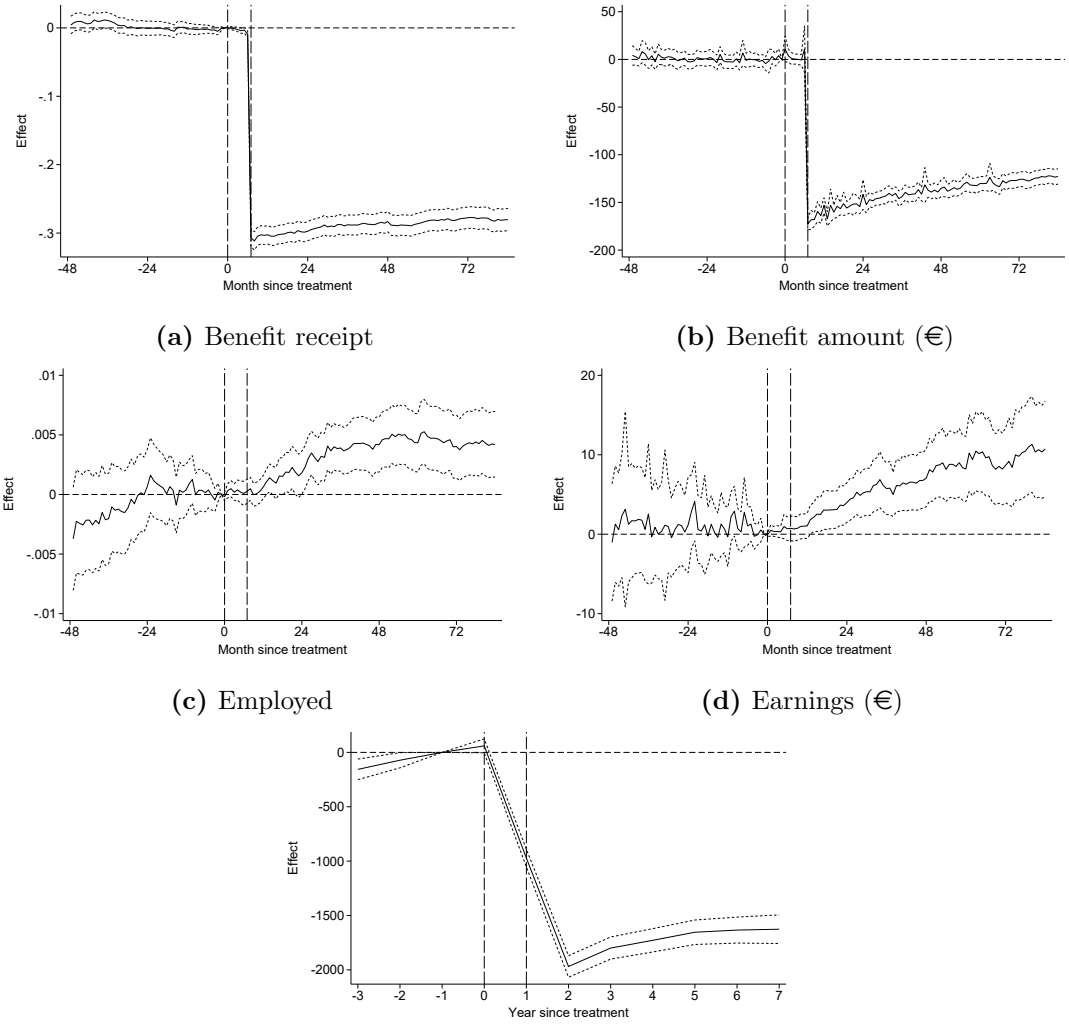
personal income.[27] The negative income effect was approximately 1,970 Euros in the year following implementation and slightly decreased to 1,626 Euros by year seven. Table 3 shows the pooled treatment effects on annual personal income. Over the full post-implementation period (year 1–7), the average annual income decrease was 1,542 Euros (including the first six months of 2015, the anticipation period of the reform), corresponding to an income reduction of about 13 percent.

## 5.2 Effects on health outcomes

Figure 4 and Table 4 present the quarterly and pooled treatment effects on health outcomes. Panels (a)–(c) of Figure 4 show essentially no significant treatment effects

[27]Note that the reform was implemented mid-year (July 2015), so the effect in the first year after treatment is roughly half the size of that in subsequent years.

**Figure 3:** Dynamic effects of the welfare cut on income-related outcomes




**(a)** Benefit receipt

**(b)** Benefit amount (€)

**(c)** Employed

**(d)** Earnings (€)

**(e)** Total personal income (€)

*Note:* The figures show monthly difference-in-differences (DiD) treatment effects with corresponding 95%-confidence intervals on income-related outcomes. For Panel (e), the figure shows yearly difference-in-differences treatment effects. The DiD estimates are obtained using Equation (2), with $t = -1$ serving as the baseline period. Dashed lines represent 95% confidence intervals constructed using robust standard errors clustered at the household level. For Panels (a)–(d), $t = 0$ and $t = 7$ indicate the *month* of announcement and implementation of the welfare cut, respectively. For Panel (e), $t = 0$ and $t = 1$ indicate the *year* of announcement and implementation of the welfare cut, respectively. Number of individuals = 42,821; number of clusters = 36,601.

on medication use for psychological disorders, pain, and inflammatory conditions in any post-treatment quarter. Consistent with this, the pooled estimates for these outcomes are close to zero and precisely estimated, with standard errors ranging from 0.2 to 0.4 percentage points (see Table 4). The 95% confidence intervals allow us to rule out effects <-0.1 (1.0%) and >0.8 (8.2%) percentage points for psychological

**Table 3:** Pooled estimates of the effect of the welfare cut on total annual personal income

| | Year 0-2 | Year 3-5 | Year 6-7 | Year 0-7 | Year 1-7 |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Coefficient | -856*** | -1,654*** | -1,555*** | -1,298*** | -1,542*** |
| (s.e.) | (39) | (53) | (62) | (45) | (48) |
| Dependent mean | 12,109 | 11,901 | 11,906 | 11,989 | 11,958 |
| N observations | 248,972 | 234,778 | 189,046 | 420,604 | 378,093 |

*Note:* The table shows pooled difference-in-differences (DiD) treatment effects on total annual personal income for each corresponding treatment period. The coefficients represent the estimated average effect on annual income. The pooled DiD estimates are obtained using Equation (3), with years $t = -3$ to $t = -1$ serving as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals = 42,821; number of clusters = 36,601. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

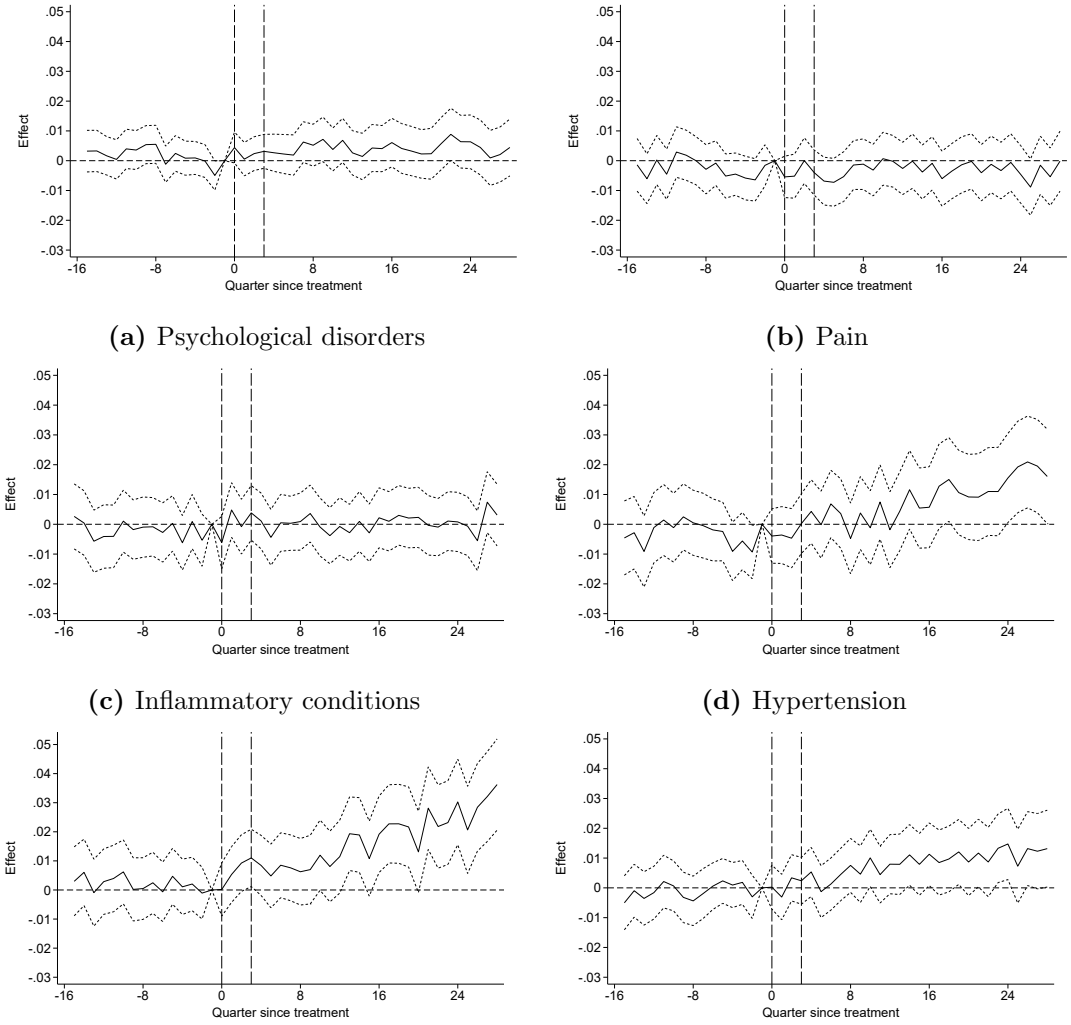
disorders, <-0.4 (4.7%) and >0.5 (5.9%) percentage points for pain, and <-0.2 (2.4%) and >0.7 (8.4%) percentage points for inflammatory conditions.

In contrast, Panels (d)–(f) of Figure 4 show that the welfare benefit cut increases medication use for lifestyle-related diseases in the longer term. For high cholesterol, the increase becomes significant after three years and progressively grows over time. For hypertension and diabetes mellitus, significant positive effects emerge in the later quarters of the post-treatment period. Specifically, the pooled estimates for quarters 19–28 indicate increases of 1.8 percentage points (+3.1%) for hypertension, 2.4 percentage points (+5.3%) for high cholesterol, and 1.2 percentage points (+3.5%) for diabetes mellitus. Although the pooled treatment effects over the full post-treatment period are slightly smaller, they are still statistically significant.

Figure E.1 and Table E.1 in Appendix E present the dynamic and pooled treatment effects of the welfare benefit cut on health care cost outcomes. Consistent with the effects on medication use, the welfare benefit cut has led to higher costs for medication use in the longer run. We find no significant treatment effects on costs for general practitioner visits, hospital care, or other services, nor on total healthcare expenditures.[28]

[28]Panel A of Table E.1 shows a significant decrease in mental health care costs. However, this cannot be conclusively attributed to the welfare benefit cut, as the abolition of co-payments for mental health care services in January 2014 may have affected the treatment and control groups differently given baseline differences (see Figure B.6).

**Figure 4:** Dynamic effects of the welfare cut on health outcomes




**(a)** Psychological disorders

**(b)** Pain

**(c)** Inflammatory conditions

**(d)** Hypertension

**(e)** High cholesterol

**(f)** Diabetes mellitus

*Note:* The figures show quarterly difference-in-differences (DiD) treatment effects with corresponding 95%-confidence intervals on health outcomes. The DiD estimates are obtained using Equation (2), with quarter $t = -1$ serving as the baseline period. Dashed lines represent 95% confidence intervals constructed using robust standard errors clustered at the household level. $t = 0$ and $t = 3$ indicate the *quarter* of announcement and implementation of the welfare cut, respectively. Number of individuals = 42,821; number of clusters = 36,601.

