The Financial and Behavioral Effects of Free Prescription Drugs: Evidence from a Policy Discontinuity in Poland

Gosia Majewska[1] and Krzysztof Zaremba[2]

[1]Department of Economics, ESSEC Business School
[2]Business School, ITAM - Instituto Tecnológico Autónomo de México

*

Abstract

We examine whether a universal drug subsidy for seniors in Poland provided effective financial protection and whether it induced ex ante moral hazard. The policy eliminated out-of-pocket costs for prescription medications while leaving all other healthcare coverage unchanged. Using detailed household expenditure data and a sharp age-based eligibility threshold, we implement a difference-in-discontinuities design to estimate causal effects. The reform reduced average medication spending and lowered the incidence of catastrophic drug expenditures by 62%, with gains concentrated in the upper tail of the spending distribution—consistent with insurance against large health shocks. On the non-medical margin, we find suggestive evidence of a modest increase in spending on a category of unhealthy goods—consistent with reduced precautionary behavior at the household level. These results highlight that while public subsidies can meaningfully reduce financial risk, they may also induce behavioral responses that partially offset intended health benefits.

JEL Classification: I10, I13, I18

1 Introduction

In the context of rapidly aging populations, pharmaceutical subsidies for seniors, who account for a disproportionately large share of medication use, have become one of the most widely debated tools in health policy. For many older adults, prescription drugs represent a significant source of household expenditure, particularly during periods of

*We thank Mariya Jojy for excellent research assistance and the participants in the workshops at ESSEC and ITAM for their useful feedback. The authors gratefully acknowledge the financial support of the ESSEC Research Center (Small Project Funding) and ITAM. Krzysztof Zaremba acknowledges support of the Asociación Mexicana de Cultura

health shocks, and high out-of-pocket costs can pose serious barriers to treatment. As a result, these financial pressures threaten not only the physical health but also the economic security of senior households. In response, many governments have implemented targeted drug subsidy programs to improve affordability and reduce financial vulnerability. While most countries rely on partial subsidies or co-payment caps, some—including Italy, the United Kingdom, Spain, and Germany—provide certain medications entirely free of charge to eligible populations. Despite the widespread use of such policies, there is limited causal evidence on their financial effects for older adults. This paper provides new evidence on these outcomes using the case of Poland's *Drugs 75+* policy. Poland provides an ideal test case due to its centralized public insurer, single-payer structure, and high pre-policy medication cost burden.

The *Drugs 75+* policy eliminated all out-of-pocket costs for eligible drugs, subject only to prescription by a healthcare provider. Unlike many pharmaceutical benefit programs that reduce prices through co-payments or tiered subsidies, the *Drugs 75+* policy represented a sharp and complete price elimination for a group of older adults, regardless of income or health status.

By removing the cost of eligible drugs, the policy directly relaxed the current-period budget constraint, with the aim of improving financial security and access to medication among seniors. In addition, it offered an insurance function by reducing exposure to the financial risks associated with future health shocks. As a result, the reform may not only influence spending on medications and other health-related goods but could also displace precautionary behaviors—potentially altering consumption patterns more broadly. Indeed, standard theoretical implications of insurance suggest that moral hazard may arise, with eligible individuals shifting their consumption toward goods that may be detrimental to health.

This paper examines how a pharmaceutical subsidy affects exposure to medication-related financial risk and alters household consumption patterns. Seniors face high and unpredictable drug costs, making them particularly vulnerable to out-of-pocket spending shocks. We study changes in financial protection and spending behavior following the introduction of Poland's *Drugs 75+* policy, using detailed household-level expenditure data and the policy's sharp age-based eligibility threshold as a natural experiment. Specifically, we implement a difference-in-discontinuities design (Grembi et al., 2016) that compares outcomes across cohorts just above and below the age-75 cutoff, before and after the policy's implementation. While the reform also aimed to improve access to medications and increase its consumption, these broader impacts lie beyond the scope of this paper and are addressed in related work (Majewska and Zaremba, 2025).

First, we find that the policy provided meaningful financial protection against high medication costs. On average, monthly out-of-pocket expenditures on prescription drugs fell by approximately \$8, representing a 23% reduction from a baseline of \$35. More substantially, the policy led to a 62% decrease in the probability that a household spends more than 10% of its disposable income on medications, a standard measure of catastrophic health expenditure. At the same time, we do not find evidence for reductions in a measure of poverty which considers income available after medication spending. The interpretation of financial protection is reinforced by the estimated quantile treatment effects, which indicate that the policy significantly compressed the right tail of the distribution. Specifically, there is no discernible effect at the first quartile or the median, but we observe substantial and statistically significant declines in spending at the third quartile and above. Heterogeneity checks indicate that the gains are concentrated among households most exposed to the policy—those with high pre-policy spending, single-person households, and those composed entirely of older adults. However, we also find that higher-income and urban households, who likely had higher pre-policy spending and have better access to healthcare and pharmacy services, disproportionately benefit. This pattern raises concerns that the policy did not improve the financial situation of the most vulnerable seniors, and it may unintentionally reinforce existing financial disparities. It also explains the lack of effects on the poverty measure: only households far from the poverty line made savings thanks to the policy.

Secondly, we find suggestive evidence of consumption responses consistent with moral hazard arising from the policy's insurance component. Monthly expenditures on unhealthy products (unhealthy foods, alcohol, and cigarettes) show a statistically significant increase of approximately \$7.5. Across different dimensions of heterogeneity, this shift is concentrated among households that experienced the largest reductions in out-of-pocket medication spending, suggesting that the financial relief provided by the policy enabled reallocation toward unhealthy goods. These patterns may reflect moral hazard: by improving access to medications that help manage chronic conditions and insuring against future health-related financial shocks, the policy may reduce incentives for precautionary behavior, such as abstaining from alcohol. On the other hand, as these results are most pronounced in larger households with seniors cohabiting with younger individuals, we cannot exclude a household-level income effect, in which the subsidy allows other household members to increase their consumption of unhealthy goods. We find no evidence of meaningful changes in other consumption categories.

This paper contributes to a growing body of literature on the financial effects of expanding public health coverage, particularly in the context of pharmaceutical benefits. Existing research has primarily focused on the United States, where insurance markets are fragmented and pricing is often nonlinear. For example, Finkelstein et al. (2012) show that gaining Medicaid coverage significantly reduces out-of-pocket medical spending and financial hardship in an experimental setting. Other work finds that Medicaid eligibility reduces medical and insurance expenses, and poverty risk among vulnerable populations (Sommers and Oellerich, 2013; Dillender, 2017). Additionally, prior work has examined Medicare Part D (e.g., Einav et al. (2018), or Park and Martin (2017) for a review), which partially subsidizes prescription drug costs through nonlinear pricing for elderly Americans.

Our study departs from this literature in two key ways. First, we provide the first causal evidence on the financial effects of a universal, age-based policy that eliminates only the prescription drug costs at a national scale. Unlike Medicaid or Medicare Part D, the Polish *Drugs 75+* policy removed all out-of-pocket costs for a defined set of medications without altering the cost of other healthcare services. This allows us to isolate the impact of eliminating drug prices directly, without confounding changes in eligibility for broader insurance coverage or nonlinear cost-sharing regimes. The setting is particularly clean: all seniors are covered by a single public insurer providing free inpatient and outpatient care, making medications the primary source of out-of-pocket spending.

The Polish context offers a highly relevant case for other countries implementing universal, non-means-tested pharmaceutical policies. Many European systems operate similarly, including those in Italy, Spain, and the UK. Moreover, the Polish context is particularly well-suited to study such a policy. Łuczak and García-Gómez (2012) and Tambor and Pavlova (2020) document that in Poland, out-of-pocket spending on prescription drugs is the primary driver of catastrophic health expenditures, defined as medical costs exceeding 10% of household income. These costs affect up to 18% of households and have been shown to push many into poverty. The burden is particularly severe among lower-income households. Furthermore, Moran and Simon (2005) show that prescription drug use rises with income, suggesting that affordability is a key barrier to access.

We contribute to this literature by leveraging the *Drugs 75+* policy as a natural experiment to estimate the effects of full medication subsidies on household-level medication spending and financial risk exposure. Moreover, while prior work has primarily focused on mean impacts of health subsidies, this paper shows that the policy operates primarily by curbing tail risk in medication spending, hence providing a substantial insurance value. While Majewska and Zaremba (2025) examine how the policy affected access and consumption, we provide complementary evidence on its financial consequences, offering a complete picture of the trade-offs and distributional implications of eliminating out-of-pocket drug costs.

Secondly, we contribute to the empirical literature on ex ante moral hazard—the notion that insurance coverage may alter preventive or health-related behaviors by lowering the financial consequences of illness. Evidence from this literature is mixed. Several studies, including the RAND Health Insurance Experiment (Newhouse and Insurance Experiment Group, 1993), and analyses by Card et al. (2008) and Courbage and Coulon (2004), find little or no impact of insurance coverage on behaviors such as smoking, drinking, or exercise. Other studies, however, document adverse behavioral responses: for instance, Klick and Stratmann (2007) find that diabetes coverage is associated with higher BMI among diabetics; Dave and Kaestner (2009) report increased engagement in unhealthy behaviors among elderly men following Medicare enrollment; and Dave et al. (2019) find increased smoking among pregnant women after coverage expansion. Similarly, research from lower-income settings shows that insurance coverage may reduce precautionary behaviors, such as malaria prevention in Ghana (Yilma et al., 2012) or waste management in India (Gitaharie et al., 2022). In contrast, other work finds health-improving effects in the longer run—for example, Soni (2020) report reductions in smoking and drinking after Medicaid expansion, though these effects emerge only over time.

Our study adds to debates on the behavioral effects of health insurance by providing suggestive evidence that eliminating out-of-pocket drug costs can prompt immediate changes in consumption, including increased spending on unhealthy goods. By focusing on a targeted pharmaceutical subsidy in a system with otherwise universal coverage, we isolate short-term behavioral responses to gaining drug insurance. This complements prior research on broader or long-term insurance expansions, highlighting how the removal of financial barriers to medication can quickly alter household spending patterns.

The rest of the paper proceeds as follows. Section 2.2 provides institutional context on the *Drugs 75+* policy and related pharmaceutical reforms in Poland. Section 2.3 describes the data. Section 3 outlines the empirical strategy and presents the main findings on financial protection and reallocation. Section 4 concludes with policy implications and directions for future research.

2 Context and Data

Poland is the fifth biggest country in the European Union, after Germany, France, Italy, and Spain. Table 1 presents basic statistics on Poland's economy and demographics compared to the EU27 average. Even though the real GDP lags behind the EU average, the country has experienced rapid growth. The population of Poland is shrinking and ageing, with the share of elderly (aged 65 or more) increasing by over 42% between 2012 and 2023 and approaching the EU average of 21.3%. Despite improvement in recent years, life expectancy at birth is below the EU average, and there is a large gap between men and women.

Table 1: Poland: GDP and demographics (source: Eurostat)

	Poland		EU27
Indicator	2012	2023	2023
Population (millions)	38.06	36.62	441.26
% share of elderly (65+)	14	19.9	21.3
Total fertility rate	1.33	1.29	1.43
GDP per capita (€)	10 000	19 920	37 930
Life expectancy at birth (total)	76.9	78.6	81.1
male:	72.6	74.8	78.9
female:	81.1	82.4	84.2

2.1 Poland's Health System

Like its European peers, Poland's health system is characterised by virtually universal public health insurance coverage. The right to healthcare is written in Article 68 of the Polish Constitution of 1997, with special weight put on the vulnerable parts of the population, including people with disabilities, pregnant women, and seniors. The public health insurance is provided through the National Health Fund (NFZ). Spending on the public health system accounted for almost 75% of total health spending in Poland in 2022 and has steadily increased from 4.33% of GDP in 2012 to 5% in 2022 (OECD Data Archive).

The NFZ contracts through tenders to provide a set of predefined health services in predefined quantities, specified in 2009 by the Ministry of Health. When demand for the publicly financed health services exceeds the contracted supply, their provision is managed via waiting lists. Patients can choose their provider, and waiting times are published on a centralized platform. There is no cost-sharing for inpatient care, primary care, or outpatient specialist care.

The Polish public health insurance system is characterized by substantial patient cost-sharing for reimbursed outpatient medications, making pharmaceuticals the most significant component of out-of-pocket health expenditures. Before the *Drugs 75+* policy, many prescription medications were eligible for partial reimbursement, but the level of support depended on the patient's diagnosed condition and the drug's classification. Out-of-pocket costs did not depend on the patient's purchasing history. Reimbursement rates followed a tiered structure, with co-payment levels of 0%, 30%, 50%, 100%, or a fixed fee (with the co-pay level related to the length and cost of the treatment), applicable only to drugs listed on the Ministry of Health's formulary. This formulary contains the majority of the most commonly prescribed medications. In practice, patient co-payments were often higher than the formal rates because reimbursement was calculated based on the lowest-priced equivalent drug, and patients were responsible for paying any difference. Finally, some specific subsets of the population, such as blood and organ donors, veterans, and active-duty soldiers, could benefit from additional discounts on their co-pays.

Additionally, some medications were not reimbursed at all. This structure generated high out-of-pocket expenses, particularly among older adults and those with chronic conditions. Socioeconomic disparities in health outcomes remain a significant public health challenge in Poland (Sowada et al., 2019). At the time of the introduction of the *Drugs 75+* program, there were no other policies specifically targeting the removal of barriers to care. Seniors had a guaranteed minimum pension (approximately 200 USD in 2014, increased to 250 USD in 2017) and, from age 75, could benefit from an additional care supplement of approximately 50 USD.[1] Low-income seniors who need help with daily activities are also eligible for caregiving services provided by local social welfare centers.

2.2 Drugs 75+

As of September 1, 2016, individuals aged 75 or older became eligible for full reimbursement of prescription medications on a designated list published bi-monthly by the Ministry of Health. This list represents a subset of the partially reimbursed drugs under the existing system. For clarity, we refer to partially reimbursed drugs as those that received government co-pay support before the policy, and to fully reimbursed drugs as those covered under the *Drugs 75+* program—a subset of the former.

To benefit from the program, patients must obtain a prescription from a primary care physician that includes a special annotation confirming eligibility.[2] Eligibility is also contingent on the drug being prescribed for specific, approved medical indications.

To define the list of products included in *Drugs 75+*, the Ministry of Health compiled a ranking of active substances based on their clinical relevance to the elderly population, safety, efficacy, and pre-policy accessibility. The initial 2016 list focused on medications for common chronic conditions among seniors, including hypertension, ischaemic heart disease, thromboembolism, asthma, chronic obstructive pulmonary disease (COPD), diabetes, depression, and dementia. This initial set accounted for approximately 28.6% of the partially reimbursed drug list, and 35% of the value of consumption of reimbursed drugs by the target population.

The program's scope has expanded over time. Throughout 2017, and most notably in May 2018, the list was expanded to include some cancer treatments, antibiotics, antiepileptics, opioids, and heparins. Following this extension, the *Drugs 75+* list encompassed roughly 50% of the medications previously covered under partial reimbursement and almost 80% of the value of reimbursed drug consumption by the target population.

[1] The medical care supplement (*dodatek pielegnacyjny* in Polish), introduced in 1998, the amount is updated every year and the cited figure refers to 2016.

[2] Renewal of an existing prescription for long-term treatments would not require scheduling a visit to the doctor; these are often managed via telephone call, and primary care visits are not subject to long waiting times.