Finally, the welfare benefit cut appears to have increased mortality risk. Figure E.2 in Appendix E presents the linear probability estimates indicating that mortality risk rises following implementation and becomes statistically significant after roughly four years. This pattern is consistent with the more pronounced increase in mortality observed in the treatment group in Figure B.7. In the long term, the effect amounts to about 1.5 percentage points. Similarly, the proportional haz-

**Table 4:** Pooled estimates of the effects of the welfare cut on health outcomes

| | Anticipation period | Post-treatment period | | | Anticipation & post-treatment period | Full post-treatment period |
|---|---|---|---|---|---|---|
| | Quarter 0-2 | Quarter 3-10 | Quarter 11-18 | Quarter 19-28 | Quarter 0-28 | Quarter 3-28 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: Psychological disorder** | | | | | | |
| Coefficient | 0.002 | 0.004 | 0.004 | 0.004 | 0.004 | 0.004 |
| (s.e.) | (0.002) | (0.002) | (0.003) | (0.004) | (0.002) | (0.003) |
| 95% CI | [-0.002, 0.006] | [-0.001, 0.008] | [-0.002, 0.010] | [-0.003, 0.011] | [-0.001, 0.008] | [-0.001, 0.009] |
| Sample mean | 0.092 | 0.094 | 0.098 | 0.101 | 0.097 | 0.097 |
| **Panel B: Pain** | | | | | | |
| Coefficient | -0.000 | -0.000 | 0.001 | 0.000 | 0.000 | 0.000 |
| (s.e.) | (0.002) | (0.002) | (0.003) | (0.003) | (0.002) | (0.002) |
| 95% CI | [-0.005, 0.004] | [-0.005, 0.004] | [-0.004, 0.007] | [-0.006, 0.006] | [-0.004, 0.005] | [-0.004, 0.005] |
| Sample mean | 0.082 | 0.085 | 0.087 | 0.083 | 0.085 | 0.085 |
| **Panel C: Inflammatory conditions** | | | | | | |
| Coefficient | 0.001 | 0.002 | 0.001 | 0.003 | 0.002 | 0.002 |
| (s.e.) | (0.003) | (0.002) | (0.003) | (0.003) | (0.002) | (0.002) |
| 95% CI | [-0.004, 0.006] | [-0.002, 0.007] | [-0.004, 0.007] | [-0.003, 0.009] | [-0.002, 0.007] | [-0.002, 0.007] |
| Sample mean | 0.106 | 0.092 | 0.081 | 0.068 | 0.083 | 0.080 |
| **Panel D: Hypertension** | | | | | | |
| Coefficient | -0.001 | 0.005 | 0.011** | 0.018** | 0.010** | 0.011** |
| (s.e.) | (0.003) | (0.004) | (0.005) | (0.006) | (0.004) | (0.005) |
| 95% CI | [-0.007, 0.006] | [-0.003, 0.013] | [0.001, 0.021] | [0.006, 0.030] | [0.002, 0.018] | [0.002, 0.020] |
| Sample mean | 0.515 | 0.526 | 0.542 | 0.557 | 0.538 | 0.542 |
| **Panel E: High cholesterol** | | | | | | |
| Coefficient | 0.004 | 0.007* | 0.015** | 0.024*** | 0.014*** | 0.016*** |
| (s.e.) | (0.003) | (0.004) | (0.005) | (0.006) | (0.004) | (0.004) |
| 95% CI | [-0.002, 0.010] | [-0.000, 0.015] | [0.006, 0.025] | [0.012, 0.036] | [0.006, 0.022] | [0.007, 0.024] |
| Sample mean | 0.411 | 0.426 | 0.446 | 0.453 | 0.438 | 0.442 |
| **Panel F: Diabetes Mellitus** | | | | | | |
| Coefficient | 0.001 | 0.005* | 0.009** | 0.012** | 0.008** | 0.009** |
| (s.e.) | (0.002) | (0.003) | (0.004) | (0.005) | (0.003) | (0.003) |
| 95% CI | [-0.004, 0.005] | [-0.001, 0.010] | [0.001, 0.017] | [0.003, 0.021] | [0.001, 0.014] | [0.002, 0.015] |
| Sample mean | 0.332 | 0.332 | 0.337 | 0.339 | 0.335 | 0.336 |
| N observations | 467,225 | 660,602 | 635,281 | 668,692 | 1,410,675 | 1,283,825 |

*Note:* The table shows pooled difference-in-differences (DiD) treatment effects on health outcomes for the corresponding treatment period. The pooled DiD estimates are obtained using Equation (3), with quarters $t = -1$ to $t = -8$ serving as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals = 42,821; number of clusters = 36,601. 95% confidence intervals are reported in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

ards model (Column 1 of Table E.2 in Appendix E) estimates an average effect of 8.1% (hazard ratio = 1.081) over the seven years after the announcement, although this effect is only marginally significant ($p = 0.052$).

## 5.3 Robustness and placebo tests

We discuss the results of three robustness checks (TWFE model, doubly-robust model, and balanced panel) and a placebo test, as described in Section 4.2. Detailed results are reported in Appendix F and Appendix G. Overall, the results of the main DiD model are robust to these alternative specifications and sample selections for all income-related outcomes (see Tables F.1-F.5) and five of the six health outcomes (see Tables G.1–G.6). For psychological disorders, pain, and inflammatory conditions, the precise null results of the main model remain consistent. For cholesterol and hypertension, we continue to find significant treatment effects in the longer run of similar magnitude. The results for these five health outcomes using the placebo cohort are almost entirely statistically insignificant and precisely estimated. In contrast, the effects on diabetes mellitus are not robust across the different specifications and the placebo cohort shows a similar upward trend over time. Accordingly, we interpret the effects on diabetes as not robust.

## 5.4 Heterogeneous treatment effects

To examine treatment effect heterogeneity, we estimate DiD effects on medication use by gender and age group. Table H.2 in Appendix H presents the estimates by gender. Over the full post-treatment period, the welfare reform reduced the average monthly benefit amount about twice as much for women (-172 Euros; 52.6%) as for men (-87 Euros; 31.9%). Despite the larger income reduction for women, adverse health effects were more pronounced among men. Specifically, medication use for psychological disorders increased by about 1 percentage point (13.3%) for men over the full post-treatment period, with no significant effect for women. For lifestyle-related conditions, effects were also larger for men, with medication use increasing 2.6 to 3.3 percentage points (6.0–7.6%) during quarters 19–28. For women, the effects were smaller (0.4–1.9 percentage points; 1.2–4.3%) and statistically significant only for high cholesterol. In contrast, for inflammatory conditions, we observe a 0.6 percentage points (6.6%) increase for women, with no significant effect for men. For mortality, we find no evidence of heterogeneous treatment effects by gender (see

Columns (4) and (5) of Appendix Table E.2). We caution against attributing these treatment effects solely to gender, as men are more likely to cohabit, whereas women are more likely to be single (see Columns (3) and (4) of Appendix Table H.1).

Table H.3 presents estimates by age group, distinguishing individuals below and above the median age. We find stronger effects among older individuals, particularly for hypertension and diabetes mellitus. In addition, we observe some indication of increased medication use for inflammatory conditions among older individuals in the longer run. For mortality, we find stronger treatment effects for the below median age groep (HR=1.187; $p$=0.010) compared to the above median age group (HR=0.992; $p$=0.879) (see Columns (2) and (3) of Appendix Table E.2).

## 5.5 Additional analyses

The welfare benefit cut may have affected not only income but also household composition. Because the revised benefit amount depends on the number of adult co-residents, the reform reduced incentives for AIO recipients to co-reside with other aldults. To examine this, we compare changes in the share of AIO recipients with cost sharers in the household in the sample cohort (November 2014) with a placebo cohort (November 2012).[29] For each cohort, Figure I.1 in Appendix I shows separate trends for individuals with and without cost sharers at month $t = -1$. For those with cost sharers, pre-treatment trends are closely aligned across cohorts but begin to diverge following the announcement of the benefit cut.

We also estimate difference-in-differences (DiD) effects for individuals residing with cost sharers, using placebo cohort individuals with cost sharers as the control group.[30] Figure I.2 in Appendix I shows a sharp decline starting shortly before the implementation of the welfare benefit cut, with a persistent negative effect of approximately 8 percentage points two years after the announcement. This suggests that the reform decreased the likelihood of co-residence with cost sharers. However,

[29]Household composition may be influenced by external factors, such as housing market conditions. We therefore rely on a temporally proximate placebo cohort rather than the 2007 cohort used for other outcomes.

[30]Individuals without cost sharers are not suitable as a control group for these outcomes, as diverging pre-treatment trends undermine a credible counterfactual (see Figure I.1).

this estimate should be interpreted cautiously, as the specification cannot fully account for housing market dynamics (e.g., via calendar-month fixed effects) and some pre-treatment patterns deviate from expected parallel pre-trends.

# 6 Discussion

Our main findings are as follows. The welfare cut reform led to a substantial and persistent reduction in the personal income of AIO recipients. We find small and statistically insignificant effects on medication use for stress-related conditions. In contrast, there is strong evidence of increased medication use for lifestyle-related conditions, particularly hypertension and high cholesterol. We also find suggestive evidence of increased mortality. In the following, we discuss potential explanations for these patterns, while emphasizing that our data do not allow definitive conclusions regarding the underlying mechanisms.[31]

The absence of significant effects of the welfare cut on medication use for stress-related conditions (psychological disorders, pain, and inflammatory conditions) may be attributed to two factors. First, the reform may not have substantially increased financial stress. This seems unlikely, as large and persistent income reductions are typically associated with increased stress, particularly among low-income households (Haushofer and Fehr, 2014). Second, even if financial stress increased, it may not have translated into higher use of medication for these conditions. This appears more plausible. AIO recipients may have relied on alternative coping mechanisms, such as support from co-residents, which could have mitigated the need for medication. An alternative explanation might be limited statistical power, given low baseline medication use for stress-related conditions (about 10 percent). Nevertheless, the 95% confidence intervals rule out increases greater than 5.9–8.4 percent. For comparison, Hämäläinen et al. (2025) find reductions of 8–11 percent in mental health medication use following income increases of 9–11 percent among unemployment benefit recipients over only a two-year horizon. Thus, while small effects cannot be excluded, the estimates rule out economically meaningful treatment effects.

[31]Note that the study population is not large enough to incorporate additional survey data, such as self-reported measures of stress, lifestyle, or health.

The positive treatment effects on medication use for hypertension and high cholesterol are most plausibly driven by the permanent negative income shock induced by the AIO benefit cut. This likely has constrained financial budgets, limiting the ability to maintain a healthy lifestyle. In addition, the income loss may have created chronic financial stress which could have contributed to these health effects, although this is not reflected in increased medication use for stress-related conditions. Selection through mortality does not explain the observed effects on medication use for lifestyle-related conditions, as the results remain robust when using a balanced sample that accounts for mortality (see Section 5.3).

Although the adverse effects on lifestyle-related conditions are plausibly driven by the permanent negative income shock, we cannot fully exclude a role for changes in household composition. Additional analyses indicate that the reform reduced the likelihood of living with co-residents, which may have negatively affected health by reducing social contact, daily support, or informal care from co-residents (Hanum et al., 2024; Smits et al., 2010). However, this mechanism appears less likely given that the estimated household composition effects are modest and that a substantial share of individuals continues to live with a partner. Moreover, placebo analyses do not indicate differential trends over time in medication use for hypertension or high cholesterol between AIO recipients with and without cost sharers, even though household composition changes naturally in the former group. Note that our data do not allow us to disentangle the role of changes in household composition from income effects. We further lack information on other potential behavioral responses of cost sharers to the reform, such as increased financial support to the household.

Heterogeneity analyses show stronger treatment effects on lifestyle-related conditions for men than for women, despite larger income losses among women. This pattern is broadly consistent with Malavasi and Ye (2024), who find mortality effects of pension income for men but not for women, even though women experience larger income gains. In our setting, however, we do not observe gender differences in mortality. One possible explanation for the differential effect by gender is that social norms may lead men to bear greater responsibility for household income, increasing their responsiveness to income shocks, which aligns with our finding of increased

medication use for psychological disorders among men. Additionally, women may receive more financial support from co-residents than men, as suggested by Kaida and Boyd (2011), potentially buffering the health impact of the income reduction. These interpretations should be treated with caution, as women in our sample are more likely to be single and men to cohabit, making it difficult to separate the role of gender from household composition. Furthermore, our data do not allow us to directly test these mechanisms.

Our estimates suggest that the welfare benefit cut, which generated a long-run average income decline of 13 percent, increased mortality by 8.1 percent. Interpreting this effect as fully driven by the negative income shock, and not by changes in household composition, implies an income-mortality elasticity of approximately –0.63. This estimate is large relative to most findings in the pension income literature (see discussions in Malavasi and Ye (2024) and Miglino et al. (2023)), but closely aligns with the elasticity reported by Malavasi and Ye (2024) in a comparable setting. Similarly, Gelber et al. (2023) found a comparable elasticity in response to reductions in disability insurance payments among low-income individuals in the U.S. context. These benchmarks are particularly relevant because they focus on low-income populations, for whom health outcomes are plausibly more sensitive to income shocks than for higher-income groups.

Although we observe long-term increases in medication use for lifestyle-related conditions, these effects appear insufficient to fully explain the rise in mortality. Mortality effects are strongest among younger AIO recipients, whereas increases in medication use for lifestyle-related conditions are largest among older recipients. We also find larger responses in medication use among men, yet mortality effects are similar for men and women. This divergence suggests that the welfare benefit cut may have affected mortality through channels beyond lifestyle-related diseases, potentially involving acute health events or other conditions (see e.g. Salm, 2011). In addition, while medication use serves as a useful proxy for underlying health status, it also functions as a treatment that may prolong life, partially offsetting mortality risk. Future research on cause-specific mortality could further clarify the mechanisms linking income shocks to death.

Our estimates may be somewhat conservative for two reasons. First, part of the control group became exposed to the welfare benefit cut over time. Panel (b) of Figure B.2 in Appendix B shows that by the end of year 7 (month 84), 6.5% of the control group had co-residents in their household. Consequently, some controls became subject to the reform over time, which may have led to a small underestimation of both the income and health effects. Second, because we measure chronic conditions using medication data, these outcomes may be understated due to under-treatment and under-diagnosis. Under-treatment is likely limited in the Dutch context (unlike in settings such as the U.S.), given universal health insurance, low financial barriers, and the absence of a clear socioeconomic gradient in the treatment of chronic conditions (Ardesch et al., 2025; Danesh et al., 2024). Under-diagnosis may be more relevant; exploratory evidence from Danesh et al. (2024) indicates higher under-diagnosis rates among individuals in the lowest income decile. As a result, our estimates may understate the true impact of the reform on the prevalence of chronic conditions.

# 7 Conclusion

This study examines the short- and long-term health effects of a welfare cut reform targeting low-income retired migrants in the Netherlands. Using a difference-in-differences design and detailed administrative data, we investigate the dynamic impact of this reform on medication use for stress-related and lifestyle-related conditions, as well as mortality.

The reform generated a large and persistent negative shock in benefit payments for the treatment group, which was only limitedly compensated by increased earnings. Overall, the welfare cut decreased personal annual income with 13% for at least seven years. Over time, the reform led to increased medication use for hypertension and high cholesterol, indicating detrimental effects on lifestyle-related health conditions. We also find suggestive evidence of higher mortality, but no significant treatment effects on medication use for stress-related conditions. Importantly, our design is well-powered to rule out economically meaningful treatment effects for

these conditions.

These findings extend and reinforce evidence that lower (higher) income levels among retirees can adversely (favorably) affect health outcomes (Golberstein, 2015; Malavasi and Ye, 2024; Pak, 2021; Salm, 2011), with especially pronounced implications for vulnerable populations, including low-income migrants. Policymakers should account for these potential health consequences when setting benefit levels for low-income elderly and other vulnerable groups.

Moreover, our results contribute to the understanding of health inequalities between low- and high-income individuals (e.g. Chetty et al., 2016; Marmot, 2015; WHO, 2019). Our results suggest that policies targeting the income position of low-income elderly individuals can mitigate health inequalities later in life, complementing prior evidence on the importance of early-life interventions (Danesh et al., 2024). This underscores the need for policies that address health inequities throughout the life course, including among the elderly.

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# Online Appendix

Health Effects of a Welfare Benefit Cut among Low-income Retired Migrants

# A Classification of medication codes

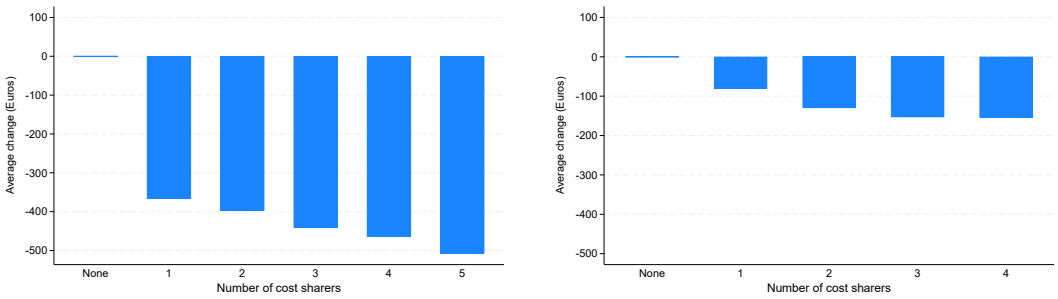
**Table A.1:** Classification of ATC-3 medication codes by chronic condition

| **Chronic condition** | **ATC-3 codes** | **ATC-3 labels** |
|---|---|---|
| Psychological disorder | N05B | Anxiolytics |
| | N05C | Hypnotics & sedatives |
| | N06A | Antidepressants |
| Pain | N02A | Opioids |
| Inflammatory conditions | M01A | Non-steroidal anti-inflammatory drugs (NSAIDs) |
| High cholesterol | C10A, C10B | Lipid-lowering agents |
| Hypertension | C03A | Low-ceiling diuretics |
| | C07A, C07B | Beta-blockers |
| | C08C, C08D | Selective calcium channel blocker |
| | C09A, C09B | ACE inhibitors |
| Diabetes mellitus | A10A | Insulins and analogues |
| | A10B | Other blood glucose-lowering drugs |

*Note:* The table presents the ATC-3 medication codes associated with each chronic condition. The classification is based on Huber et al. (2013) and adapted to the Dutch context by Yildiz et al. (2020). Medications that are rarely prescribed were not available in our dataset. For Psychological disorders: N06B; Pain: N02B (not registered since 2019); Inflammatory conditions: H02A, H02B, M01C, and M02A; Hypertension: C02A, C02C, C02D, C02K, C07C, and C08G.

# B Additional descriptive statistics

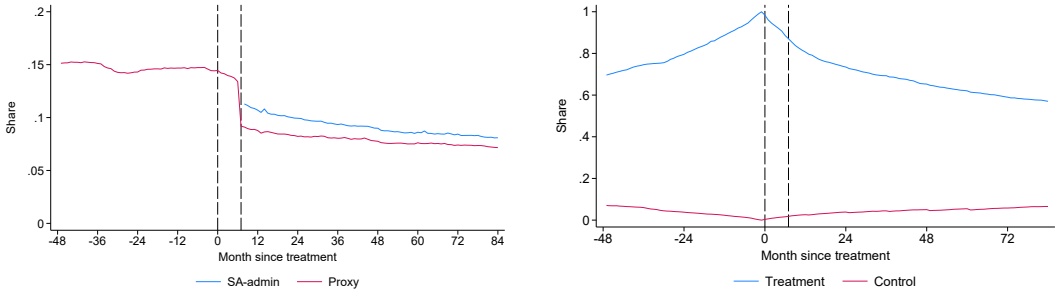
**Figure B.1:** Change in AIO benefit payments by number of cost sharers




**(a)** Singles **(b)** Cohabits

*Note:* The figures show the average change in the amount of AIO benefit payments between June and July 2015 for AIO recipients by the number of cost sharers (excluding AIO recipients themselves) in their household. Panel (a) reflects changes for single AIO recipients and Panel (b) for cohabiting AIO recipients. The number of cost sharers is defined at July 2015 using administrative records. AIO recipients in June 2015 who lost their benefit in July 2015 are excluded. The calculations are executed by the authors using individual-level welfare data from Statistics Netherlands. Total N singles = 22,443; total N cohabits = 15,658.

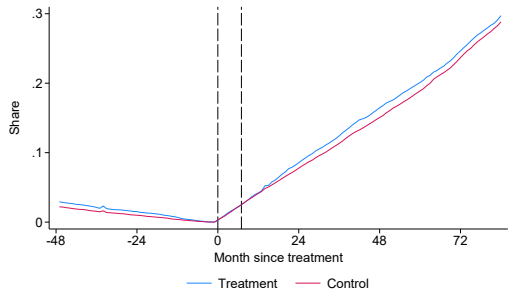
**Figure B.2:** Trends in the share of AIO recipients living with cost sharers




**(a)** All AIO recipients **(b)** Treatment vs. control group

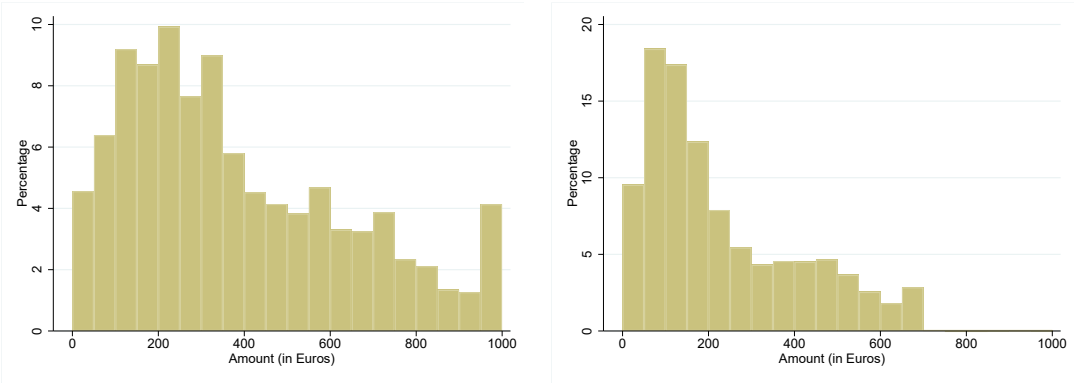
*Note:* The figures show the trend over time in the share of AIO recipients who have cost sharers in their household. Panel (a) presents this share for all AIO recipients in the corresponding month. SA-admin denotes the share with cost sharers based on administrative data; proxy denotes the constructed proxy measure used in this research. Panel (b) shows the share with cost sharers separately for the treatment and control groups, defined at $t = -1$. $t = 0$ indicates the month of first announcement of the welfare cut (month of treatment). $t = 7$ is the month of the introduction of the welfare cut.

**Figure B.3:** Share of individuals not registered in the Personal Records Database




*Note:* The figure shows the trend over time in the share of individuals in the treatment and control group who are not registered in the Personal Records Database (BRP) in the corresponding month. $t = 0$ indicates the month of first announcement of the welfare cut. $t = 7$ is the month of the introduction of the welfare cut. Number of individuals = 42,821.

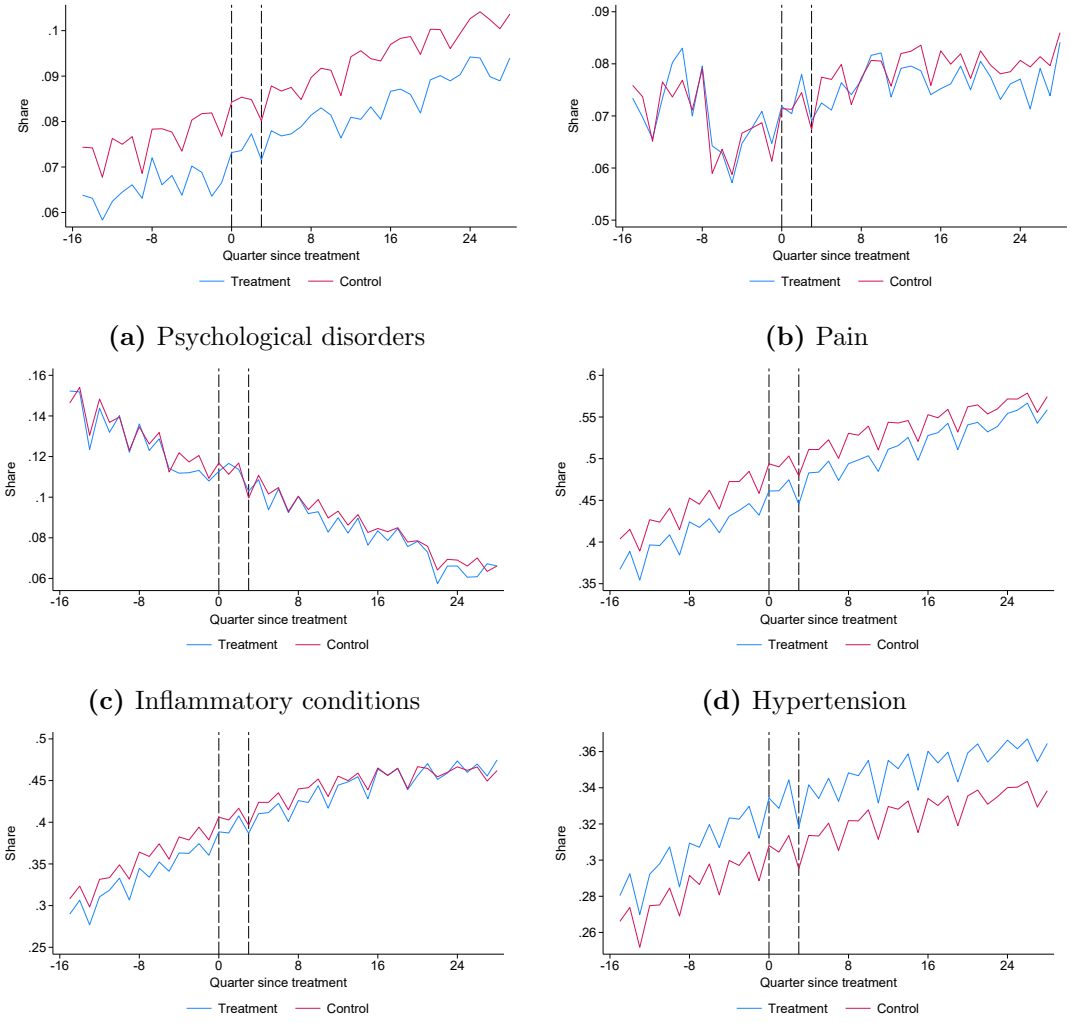
**Figure B.4:** Distribution in AIO benefit payments




**(a)** Singles **(b)** Cohabits

*Note:* The figures show the destribution in the AIO benefit payments at the month before the announcement of the welfare cut (November 2014). A small number of individuals with occassionaly higher payments were excluded. Total N singles = 25,130; total N cohabits = 17,557.