Table 2: Summary of Per-Person Monthly Prescription Medication Consumption Before and After Policy Change

	Policy	
Metric	Before	After
Panel A: Age < 75		
Out of Pocket per Person	1.603	1.685
Packages per Person	0.703	0.727
Total Cost per Person	4.965	5.283
Panel B: Age ≥ 75		
Out of Pocket per Person	6.533	2.680
Packages per Person	3.108	3.431
Total Cost per Person	18.465	21.237

Note: Based on administrative data on medication reimbursement obtained from the Polish government. The denominator for the per capita measures is the total population in Poland for the given age group in 2015. Pre-policy corresponds to year 2015 and post-policy to year 2018

Table 2 summarizes per-person monthly prescription medication consumption and spending before and after the implementation of the *Drugs 75+* policy, disaggregated by age group. Before the reform, individuals aged 75 and older consumed nearly 4.5 times as many prescription medications as those under 75 and incurred proportionally higher out-of-pocket costs. In 2015, the average monthly cost of prescription medications among seniors was approximately \$18.50 per person, of which \$6.53 was paid out of pocket. Following the reform, out-of-pocket spending for this age group declined to \$2.68, reflecting the impact of full reimbursement for a subset of medications. However, out-of-pocket costs did not fall to zero, as not all partially reimbursed drugs were included in the *Drugs 75+* list.

2.3 Household Budgets Survey

We use microdata from the Polish Household Budget Survey (HBS), a flagship study of Statistics Poland, for the years 2015–2018.[3] Each month, a nationally representative sample of households is asked to complete a detailed expenditure diary for that month. Participating households keep a meticulous day-by-day record of all monetary outlays (cash, debit/credit card, or electronic transfer) as well as in-kind receipts, inter-household gifts, and other monetary transfers.[4] Statistics Poland defines the highest-earning member as the head of the household, but there is no designated primary respondent to the survey. Instead, the households are visited regularly by a trained enumerator, who reviews the diary every two weeks for completeness and coherence, and conducts follow-up interviews (e.g., concerning the consumption of durable goods over the course of a year). Moreover, the households are obliged to keep receipts of all their expenses for the enumerator to

[3]The data have been used in previous research, such as Gromadzki (2024).

[4]The monetary expenditures are originally in PLN which we express in USD using the average exchange rate in 2016.

review.

Expenditures then are categorized by the enumerators at a highly disaggregated level. Our analysis focuses on health-related spending, particularly the subcategory of medical products. This subcategory separately identifies pharmaceuticals, distinct from other items such as medical devices (e.g., eyeglasses or hearing aids), allowing us to isolate changes in drug consumption from other types of health spending.[5]

Each household participates in the survey twice over a two-year period, and half of the sample is replaced annually. The dataset includes rich demographic information, including birthdates of all household members, which allows us to identify eligibility for the pharmaceutical subsidies at the time of the interview.

Measurement error is an important concern when relying on survey data. In our context, such error may arise if older household members face cognitive limitations or are marginalized in larger, multigenerational households, potentially affecting the accuracy of key variables. Statistics Poland acknowledges several sources of non-random error in the HBS, though these primarily relate to underreporting of alcohol, tobacco, sweets, and income from informal or illegal sources (GUS, 2011). Importantly, the survey methodology emphasizes the role of trained enumerators in verifying consistency between reported expenditures and household demographics, which helps mitigate some concerns. Crucially, we assume that any such measurement issues do not vary discontinuously at the exact age 75 threshold, which underpins our identification strategy.

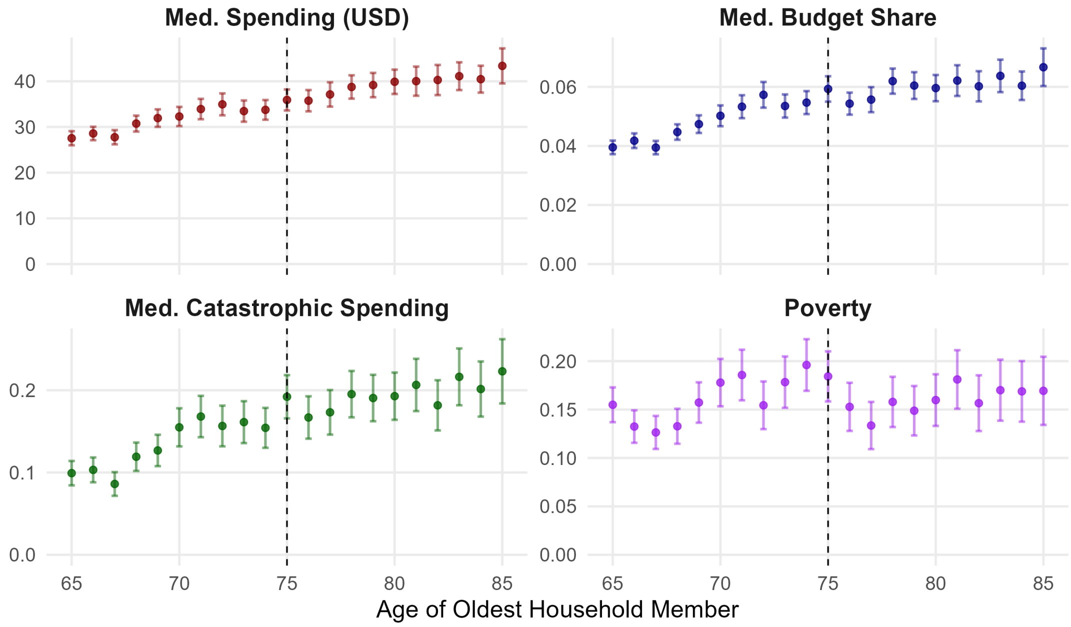
Our analysis focuses on people close to crossing the eligibility threshold, so we restrict the sample to households in which the oldest member is aged 65–85, yielding 46,976 observations. In Figure 1, we present average pre-policy pharmaceutical spending as a function of the age of the oldest household member in our sample. The data reveal a clear upward trend: medication expenditures rise with household age. Among households with members aged 75 and above, pharmaceutical spending accounts for approximately 8% of disposable income on average, with this share increasing with age. Nearly 20% of these households face catastrophic pharmaceutical expenditures, defined as spending exceeding 10% of disposable income. In addition, the poverty rate, measured using the European Union's relative poverty threshold[6], fluctuates between 15% and 20%. These patterns reveal the substantial financial burden that pharmaceuticals impose on elderly households and highlight their vulnerability to catastrophic health expenditures. These high expenditures are likely triggered by adverse health shocks—whether transitory (e.g., infections) or persistent (e.g., chronic diseases). Taken together, the evidence shows considerable scope for pharmaceutical policy intervention to alleviate financial hardship among seniors.

Tables A2 and A3 in the Appendix present detailed summary statistics on demographic and expenditure patterns for households in the full analysis sample and near the age 75 eligibility threshold. Specifically, in Table A3 we report pre- and post-policy means for households with the oldest member within 2.5 years below and above the cutoff, providing a clear comparison of treatment and control groups. While older households naturally exhibit higher levels of pharmaceutical spending and distinct demographic profiles, Table A4 in the Appendix formally tests for covariate continuity at the threshold.

[5] Such underreporting would make our estimates conservative.

[6] A household is considered poor if its equivalized disposable income—net of pharmaceutical expenditures—is below 60% of the national median.

Figure 1: Medication Expenditures by Age



Note: This figure shows pre-policy average expenditure on medication, budget share dedicated to medication, share of households with catastrophic spending on medication and households facing poverty. The vertical line marks 75 years. Error bars represent 95% confidence intervals. Sample size is 18 801.

The results confirm that observable characteristics evolve smoothly across the cutoff.

3 Estimation of the Policy Effects

3.1 Difference-in-Discontinuities

To evaluate the effects of the policy, we employ a difference-in-discontinuities approach, leveraging both the eligibility age cutoff for free drug access and the temporal dimension of the policy introduction in September 2016. This method builds on the framework introduced by Grembi et al. (2016), extending the traditional regression discontinuity design (RDD).

Our objective is to estimate the causal effect of the *Drugs 75+* program on household medication expenditures and financial risk exposure. The policy eliminates out-of-pocket costs for eligible prescription drugs for individuals aged 75 and older. Let $Y_{it}(d)$ denote the potential outcome for household i at time t under treatment status $d \in \{0, 1\}$, where $d = 1$ indicates eligibility for the *Drugs 75+* program. Let a_{it} denote the exact age (in years and fractions thereof) of the oldest household member in household i at time t, and let t_0 denote the date of policy implementation. Eligibility is granted if and only if $a_{it} \geq 75$ and $t \geq t_0$.

The causal effect of interest is the difference between the outcome under the policy, $Y_{it}(1)$, and the counterfactual outcome in its absence, $Y_{it}(0)$. Formally, we define the parameter of interest as the following conditional expectation:

$$\tau \equiv \mathbb{E}\left[Y_{it}(1) - Y_{it}(0) \,|\, a_{it} = 75\right], \tag{1}$$

which captures the *local average treatment effect* (LATE) of the *Drugs 75+* program for households whose oldest member is exactly at the eligibility threshold.

While eligibility is determined by age, age itself is correlated with many other factors that affect outcomes, so comparing younger and older households directly may be misleading. However, households near the age 75 threshold are likely to be similar in all respects, enabling credible local comparisons using regression discontinuity design.

In a standard RDD, the assumption is that counterfactual outcomes are continuous at the cutoff, ensuring that individuals just below and above the threshold are comparable except for their eligibility. Accordingly, any observed differences in outcomes near the cutoff can be attributed to the policy, providing a local treatment effect at the eligibility threshold. In our case, this effect pertains specifically to households whose oldest member has just crossed the eligibility cutoff at 75.[7]

However, a traditional RDD is not applicable in this context due to a pre-existing program with the same age cutoff. Specifically, individuals aged 75 and older are eligible for a pension supplement (care supplement) of approximately 50 USD per month. This overlap creates ambiguity, as changes in outcomes at the discontinuity could be driven by either the pension supplement or the free drug policy introduced in 2016.

If the care supplement affects outcomes, a standard RDD around the age 75 cutoff would not necessarily separately identify the effect of the drug policy, but rather a combination of the effects of both the *Drugs 75+* program and the pre-existing care supplement. Let τ^{D} denote the effect of the drug subsidy, and τ^{C} the effect of the care supplement. Then the observed discontinuities at the threshold can be expressed as:[8]

$$\text{Pre-policy RDD:} \quad \tau^{\mathrm{pre}} = \lim_{a \downarrow 75} \mathbb{E}[Y_{it} \mid a, t < t_0] - \lim_{a \uparrow 75} \mathbb{E}[Y_{it} \mid a, t < t_0] = \tau^{\mathrm{C}} \tag{2}$$

$$\text{Post-policy RDD:} \quad \tau^{\mathrm{post}} = \lim_{a \downarrow 75} \mathbb{E}[Y_{it} \mid a, t \geq t_0] - \lim_{a \uparrow 75} \mathbb{E}[Y_{it} \mid a, t \geq t_0] = \tau^{\mathrm{C}} + \tau^{\mathrm{D}} \tag{3}$$

To disentangle these effects, we apply a difference-in-discontinuities approach, comparing changes in outcomes at the age-75 cutoff before and after the introduction of the free drug policy. Identification relies on an additivity assumption between the care supplement and Drugs 75+. Acknowledging this limitation is important, as the supplement could in principle affect prescription drug consumption on its own. In Appendix A, we discuss this key identification assumption in detail. One potential channel is that the supplement induces substitution away from refundable prescription drugs toward more expensive, branded non-refundable alternatives. In this case, the estimated effect of free drug eligibility would be attenuated and therefore conservative, a mechanism likely more relevant for relatively wealthier households. A more consequential concern is that the supplement relaxes liquidity constraints—particularly among poorer households—enabling seniors to finance prescription drug purchases they would not otherwise afford. In this

[7]Because eligibility is based on individual age but the unit of observation is the household, this may attenuate treatment effects in multi-person households where only one member is eligible.

[8]Under the assumption of additive effects between the two policies.

case, the estimated impact of free drug eligibility could be mechanically amplified relative to a counterfactual setting without the supplement. Moreover, to the extent that the care supplement induces lifestyle or consumption adjustments that materialize sufficiently quickly within the estimation bandwidth, such mechanisms could also generate a discontinuity in medication consumption at the cutoff.

In the Results section and Appendix A, we present a set of empirical checks designed to assess the quantitative relevance of these concerns. Exploiting the pre-policy period—when only the care supplement was determined by the 75 threshold—we find no discontinuity in prescription drug spending, no evidence of stronger responses among poorer households where liquidity constraints should be most binding, and no differential effects among richer households. Consistent with this, we also detect no meaningful changes in household income or non-medical expenditures attributable to the supplement alone. Using healthcare utilization data, we further show that increases in primary care visits arise only after the introduction of Drugs 75+ and are absent in the pre-policy period, a pattern consistent with medication uptake responding to drug eligibility rather than to the care supplement itself. While these results alleviate concerns that violations of additivity are quantitatively important and make extrapolation to other settings plausible, we emphasize that the estimates should be interpreted as local policy effects within an institutional environment where the care supplement—amounting to a modest, fixed cash transfer—remains in place.

The difference-in-discontinuities estimate is calculated by first estimating RDD effects separately for the pre-policy and post-policy periods, and then taking the difference between them. In each RDD, the running variable is the age of the oldest household member at the end of a given month. We leverage precise birth-date data to calculate exact ages, expressed in fractions of years. The focus on age at the end of the month is critical because eligibility depends on age at the time of purchase, allowing for potential delays in purchases until eligibility is attained. Our identification relies on the following assumptions: (1) potential outcomes evolve smoothly across age; (2) the effect of the pre-existing pension supplement is stable over time and additive; (3) there is no manipulation of age reporting or birth timing.

Under these assumptions, we can identify the parameter of interest as the difference between the post-policy and pre-policy discontinuities:

$$\tau = \tau_{\text{post}} - \tau_{\text{pre}}, \tag{4}$$

We estimate each component in Equation (4) using nonparametric local linear regression discontinuity estimators on either side of the age 75 cutoff, applied separately to the pre-policy and post-policy periods. Formally, our estimator is:

$$\hat{\tau} = \left[\hat{\mu}_{+}^{\text{post}}(75) - \hat{\mu}_{-}^{\text{post}}(75)\right] - \left[\hat{\mu}_{+}^{\text{pre}}(75) - \hat{\mu}_{-}^{\text{pre}}(75)\right], \tag{5}$$

where $\hat{\mu}_{+}^{\text{post}}(75)$ and $\hat{\mu}_{-}^{\text{post}}(75)$ denote the estimated conditional means just above and below the threshold in the post-policy period, and analogously for the pre-policy period.

We follow the bias-corrected local polynomial estimation framework of Calonico et al. (2020), using a triangular kernel and data-driven bandwidths selected to minimize the mean squared error (MSE). The approach is nonparametric: rather than imposing a global functional form between age and the outcome, we fit separate local linear regressions on either side of the cutoff using observations within a neighborhood around 75. For example, the conditional mean just above the threshold in the post-policy period is given by:

$$\hat{\mu}_{+}^{\text{post}}(75) = \hat{\beta}_{0}^{+,\text{post}}, \tag{6}$$

where $\hat{\beta}_{0}^{+,\text{post}}$ solves the weighted least squares problem:

$$\min_{\beta_0,\beta_1} \sum_{i:t\geq t_0} \mathbf{1}(a_{it} \geq 75)\left(Y_{it} - \beta_0 - \beta_1(a_{it} - 75)\right)^2 K_h(a_{it} - 75). \tag{7}$$

A symmetric procedure is applied to observations below the cutoff and to the pre-policy period.[9] All models are weighted using national survey weights to ensure population representativeness.

Inference is conducted using a nonparametric bootstrap procedure clustered at the household level. Specifically, we repeatedly resample households with replacement. For each bootstrap iteration, we re-estimate the pre-policy and post-policy discontinuities and compute the resulting difference-in-discontinuities estimator. Repeating this process 1,000 times yields the empirical distribution of the estimator, from which we compute bootstrap standard errors and confidence intervals.