**Figure B.5:** Trends in health outcomes – balanced panel




**(a)** Psychological disorders

**(b)** Pain

**(c)** Inflammatory conditions

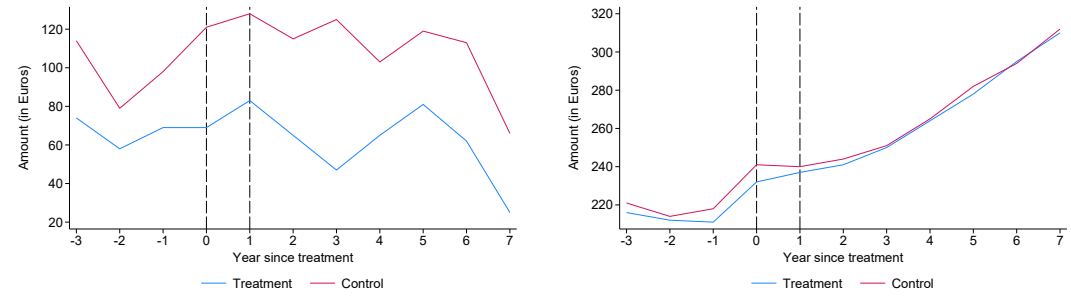
**(d)** Hypertension

**(e)** High cholesterol

**(f)** Diabetes mellitus

*Note:* Trends in health outcomes for treated and control groups are shown for a balanced panel; individuals with any missing observations are excluded. All individuals are observed over the full 11-year period. $t = 0$ indicates the quarter of first announcement of the welfare cut (Q4 2014). $t = 3$ is the quarter of the introduction of the welfare cut (Q3 2015). Number of individuals = 29,306.

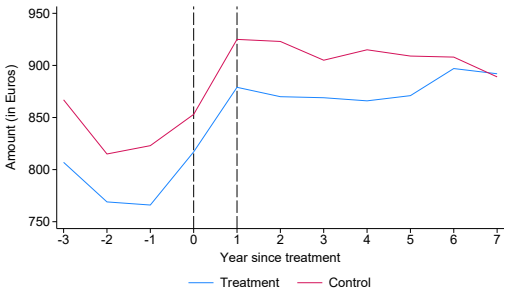
**Figure B.6:** Trends in health care cost outcomes



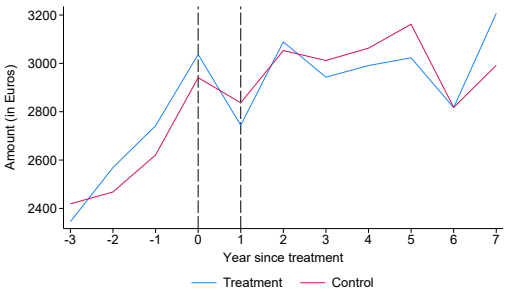

**(a)** Mental health care



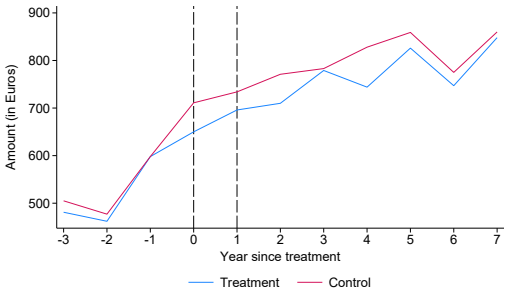

**(b)** General practitioner



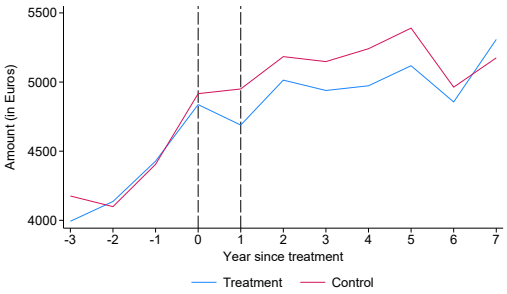

**(c)** Medication




**(d)** Hospital



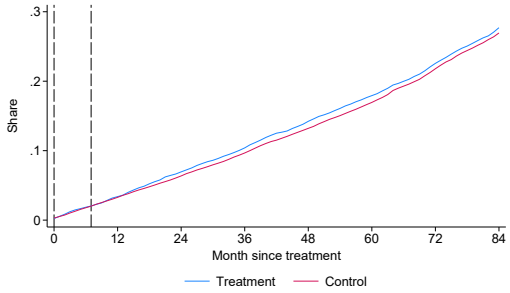

**(e)** Other




**(f)** Total

*Note:* Trends in health care cost outcome variables for treated versus control groups. $t = 0$ indicates the year of first announcement of the welfare cut (2014). $t = 1$ is the year of the start of the welfare cut (2015). Number of individuals = 42,821.

**Figure B.7:** Trends in mortality




*Note:* The figure shows monthly mortality rates (share deceased) for individuals in the treatment and control groups. $t = 0$ indicates the month of first announcement of the welfare cut. $t = 7$ is the month of the introduction of the welfare cut. Number of individuals = 42,821.

# C Empirical strategy for mortality

We estimate treatment effects of the welfare cut on mortality using a linear probability model and a proportional hazard model in a cohort-based difference-in-differences framework. Rather than comparing outcomes before and after the introduction of the welfare benefit cut, we compare the November 2014 cohort to a placebo cohort from 2007. Within each cohort, we contrast AIO recipients with and without cost sharers in their household (measured at month $t = -1$).

For the linear probability model, we estimate for each month separately:

$$y_{it} = \alpha_t + \beta_t \,\text{Cohort}_i + \tau_t \,\text{CS}_i + \delta_t \,(\text{Cohort}_i \times \text{CS}_i) + \mathbf{X}_i \,\boldsymbol{\gamma_t} + \epsilon_{it} \tag{4}$$

where $y_{it}$ equals 1 if individual $i$ dies in month $t$. $\text{Cohort}_i$ equals 1 for the 2014 cohort and 0 for the 2007 placebo cohort, and $\text{CS}_i$ indicates the presence of cost sharers in the household (at $t = -1$). The coefficient $\beta_t$ captures the main effect of belonging to the 2014 cohort, $\tau_t$ captures the main effect of having cost sharers, and $\delta_t$ measures the differential effect of the welfare cut on the 2014 cohort relative to the placebo cohort. $\mathbf{X}_i$ is a vector of time-invariant controls, including separate dummies for each age in years, gender, country of origin, and household type. Standard errors are robust and clustered at the household level.

We also estimate a Cox proportional hazard model exploiting the full seven-year post-announcement period:

$$\begin{aligned} h_i(t) = h_0(t) \,\exp\Big\{ & \beta \,\text{Cohort}_i + \tau \,\text{CS}_i + \delta \,(\text{Cohort}_i \times \text{CS}_i) + \mathbf{X}_i \,\boldsymbol{\gamma} + \\ & \theta_1 \text{Age}_{it} + \theta_2 \text{Age}_{it}^2 + \theta_3 \text{Age}_{it}^3 \Big\} \end{aligned} \tag{5}$$

where $h_i(t)$ is the hazard of death for individual $i$ at month $t$, and $h_0(t)$ denotes the baseline hazard. The vector $\mathbf{X}_i$ includes controls for household type, country of origin, and gender. The model is estimated in person-month data. Age is included as a time-varying covariate in cubic form to flexibly capture age-related changes in mortality risk over time. Standard errors are robust and clustered at the household level. As in the linear probability model, $\delta$ captures the differential association for the 2014 cohort by baseline cost-sharing status, while $\beta$ and $\tau$ capture proportional differences in the hazard associated with cohort and cost-sharing status, respectively.

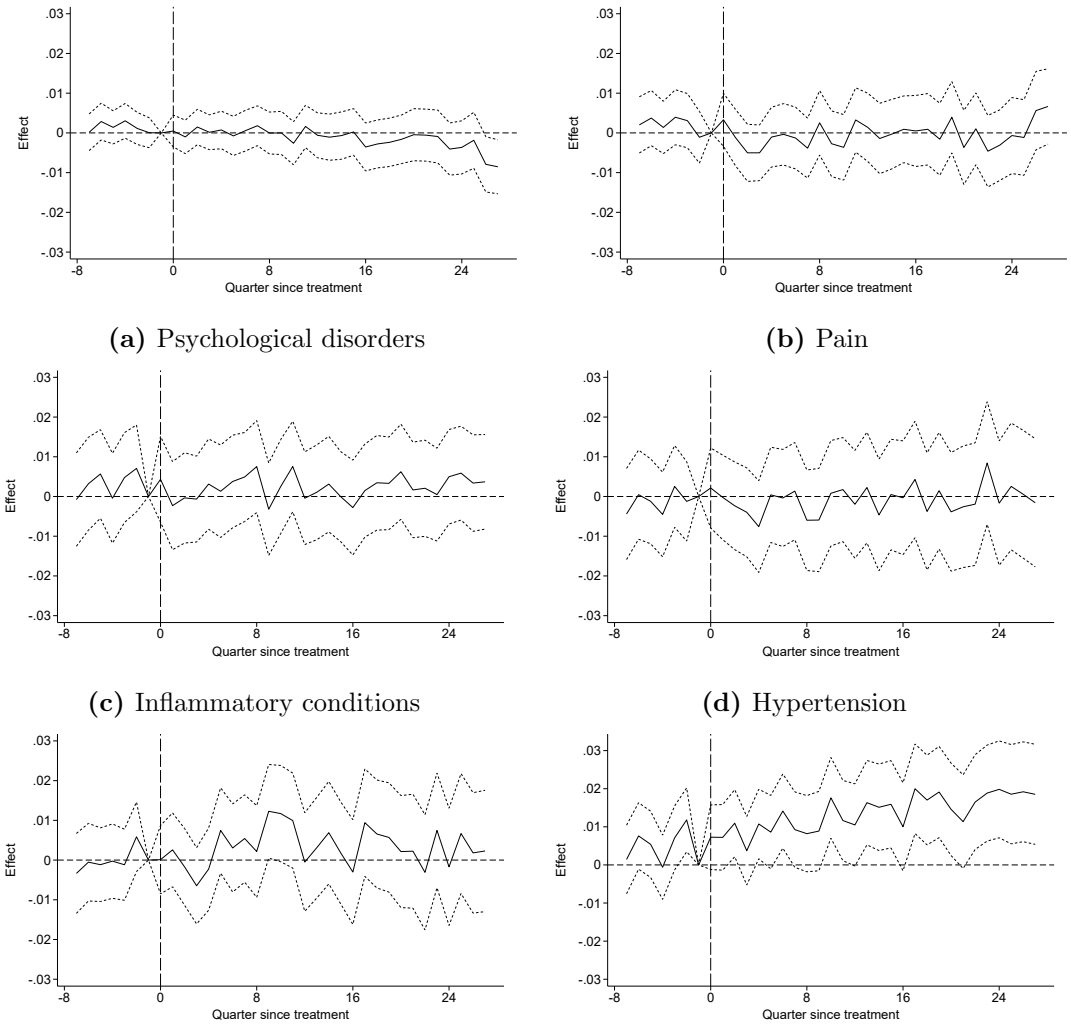
# D Additional validity checks

**Table D.1:** Tests of parallel pre-treatment trends

| | Pre-trend 8 quarters | | | Pre-trend 14 quarters | | |
|---|---|---|---|---|---|---|
| | coef | se | p-value | coef | se | p-value |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: Income-related outcomes** | | | | | | |
| AIO benefit receipt | -0.002 | 0.003 | 0.649 | 0.002 | 0.004 | 0.608 |
| AIO benefit amount (€) | -0.173 | 2.736 | 0.950 | 0.652 | 2.945 | 0.825 |
| Employed | 0.000 | 0.001 | 0.667 | -0.001 | 0.001 | 0.637 |
| Earnings (€) | 1.020 | 1.428 | 0.475 | 1.037 | 2.082 | 0.619 |
| Total personal income (€) (year) | -114*** | 38 | 0.003 | - | - | - |
| **Panel B: Health outcomes** | | | | | | |
| Psychological disorder | 0.001 | 0.002 | 0.629 | 0.002 | 0.002 | 0.458 |
| Pain | -0.003 | 0.003 | 0.236 | -0.002 | 0.003 | 0.410 |
| Inflammatory conditions | -0.002 | 0.004 | 0.568 | -0.002 | 0.004 | 0.612 |
| Hypertension | -0.003 | 0.004 | 0.428 | -0.003 | 0.004 | 0.476 |
| Cholesterol | 0.001 | 0.004 | 0.768 | 0.002 | 0.004 | 0.601 |
| Diabetes Mellitus | -0.001 | 0.003 | 0.786 | -0.001 | 0.003 | 0.739 |