Because identification relies on the assumption that all other determinants of the outcome evolve smoothly at the threshold, we do not include additional covariates in the main specification. As a robustness check, we formally test for the continuity of baseline covariates at the cutoff and do not see any significant discontinuities (see Table A4).

We analyze four primary outcomes: (1) total spending on medication in (USD), (2) the share of disposable income allocated to medication, (3) a binary indicator for high (catastrophic) medication expenditure, defined as spending exceeding 10% of disposable income,[10] (4) a binary indicator for whether the household is considered in poverty after the medication expenditures.

The poverty indicator is defined as having a per-capita disposable income, net of medication expenditures, that falls below 60% of the national median disposable income, following the European Union's definition of relative poverty.[11] Although the gross household income may remain unchanged, this poverty measure may react to the policy because it focuses on income available after medication expenditures. A reduction in out-of-pocket medication costs can directly shift households above or below this modified poverty threshold. Overall, these outcomes capture both direct financial impacts and the insurance value of the policy, potentially protecting against high expenditure stemming from negative health shocks.

We next examine whether the policy induces shifts in household spending across non-targeted categories. Two mechanisms may drive such reallocation: a direct liquidity effect

[9] Estimation is implemented using the `rdrobust` package in R.

[10] This threshold corresponds to approximately the third quartile for households with an oldest member aged 74.

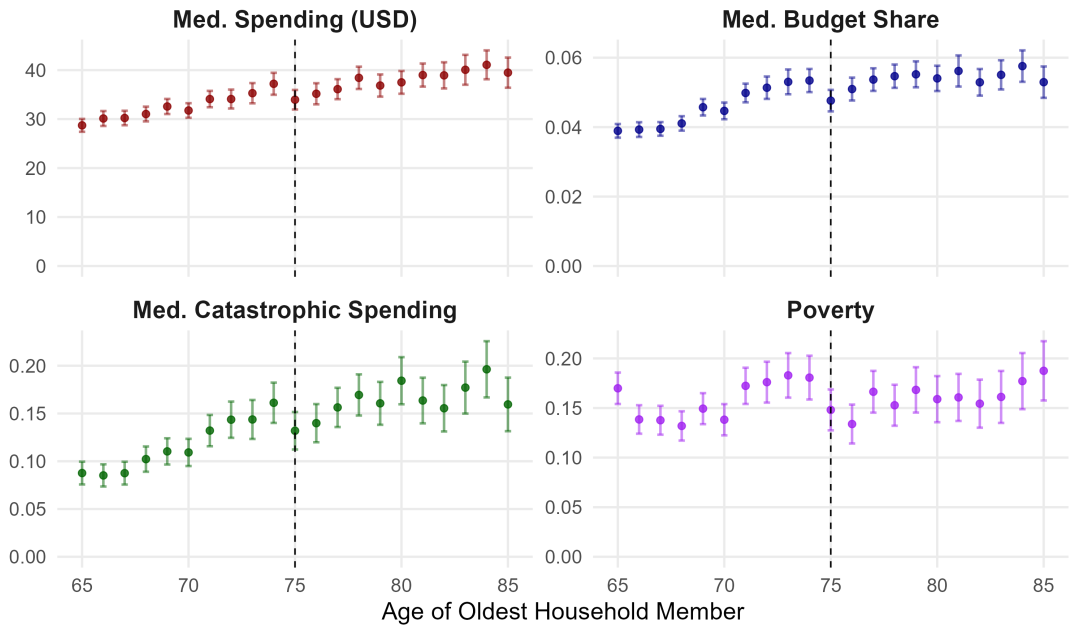
[11] We adjust household disposable income by an equivalence scale that accounts for household composition. Specifically, the household head is assigned a weight of 1.0, each additional member over 13 receives a weight of 0.7, and each child aged 13 or younger is assigned a weight of 0.5. Disposable income is then divided by the weighted household size to yield equivalized disposable income. We compute the median of this equivalized income distribution and define the relative poverty threshold as 60% of this median. A household is considered at risk of poverty after medical expenses if its equivalized disposable income, net of per-capita out-of-pocket medical expenditures, falls below this threshold.

stemming from a relaxed budget constraint, and an insurance effect that reduces the need for precautionary behaviors. The insurance channel, in particular, may lead households to reallocate spending toward goods that are riskier for health.

3.1.1 Results

We begin by visualizing average raw outcomes by age following the policy implementation in Figure 2. Across all outcomes, there is a distinct and sharp decline at age 75. While outcomes continue to rise gradually with age post-policy, they remain systematically lower than pre-policy levels. This contrasts sharply with the pre-policy trends in Figure 1, where no clear discontinuities are observed. While not apparent, given the potential confounding effect of a universal cash transfer occurring at age 75, we employ a difference-in-discontinuities design to isolate the policy's impact.

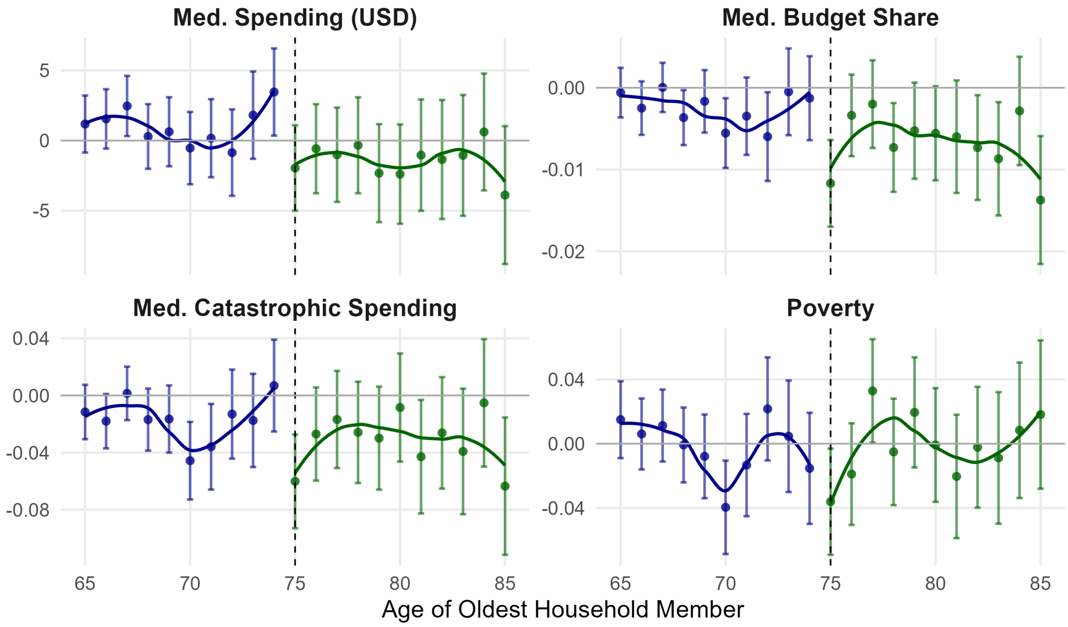
Figure 2: Post Policy Medication Expenditures by Age



Note: This figure shows post-policy average expenditure on medications, budget share dedicated to medications, share of households with catastrophic spending on medications and households facing poverty. The vertical line marks 75 years. Error bars represent 95% confidence intervals. Sample size is 28 175.

To visualize this design, Figure 3 plots the difference in average outcomes between the post- and pre-policy periods for each age, overlaid with a smoothed loess curve. Since the confounding policy was present both before and after the introduction of free medication, its effect is differenced out. The graph confirms a pronounced break at the 75-year threshold for most outcomes, except poverty. We further quantify these effects in the formal analysis presented below.

Figure 3: Difference in Medication Expenditure by Age



Note: This figure shows post vs pre policy difference in outcomes by age of the oldest household member. The outcomes are average expenditure on medications, budget share dedicated to medications, share of households with catastrophic spending on medications, and households facing poverty. The vertical line marks 75 years. Error bars represent 95% confidence intervals, and a smoothed curve is fitted using loess. Sample size is 46 976.

Table 3 reports the formal estimates from the difference-in-discontinuities analysis. We begin by examining the regression discontinuity estimates prior to the policy implementation. While the pre-existing care supplement could, in principle, generate discontinuities at age 75, Section C in the Appendix demonstrates that there is no detectable income jump at the threshold in the general sample. Although a discontinuity is observed among single-person households, this additional income appears to be entirely saved rather than spent. Consistent with these findings, we observe only a small and statistically insignificant increase in medication spending of \$2.63 at age 75 before the policy (p-value = 0.26). Similarly, the pre-policy estimates for the share of income spent on medications, the incidence of catastrophic spending, and poverty are all statistically insignificant. While these results are reassuring, the estimates are imprecise, and we cannot fully rule out small pre-existing discontinuities. This motivates the use of the difference-in-discontinuities approach.

Following the policy introduction, we document a substantial and statistically significant decline in medication expenditures at the threshold. Specifically, spending falls by \$5.72 (significant at the 5% level), and the estimated difference-in-discontinuities effect is a reduction of \$8.36, with a 95% confidence interval of [\$2.10, \$15.92]. This effect corresponds to a 23% decrease relative to the average spending of \$35.75 at age 74, providing compelling evidence that the policy meaningfully reduced out-of-pocket prescription drug costs. Nevertheless, the persistence of non-negligible spending levels suggests incomplete coverage—many medications commonly used by older adults may not be included in the program. Some attenuation may also reflect increased consumption of complementary non-covered drugs or intra-household shifting of medication purchases to younger members.

The policy also yields a sizable reduction in the share of disposable income allocated to medications. The estimated decline at the threshold is 2 percentage points (statistically significant at the 5% level), a large effect given that households with a 74-year-old oldest member spend approximately 5.4% of their disposable income on medications.

The most pronounced effect is observed in financial risk protection. The share of households incurring catastrophic medication expenditures falls by 9.8 percentage points (significant at the 5% level), representing a 62% reduction relative to the pre-policy mean of 16%. This finding highlights the strong insurance value of the policy, which substantially reduces exposure to major health-related financial shocks. In the Polish context, where out-of-pocket medication spending constitutes the primary source of out-of-pocket healthcare costs, this result points to a near elimination of catastrophic risk at the eligibility threshold.

By contrast, the estimated effect on poverty incidence is modest and statistically insignificant, corresponding to a reduction of 1.3 percentage points. The lack of a significant poverty effect suggests that out-of-pocket medication expenditures, while burdensome for some, are not the principal driver of poverty status in this setting. This is consistent with the observation that medication spending is a normal good that rises with income (see Figure G1 in the Appendix). As a result, higher-income households—who spent more on medications prior to the policy—realize greater absolute savings, but these households were not at risk of poverty. Consequently, the increase in disposable income net of medication expenditures does not translate into measurable changes in poverty rates. The heterogeneity of the policy's financial impact is explored in detail in the following section.

We conduct a series of robustness checks to assess the credibility and stability of our main findings. First, to address potential anticipatory behavior around the eligibility threshold, we implement an event-study analysis of outcome dynamics in the months surrounding the 75th birthday (see Appendix E). While we observe a modest decline in spending in the month preceding eligibility—suggesting some strategic timing of purchases—this effect is limited in magnitude and implies that our main estimates are, if anything, conservative.

Second, we estimate a donut RDD specification (Noack and Rothe, 2023) that excludes observations within three weeks of the 75th birthday to mitigate concerns about behavioral adjustments at the threshold. The resulting estimates (Table G1 in the Appendix) are virtually unchanged from the baseline, confirming that our results are not driven by anticipatory behaviors.

Third, we test the sensitivity of our findings to alternative bandwidth selection procedures and kernel choices. As reported in Table G2, the estimated effects remain stable across a range of plausible specifications. Similarly, re-estimating the models without survey weights (Table G3) yields nearly identical results.

Finally, we estimate fully parametric specifications as an additional robustness check (Table G4 in the Appendix). While these models yield somewhat smaller point estimates—ranging from \$9 to \$4 in spending reductions—all effects remain statistically significant and substantively meaningful.

While the survey offers limited direct measures of healthcare utilization, we supplement our analysis with two additional indicators. First, we examine households' self-reported satisfaction with their ability to afford necessary healthcare. Second, we

Table 3: Difference-in-Discontinuities Estimates

Outcome	Pre	Post	Diff.	95% CI
Med. Expenditures	2.633	-5.723*	**-8.356***	[-15.924, -2.095]
	(2.373)	(2.354)	(3.721)	
bandwidth	3.12	3.41		
p-value	0.268	0.015	0.021	
Med. Budget Share	0.010	-0.009**	**-0.020***	[-0.036, -0.005]
	(0.006)	(0.003)	(0.008)	
bandwidth	2.30	3.98		
p-value	0.087	0.004	0.011	
Catastrophic Spending	0.057	-0.041*	**-0.098***	[-0.190, -0.021]
	(0.032)	(0.021)	(0.042)	
bandwidth	2.43	3.57		
p-value	0.073	0.049	0.021	
Poverty	-0.028	-0.041	**-0.013**	[-0.085, 0.093]
	(0.031)	(0.021)	(0.045)	
bandwidth	2.53	3.95		
p-value	0.361	0.052	0.774	

Notes: This table reports the difference-in-discontinuities estimates from the main specification for four outcomes: medication expenditures (in USD), budget share dedicated to medications, catastrophic spending on medication, and poverty. Catastrophic spending is defined as the share of disposable income spent on medication exceeding 10%. Poverty is measured using the European Union relative poverty definition, weighted per capita household disposable income (net of medication expenditure) being below 60% of the national median. The first two columns represent RDD estimates pre- and post-policy, respectively, and the third is the difference-in-discontinuities. Sample size is 46 976. Errors are clustered at the household level. Confidence intervals are calculated as percentiles of the distribution of differences from 1000 bootstrap iterations. P-value is also bootstrap based and is defined as the smallest significance level at which the confidence interval does not include zero. These bootstrap iterations also derive the average bandwidths applied in the pre- and post-policy periods. The bandwidth is MSE-optimal, the same on both sides of the cutoff. The table also reports standard errors of pre, post, and difference estimates, as well as corresponding p-values. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

analyze administrative data on healthcare visits, including outpatient, inpatient, and primary care encounters. Details on methodology and results are provided in Section D of the Appendix. In the post-policy period, we find a statistically significant improvement in perceived affordability of healthcare and an increase in primary care visits among individuals who recently turned 75. Although the difference-in-discontinuities estimates for both outcomes are directionally consistent with improved satisfaction and increased utilization, they do not reach conventional thresholds for statistical significance.

3.2 Heterogeneity in Policy Effects

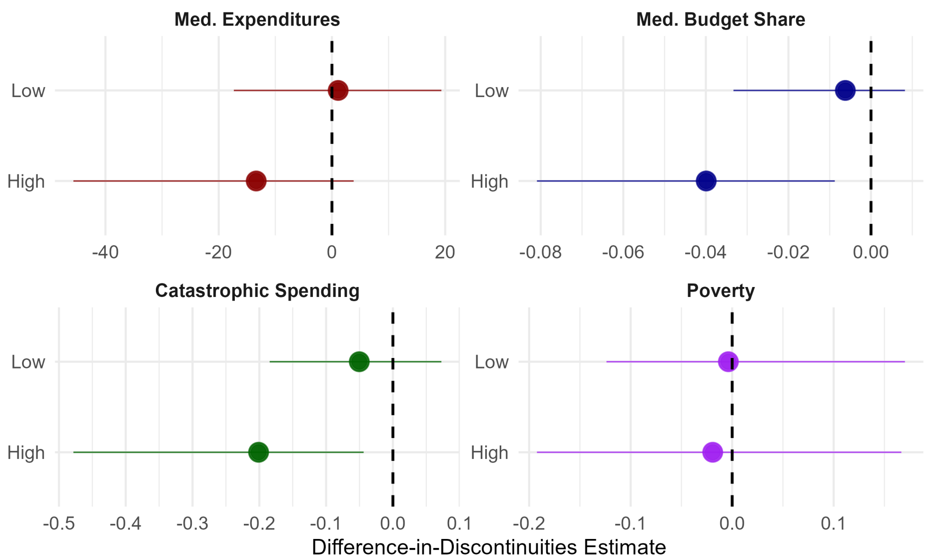
To better understand who experiences larger financial gains from the policy, we examine how its effects vary across key household characteristics. Figures 4 through 9 present estimates from the difference-in-discontinuities design, stratified by pre-policy medication burden, household size, age composition, income, and population density. Figure G2 in the Appendix confirms a low degree of correlation between these characteristics. Each subplot reports subgroup-specific effects on four outcomes: medication expenditures, the budget share allocated to medications, the incidence of catastrophic spending, and poverty. Error bars represent 95% confidence intervals based on bootstrap replications.

The largest effects are concentrated among households with the highest exposure and financial need: specifically, those with high pre-policy medication spending, single-person households, and households composed entirely of older adults. Interestingly, we also observe relatively strong effects among higher-income households and those residing in densely populated areas. These patterns may reflect higher pre-policy spending and better access to physician appointments or pharmacies among high-income households and in urban settings.

1. Policy Exposure

We begin by examining heterogeneity based on households' exposure to the policy. First, we consider the households' burden of medication expenditure. To do so, we restrict the sample to households observed at least once before the policy. We then divide them into high and low spenders based on their pre-policy medication budget share, using the sample median of 3.5% as the cutoff. We then estimate the difference-in-discontinuities separately for the high- and low-spending groups.

Figure 4: Effects by Pre-Policy Medication Budget Share

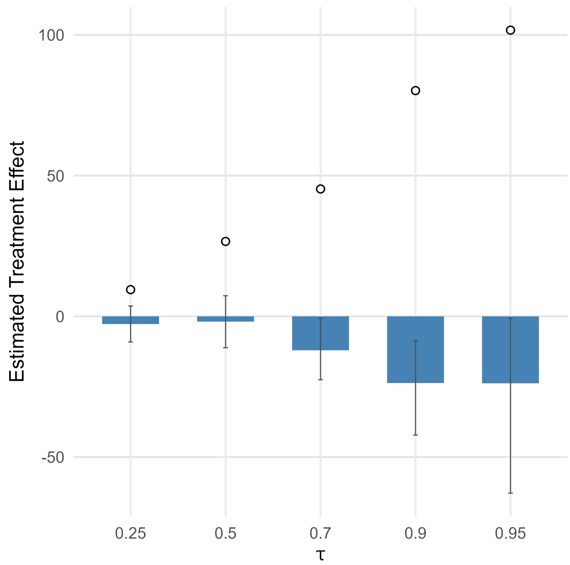


Note: This figure shows the heterogeneity of the effects of the policy by the pre-policy budget share of the household dedicated to medication. It is divided into two groups: low and high, with the median as the cutoff. The dot represents the difference-in-discontinuities estimate. Vertical line marks 0. Error bars represent 95% confidence intervals from bootstrap. Sample size: below median (*Low*): 23 474, above median (*High*): 23 502.

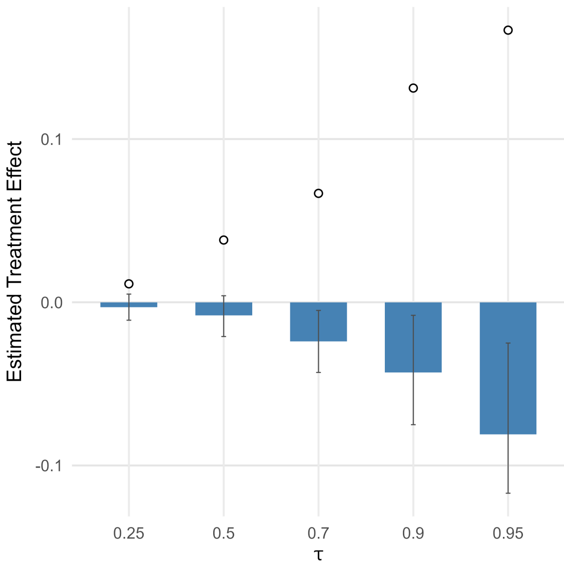
As shown in Figure 4, households with above-median pre-policy medication spending experienced substantially larger improvements across nearly all outcomes. While the decline in absolute medication expenditures is sizable, it is not statistically significant at the 5% level. However, the reduction in the budget share allocated to medications is both large and significant, amounting to approximately 4 percentage points. The most pronounced effect is observed in the incidence of catastrophic medication spending, which declines by 20 percentage points among high-exposure households—a substantial reduction in financial risk. In contrast, effects for the low-exposure group are small and statistically indistinguishable from zero across all outcomes. As in prior analyses, there is no meaningful impact on poverty in either subgroup, and the estimates remain imprecise with wide confidence intervals. These findings confirm that the policy delivers the greatest financial benefits to households most burdened by medication costs before its implementation.

To unpack further the distributional origins of the average treatment effects, we examine how the policy influenced different points in the outcome distribution. Specifically, we estimate quantile treatment effects (QTEs) using the methodology developed by Qu et al. (2024), which extends the regression discontinuity design (RDD) to quantile outcomes. We adapt this approach to our difference-in-discontinuities framework. We estimate treatment effects for the 0.25, 0.50, 0.70, 0.90, and 0.95 quantiles of both monthly medication spending and the budget share allocated to medications.

Figure 5: Quantile Treatment Effects on Medication Outcomes



(a) Total Medication Spending (USD)



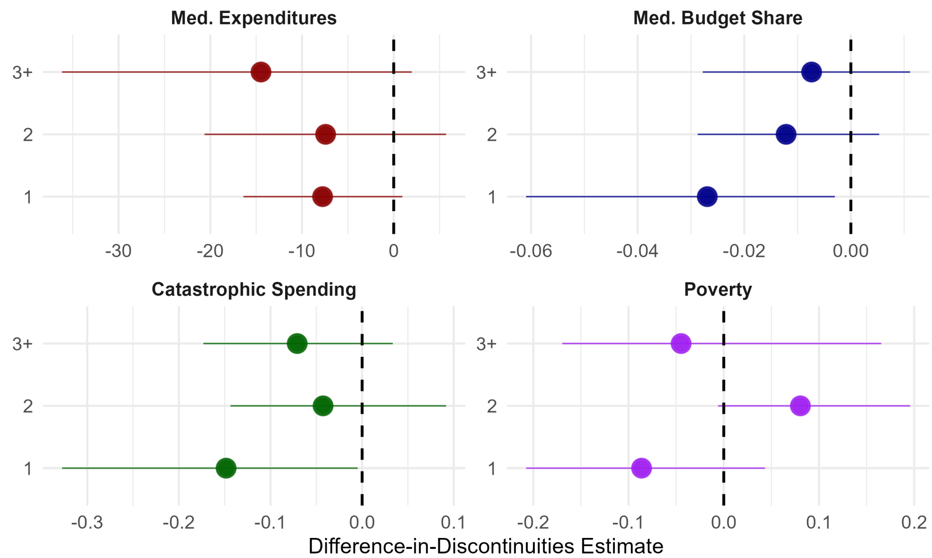
(b) Budget Share of Medication

Note: These figures present estimated quantile treatment effects of the policy on the distribution of medication-related outcomes, using difference-in-discontinuities in quantiles. Blue bars represent the estimated difference at each quantile τ in (a) monthly medication spending in USD (left) and (b) the share of income spent on medications (right), comparing households just above and just below the age 75 threshold before and after the policy. Vertical lines denote 95% confidence intervals obtained from bootstrap. Hollow circles denote the corresponding quantiles at 74, allowing comparison to baseline values at each τ. Sample size is 46 976.

As illustrated in Figures 5a and 5b, the effects are highly concentrated in the upper tail of the distribution. We find no statistically significant changes at the 25th or 50th percentiles for either outcome, suggesting that lower and median spenders were largely unaffected. In contrast, substantial and statistically significant reductions are observed at the 70th, 90th, and 95th percentiles. This pattern indicates that the policy was particularly effective in mitigating extreme out-of-pocket medication costs, providing strong protection against financial risk for households facing the highest burdens. For the 90th and 95th percentiles of the budget share of medication, exceeding the 10% threshold for catastrophic spending, the policy succeeds at, on average, bringing these households below this threshold.

Next, we examine heterogeneity in policy effects by household size—another important exposure dimension. Since our running variable is the age of the oldest member, in single-person households, 100% of household members are potentially eligible for the policy once the age threshold is reached. In contrast, in larger households, only one individual is likely to qualify at the cutoff, meaning that a smaller share of the household benefits directly from the reform. While cases where spouses or cohabiting adults cross the eligibility threshold simultaneously are possible, such scenarios are rare. We therefore stratify households into three groups: single-person, two-person, and households with three or more members.

Figure 6: Effects by Household Size



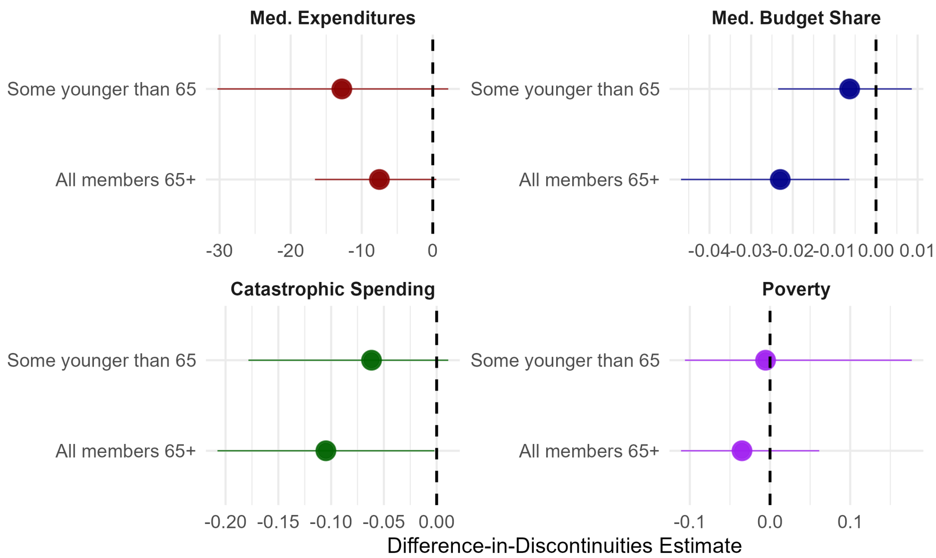
Note: This figure shows the heterogeneity of the effects of the policy by household size. The dot represents the difference-in-discontinuities estimate. Vertical line marks 0. Error bars represent 95% confidence intervals from bootstrap. Sample size: for n=1: 14 821, for n=2: 20 775, for n=3+: 11 380.

As shown in Figure 6, the results are somewhat noisy but broadly consistent with the hypothesis that smaller households, where a larger share of the household is affected, experience more substantial relative effects. While the point estimate for the decline in medication expenditures is slightly larger for larger households, it is not statistically significant and likely reflects higher baseline spending and incomes in those groups.

More tellingly, the most significant reductions in the medication budget share and the incidence of catastrophic spending are observed among single-person households. While the cross-group differences are not statistically significant—likely due to limited statistical power—they are directionally consistent and economically meaningful. Once again, we find no significant effect on poverty across any household size category. These patterns suggest that the policy offers powerful financial protection to single-person households, arguably among the most vulnerable to health-related financial shocks. With no capacity for intra-household risk pooling and only a single income to buffer medical expenses, such households would face heightened exposure to out-of-pocket health costs absent the policy.

As the final dimension of heterogeneity in exposure, we examine household age composition. Households composed entirely of older adults will likely be more exposed to the policy's benefits, as they allocate a larger share of their budget to medications. Even if only one member is formally eligible, there may be within-household sharing or cross-use of subsidized medications. To assess this, we divide the sample into households with all members aged 65 or older and at least one younger member.

Figure 7: Effects by Household Age Composition



Note: This figure shows the heterogeneity of the effects of the policy by household age composition. The dot represents the difference-in-discontinuities estimate. Vertical line marks 0. Error bars represent 95% confidence intervals from bootstrap. Sample size: households with only members above 65: 27 033, households with at least one member below 65: 19 943.

As shown in Figure 7, the decline in the medication budget share is more pronounced in older-only households, consistent with their greater financial exposure to medical expenses. For the other outcomes, the differences between household types are relatively small and not statistically significant.

Overall, the heterogeneity analysis shows that the policy offers the most financial relief to households with the greatest need, particularly those that spent more on medication before the policy, live alone, or consist only of seniors. These are also the households where we expect the survey to best reflect the situation of seniors, as in larger, multi-generational households, younger members might be taking the lead in data collection and possibly under-report the expenditures of older members.

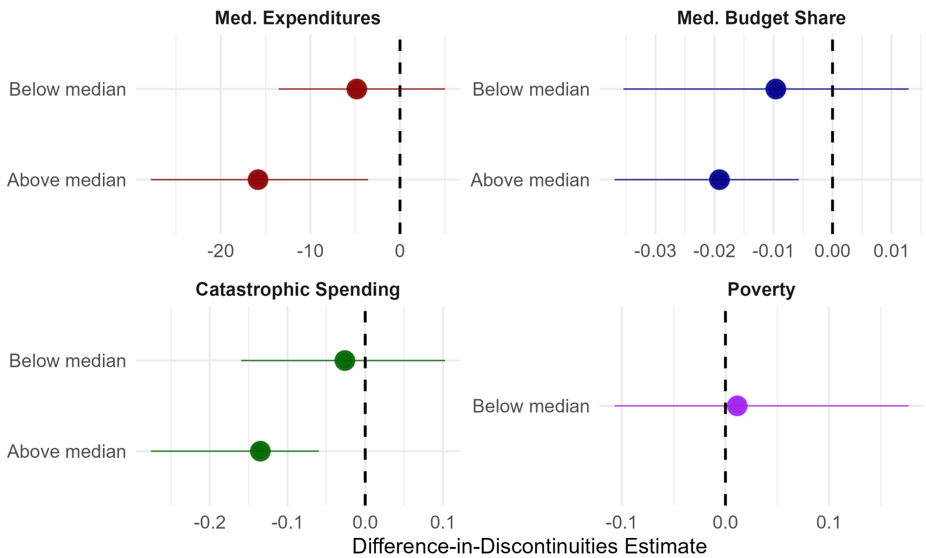
These results point to a degree of effective targeting. However, we now turn to distributional dimensions such as household income and geographic context to assess the broader equity implications.

2. Economic Vulnerability

We next examine whether the policy disproportionately affects economically disadvantaged groups, focusing on two dimensions: household income and the population density of the household's location.

We begin by analyzing heterogeneity by income, dividing households into two groups: those above and below the median level of per-capita disposable income. There are several reasons to expect differential effects by income. Higher-income households may spend more on medications at the baseline, given that prescription drugs are a normal good. They may also have better access to healthcare providers, which is a prerequisite for obtaining prescriptions and thus for benefiting from the policy. In contrast, lower-income households may have had limited medication consumption before the policy due to affordability constraints. While the policy may enable them to increase their medication use, this may not be reflected in observed expenditures, since covered medications are provided free of charge.

Figure 8: Effects by Household Income



Note: This figure shows the heterogeneity of the policy's effects by the household's per-capita disposable income. It is divided into two groups: below and above the median. The dot represents the difference-in-discontinuities estimate. Vertical line marks 0. Error bars represent 95% confidence intervals from bootstrap. Sample size: below median: 23 474, above median: 23 502.