*Note:* The table presents the coefficients, robust standard errors, and corresponding p-values of the pre-trend analysis for 8-quarter (Columns (1)–(3)) and 14-quarter (Columns (4)–(6)) periods. Each coefficient represents the average difference-in-differences estimate for the respective outcome in the corresponding pre-treatment period, obtained via Stata's lincom command. Panel A reports average monthly estimates, except for total personal income, which is presented on an annual basis and only for years t=-2 and t=-3. Panel B provides average quarterly estimates. Standard errors are robust and clustered at the household level. Number of individuals = 42,821; number of clusters = 36,601. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Figure D.1:** Placebo dynamic DiD estimates for health outcomes




**(a)** Psychological disorders

**(b)** Pain

**(c)** Inflammatory conditions

**(d)** Hypertension

**(e)** High cholesterol

**(f)** Diabetes mellitus

*Note:* The figures show quarterly placebo difference-in-differences (DiD) treatment effects with corresponding 95%-confidence intervals on health outcomes for a placebo-cohort of AIO recipients at November 2007. Psychological disorders are proxied using ATC-3 code N06A (antidepressants) only, as the alternative categories (N05B and N05C) exhibit a structural trend break beginning in Q1 2009. The DiD estimates are obtained using Equation (2), with quarter $t = -1$ serving as the baseline period (Q3 2007). The 95% confidence interval (dashed lines) is constructed using robust standard errors clustered at the household level. Number of individuals = 31,962; number of clusters = 26,958

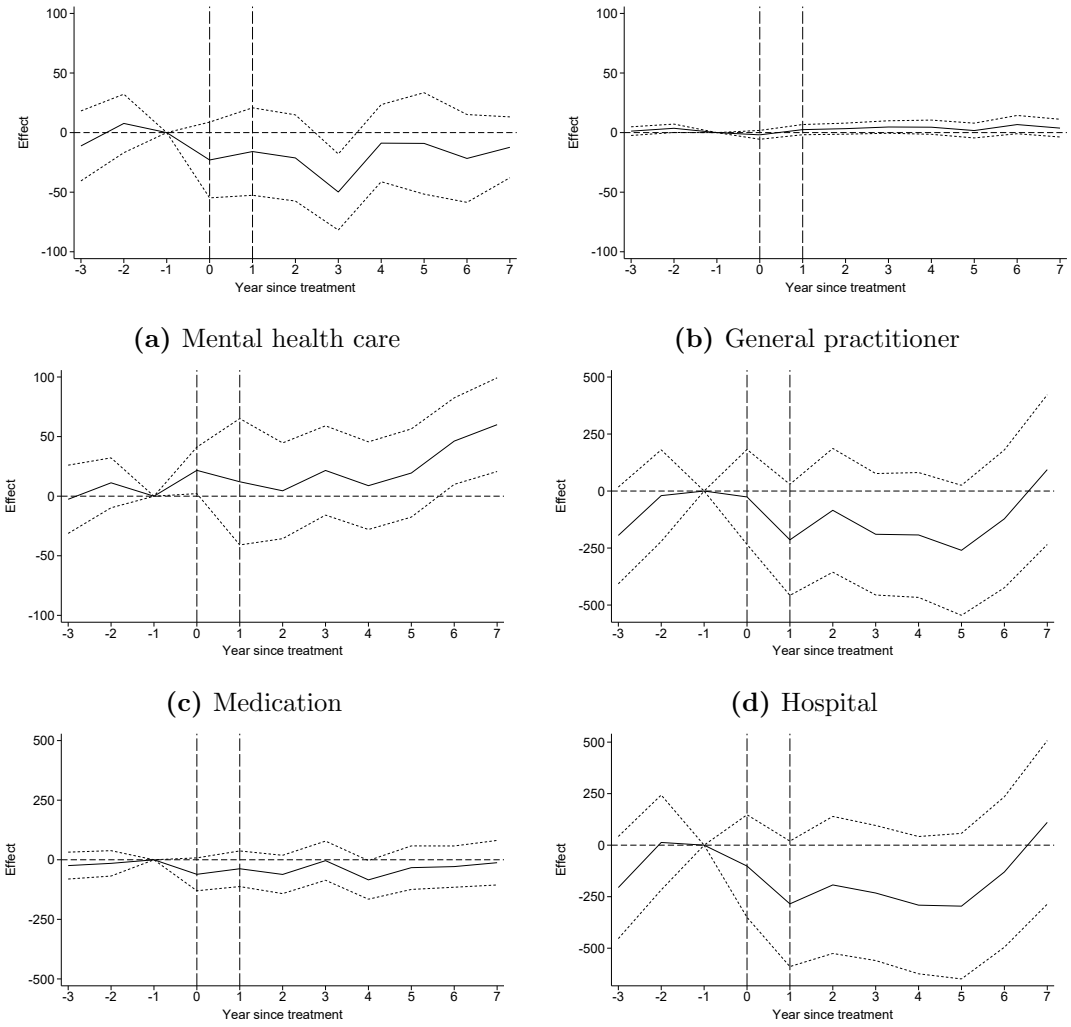
**Table D.2:** Compositional stability test

| | First period vs. baseline | Last period vs. baseline |
|---|---|---|
| | (1) | (2) |
| Female | -0.01 | -0.03 |
| Age (in years) | -0.01 | -0.01 |
| 65–69 years | 0.00 | 0.00 |
| 70–74 years | 0.00 | 0.01 |
| 75–79 years | 0.00 | -0.01 |
| 80–84 years | 0.00 | 0.00 |
| 85+ years | -0.01 | -0.01 |
| Country of origin | | |
| Turkey | 0.00 | 0.00 |
| Morocco | 0.01 | 0.03 |
| Surinam | 0.00 | 0.00 |
| Dutch Antilles / Aruba | 0.00 | -0.01 |
| Western | -0.01 | -0.01 |
| Other non-Western | 0.00 | -0.02 |
| Unknown | -0.01 | -0.02 |
| Benefit type: Single | -0.01 | -0.03 |
| Benefit type: Cohabit | 0.01 | 0.03 |
| # chronic diseases (all) | 0.01 | 0.02 |

*Note:* The table presents the results of a normalized difference test to assess differential compositional change due to attrition over time. All covariates are measured at baseline to ensure comparability over time. In Column (1), we compute the normalized differences between the treatment and control groups for both the first observed period (remaining sample in January 2011) and the baseline period, then calculate the difference between these values. Column (2) applies the same procedure using the last observed period (remaining sample in December 2021) and the baseline. Number of individuals: first period = 41,832; baseline period = 42,821; last period = 30,414.

# E Additional health outcomes

**Figure E.1:** Dynamic effects of the welfare cut on health care cost outcomes




**(a)** Mental health care

**(b)** General practitioner

**(c)** Medication

**(d)** Hospital

**(e)** Other

**(f)** Total

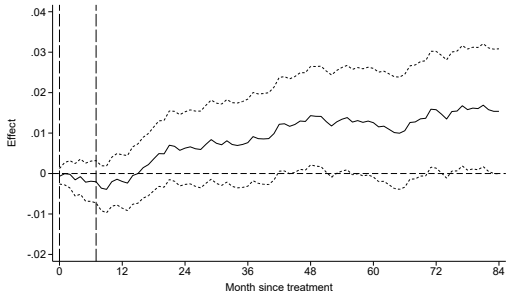
*Note:* The figures show annual difference-in-differences (DiD) treatment effects on health care cost outcomes, along with corresponding 95%-confidence intervals. The DiD estimates are obtained using Equation (2), with year $t = -1$ serving as the baseline period. Dashed lines represent 95% confidence intervals constructed using robust standard errors clustered at the household level. $t = 0$ and $t = 1$ indicate the *year* of announcement and implementation of the welfare cut, respectively. Number of individuals = 42,821; number of clusters = 36,601.

**Table E.1:** Pooled estimates of the effects of the welfare cut on health care cost outcomes

| | Specification | | | | Sample |
|---|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Non-winsorized | Balanced panel |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Mental health care** | | | | | |
| Coefficient | -19** | -19* | -16* | -33 | -16* |
| (s.e.) | (9) | (10) | (9) | (23) | (9) |
| Sample mean | 105 | 105 | 105 | 142 | 88 |
| **Panel B: GP** | | | | | |
| Coefficient | 1 | 1 | 1 | 2 | 0 |
| (s.e.) | (2) | (2) | (2) | (2) | (2) |
| Sample mean | 263 | 263 | 263 | 264 | 254 |
| **Panel C: Medication** | | | | | |
| Coefficient | 20 | 18 | 24* | 1 | 18 |
| (s.e.) | (14) | (13) | (14) | (20) | (13) |
| Sample mean | 898 | 898 | 898 | 916 | 805 |
| **Panel D: Hospital** | | | | | |
| Coefficient | -56 | 25 | -38 | -77 | 33 |
| (s.e.) | (83) | (78) | (83) | (87) | (71) |
| Sample mean | 2,982 | 2,982 | 2,982 | 3,016 | 2,390 |
| **Panel E: Other** | | | | | |
| Coefficient | -28 | -26 | -20 | -27 | -1 |
| (s.e.) | (19) | (19) | (19) | (21) | (17) |
| Sample mean | 780 | 780 | 780 | 790 | 626 |
| **Panel F: Total** | | | | | |
| Coefficient | -120 | -42 | -71 | -134 | -8 |
| (s.e.) | (100) | (94) | (100) | (106) | (86) |
| Sample mean | 5,090 | 5,090 | 5,090 | 5,127 | 4,209 |
| Number of observations | 42,821 | 42,821 | 42,821 | 42,821 | 29,306 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on health care cost outcomes for the full post-announcement period for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) presents results for non-winsorized outcomes. Column (5) restricts the main model to a balanced panel of individuals observed over the full 11-year period. All pooled DiD estimates use quarters $t = -1$ to $t = -3$ as the baseline period. Standard errors are robust and clustered at the household level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Figure E.2:** Dynamic effects of the welfare cut on mortality




*Note:* The figure presents difference-in-differences estimates of the effect of the welfare benefit cut on mortality. Treatment effects are calculated for the sample cohort (November 2014) relative to a placebo cohort (November 2007) of AIO recipients. Regressions are estimated separately for each month using Equation (4), controlling for a full set of baseline covariates. Dashed lines represent 95% confidence intervals constructed using robust standard errors clustered at the household level. $t = 0$ indicates the month of first announcement of the welfare cut. $t = 7$ is the month of the introduction of the welfare cut. Number of individuals = 74,783.

**Table E.2:** Proportional hazards estimates of the effect of the welfare cut on mortality

| | Full sample | By age | | By gender | |
|---|---|---|---|---|---|
| | | Below median | Above median | Male | Female |
| | (1) | (2) | (3) | (4) | (5) |
| Effect | 1.081 | 1.187 | 0.992 | 1.092 | 1.070 |
| (s.e.) | (0.043) | (0.079) | (0.051) | (0.065) | (0.058) |
| p-value | 0.052 | 0.010 | 0.879 | 0.139 | 0.213 |
| Number at risk | 5,477,840 | 3,212,301 | 2,265,539 | 2,199,146 | 3,278,694 |
| Number of deaths | 18,181 | 6,268 | 11,913 | 8,506 | 9,675 |

*Note:* This table presents the estimated hazard ratios from proportional hazard models. Column (1) shows results for the full sample, while Columns (2)–(3) and (4)–(5) present subsample analyses by age and gender. All regressions are estimated using Equation (5), controlling for a full set of baseline covariates and time-varying age. Coefficients are expressed as hazard ratios, and standard errors are clustered at the household level.