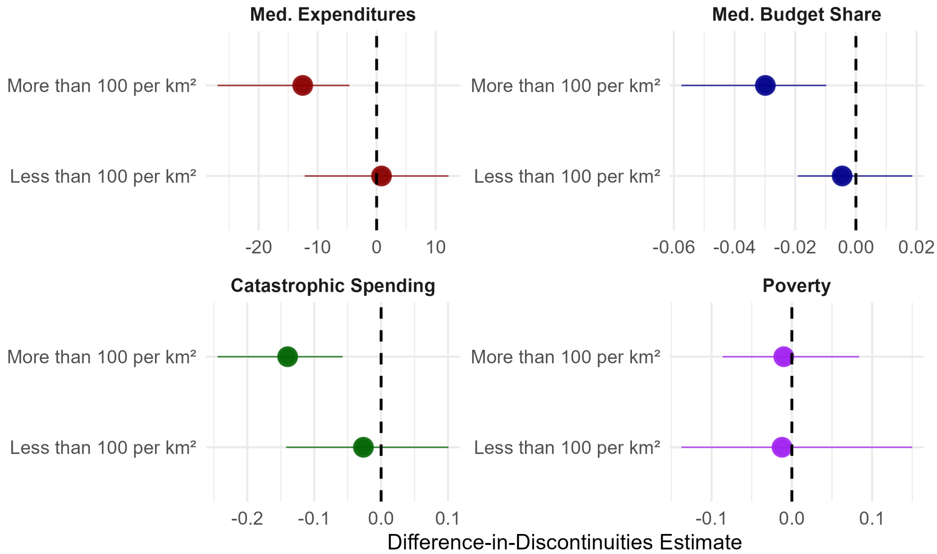
As shown in Figure 8, higher income households exhibit substantially larger reductions in absolute medication spending and the budget share devoted to medications. This does not necessarily mean that the policy does not provide relief to poor households. Instead, they might increase free consumption without a change in expenditures or reallocate the savings from the policy to a higher degree to other medications, not covered by *Drugs 75+*. Section B of the Appendix formalizes theoretically why poorer households experience lower financial effects. We show that the lack of effect on medication expenditures could stem from the poorest households struggling to afford the medication they needed before the reform. Underconsumption of prescription drugs (due to their affordability) decreases the financial gain from *Drugs 75+*. Rather than pointing to problems with the policy's targeting, the small effect on expenditures of the lower-income households can suggest that drug prices are an important barrier to access to treatment in Poland. Moreover, survey data from low-income households can be more prone to measurement error, attenuating the estimates (Bound et al., 2001).

Higher-income households also experience a more substantial decline in the probability of catastrophic spending, indicating that the policy offered more effective financial protection to this group. On the other hand, the effect for the lower-income group could be attenuated by households whose income is too low to afford high medication spending. Last, the policy does not appear to significantly affect poverty rates.[12] This is unsurprising, given the lack of significant effects among poorer households.

Finally, we examine whether the policy's effects vary by household location, specifically population density. We divide households into two groups based on whether they reside in areas with more or fewer than 100 inhabitants per square kilometer. Residents in less densely populated areas often face a range of socioeconomic disadvantages. More importantly, they may encounter greater barriers to healthcare access, including fewer providers authorized to prescribe medications and more limited proximity to pharmacies, which may hinder their ability to benefit from the policy. Indeed, using 2018 data, Statistics Poland reports that seniors living in urban areas had on average 9.9 primary-care visits per year, compared to 5.9 visits among those residing in rural areas (GUS, 2019). It needs to be noted, however, that the differences by income and location may also capture unobserved variation in health status or care-seeking behavior rather than just in healthcare access.

[12]By construction, households above the median income are not classified as poor, so no poverty effect is observed for them.

Figure 9: Effects by Location Density



Note: This figure shows the heterogeneity of the effects of the policy by population density. It is divided into two groups: below vs. above 100 inhabitants per km^2. The dot represents the difference-in-discontinuities estimate. Vertical line marks 0. Error bars represent 95% confidence intervals from bootstrap. Sample size: low density: 20 571, high density: 26 405.

As shown in Figure 9, the entire effect of the policy is concentrated among households living in densely populated areas. These households experience substantial reductions in medication expenditures, budget share, and the incidence of catastrophic spending. In contrast, households in sparsely populated areas show no statistically or economically meaningful change across any outcome. As in previous analyses, we find no significant effect on poverty in either group.

The heterogeneity analysis confirms that the policy is most effective in financially protecting households with a high pre-policy medication burden and those most exposed due to household size and composition. However, the financial benefits appear to accrue disproportionately to households with greater socioeconomic advantage—those with higher incomes and those living in more urban areas. These groups are more likely to have access to the healthcare infrastructure necessary to utilize the policy fully. While the policy aims to alleviate financial risk among vulnerable senior households, these findings raise important concerns about distributional equity. The reform may inadvertently reinforce existing financial inequalities if more advantaged households are better positioned to capture its benefits. At the same time, while the Household Budget Survey adheres to high methodological standards comparable to other national expenditure surveys that have supported extensive economic research, some measurement error is possible. In particular, reporting quality may vary systematically across household types, and such differences cannot be entirely ruled out as a contributing factor to observed heterogeneity.

3.2.1 Spending Reallocation

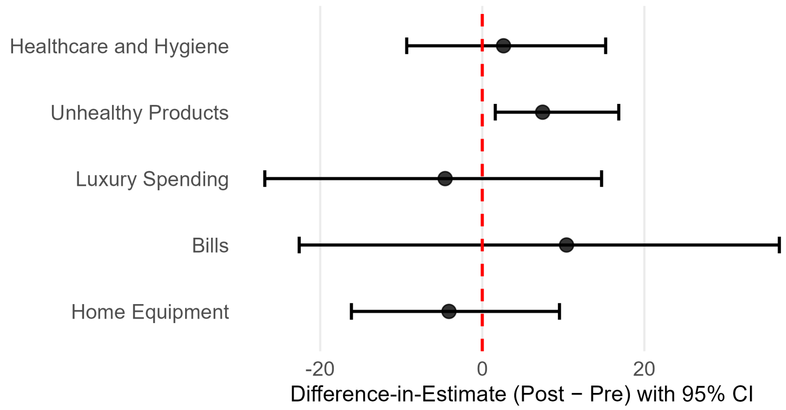
We find that the policy leads to a reduction of approximately \$8 in monthly medication spending among seniors, along with a substantial decline in the probability of incurring high medication expenditures. These patterns suggest that the policy modestly relaxes the current-period budget constraint while also providing insurance against future health-related financial risks. In light of these two effects, we may expect households to reallocate spending toward other categories.

To examine this possibility, Figures 10 and 11 report difference-in-discontinuity estimates of the policy's impact on spending in alternative categories. We focus on consumption groups similar to those analyzed by Gromadzki (2024), with additional emphasis on categories particularly relevant for older households. In particular, following their approach, we classify food items by nutritional quality—from A (most healthful) to E (least healthful)—to assess whether the policy induces changes in dietary composition.[13]

Figure 10 presents results for five economically meaningful aggregate categories: Healthcare and Hygiene, Unhealthy Products, Luxury Spending, Bills, and Home Equipment[14].

It shows that the only statistically significant response occurs in Unhealthy Products, which increase by approximately \$7.5, with statistical significance in conventional single-hypothesis tests. All other aggregate coefficients are statistically indistinguishable from zero.

Figure 10: Diff-in-Disc Estimates for Aggregated Alternative Categories



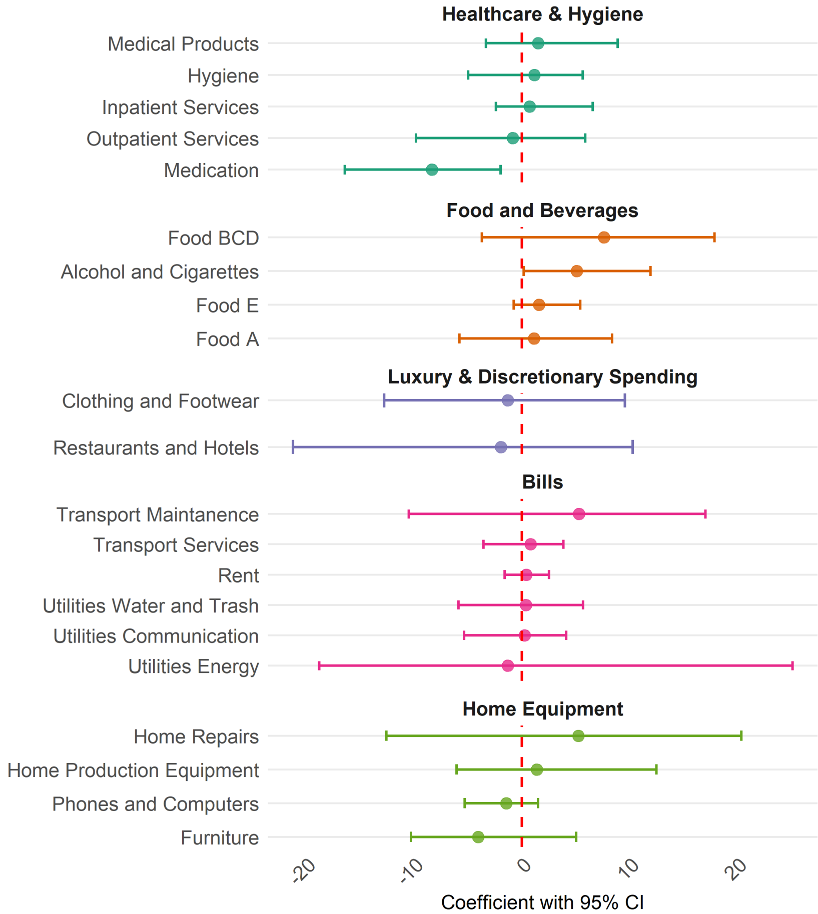
Note: This figure presents difference-in-discontinuity estimates from the main specification for alternative aggregated spending categories. Regressions are weighted using survey weights. Confidence intervals are constructed using 1000 bootstrap replications. Sample size is 46 976.

Figure 11 fully disaggregates spending categories, including individual food groups and medication expenditures. Consistent with the aggregate results, the \$8 decline in medication spending is accompanied by an increase of roughly \$5 in expenditures on alcohol and cigarettes, which accounts for most of the rise in unhealthy consumption. Given that average monthly spending on alcohol and cigarettes at age 74 is \$13.1, this shift is economically modest but not negligible. This outcome also yields the smallest unadjusted p-value among all disagregated spending categories.

[13] Appendix Section G.1 lists the items included in each food category.

[14] The Healthcare and Hygiene category includes medical products, hygiene items, and inpatient and outpatient services, but excludes medications. Unhealthy Products comprise Alcohol, Cigarettes, and Food E. Luxury Spending includes clothing and footwear as well as restaurants and hotels. Bills consist of transportation, rent, and utilities, while Home Equipment covers home repairs, household production equipment, furniture, and information and communication devices.

Figure 11: Diff-in-Disc Estimates for Alternative Outcomes



Note: This figure presents difference-in-discontinuity estimates from the main specification for alternative spending categories. Regressions are weighted using survey weights. Confidence intervals are constructed using 1000 bootstrap replications. Sample size is 46 976.

At the same time, these findings should be interpreted with caution. Because we examine a large number of outcomes, Appendix F reports formal multiple-testing adjustments for both disaggregated and aggregated categories, including indices following Anderson (2008). When all disaggregated outcomes are considered jointly, the alcohol and cigarettes estimate does not survive the full set of corrections, reflecting the substantial sampling variability inherent in testing many margins of adjustment simultaneously. By contrast, the aggregated Unhealthy Products category yields a raw p-value of 0.016, an Anderson-sharpened q-value of 0.087, and adjusted p-values of approximately 0.08. Taken together, these results should therefore be viewed as tentative evidence of increased spending on unhealthy goods.

This substitution effect is further corroborated by heterogeneity analyses, which indicate that groups experiencing larger reductions in medication spending also tend to show greater increases in alcohol and cigarette consumption. Figures G3, G4, G5, G6, and G7 in the Appendix present results stratified by pre-policy medication burden, household size, age composition, income level, and population density. We find that the reallocation from medication to cigarette and alcohol spending is the most pronounced in larger households (3 or more persons), households located in areas with higher population density, and those with some members younger than 65. Table G5 suggests that this pattern, although less precisely estimated due to a smaller sample size, also emerges among single-person households: groups with larger declines in medication spending tend to experience greater increases in alcohol, cigarette, or unhealthy product consumption. While these subgroup patterns are generally consistent, they should be interpreted as suggestive rather than definitive due to the low power.

Our statistical evidence does not allow us to distinguish between two possible mechanisms that explain this pattern of reallocation. On one hand, we could consider a potential moral–hazard–type response. A more relaxed budget constraint, combined with improved access to medications and greater protection against future health-related financial shocks, may reduce the perceived costs of engaging in risky behaviors. For example, easier access to medications for chronic conditions could lower the perceived health consequences of alcohol consumption, weakening incentives to abstain or moderate intake among individuals at metabolic risk. On the other hand, the heterogeneity results do not allow us to exclude that this effect could be driven by changes in consumption by younger members, who benefit from increased household disposable income, rather than by the eligible seniors themselves.

These findings suggest that financial relief in one health domain may inadvertently lead to increased consumption of health-adverse goods, highlighting the importance of anticipating behavioral spillovers in the design of social policies.

4 Discussion and Conclusion

This paper evaluates the financial effects of Poland's *Drugs 75+* policy, which introduced universal, age-based access to free prescription drugs for individuals aged 75 and older. Using a difference-in-discontinuities approach, we find that the policy led to modest reductions in average medication expenditures but a substantial decline in the incidence of catastrophic spending. From a normative perspective, the *Drugs 75+* policy provides insurance value by reducing exposure to catastrophic health spending, particularly among seniors with high medication needs. While the policy might improve welfare by smoothing consumption in the presence of health shocks, the full welfare calculation would also require considering medication consumption, public payer costs, and the health effects of the policy.

Our heterogeneity analysis reveals that the benefits are not evenly distributed. The largest financial gains accrue to households with high pre-policy medication spending, those living alone, and those composed entirely of older adults—groups that face the greatest exposure to medication costs. However, we also find that higher-income households and those in more densely populated areas capture a disproportionate share of the gains, likely due to higher baseline spending, better access to healthcare providers, and prescription and pharmacy infrastructure. These findings raise important concerns about the equity implications of universal subsidies: while the policy aims to protect vulnerable seniors, its design and delivery may unintentionally reinforce existing inequalities if better-off groups benefit doubly from the policy, by having free access to the needed medication and financially, by making savings.

These findings have important implications for the design of pharmaceutical subsidy programs—an increasingly common policy instrument aimed at improving the financial well-being of seniors. While such programs, including Poland's *Drugs 75+*, Medicare Part D in the U.S., and age-based exemptions in Western Europe, are often framed as universal and equitable, our results highlight that financial benefits may disproportionately accrue to more advantaged groups with better access to healthcare infrastructure. This presents a policy challenge: removing price barriers alone may not be sufficient to ensure equitable outcomes. Structural barriers can limit the reach of such programs, suggesting that improving access to providers and pharmacies may be equally important. Moreover, our findings reveal behavioral adjustments to the policy, specifically a reallocation of spending toward less healthy goods such as alcohol and cigarettes. This raises further concerns for policy design, as the intended health benefits of pharmaceutical subsidies may be partially offset by unintended consumption responses.

While our analysis provides robust evidence on the financial effects of the *Drugs 75+* policy, several limitations remain. First, we focus exclusively on spending outcomes and financial protection among seniors, which capture only part of the policy's total impact. Importantly, the policy also affected medication consumption, an issue analyzed in Majewska and Zaremba (2025) and, potentially, health outcomes—effects we cannot observe in our data.[15] Recent evidence suggests that drug prices affect health (Chandra et al., 2024), pointing to a potentially significant omitted margin. This is a critical omission for assessing the full welfare impact of the policy. Second, our data do not distinguish between prescription and over-the-counter medications, limiting our ability to analyze substitution patterns within pharmaceutical consumption. Finally, the interpretation of an increase in the alcohol and cigarettes spending should be treated with caution. In the absence of direct measures of individual health behaviors, we cannot definitively attribute these consumption changes to ex ante moral hazard. Alternative mechanisms such as increased discretionary income could play a role.

Future research should examine the effects of such policies on health outcomes and long-term healthcare expenditures to evaluate their full impact in a cost -benefit analysis framework. Additionally, investigating how prescription behavior and provider-side responses evolve post-reform would help clarify the mechanisms through which these policies operate. Cross-country research could also examine whether similar age-based drug subsidies have differential impacts in more decentralized or privately financed systems.

Declaration of Generative AI and AI-Assisted Technologies in the Writing Process. During the preparation of this work, the author used ChatGPT to correct grammar, spelling, and writing. After using this tool, the author reviewed and edited the content as needed and take full responsibility for the content of the published article.

[15]But e.g. Kardas et al. (2020) shows lower rates of non-adherence to treatment for medication covered by *Drugs 75+*

References

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American statistical Association*, 103(484):1481–1495.

Bound, J., Brown, C., and Mathiowetz, N. (2001). Measurement error in survey data. In *Handbook of econometrics*, volume 5, pages 3705–3843. Elsevier.

Calonico, S., Cattaneo, M. D., and Farrell, M. H. (2020). Optimal bandwidth choice for robust bias-corrected inference in regression discontinuity designs. *The Econometrics Journal*, 23(2):192–210.

Card, D., Dobkin, C., and Maestas, N. (2008). The Impact of Nearly Universal Insurance Coverage on Health Care Utilization: Evidence from Medicare. *American Economic Review*, 98(5):2242–2258.

Chandra, A., Flack, E., and Obermeyer, Z. (2024). The Health Costs of Cost Sharing. *The Quarterly Journal of Economics*, 139(4):2037–2082.

Courbage, C. and Coulon, A. d. (2004). Prevention and Private Health Insurance in the U.K. *The Geneva Papers on Risk and Insurance - Issues and Practice*, 29(4):719–727.

Dave, D. and Kaestner, R. (2009). Health insurance and ex ante moral hazard: evidence from Medicare. *International Journal of Health Care Finance and Economics*, 9(4):367–390.

Dave, D. M., Kaestner, R., and Wehby, G. L. (2019). Does public insurance coverage for pregnant women affect prenatal health behaviors? *Journal of Population Economics*, 32(2):419–453.

Dillender, M. (2017). Medicaid, family spending, and the financial implications of crowd-out. *Journal of Health Economics*, 53:1–16.

Einav, L., Finkelstein, A., and Polyakova, M. (2018). Private Provision of Social Insurance: Drug-Specific Price Elasticities and Cost Sharing in Medicare Part D. *American Economic Journal: Economic Policy*, 10(3):122–153.

Finkelstein, A., Taubman, S., Wright, B., Bernstein, M., Gruber, J., Newhouse, J. P., Allen, H., Baicker, K., and Oregon Health Study Group (2012). The Oregon Health Insurance Experiment: Evidence from the First Year*. *The Quarterly Journal of Economics*, 127(3):1057–1106.

Gitaharie, B. Y., Nasrudin, R., Bonita, A. P. A., Putri, L. A. M., Rohman, M. A., and Handayani, D. (2022). Is there an ex-ante moral hazard on Indonesia's health insurance? An impact analysis on household waste management behavior. *PloS One*, 17(12):e0276521.

Grembi, V., Nannicini, T., and Troiano, U. (2016). Do Fiscal Rules Matter? *American Economic Journal: Applied Economics*, 8(3):1–30.

Gromadzki, J. (2024). Labor supply effects of a universal cash transfer. *Journal of Public Economics*, 239:105248.

GUS (2011). *Metodologia Badania Budżetów Gospodarstw Domowych.*

GUS (2018). Informacja o sytuacji osób starszych na podstawie badań Głównego Urzedu Statystycznego.

GUS (2019). The situation of older people in Poland in 2018.

Kardas, P., Cieszyński, J., Czech, M., Banaś, I., and Lewek, P. (2020). Primary nonadherence to medication and its drivers in poland: findings from the electronic prescription pilot analysis. *Pol Arch Intern Med*, 130(1):8–16.

Klick, J. and Stratmann, T. (2007). Diabetes Treatments and Moral Hazard. *The Journal of Law and Economics*, 50(3):519–538.

Majewska, M. and Zaremba, K. (2025). Universal Subsidies in Pharmaceutical Markets: Lessons from Poland's Drugs 75+ Policy. *Working Paper.*

Moran, J. R. and Simon, K. I. (2005). Income and the Use of Prescription Drugs by the Elderly: Evidence from the Notch Cohorts.

Newhouse, J. P. and Insurance Experiment Group (1993). Free for All? Lessons from the RAND Health Insurance Experiment.

Noack, C. and Rothe, C. (2023). Donut Regression Discontinuity Designs. arXiv:2308.14464.

Park, Y. J. and Martin, E. G. (2017). Medicare Part D's Effects on Drug Utilization and Out-of-Pocket Costs: A Systematic Review. *Health Services Research*, 52(5):1685–1728.

Qu, Z., Yoon, J., and Perron, P. (2024). Inference on Conditional Quantile Processes in Partially Linear Models with Applications to the Impact of Unemployment Benefits. *Review of Economics and Statistics*, 106(2):521–541.

Sommers, B. D. and Oellerich, D. (2013). The poverty-reducing effect of Medicaid. *Journal of Health Economics*, 32(5):816–832.

Soni, A. (2020). The effects of public health insurance on health behaviors: Evidence from the fifth year of Medicaid expansion. *Health Economics*, 29(12):1586–1605.

Sowada, C., Sagan, A., and Kowalska-Bobko, I. (2019). *Poland: Health system review*, volume 21 of *Health Systems in Transition*. World Health Organization, Regional Office for Europe.

Tambor, M. and Pavlova, M. (2020). Financial protection against out-of-pocket health expenditure in Poland. *European Journal of Public Health*, 30(Supplement_5):ckaa165.392.

Yilma, Z., van Kempen, L., and de Hoop, T. (2012). A perverse 'net' effect? Health insurance and ex-ante moral hazard in Ghana. *Social Science & Medicine*, 75(1):138–147.

Łuczak, J. and García-Gómez, P. (2012). Financial burden of drug expenditures in Poland. *Health Policy*, 105(2):256–264.

Appendix A Identification

In this section, we discuss the identification of the effect of *Drugs 75+* in the framework of differences-in-discontinuities. Adapting the notation in Grembi et al. (2016), we have two treatments that affect individuals indexed by i observed at time t, i.e. polish seniors, on their 75th birthday, $a_{it} = 75 = a_c$. First, they start receiving the care supplement with their pension, C_{it} and, from September 2016 on, $t = t_0$, they become eligible for free prescription drugs D_{it}. We can summarize the assignment mechanism by:

$$C_{it} = \begin{cases} 1 & \text{if } a_{it} \geq 75 \\ 0 & \text{otherwise} \end{cases}$$

$$D_{it} = \begin{cases} 1 & \text{if } a_{it} \geq 75 \wedge t \geq t_0 \\ 0 & \text{otherwise} \end{cases}$$

We define potential outcomes $Y_{it}(c, d)$ when $C_{it} = c$ and $D_{it} = d$. We are interested in the treatment effect of D_{it} on Y_{it} in presence of the confounding policy C_{it}. Assuming continuity and the time-invariance of the confounding effects we can rewrite Equation (5) as:

$$\begin{aligned} \hat{\tau} &= [Y(1,1)^- - Y(0,0)^+] - [\tilde{Y}(1,0)^- - \tilde{Y}(0,0)^+] \\ &= Y(1,1) - Y(1,0) = \mathbb{E}[Y_{it}(1,1) - Y_{it}(1,0) | a_{it} = 75] \end{aligned}$$

We obtain the causal effect of *Drugs 75+* with the confounding policy of the care supplement. To identify a more general treatment effect, we would also need to assume additivity, i.e. that the effect of *Drugs 75+* on spending on medication does not depend on the care supplement: $Y(1,1) - Y(1,0) = Y(0,1) - Y(0,0)$.

In our setting, the additivity assumption would be violated if the care supplement influences spending on medication. This could occur through two main channels. First, if recipients use the additional funds from the care supplement to adjust their consumption of medications covered by the *Drugs 75+* program after its introduction in September 2016. Second, if the supplement enables lifestyle changes that improve health status among eligible individuals, thereby altering their need for prescription medication.

While we cannot rule out this latter channel, it is important to be precise about its implications for identification. Lifestyle or behavioral adjustments induced by the care supplement may affect prescription drug consumption, and some responses may occur immediately—for example, through changes in expectations or awareness of improved access. Other adjustments are likely to arise more gradually and accumulate over time rather than materialize sharply at the age threshold. A regression discontinuity design is designed to detect discrete changes at the cutoff; however, we acknowledge that even gradual post-eligibility adjustments could still generate a discontinuity if they materialize sufficiently quickly and individuals above the threshold are observed with heterogeneous durations of exposure within the estimation bandwidth. At the same time, the policy primarily covers medications used to treat chronic conditions—such as hypertension, diabetes, and dementia—for which short-run behavioral or lifestyle changes are less likely to translate into immediate changes in prescription needs, particularly among older adults.

We are also concerned about potential interactions between the care supplement and the drug reimbursement policy for certain subpopulations, particularly the poorest households. If the care supplement enables access to medications that would otherwise be unaffordable, these households may only benefit from the *Drugs 75+* program because the supplement first allows them to purchase the reimbursed drugs. In such cases, there would be no baseline consumption to generate savings absent the supplement. Formally, for the poorest households, it is possible that $Y(1,1) - Y(1,0) < Y(0,1) - Y(0,0)$, leading to an upward bias (in absolute terms[16]) in our estimate of the drug policy's effect. This interaction highlights the possibility that our estimates may partially capture the enabling role of the care supplement, rather than the impact of the reimbursement policy alone.

We provide two empirical arguments suggesting that interactions between the care supplement and the reimbursement policy are unlikely to bias our results. Crucially, our setting allows us to identify the effect of the care supplement on medication purchases by focusing on the pre-policy period—before the introduction of the *Drugs 75+* program. During this period, any discontinuity at age 75 reflects the impact of the care supplement alone. As shown in Table 3, we find no evidence of a discontinuity in medication spending at the age 75 cutoff prior to the policy. Since any confounding from this channel should be most pronounced among the poorest households, we also disaggregate the analysis by income per capita tertile. The results, presented in Table A1, show no significant increase in medication expenditures at age 75 for households in the bottom income tertile.

Second, this concern is only relevant if the care supplement were used to purchase prescription medications covered under the *Drugs 75+* program. Accessing these drugs requires a physician's prescription, typically obtained through a primary care visit. If the supplement were facilitating such purchases, we would expect to observe a corresponding increase in medical consultations at age 75. In Section D, we examine the effect of turning 75 on the frequency of medical visits, both before and after the policy's introduction. Using RDD estimates from the pre-policy period, we find no evidence of a significant effect of the care supplement on visit frequency: the estimated effect on log(visits) is 0.007 (standard error = 0.027; p-value = 0.798). By contrast, we observe a statistically significant 5.1% increase in visits after the policy was introduced (p-value = 0.034).

Together, these two pieces of evidence suggest that the care supplement is not primarily driving increased spending on prescription drugs covered under the 75+ policy.

In contrast, if the care supplement enables some households to substitute toward more expensive, non-reimbursed medications, the net effect of the *Drugs 75+* policy on their medication spending could be attenuated. In this case, the treatment effect would be smaller in the presence of the supplement—formally, $Y(1,1) - Y(1,0) > Y(0,1) - Y(0,0)$[17], leading us to underestimating the treatment effect. Again, we can test this assumption using RDD on medication spending by per capita income tertile on the pre-September 2016 data (Table A1). We do not find a significant effect of the care supplement on medication spending in any income tertile, reassuring us that our diff-in-disc results are not systematically biased.

[16]If Y is medication spending, we would expect $Y(1,1) - Y(1,0)$ to be more negative than $Y(0,1) - Y(0,0)$

[17]That is, $Y(1,1) - Y(1,0)$ is less negative than $Y(0,1) - Y(0,0)$

Table A1: RD Estimates of Medication Spending Prior to September 2016, by Income Tertile

Income Tertile	Robust Coef.	Robust SE	p-value	BW (h)
Bottom tertile	2.998	3.593	0.404	2.554
Middle tertile	0.116	3.697	0.975	3.435
Top tertile	4.344	5.078	0.392	3.493

Notes: Each row reports robust local linear RD estimates of monthly medication spending (USD) at the age 75 cutoff, estimated separately by per capita income tertile using pre-September 2016 data. All models use triangular kernel weights and `rdrobust` MSE-optimal bandwidths. Observations are weighted by survey weights.

To lend support to the continuity and time-invariance assumptions, we present the means of the main household characteristics in Table A2, means within 2.5 years of the threshold A3, and test for their continuity in Table A4.

Table A2: Descriptive Statistics: Full Sample

Variable	Mean	SD	N
Household Size	2.212	1.356	46976
Number of Children	0.177	0.570	46976
Number of Adults below 65	0.701	1.007	46976
Number of Adults 65+	1.334	0.475	46976
Oldest Adult: Married	0.474	0.499	46976
Oldest Adult: Bachelor or More	0.139	0.346	46976
Share of Females	0.607	0.288	46976
Presence of Disabled Member	0.239	0.426	46976
High Population Density Area	0.333	0.471	46976
Income	1088.596	1581.823	46976
Expenditure	1100.077	1534.376	46976
Pension Income	579.873	314.659	46976
Med. Expenditures	34.042	35.537	46976
Med. Budget Share	0.049	0.056	46733
Catastrophic Spending	0.138	0.345	46733
Poverty	0.135	0.342	46976

Notes: This table reports descriptive statistics for the full analysis sample, consisting of all households in which the oldest household member is between ages 65 and 85. Income, expenditure, pension income, and medication expenditures are reported in monthly USD. All characteristics are measured at the household level. The number of observations varies slightly across variables due to missing values.