# F Robustness of the income-related outcomes

**Table F.1:** Robustness of pooled DiD estimates of the effect of the welfare cut on benefit receipt

| | Specification | | | Sample |
|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel |
| | (1) | (2) | (3) | (4) |
| **Panel A: Month 0-6** | | | | |
| Coefficient | -0.001 | -0.000 | -0.002 | -0.000 |
| (s.e.) | (0.003) | (0.003) | (0.002) | (0.004) |
| Sample mean | 0.989 | 0.989 | 0.989 | 0.990 |
| **Panel B: Month 7-30** | | | | |
| Coefficient | -0.298*** | -0.298*** | -0.283*** | -0.312*** |
| (s.e.) | (0.007) | (0.007) | (0.007) | (0.009) |
| Sample mean | 0.888 | 0.889 | 0.888 | 0.897 |
| **Panel C: Month 31-54** | | | | |
| Coefficient | -0.286*** | -0.286*** | -0.271*** | -0.302*** |
| (s.e.) | (0.008) | (0.008) | (0.007) | (0.009) |
| Sample mean | 0.853 | 0.853 | 0.853 | 0.866 |
| **Panel D: Month 55-84** | | | | |
| Coefficient | -0.280*** | -0.279*** | -0.265*** | -0.292*** |
| (s.e.) | (0.008) | (0.008) | (0.007) | (0.009) |
| Sample mean | 0.829 | 0.829 | 0.829 | 0.837 |
| **Panel E: Month 0-84** | | | | |
| Coefficient | -0.261*** | -0.256*** | -0.254*** | -0.277*** |
| (s.e.) | (0.007) | (0.007) | (0.006) | (0.008) |
| Sample mean | 0.869 | 0.869 | 0.869 | 0.875 |
| **Panel F: Month 7-84** | | | | |
| Coefficient | -0.288*** | -0.287*** | -0.274*** | -0.302*** |
| (s.e.) | (0.007) | (0.007) | (0.007) | (0.009) |
| Sample mean | 0.856 | 0.856 | 0.856 | 0.864 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on benefit receipt over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. All pooled DiD estimates use months $t = -1$ to $t = -24$ as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table F.2:** Robustness of pooled DiD estimates of the effect of the welfare cut on benefit amount

| | Specification | | | Sample |
|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel |
| | (1) | (2) | (3) | (4) |
| **Panel A: Month 0-6** | | | | |
| Coefficient | 4 | 3 | 10*** | 2 |
| (s.e.) | (3) | (2) | (2) | (2) |
| Sample mean | 353 | 353 | 353 | 339 |
| **Panel B: Month 7-30** | | | | |
| Coefficient | -155*** | -154*** | -145*** | -149*** |
| (s.e.) | (3) | (2) | (2) | (3) |
| Sample mean | 312 | 312 | 312 | 307 |
| **Panel C: Month 31-54** | | | | |
| Coefficient | -139*** | -135*** | -130*** | -132*** |
| (s.e.) | (3) | (3) | (3) | (3) |
| Sample mean | 306 | 306 | 306 | 303 |
| **Panel D: Month 55-84** | | | | |
| Coefficient | -127*** | -121*** | -118*** | -121*** |
| (s.e.) | (3) | (3) | (3) | (3) |
| Sample mean | 300 | 300 | 300 | 294 |
| **Panel E: Month 0-84** | | | | |
| Coefficient | -127*** | -123*** | -119*** | -122*** |
| (s.e.) | (3) | (2) | (2) | (3) |
| Sample mean | 310 | 310 | 310 | 304 |
| **Panel F: Month 7-84** | | | | |
| Coefficient | -140*** | -139*** | -129*** | -133*** |
| (s.e.) | (3) | (2) | (2) | (3) |
| Sample mean | 306 | 306 | 306 | 301 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on benefit amount over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. All pooled DiD estimates use months $t = -1$ to $t = -24$ as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table F.3:** Robustness of pooled DiD estimates of the effect of the welfare cut on being employed

| | Specification | | | Sample |
|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel |
| | (1) | (2) | (3) | (4) |
| **Panel A: Month 0-6** | | | | |
| Coefficient | -0.000 | -0.000 | -0.001 | -0.001 |
| (s.e.) | (0.001) | (0.001) | (0.001) | (0.001) |
| Sample mean | 0.002 | 0.002 | 0.002 | 0.003 |
| **Panel B: Month 7-30** | | | | |
| Coefficient | 0.001 | 0.001 | 0.002* | 0.001 |
| (s.e.) | (0.001) | (0.001) | (0.001) | (0.001) |
| Sample mean | 0.002 | 0.002 | 0.002 | 0.002 |
| **Panel C: Month 31-54** | | | | |
| Coefficient | 0.004*** | 0.003** | 0.004*** | 0.003** |
| (s.e.) | (0.001) | (0.001) | (0.001) | (0.002) |
| Sample mean | 0.002 | 0.002 | 0.002 | 0.003 |
| **Panel D: Month 55-84** | | | | |
| Coefficient | 0.004*** | 0.003** | 0.003** | 0.003* |
| (s.e.) | (0.001) | (0.002) | (0.001) | (0.002) |
| Sample mean | 0.002 | 0.002 | 0.002 | 0.002 |
| **Panel E: Month 0-84** | | | | |
| Coefficient | 0.003** | 0.002* | 0.003*** | 0.002 |
| (s.e.) | (0.001) | (0.001) | (0.001) | (0.001) |
| Sample mean | 0.002 | 0.002 | 0.002 | 0.002 |
| **Panel F: Month 7-84** | | | | |
| Coefficient | 0.003*** | 0.002** | 0.003*** | 0.003* |
| (s.e.) | (0.001) | (0.001) | (0.001) | (0.001) |
| Sample mean | 0.002 | 0.002 | 0.002 | 0.002 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on being employed over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. All pooled DiD estimates use months $t = -1$ to $t = -24$ as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table F.4:** Robustness of pooled DiD estimates of the effect of the welfare cut on earnings

| | Specification | | | Sample |
|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel |
| | (1) | (2) | (3) | (4) |
| **Panel A: Month 0-6** | | | | |
| Coefficient | -0.58 | -0.62 | -0.97 | -1.50 |
| (s.e.) | (1.39) | (1.40) | (1.35) | (1.95) |
| Sample mean | 0.92 | 0.92 | 0.92 | 1.11 |
| **Panel B: Month 7-30** | | | | |
| Coefficient | 1.89 | 1.50 | 3.08* | 1.32 |
| (s.e.) | (1.62) | (1.67) | (1.58) | (2.20) |
| Sample mean | 1.33 | 1.33 | 1.33 | 1.46 |
| **Panel C: Month 31-54** | | | | |
| Coefficient | 5.76*** | 4.87** | 6.31*** | 5.15* |
| (s.e.) | (2.10) | (2.21) | (2.06) | (2.69) |
| Sample mean | 2.16 | 2.16 | 2.16 | 2.20 |
| **Panel D: Month 55-84** | | | | |
| Coefficient | 8.52*** | 7.52*** | 8.61*** | 7.91** |
| (s.e.) | (2.69) | (2.85) | (2.67) | (3.22) |
| Sample mean | 2.65 | 2.65 | 2.65 | 2.70 |
| **Panel E: Month 0-84** | | | | |
| Coefficient | 4.83*** | 3.99** | 6.36*** | 4.50* |
| (s.e.) | (1.87) | (1.88) | (1.83) | (2.46) |
| Sample mean | 1.94 | 1.94 | 1.94 | 2.08 |
| **Panel F: Month 7-84** | | | | |
| Coefficient | 5.40*** | 4.45** | 6.81*** | 5.03** |
| (s.e.) | (1.96) | (1.98) | (1.93) | (2.54) |
| Sample mean | 2.05 | 2.05 | 2.05 | 2.16 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on earnings over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. All pooled DiD estimates use months $t = -1$ to $t = -24$ as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table F.5:** Robustness of pooled DiD estimates of the effect of the welfare cut on annual personal income

| | Specification | | | Sample |
|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel |
| | (1) | (2) | (3) | (4) |
| **Panel A: Year 0-2** | | | | |
| Coefficient | -856*** | -852*** | -1,468*** | -850*** |
| (s.e.) | (39) | (39) | (38) | (49) |
| Sample mean | 12,109 | 12,109 | 12,109 | 12,170 |
| **Panel B: Year 3-5** | | | | |
| Coefficient | -1,654*** | -1,699*** | -1,713*** | -1,625*** |
| (s.e.) | (53) | (54) | (52) | (63) |
| Sample mean | 11,901 | 11,901 | 11,901 | 11,992 |
| **Panel C: Year 6-7** | | | | |
| Coefficient | -1,555*** | -1,576*** | -1,697*** | -1,552*** |
| (s.e.) | (62) | (67) | (61) | (70) |
| Sample mean | 11,906 | 11,906 | 11,906 | 11,947 |
| **Panel D: Year 0-7** | | | | |
| Coefficient | -1,298*** | -1,291*** | -1,484*** | -1,316*** |
| (s.e.) | (45) | (44) | (44) | (55) |
| Sample mean | 11,989 | 11,989 | 11,989 | 12,047 |
| **Panel E: Year 1-7** | | | | |
| Coefficient | -1,542*** | -1,572*** | -1,659*** | -1,525*** |
| (s.e.) | (48) | (49) | (48) | (59) |
| Sample mean | 11,958 | 11,958 | 11,958 | 12,024 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on annual personal income over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. All pooled DiD estimates use years $t = -1$ to $t = -3$ as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

# G Robustness of the health-related outcomes

**Table G.1:** Robustness of pooled DiD estimates of the effect of the welfare cut on psychological disorder

| | Specification | | | Sample | Placebo |
|---|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel | DiD |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Quarter 0-2** | | | | | |
| Coefficient | 0.002 | 0.002 | 0.001 | 0.001 | -0.001 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Sample mean | 0.092 | 0.092 | 0.092 | 0.083 | 0.040 |
| **Panel B: Quarter 3-10** | | | | | |
| Coefficient | 0.004 | 0.003 | 0.003 | 0.002 | -0.001 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) |
| Sample mean | 0.094 | 0.094 | 0.094 | 0.086 | 0.043 |
| **Panel C: Quarter 11-18** | | | | | |
| Coefficient | 0.004 | 0.002 | 0.002 | -0.001 | -0.002 |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) |
| Sample mean | 0.098 | 0.098 | 0.098 | 0.093 | 0.047 |
| **Panel D: Quarter 19-28** | | | | | |
| Coefficient | 0.004 | 0.002 | 0.002 | 0.001 | -0.004* |
| (s.e.) | (0.004) | (0.003) | (0.004) | (0.004) | (0.003) |
| Sample mean | 0.101 | 0.101 | 0.101 | 0.099 | 0.053 |
| **Panel E: Quarter 0-28** | | | | | |
| Coefficient | 0.004 | 0.002 | 0.002 | 0.001 | -0.002 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) |
| Sample mean | 0.097 | 0.097 | 0.097 | 0.092 | 0.047 |
| **Panel F: Quarter 3-28** | | | | | |
| Coefficient | 0.004 | 0.002 | 0.002 | 0.001 | -0.003 |
| (s.e.) | (0.003) | (0.002) | (0.003) | (0.003) | (0.002) |
| Sample mean | 0.097 | 0.097 | 0.097 | 0.093 | 0.048 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on psychological disorders over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. Column (5) shows placebo estimates for a cohort of AIO recipients at November 2007. In the latter cohort, psychological disorders are measured differently and include only ATC-3 code N06A (antidepressants). All pooled DiD estimates use quarters $t = -1$ to $t = -8$ as the baseline period, except column (5) which uses $t = -1$ to $t = -6$. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306; for Column 5 = 31,962. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table G.2:** Robustness of pooled DiD estimates of the effect of the welfare cut on pain

| | Specification | | | Sample | Placebo |
|---|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel | DiD |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Quarter 0-2** | | | | | |
| Coefficient | -0.000 | -0.001 | 0.000 | 0.000 | -0.003 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) |
| Sample mean | 0.082 | 0.082 | 0.082 | 0.073 | 0.073 |
| **Panel B: Quarter 3-10** | | | | | |
| Coefficient | -0.000 | -0.002 | -0.001 | -0.002 | -0.004* |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) |
| Sample mean | 0.085 | 0.085 | 0.085 | 0.076 | 0.076 |
| **Panel C: Quarter 11-18** | | | | | |
| Coefficient | 0.001 | -0.001 | -0.002 | -0.004 | -0.001 |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Sample mean | 0.087 | 0.087 | 0.087 | 0.080 | 0.085 |
| **Panel D: Quarter 19-28** | | | | | |
| Coefficient | 0.000 | -0.003 | -0.002 | -0.004 | -0.001 |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Sample mean | 0.083 | 0.083 | 0.083 | 0.080 | 0.083 |
| **Panel E: Quarter 0-28** | | | | | |
| Coefficient | 0.000 | -0.002 | -0.004* | -0.003 | -0.002 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Sample mean | 0.085 | 0.085 | 0.085 | 0.078 | 0.080 |
| **Panel F: Quarter 3-28** | | | | | |
| Coefficient | 0.000 | -0.002 | -0.004* | -0.004 | -0.002 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Sample mean | 0.085 | 0.085 | 0.085 | 0.079 | 0.081 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on pain over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. Column (5) shows placebo estimates for a cohort of AIO recipients at November 2007. All pooled DiD estimates use quarters $t = -1$ to $t = -8$ as the baseline period, except column (5) which uses $t = -1$ to $t = -6$. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306; for Column 5 = 31,962. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table G.3:** Robustness of pooled DiD estimates of the effect of the welfare cut on inflammatory conditions

| | Specification | | | Sample | Placebo |
|---|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel | DiD |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Quarter 0-2** | | | | | |
| Coefficient | 0.001 | 0.001 | -0.001 | 0.003 | -0.002 |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Sample mean | 0.106 | 0.106 | 0.106 | 0.115 | 0.156 |
| **Panel B: Quarter 3-10** | | | | | |
| Coefficient | 0.002 | 0.002 | 0.000 | 0.001 | -0.000 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.003) | (0.003) |
| Sample mean | 0.092 | 0.092 | 0.092 | 0.100 | 0.138 |
| **Panel C: Quarter 11-18** | | | | | |
| Coefficient | 0.001 | 0.001 | 0.001 | 0.000 | -0.001 |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Sample mean | 0.081 | 0.081 | 0.081 | 0.086 | 0.120 |
| **Panel D: Quarter 19-28** | | | | | |
| Coefficient | 0.003 | 0.002 | 0.003 | 0.000 | 0.001 |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.003) | (0.004) |
| Sample mean | 0.068 | 0.068 | 0.068 | 0.070 | 0.102 |
| **Panel E: Quarter 0-28** | | | | | |
| Coefficient | 0.002 | 0.002 | 0.000 | 0.001 | -0.000 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.003) | (0.003) |
| Sample mean | 0.083 | 0.083 | 0.083 | 0.087 | 0.125 |
| **Panel F: Quarter 3-28** | | | | | |
| Coefficient | 0.002 | 0.002 | 0.000 | 0.001 | -0.000 |
| (s.e.) | (0.002) | (0.003) | (0.002) | (0.003) | (0.003) |
| Sample mean | 0.080 | 0.080 | 0.080 | 0.084 | 0.121 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on inflammatory conditions over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. Column (5) shows placebo estimates for a cohort of AIO recipients at November 2007. All pooled DiD estimates use quarters $t = -1$ to $t = -8$ as the baseline period, except column (5) which uses $t = -1$ to $t = -6$. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306; for Column 5 = 31,962. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table G.4:** Robustness of pooled DiD estimates of the effect of the welfare cut on hypertension

| | Specification | | | Sample | Placebo |
|---|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel | DiD |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Quarter 0-2** | | | | | |
| Coefficient | -0.001 | -0.001 | -0.005 | 0.003 | 0.001 |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) |
| Sample mean | 0.515 | 0.515 | 0.515 | 0.491 | 0.423 |
| **Panel B: Quarter 3-10** | | | | | |
| Coefficient | 0.005 | 0.002 | -0.001 | 0.002 | -0.001 |
| (s.e.) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Sample mean | 0.526 | 0.526 | 0.526 | 0.511 | 0.439 |
| **Panel C: Quarter 11-18** | | | | | |
| Coefficient | 0.011** | 0.007 | 0.009 | 0.009 | 0.001 |
| (s.e.) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Sample mean | 0.542 | 0.542 | 0.542 | 0.537 | 0.474 |
| **Panel D: Quarter 19-28** | | | | | |
| Coefficient | 0.018*** | 0.014** | 0.012* | 0.015** | 0.001 |
| (s.e.) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| Sample mean | 0.557 | 0.557 | 0.557 | 0.560 | 0.510 |
| **Panel E: Quarter 0-28** | | | | | |
| Coefficient | 0.010** | 0.006 | 0.004 | 0.008* | 0.000 |
| (s.e.) | (0.004) | (0.004) | (0.004) | (0.005) | (0.004) |
| Sample mean | 0.538 | 0.538 | 0.538 | 0.533 | 0.468 |
| **Panel F: Quarter 3-28** | | | | | |
| Coefficient | 0.011** | 0.007 | 0.006 | 0.009* | 0.000 |
| (s.e.) | (0.005) | (0.004) | (0.005) | (0.005) | (0.004) |
| Sample mean | 0.542 | 0.542 | 0.542 | 0.538 | 0.474 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on hypertension over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. Column (5) shows placebo estimates for a cohort of AIO recipients at November 2007. All pooled DiD estimates use quarters $t = -1$ to $t = -8$ as the baseline period, except column (5) which uses $t = -1$ to $t = -6$. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306; for Column 5 = 31,962. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table G.5:** Robustness of pooled DiD estimates of the effect of the welfare cut on high cholesterol

| | Specification | | | Sample | Placebo |
|---|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel | DiD |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Quarter 0-2** | | | | | |
| Coefficient | 0.004 | 0.004 | 0.005* | 0.005 | 0.000 |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) |
| Sample mean | 0.411 | 0.411 | 0.411 | 0.406 | 0.263 |
| **Panel B: Quarter 3-10** | | | | | |
| Coefficient | 0.007* | 0.004 | 0.007* | 0.006 | 0.004 |
| (s.e.) | (0.004) | (0.002) | (0.004) | (0.004) | (0.004) |
| Sample mean | 0.426 | 0.426 | 0.426 | 0.427 | 0.287 |
| **Panel C: Quarter 11-18** | | | | | |
| Coefficient | 0.015** | 0.011** | 0.016** | 0.014** | 0.004 |
| (s.e.) | (0.005) | (0.003) | (0.005) | (0.005) | (0.005) |
| Sample mean | 0.446 | 0.446 | 0.446 | 0.452 | 0.334 |
| **Panel D: Quarter 19-28** | | | | | |
| Coefficient | 0.024*** | 0.019*** | 0.025*** | 0.021*** | 0.003 |
| (s.e.) | (0.006) | (0.003) | (0.006) | (0.006) | (0.006) |
| Sample mean | 0.453 | 0.453 | 0.453 | 0.460 | 0.381 |
| **Panel E: Quarter 0-28** | | | | | |
| Coefficient | 0.014*** | 0.010** | 0.016*** | 0.013*** | 0.003 |
| (s.e.) | (0.004) | (0.002) | (0.004) | (0.004) | (0.004) |
| Sample mean | 0.438 | 0.438 | 0.438 | 0.443 | 0.325 |
| **Panel F: Quarter 3-28** | | | | | |
| Coefficient | 0.016*** | 0.011** | 0.017*** | 0.014*** | 0.004 |
| (s.e.) | (0.004) | (0.003) | (0.004) | (0.005) | (0.004) |
| Sample mean | 0.442 | 0.442 | 0.442 | 0.447 | 0.333 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on high cholesterol over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. Column (5) shows placebo estimates for a cohort of AIO recipients at November 2007. All pooled DiD estimates use quarters $t = -1$ to $t = -8$ as the baseline period, except column (5) which uses $t = -1$ to $t = -6$. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306; for Column 5 = 31,962. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table G.6:** Robustness of pooled DiD estimates of the effect of the welfare cut on diabetes mellitus

| | Specification | | | Sample | Placebo |
|---|---|---|---|---|---|
| | Main model | TWFE | Doubly robust | Balanced panel | DiD |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Quarter 0-2** | | | | | |
| Coefficient | 0.001 | 0.001 | -0.000 | 0.004 | 0.004 |
| (s.e.) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Sample mean | 0.332 | 0.332 | 0.332 | 0.313 | 0.296 |
| **Panel B: Quarter 3-10** | | | | | |
| Coefficient | 0.005* | -0.000 | 0.004 | 0.002 | 0.005* |
| (s.e.) | (0.003) | (0.002) | (0.003) | (0.003) | (0.003) |
| Sample mean | 0.332 | 0.332 | 0.332 | 0.319 | 0.301 |
| **Panel C: Quarter 11-18** | | | | | |
| Coefficient | 0.009** | -0.000 | 0.004 | 0.001 | 0.010** |
| (s.e.) | (0.004) | (0.003) | (0.004) | (0.004) | (0.004) |
| Sample mean | 0.337 | 0.337 | 0.337 | 0.331 | 0.320 |
| **Panel D: Quarter 19-28** | | | | | |
| Coefficient | 0.012** | 0.001 | 0.006 | 0.001 | 0.013** |
| (s.e.) | (0.005) | (0.004) | (0.005) | (0.004) | (0.005) |
| Sample mean | 0.339 | 0.339 | 0.339 | 0.339 | 0.337 |
| **Panel E: Quarter 0-28** | | | | | |
| Coefficient | 0.008** | -0.000 | 0.002 | 0.002 | 0.009*** |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Sample mean | 0.335 | 0.335 | 0.335 | 0.328 | 0.316 |
| **Panel F: Quarter 3-28** | | | | | |
| Coefficient | 0.009** | -0.000 | 0.003 | 0.001 | 0.009*** |
| (s.e.) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Sample mean | 0.336 | 0.336 | 0.336 | 0.330 | 0.319 |

*Note:* The table presents pooled difference-in-differences (DiD) treatment effects on diabetes mellitus over various treatment periods for different model specifications and samples. Column (1) presents results from the main specification. Column (2) adds individual fixed effects (two-way fixed effects). Column (3) provides results from a doubly robust DiD method following Callaway and Sant'Anna (2021) and including covariates for female, country of origin, age (years), and type of benefit (cohabit). Column (4) restricts the main model to a balanced panel of individuals observed over the full 11-year period. Column (5) shows placebo estimates for a cohort of AIO recipients at November 2007. All pooled DiD estimates use quarters $t = -1$ to $t = -8$ as the baseline period, except column (5) which uses $t = -1$ to $t = -6$. Standard errors are robust and clustered at the household level. Number of individuals: for Columns 1–3 = 42,821; for Column 4 = 29,306; for Column 5 = 31,962. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

# H Heterogeneous treatment effects

**Table H.1:** Descriptive statistics by subgroup

| | By age | | By gender | |
|---|---|---|---|---|
| | Below median | Above median | Male | Female |
| | (1) | (2) | (3) | (4) |
| Female | 0.588 | 0.609 | - | - |
| Age (in years) | 68.7 | 78.2 | 73.1 | 73.7 |
| 65–69 years | 0.677 | - | 0.345 | 0.335 |
| 70–74 years | 0.323 | 0.286 | 0.316 | 0.297 |
| 75–79 years | - | 0.421 | 0.218 | 0.205 |
| 80–84 years | - | 0.199 | 0.091 | 0.105 |
| 85+ years | - | 0.093 | 0.030 | 0.058 |
| Country of origin | | | | |
| Western | 0.176 | 0.166 | 0.153 | 0.183 |
| Turkey | 0.131 | 0.149 | 0.143 | 0.138 |
| Morocco | 0.156 | 0.191 | 0.218 | 0.143 |
| Surinam | 0.164 | 0.213 | 0.135 | 0.224 |
| Dutch Antilles/Aruba | 0.049 | 0.041 | 0.031 | 0.055 |
| Other non-Western | 0.321 | 0.239 | 0.318 | 0.254 |
| Unknown | 0.003 | 0.002 | 0.002 | 0.002 |
| Benefit type: Single | 0.571 | 0.608 | 0.364 | 0.740 |
| Benefit type: Cohabit | 0.429 | 0.392 | 0.636 | 0.260 |
| Has cost sharers in household | 0.150 | 0.155 | 0.145 | 0.158 |
| N | 21,451 | 21,370 | 17,196 | 25,625 |

*Note:* Descriptive statistics for subgroups in the month prior to treatment (November 2014).