Table A3: Pre- and Post-Policy Means Below and Above the Eligibility Cutoff

	Pre-Policy			Post-Policy		
Outcome	Below	Above	Diff.	Below	Above	Diff.
Household Size	2.190 (0.031)	2.199 (0.031)	0.008	2.131 (0.024)	2.129 (0.024)	-0.002
Number of Children	0.201 (0.013)	0.183 (0.013)	-0.018	0.174 (0.010)	0.157 (0.010)	-0.017
Adults < 65	0.563 (0.021)	0.609 (0.022)	0.046	0.491 (0.016)	0.557 (0.018)	0.066**
Adults 65+	1.427 (0.011)	1.407 (0.011)	-0.020	1.466 (0.009)	1.416 (0.009)	-0.051**
Oldest Adult Married	0.476 (0.011)	0.432 (0.011)	-0.044**	0.497 (0.009)	0.433 (0.009)	-0.064**
Oldest Adult BA+	0.145 (0.008)	0.121 (0.007)	-0.025*	0.163 (0.007)	0.141 (0.006)	-0.022*
Share Female	0.621 (0.006)	0.621 (0.006)	0.000	0.617 (0.005)	0.624 (0.005)	0.007
Disabled Member	0.239 (0.009)	0.264 (0.010)	0.025*	0.221 (0.008)	0.209 (0.008)	-0.012
High-density Area	0.330 (0.010)	0.305 (0.010)	-0.025*	0.346 (0.009)	0.335 (0.009)	-0.011
Gross Income	960.942 (16.488)	969.514 (15.625)	8.572	1066.939 (19.600)	1156.690 (73.108)	89.751
Gross Expenditures	976.438 (16.708)	982.875 (16.659)	6.438	1084.788 (20.375)	1163.428 (72.644)	78.639
Pension Income	538.652 (6.449)	551.441 (6.329)	12.790	600.942 (6.011)	589.796 (5.771)	-11.146
Med. Expenditures	33.764 (0.725)	36.003 (0.773)	2.239*	35.753 (0.682)	35.124 (0.677)	-0.629
Med. Budget Share	0.055 (0.001)	0.057 (0.001)	0.002	0.053 (0.001)	0.051 (0.001)	-0.003*
Catastrophic Spending	0.160 (0.008)	0.179 (0.008)	0.019	0.151 (0.007)	0.141 (0.006)	-0.010
Poverty	0.166 (0.008)	0.145 (0.008)	-0.021*	0.150 (0.007)	0.115 (0.006)	-0.034**

Notes: Pre- and post-policy mean outcomes are shown for households lying 2.5 years below and 2.5 years above the eligibility cutoff. The observations are not weighted and no kernel is used. Differences are computed as Above − Below. Standard errors are in parentheses. BA+ stands for Bachelor's degree or higher. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

Table A4: Covariates Continuity Tests

	Estimate			Avg. BW	p-value			Mean
Outcome	Pre	Post	Diff.	(Pre/Post)	Pre	Post	Diff.	at age 74
Household Size	-0.035	-0.114	-0.079	3.85 / 4.01	0.668	0.246	0.657	2.197
Number of Children	-0.021	-0.079	-0.058	3.40 / 2.61	0.681	0.079	0.460	0.199
Num. of Adults <65	-0.053	-0.043	0.010	2.60 / 3.45	0.521	0.522	0.935	0.555
Num. of Adults 65+	-0.019	-0.012	0.006	2.54 / 3.33	0.624	0.675	0.917	1.443
Oldest Married	-0.031	0.001	0.032	2.72 / 3.41	0.424	0.971	0.530	0.477
Oldest BA+	-0.045	0.000	0.045	3.97 / 3.52	0.059	1.000	0.284	0.150
Share Female	0.028	0.007	-0.021	3.23 / 2.79	0.210	0.696	0.575	0.620
Disabled Member	0.015	-0.064*	-0.079	2.79 / 3.14	0.630	0.016	0.138	0.237
High Pop. Density	-0.028	0.016	0.044	3.08 / 3.93	0.459	0.552	0.394	0.339
Gross Income	-75.749	-20.742	55.008	2.35 / 2.79	0.232	0.795	0.616	1038.988
Pension Income	5.086	18.482	13.397	2.61 / 3.26	0.778	0.362	0.691	569.748

Notes: This table presents continuity tests for predetermined household characteristics around the age-75 threshold. For each outcome, we report the pre-policy and post-policy RD estimates, their difference, and corresponding confidence intervals. The table also includes bandwidths and p-values from each estimation, as well as the average outcome at age 74. The estimations use the same framework as the main results. BA+ stands for Bachelor's degree or higher. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

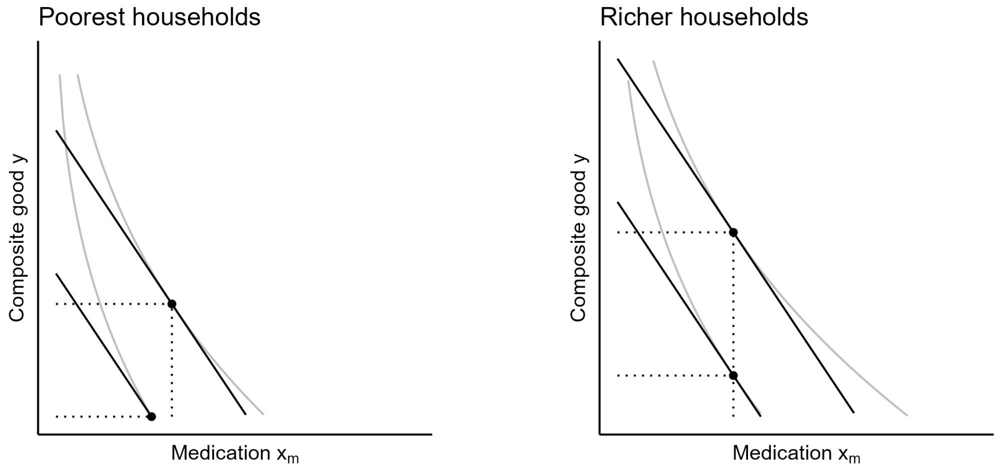
Appendix B Theoretical Predictions

The basic microeconomics concept of optimal choice under a changing budget constraint can provide intuitions and guide our interpretation of the empirical results. We can assume that households allocate their budgets to medication and a composite good, aggregating the consumption of everything else than medication. Within medication, we distinguish prescription drugs covered by *Drugs 75+*, for which the households spend e_m before the introduction of the policy and other drugs consumed, denoted by x_m acquired by the household at price p_x. The composite good is denoted by y and priced at p_y.

Drugs 75+ affects the optimal bundle consumed by a household and the budget at its disposal for x_m and y. Before the policy, the household's consumption is limited by the budget m: $p_x x_m + e_m + p_y y \leq m$. After the reform, households save e_m on prescription drugs and can allocate that amount to x_m and y, giving a budget constraint: $p_x x'_m + p_y y' \leq m$.

From the point of view of the optimal bundle in the (x_m, y) space, the policy results in a shift of the budget constraint to the right. The change in consumption depends on the shape of the household's preferences and its relative wealth. In Figure B1 we show the impact of the policy under quasi-linear utility, for relatively richer and poorer households (keeping the preferences and budget change constant).

Figure B1: Change in consumption due to the policy under quasi-linear utility $U(x_m, y) = bx_m^a + y$



Under quasi-linear utility, the optimal level of consumption of the good x_m depends on the prices and the shape parameters of the utility function. The savings from prescription drugs covered by the policy are fully allocated to the consumption of y unless the household was not able to purchase the optimal amount of medication before the policy and was in a corner solution as shown in the left-hand graph of Figure B1.[18] Assuming quasi-linear utility implies that the total budget affects the consumption of medication of the poorest households, but its optimal level - or the need - does not vary with income.

[18]The corner solution implies 0 consumption of other goods. We assume the household already satisfies some minimum basic needs before allocating the remaining budet m to x_m and y.

The poorest households will first allocate the savings from the policy to other medication, and only once their needs are satisfied in this area will they consume y. For households that are in the corner solution before and after *Drugs 75+*, we will not observe a change in total spending on medication: $p_x x'_m = p_x x_m + e_m$.

For households who were in a corner solution before the policy but after are not, the spending on medication will slightly decrease as part of the savings is allocated to y: $p_x x'_m < p_x x_m + e_m$. This decrease entails a small drop in the budget share of medication for these households. Finally, the richer households that were able to satisfy all their medication needs already before the policy do not change their consumption of x_m and allocate all their savings from free prescription drugs towards y. Only these households will see a decrease in overall medication spending equal to e_m.

Summarizing, we expect that the impact of the policy on household spending on medication and its budget share will depend on the income level of the household. For the poor, the policy does not affect spending at all or only to a small degree. The decrease in spending is the highest for households that are relatively richer and were able to buy all the needed medication already before the policy. Moreover, the impact of the policy will depend on the saving made, so the pre-policy spending on the covered prescription drugs. If poorer households were restricting their consumption of the covered drugs before the policy than equivalent (in terms of health status), richer households, their savings will also be smaller.

Appendix C Income Analysis

Turning 75 in Poland triggers eligibility for both the *Drugs 75+* program and a care supplement of approximately 50 USD per month. Our difference-in-discontinuities identification strategy is motivated by this institutional feature: under the assumption that the care supplement's effect is time-invariant and additive, difference-in-discontinuities isolates the causal effect of *Drugs 75+* on medication spending, net of confounding policy changes at the same threshold.

Nevertheless, it is important to empirically assess whether the care supplement could itself affect household behaviors.[19] To this end, we first estimate the effect of turning 75 on disposable income using a sharp regression discontinuity design, pooling all available periods. This approach is justified because the *Drugs 75+* program, by construction, should not mechanically affect disposable income (which is defined as total income net of taxes and transfers), and pooling increases statistical power.

Table C1 presents the results. In the full sample, we find no statistically significant effect of turning 75 on disposable income. This null result may reflect both statistical noise—since many households have multiple income sources and members—and potential intra-household behavioral responses. For example, prior research (e.g., Gromadzki (2024)) documents that households receiving new transfers may partially offset them by reducing labor supply or other income sources, especially in multi-generational or multi-person households where income pooling and resource sharing are common. Such offsetting could attenuate the observed effect of the supplement on measured disposable income.

[19] We do not see changes in medication spending around the cutoff before the policy

To address this, we repeat the RDD analysis restricting to single-person households, where income substitution and intra-household adjustments are minimized. In this subsample, we find a marginally significant increase in disposable income of about 45 USD per month—closely matching the value of the care supplement. However, further analysis reveals that this additional income is not translated into higher consumption: the estimated effect on total consumption spending is small and statistically insignificant, while the effect on savings is positive and significant, nearly one-for-one with the supplement amount. This suggests that, among single-person households, the supplement is almost entirely saved rather than spent.

Taken together, these results provide evidence that the care supplement does not meaningfully alter household spending patterns at the age-75 threshold, either in the full sample or among those most likely to be affected. Thus, even absent the difference-in-discontinuities design, confounding from the supplement is likely minimal.

Table C1: RDD Estimates of Disposable Income (USD per Month)

Sample	Estimate	95% CI	p-value	BW	N
Disposable Income Full Sample	0.412 (45.974)	[-89.695, 90.518]	0.993	2.707	46 974
Disposable Income Single-Person HH	44.809 (24.101)	[-2.429, 92.046]	0.063	3.091	14 821
Savings Single-Person HH	52.971* (25.990)	[2.032, 103.910]	0.042	2.782	14 821
Consumption Spending Single-Person HH	-6.921 (19.369)	[-44.885, 31.042]	0.721	3.244	14 821

Notes: Estimates are robust bias-corrected (RBC) sharp RDD effects from `rdrobust`, with triangular kernel and MSE-optimal bandwidth selection (mserd). Outcome is monthly disposable income (in USD), computed as quarterly DDP divided by four. Standard errors, in parentheses, correspond to the robust (RBC) variance estimator. Bandwidth h is the optimal RDD bandwidth on each side of the cutoff (symmetric). Weights equal household size. Rows report the full sample and the subset of single-person households. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

Appendix D Healthcare Outcomes

D.1 Healthcare Needs

To evaluate the effect on healthcare behaviors, we first examine self-reported ability to afford healthcare needs as captured in the survey. Respondents rate their ability on a scale from 1 (can afford all healthcare needs) to 5 (cannot afford healthcare needs). We use this index as the main outcome variable, and also construct a binary indicator for unmet healthcare needs (index values of 4 or 5).

Table D1 presents the difference-in-discontinuities results. Before the policy, there is no significant discontinuity at age 75 for either outcome. After the policy, however, we observe statistically significant improvements: the probability of unmet healthcare needs decreases by 4 percentage points (from an average of 12.6% at age 74), and the index value declines by 0.149 (from an average of 2.328 at age 74), both significant at the 5% level. This suggests that, following the policy, individuals above age 75 are more likely to have their healthcare needs satisfied. However, the difference-in-discontinuities estimates, while directionally consistent with improved affordability, are not statistically significant.

Table D1: Difference-in-Discontinuities in Unmet Health Needs and Health Needs Index

	Pre	Post	Difference
Health Needs Unsatisfied	-0.027	-0.040*	-0.013
	(0.026)	(0.019)	(0.037)
bandwidth	3.10	3.73	
p-value	0.282	0.036	0.707
Health Needs Index	-0.003	-0.149*	-0.146
	(0.089)	(0.062)	(0.120)
bandwidth	2.60	4.07	
p-value	0.963	0.015	0.222

Notes: Table reports pre- and post-policy local linear RDD estimates and their difference-in-discontinuities (Diff.) for two outcomes: a binary indicator for unmet healthcare needs (index values of 4 or 5) and the continuous Health Needs Index (1 = can afford all needs, 5 = cannot afford any). Standard errors are in parentheses; bandwidths are average optimal values in years. Sample size is 46 976. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

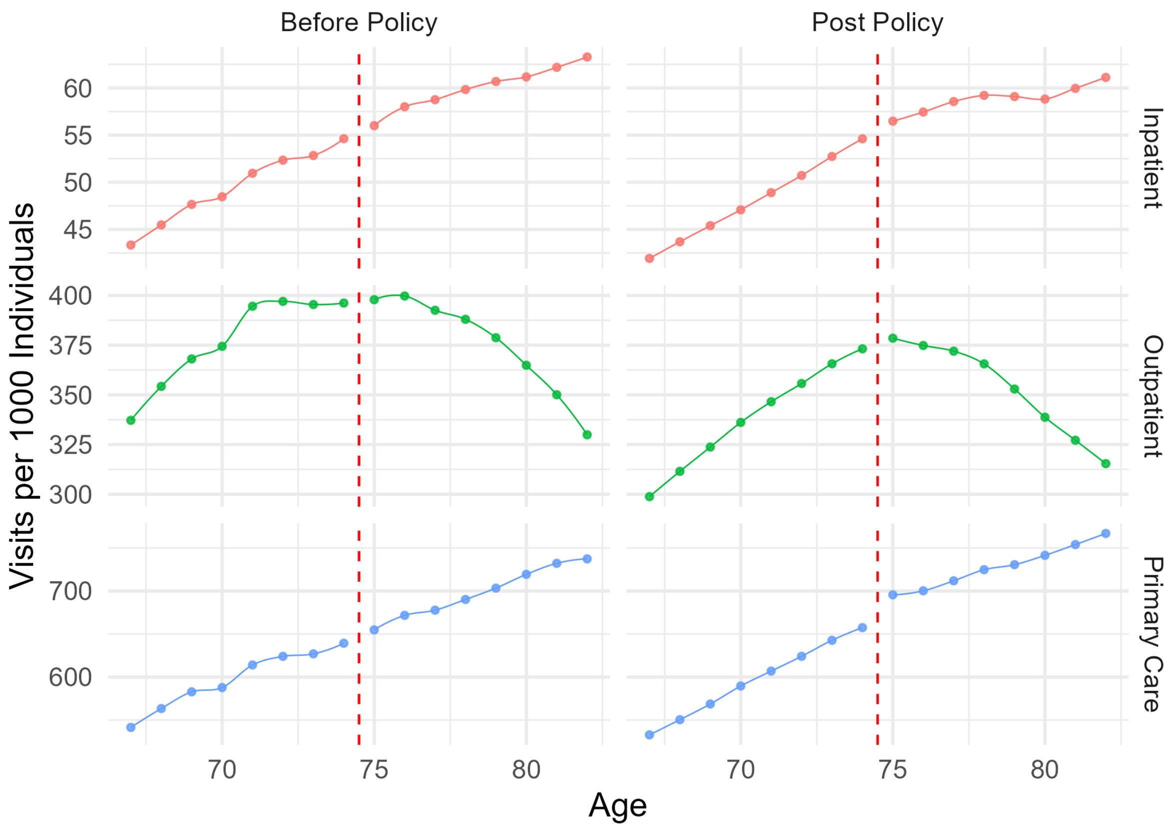
D.2 Healthcare Visits

To further examine the policy's impact on healthcare utilization, we analyze administrative data on healthcare visits provided by the National Health Fund (NFZ). Unlike the survey-based outcomes, these data capture the universe of healthcare visits—classified as inpatient, outpatient (specialists), and primary care—aggregated by cohorts defined by birth year and month, and tracked monthly over time.