**Table H.2:** Effects of the welfare cut for subgroups: Men vs. women

| | Benefit receipt | Benefit amount | Psychological disorder | Pain | Inflammatory conditions | Hypertension | High cholesterol | Diabetes mellitus |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Panel A: Quarter 0-2** | | | | | | | | |
| 1. Men | | | | | | | | |
| Coefficient | -0.003 | -4* | 0.005 | 0.002 | -0.001 | -0.006 | 0.006 | 0.003 |
| (s.e.) | (0.005) | (3) | (0.003) | (0.003) | (0.004) | (0.005) | (0.005) | (0.004) |
| Sample mean | 0.985 | 306 | 0.073 | 0.066 | 0.091 | 0.491 | 0.424 | 0.327 |
| 2. Women | | | | | | | | |
| Coefficient | 0.000 | 9* | 0.000 | -0.002 | 0.002 | 0.003 | 0.003 | -0.001 |
| (s.e.) | (0.004) | (4) | (0.003) | (0.003) | (0.003) | (0.004) | (0.004) | (0.003) |
| Sample mean | 0.991 | 384 | 0.104 | 0.093 | 0.117 | 0.531 | 0.402 | 0.336 |
| **Panel B: Quarter 3-10** | | | | | | | | |
| 1. Men | | | | | | | | |
| Coefficient | -0.246*** | -102*** | 0.009** | -0.001 | -0.004 | 0.008 | 0.012* | 0.008* |
| (s.e.) | (0.010) | (3) | (0.003) | (0.003) | (0.004) | (0.006) | (0.006) | (0.004) |
| Sample mean | 0.889 | 279 | 0.072 | 0.067 | 0.079 | 0.501 | 0.440 | 0.327 |
| 2. Women | | | | | | | | |
| Coefficient | -0.330*** | -187*** | 0.000 | 0.000 | 0.007** | 0.004 | 0.004 | 0.003 |
| (s.e.) | (0.009) | (3) | (0.003) | (0.003) | (0.003) | (0.005) | (0.005) | (0.004) |
| Sample mean | 0.887 | 334 | 0.108 | 0.097 | 0.101 | 0.542 | 0.417 | 0.335 |
| **Panel C: Quarter 11-18** | | | | | | | | |
| 1. Men | | | | | | | | |
| Coefficient | -0.239*** | -84*** | 0.014*** | 0.002 | -0.004 | 0.020** | 0.029*** | 0.021*** |
| (s.e.) | (0.011) | (4) | (0.004) | (0.004) | (0.004) | (0.008) | (0.008) | (0.006) |
| Sample mean | 0.847 | 272 | 0.075 | 0.069 | 0.070 | 0.518 | 0.465 | 0.334 |
| 2. Women | | | | | | | | |
| Coefficient | -0.314*** | -171*** | -0.002 | 0.001 | 0.006 | 0.006 | 0.007 | 0.002 |
| (s.e.) | (0.009) | (4) | (0.004) | (0.004) | (0.004) | (0.006) | (0.006) | (0.005) |
| Sample mean | 0.857 | 328 | 0.113 | 0.098 | 0.088 | 0.557 | 0.434 | 0.338 |
| **Panel D: Quarter 19-28** | | | | | | | | |
| 1. Men | | | | | | | | |
| Coefficient | -0.237*** | -74*** | 0.009* | -0.000 | -0.003 | 0.032*** | 0.033*** | 0.026*** |
| (s.e.) | (0.012) | (4) | (0.005) | (0.004) | (0.005) | (0.010) | (0.010) | (0.008) |
| Sample mean | 0.821 | 266 | 0.077 | 0.064 | 0.057 | 0.533 | 0.478 | 0.340 |
| 2. Women | | | | | | | | |
| Coefficient | -0.305*** | -158*** | 0.002 | 0.001 | 0.007* | 0.009 | 0.019** | 0.004 |
| (s.e.) | (0.010) | (4) | (0.005) | (0.004) | (0.004) | (0.008) | (0.008) | (0.006) |
| Sample mean | 0.833 | 320 | 0.116 | 0.095 | 0.074 | 0.572 | 0.438 | 0.338 |
| **Panel E: Quarter 0-28** | | | | | | | | |
| 1. Men | | | | | | | | |
| Coefficient | -0.218*** | -79*** | 0.010** | 0.000 | -0.003 | 0.017** | 0.022*** | 0.017*** |
| (s.e.) | (0.010) | (3) | (0.003) | (0.003) | (0.003) | (0.007) | (0.006) | (0.005) |
| Sample mean | 0.865 | 276 | 0.074 | 0.067 | 0.071 | 0.514 | 0.456 | 0.333 |
| 2. Women | | | | | | | | |
| Coefficient | -0.287*** | -155*** | 0.000 | 0.000 | 0.006** | 0.006 | 0.009* | 0.003 |
| (s.e.) | (0.008) | (3) | (0.003) | (0.003) | (0.003) | (0.005) | (0.005) | (0.004) |
| Sample mean | 0.871 | 332 | 0.111 | 0.096 | 0.091 | 0.554 | 0.426 | 0.337 |
| **Panel F: Quarter 3-28** | | | | | | | | |
| 1. Men | | | | | | | | |
| Coefficient | -0.241*** | -87*** | 0.010** | 0.000 | -0.004 | 0.020*** | 0.024*** | 0.018*** |
| (s.e.) | (0.011) | (4) | (0.004) | (0.003) | (0.004) | (0.007) | (0.007) | (0.006) |
| Sample mean | 0.853 | 273 | 0.075 | 0.067 | 0.069 | 0.517 | 0.461 | 0.334 |
| 2. Women | | | | | | | | |
| Coefficient | -0.316*** | -172*** | 0.000 | 0.001 | 0.006** | 0.006 | 0.010* | 0.002 |
| (s.e.) | (0.009) | (4) | (0.004) | (0.003) | (0.003) | (0.006) | (0.006) | (0.004) |
| Sample mean | 0.859 | 327 | 0.112 | 0.097 | 0.088 | 0.557 | 0.429 | 0.337 |

*Note:* The table shows pooled difference-in-differences (DiD) treatment effects on health outcomes for the corresponding treatment period, separately for men (subpanel 1) and women (subpanel 2). The pooled DiD estimates are obtained using Equation (3), with quarters $t = -1$ to $t = -8$ serving as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals: men = 17,196; women = 25,625. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

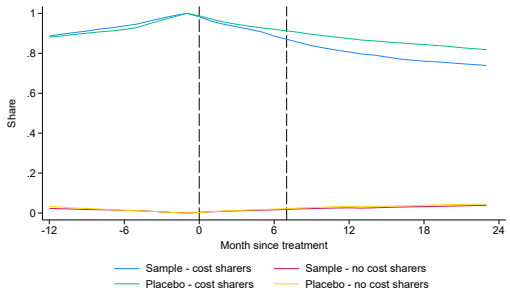
**Table H.3:** Effects of the welfare cut for subgroups: Younger vs. older individuals

| | Benefit receipt | Benefit amount | Psychological disorder | Pain | Inflammatory conditions | Hypertension | High cholesterol | Diabetes mellitus |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Panel A: Quarter 0-2** | | | | | | | | |
| 1. Age below median | | | | | | | | |
| Coefficient | 0.000 | -1 | 0.003 | -0.002 | 0.002 | 0.001 | 0.005 | 0.000 |
| (s.e.) | (0.006) | (3) | (0.003) | (0.003) | (0.004) | (0.004) | (0.004) | (0.003) |
| Sample mean | 0.988 | 338 | 0.091 | 0.075 | 0.120 | 0.475 | 0.400 | 0.319 |
| 2. Age above median | | | | | | | | |
| Coefficient | -0.000 | 10** | 0.000 | 0.002 | 0.000 | -0.002 | 0.002 | 0.001 |
| (s.e.) | (0.003) | (4) | (0.003) | (0.003) | (0.004) | (0.005) | (0.004) | (0.003) |
| Sample mean | 0.989 | 368 | 0.092 | 0.089 | 0.092 | 0.555 | 0.422 | 0.346 |
| **Panel B: Quarter 3-10** | | | | | | | | |
| 1. Age below median | | | | | | | | |
| Coefficient | -0.292*** | -130*** | 0.002 | -0.001 | 0.001 | 0.006 | 0.009 | -0.001 |
| (s.e.) | (0.010) | (3) | (0.003) | (0.003) | (0.004) | (0.005) | (0.005) | (0.004) |
| Sample mean | 0.893 | 306 | 0.093 | 0.079 | 0.105 | 0.493 | 0.423 | 0.324 |
| 2. Age above median | | | | | | | | |
| Coefficient | -0.301*** | -179*** | 0.004 | -0.000 | 0.004 | 0.004 | 0.005 | 0.010** |
| (s.e.) | (0.009) | (4) | (0.004) | (0.003) | (0.003) | (0.006) | (0.006) | (0.004) |
| Sample mean | 0.883 | 318 | 0.094 | 0.092 | 0.079 | 0.561 | 0.429 | 0.340 |
| **Panel C: Quarter 11-18** | | | | | | | | |
| 1. Age below median | | | | | | | | |
| Coefficient | -0.279*** | -111*** | -0.002 | -0.005 | -0.001 | 0.006 | 0.015** | 0.002 |
| (s.e.) | (0.011) | (4) | (0.004) | (0.004) | (0.004) | (0.007) | (0.007) | (0.005) |
| Sample mean | 0.863 | 303 | 0.098 | 0.081 | 0.093 | 0.518 | 0.454 | 0.335 |
| 2. Age above median | | | | | | | | |
| Coefficient | -0.291*** | -166*** | 0.009** | 0.007 | 0.002 | 0.016** | 0.015 | 0.014** |
| (s.e.) | (0.010) | (4) | (0.005) | (0.004) | (0.004) | (0.008) | (0.008) | (0.006) |
| Sample mean | 0.842 | 309 | 0.098 | 0.093 | 0.068 | 0.568 | 0.437 | 0.339 |
| **Panel D: Quarter 19-28** | | | | | | | | |
| 1. Age below median | | | | | | | | |
| Coefficient | -0.264*** | -99*** | 0.000 | -0.004 | -0.004 | 0.013 | 0.025*** | 0.002 |
| (s.e.) | (0.011) | (4) | (0.005) | (0.004) | (0.005) | (0.008) | (0.008) | (0.006) |
| Sample mean | 0.842 | 298 | 0.098 | 0.079 | 0.078 | 0.541 | 0.474 | 0.343 |
| 2. Age above median | | | | | | | | |
| Coefficient | -0.295*** | -157*** | 0.007 | 0.004 | 0.009** | 0.022** | 0.020** | 0.020*** |
| (s.e.) | (0.011) | (5) | (0.005) | (0.005) | (0.004) | (0.009) | (0.009) | (0.008) |
| Sample mean | 0.813 | 301 | 0.104 | 0.090 | 0.055 | 0.576 | 0.427 | 0.333 |
| **Panel E: Quarter 0-28** | | | | | | | | |
| 1. Age below median | | | | | | | | |
| Coefficient | -0.253*** | -103*** | 0.000 | -0.003 | -0.001 | 0.008 | 0.015*** | 0.001 |
| (s.e.) | (0.010) | (3) | (0.003) | (0.003) | (0.003) | (0.006) | (0.005) | (0.004) |
| Sample mean | 0.876 | 305 | 0.096 | 0.079 | 0.094 | 0.514 | 0.445 | 0.333 |
| 2. Age above median | | | | | | | | |
| Coefficient | -0.266*** | -149*** | 0.006 | 0.003 | 0.004 | 0.012* | 0.012* | 0.013*** |
| (s.e.) | (0.009) | (4) | (0.004) | (0.003) | (0.003) | (0.006) | (0.006) | (0.005) |
| Sample mean | 0.861 | 316 | 0.098 | 0.091 | 0.071 | 0.566 | 0.430 | 0.338 |
| **Panel F: Quarter 3-28** | | | | | | | | |
| 1. Age below median | | | | | | | | |
| Coefficient | -0.278*** | -113*** | -0.000 | -0.003 | -0.001 | 0.008 | 0.017*** | 0.001 |
| (s.e.) | (0.010) | (3) | (0.004) | (0.003) | (0.004) | (0.006) | (0.006) | (0.004) |
| Sample mean | 0.865 | 302 | 0.096 | 0.079 | 0.091 | 0.518 | 0.451 | 0.334 |
| 2. Age above median | | | | | | | | |
| Coefficient | -0.296*** | -167*** | 0.007 | 0.003 | 0.005 | 0.014** | 0.013* | 0.014*** |
| (s.e.) | (0.009) | (4) | (0.004) | (0.003) | (0.003) | (0.007) | (0.007) | (0.005) |
| Sample mean | 0.847 | 310 | 0.099 | 0.091 | 0.068 | 0.568 | 0.431 | 0.337 |

*Note:* The table shows pooled difference-in-differences (DiD) treatment effects on health outcomes for the corresponding treatment period, separately for individuals below the median age (subpanel 1) and above the median age (subpanel 2). The pooled DiD estimates are obtained using Equation (3), with quarters $t = -1$ to $t = -8$ serving as the baseline period. Standard errors are robust and clustered at the household level. Number of individuals: below median = 21,451; above median = 21,370. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

# I Additional analyses

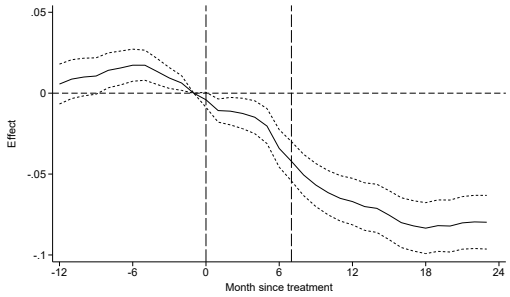
**Figure I.1:** Trends in having cost sharers: Sample vs. placebo cohort




**(a)** Has cost sharers in household

*Note:* The figure shows trends in the share of AIO recipients with cost sharers in the household for both the sample cohort (November 2014) and the placebo cohort (November 2012). For each cohort, trends are shown separately for individuals with and without cost sharers in month $t = -1$. In the sample cohort, $t = 0$ indicates the announcement of the welfare cut, and $t = 7$ its implementation. Number of individuals sample cohort: with cost sharers = 6,533; no cost sharers = 36,288; Number of individuals placeb cohort: with cost sharers = 6,411; no cost sharers = 35,461.

**Figure I.2:** Dynamic DiD estimates for the effect of the welfare cut on having cost sharers




**(a)** Has cost sharers in household

*Note:* The figure presents difference-in-differences (DiD) estimates, with corresponding 95%-confidence intervals, of the effect of the welfare benefit cut on the probability of co-residing with cost sharers. The treatment group comprises AIO recipients with cost sharers in November 2014, while the control group comprises AIO recipients with cost sharers in November 2012. Estimates are based on Equation (2), with month $t = -1$ serving as the baseline period. Dashed lines represent 95% confidence intervals constructed using robust standard errors clustered at the household level. Number of individuals = 12,944.