This cohort-by-month structure enables us to precisely assign age in months to each group, allowing for a sharp regression discontinuity design around the 75th birthday, both before and after the policy introduction. By leveraging this granularity, we can robustly estimate changes in healthcare use at the eligibility threshold, mirroring the main empirical strategy.

Figure D1 illustrates these patterns by plotting the average number of visits per 1,000 individuals as a function of age, centered at 75. The vertical dashed line denotes the age-75 cutoff. The left panel presents data from the pre-policy period, while the right panel shows the post-policy period. A visible discontinuity in primary care visits emerges only after the introduction of the policy, with no corresponding jump in the pre-policy period. Table D2 reports the corresponding difference-in-discontinuities estimates, using the log number of visits as the outcome for ease of interpretation.

Figure D1: Healthcare Visits



Note: This figure plots the average number of visits per 1,000 individuals by single year of age. Outpatient corresponds to specialists visits, and inpatient to hospitalizations. The running variable is age (in years), centered at the age–75 eligibility cutoff. Separate local–smoothed fits (LOESS) are shown on each side of the cutoff. The left column presents data from the period *before* the policy was introduced, while the right column presents data from the *post–policy* period. Each panel corresponds to a different category of medical services. Sample size is 34 656.

The results in both Figure D1 and Table D2 indicate no evidence of discontinuous changes in inpatient or outpatient specialist visits at the age-75 threshold, either before or after the policy. The corresponding difference-in-discontinuities estimates are small in magnitude and statistically insignificant. This null result suggests that the policy did not affect utilization of these services in the short run. This is consistent with the notion that such visits are primarily determined by underlying health needs, which are unlikely to shift abruptly at the cutoff, but rather are formed by long term medication exposure.

In contrast, we find a significant increase in primary care visits at age 75 after the policy's introduction. The post-policy discontinuity is statistically significant at the 5% level, indicating an increase of about 5.1% in monthly visits for newly eligible individuals compared to those just below the cutoff. However, the difference-in-discontinuities estimate, while positive (4.4% increase), is not statistically significant. This direction of the results aligns with the mechanism that improved access to free prescription drugs raises demand for primary care, as physician visits are a mechanism to obtain prescriptions.

Table D2: Difference-in-Discontinuities in Health Service Use

Outcome	Pre	Post	Diff.
Visits Inpatient	0.002	0.003	0.002
	(0.027)	(0.024)	(0.034)
bandwidth	1.84	2.13	
p-value	0.955	0.888	0.961
Visits Outpatient	0.006	0.007	0.001
	(0.030)	(0.024)	(0.037)
bandwidth	1.92	1.94	
p-value	0.830	0.766	0.989
Visits Primary Care	0.007	0.051*	0.044
	(0.027)	(0.024)	(0.036)
bandwidth	1.83	1.71	
p-value	0.798	0.034	0.226

Notes: Estimates show pre- and post-policy local linear RDD coefficients and their difference-in-discontinuities (Diff.) for visits by service type. Visits correspond to number of visits in a given month by a given cohort defined by birth year and month. Outpatient corresponds to specialists visits, and inpatient to hospitalizations. Standard errors are in parentheses, with average optimal bandwidths shown for each period. The running variable is age (in years) centered at 75, and the bandwidth is measured in years. Sample size is 34 656. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

Appendix E Event Study

As people know they will turn 75 and have free medication, they may anticipate the policy and change their behavior before reaching the age cutoff. To investigate this possibility, we conduct an event study analysis focusing on monthly medication spending around the 75th birthday. We estimate the following specification:

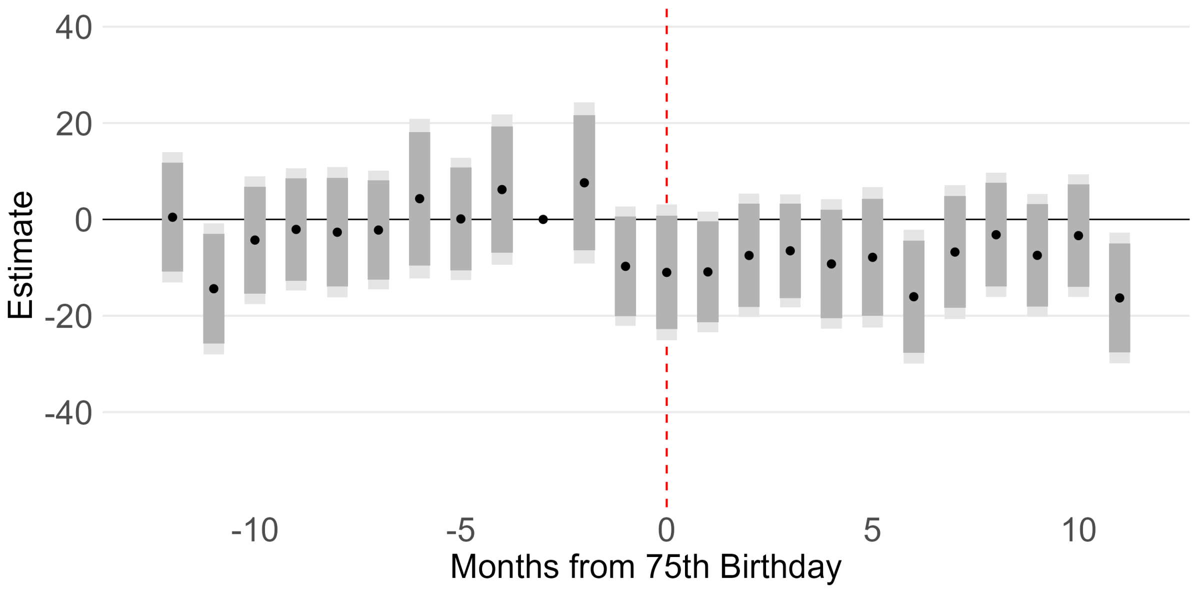
$$MedSpending_{it} = \alpha + \sum_{k=-12,k\neq-3}^{12} [\theta_k(D_{k,it} \times Post_t)] + X_{it}'\gamma + \delta_t + \epsilon_{it}$$

where $MedSpending_{it}$ is monthly medication spending for household i at time t; $D_{k,it}$ are event-time dummies for each month k relative to the 75th birthday (with $k = -3$ rather than -1 as the omitted reference to allow for anticipatory behaviors); $Post_t$ indicates the post-policy period; X_{it} is a vector of household-level controls (region, household size, age in months of the oldest household member, number of children, number of seniors aged 65+, marital status, and education of the oldest adult); and δ_t are time fixed effects. The coefficients θ_k trace the month-by-month effect of policy eligibility on medication spending, allowing for dynamic and anticipatory responses around the age threshold. This specification effectively compares spending trajectories before and after policy implementation for individuals approaching and just surpassing age 75.

Figure E1 presents the estimated event-study coefficients for the period spanning one year before and after the 75th birthday. Due to the relatively small number of observations in each month around the birthday, the results are somewhat noisy; however, a clear pattern emerges. The results reveal a persistent decline in medication spending following eligibility, closely mirroring the magnitude of our main difference-in-discontinuities estimates. Importantly, the pre-policy coefficients for months later than -2 onward are near zero and statistically insignificant, providing support for the parallel trends assumption underlying the event study methodology.

Notably, we observe a decline in spending beginning one month prior to the 75th birthday, suggestive of anticipatory behavior. Individuals may postpone medication purchases in anticipation of imminent eligibility for free drugs. This pattern, however, would mechanically attenuate our main estimates from the difference in discontinuities, implying that the true policy effect may be even larger than reported. To address this concern, we conduct a "donut" difference-in-discontinuities analysis, excluding the three weeks surrounding the 75th birthday. The results (in Table G1) remain robust and quantitatively similar to the main ones, indicating that anticipatory adjustments do not drive our main findings.

Figure E1: Event Study: Medication Spending



Note: This figure presents event study estimates of monthly medication spending around the 75th birthday. The running variable is months relative to turning 75, interacted with the post-policy indicator to estimate dynamic treatment effects. The specification controls for time, region, household size, age in months, number of children, number of seniors (65+) in the household, marital status, and education of the oldest adult. Estimates are weighted by survey weights, with standard errors clustered at the household level. The vertical dashed line marks the age-75 eligibility cutoff. The omitted category is month -3 in the pre-policy period. Shaded areas represent 90% (dark) and 95% (light) confidence intervals. Sample size is 46 976.

Appendix F Multiple Hypothesis Testing

This appendix describes the procedures used to account for multiple hypothesis testing in the analysis of household expenditure reallocation. We begin by constructing baseline p-values for each category-specific difference-in-discontinuities estimate. The p-values are defined as the lowest level of significance for which the bootstrap percentile confidence interval does not contain zero.

Given the large number of outcomes tested jointly, we apply three widely used adjustments to account for the increased probability of false positives: (i) the Holm step-down procedure, which strongly controls the family-wise error rate; (ii) the Benjamini–Hochberg (BH) procedure, which controls the false discovery rate; and (iii) Anderson-sharpened q-values, which increase power in the presence of correlated outcomes by exploiting the dependence structure among tests.

We also follow the methodology motivated by Anderson (2008) to construct (a) composite indices of conceptually related outcomes and (b) aggregated spending categories. These transformations reduce dimensionality, improve power, and provide economically interpretable group-level treatment effects.

F.1 Detailed Categories

Table F1 reports the difference-in-discontinuities estimates for 22 detailed expenditure categories. The raw bootstrap p-values already suggest limited statistical significance. Once we apply the Holm and BH adjustments, no category remains statistically significant. This is unsurprising given the large number of simultaneous tests and the modest effect sizes for most outcomes. The Anderson-sharpened q-values are also close to one for all categories, reflecting the absence of systematic multivariate evidence against the null.

The only category with a comparatively low unadjusted p-value is Alcohol & Cigarettes, but even this estimate does not survive family-wise or FDR adjustment.

Table F1: Difference-in-Discontinuities Estimates for Detailed Expenditure Categories

Outcome	Diff.	p	Holm	BH	Anderson q
Food A	1.106	0.666	1	0.901	1
Food BCD	7.604	0.216	1	0.901	1
Food E	1.575	0.132	1	0.901	1
Medical Products	1.500	0.380	1	0.901	1
Outpatient Services	-0.853	0.722	1	0.901	1
Inpatient Services	0.701	0.388	1	0.901	1
Transport Vehicles	-35.266	0.126	1	0.901	1
Transport Maintenance	5.290	0.658	1	0.901	1
Transport Services	0.813	0.802	1	0.901	1
Home Production Equipment	1.391	0.780	1	0.901	1
Education	0.137	0.862	1	0.901	1
Restaurants & Hotels	-1.965	0.626	1	0.901	1
Alcohol & Cigarettes	5.088	0.044	1	0.901	1
Clothing & Footwear	-1.311	0.828	1	0.901	1
Rent	0.401	0.560	1	0.901	1
Home Repairs	5.233	0.444	1	0.901	1
Utilities: Water & Trash	0.373	0.830	1	0.901	1
Utilities: Energy	-1.309	0.772	1	0.901	1
Utilities: Communication	0.260	0.996	1	0.996	1
Furniture	-4.078	0.472	1	0.901	1
Phones & Computers	-1.455	0.298	1	0.901	1
Foreign Expenses	-1.049	0.480	1	0.901	1
Hygiene & Wellness	1.136	0.706	1	0.901	1

Notes: Table reports difference-in-discontinuities estimates for disaggregated expenditure categories. Inference is based on the p-values from bootstrapping (lowest alpha such that the confidence interval excludes zero). Multiple-testing corrections include Holm and Benjamini–Hochberg procedures. Anderson–sharpened q-values are also reported. Sample size is 46 976.

F.2 Aggregated Spending Categories

To address low power arising from many tests, we also use the aggregate of the detailed categories into five economically meaningful groups as presented in the main results: Healthcare & Hygiene[20], Unhealthy Products,[21] Luxury Spending, Bills, and Home Equipment. The results are shown in Table F2.

Aggregating outcomes reduces the number of tests substantially, but the general pattern remains similar. The Unhealthy Products category experiences an increase and yields the lowest p-values ($p = 0.016$ unadjusted). The Holm correction ($p = 0.08$), the BH-adjusted value ($p = 0.08$) and the Anderson-sharpened q-value of 0.087 indicate that this category is the most affected among the aggregates, even though it is only significant at 10%. This increase would be in line with the growing overall consumption of alcohol in Poland. Repeated surveys conducted by the public polling institute CBOS indicate that alcohol consumption among seniors is widespread and has increased over time: in 2019, 83% of men and 63% of women aged 65 or older reported consuming alcohol, corresponding to increases of approximately 10 and 28 percentage points relative to 2010, respectively. Complementary evidence from the Statistics Poland (GUS, 2018) for year 2017, shows that alcohol consumption remains common well into older ages. More than 70% of individuals aged 60-69 reported drinking alcohol in the preceding year, and even among those aged 70-79 the share exceeds 50%. Smoking displays a different age profile: while smoking remains relatively common among individuals aged 60-69 (27.2%), prevalence declines at older ages, falling to 11.2% among those aged 70-79.

The other aggregated categories exhibit small and imprecise estimates.

Table F2: Difference-in-Discontinuities for Aggregated Spending Categories

Outcome	Diff.	p	Holm	BH	Anderson q
Healthcare & Hygiene	2.623	0.594	1.00	0.594	0.906
Unhealthy Products	7.447	0.016	0.08	0.080	0.087
Luxury Spending	-4.582	0.576	1.00	0.594	0.906
Bills	10.396	0.552	1.00	0.594	0.906
Home Equipment	-4.111	0.512	1.00	0.594	0.906

Notes: Table reports difference-in-discontinuities estimates for aggregated household expenditure categories. Inference is based on p-values from bootstrapping (lowest level of alpha such that the confidence interval does not contain 0). Multiple-testing corrections include Holm and Benjamini–Hochberg (BH). Anderson–sharpened q-values are also reported. Unhealthy products include alcohol, cigarettes, and unhealthy food (Food E). Sample size is 46 976.

F.3 Composite Spending Indices

Finally, following Anderson (2008), we construct weighted composite indices for each spending group, normalizing individual outcomes by pre-policy variances and weighting them by the inverse covariance matrix within each group. These indices compress multiple correlated outcomes into a single measure, increasing power to detect underlying patterns of substitution.

[20]Excluding Medication to focus on the substitution patterns.

[21]It represents Alcohol, Cigarettes, and Food E.

Table F3 presents the index-level difference-in-discontinuities estimates. Consistent with the aggregated categories, the Unhealthy Products Index again yields the smallest p-value ($p = 0.020$ unadjusted) and adjusted p-values of 10%, and the lowest Anderson-sharpened q-value (0.112). Although the estimate does not reach conventional levels of statistical significance, the repeated emergence of the same category across independent levels of aggregation suggests that the upward shift in spending on unhealthy goods is not a statistical artifact of high-dimensional testing.

The indices for Healthcare & Hygiene, Luxury Spending, Bills, and Home Equipment show no detectable effects.

Table F3: Difference-in-Discontinuities Estimates for Spending Index Categories

Outcome	Diff.	p	Holm	BH	Anderson q
Index: Healthcare and Hygiene	0.035	0.508	1.00	0.600	0.924
Index: Unhealthy Products	0.192	0.020	0.10	0.100	0.112
Index: Luxury Spending	-0.033	0.600	1.00	0.600	0.924
Index: Bills	0.035	0.488	1.00	0.600	0.924
Index: Home Equipment	-0.050	0.418	1.00	0.600	0.924

Notes: Table reports difference-in-discontinuities estimates for spending indices. Inference is based on p-values from bootstrapping (lowest level of alpha such that the confidence interval does not contain 0). Multiple-testing corrections include Holm and Benjamini–Hochberg. Anderson–sharpened q-values are also reported. Sample size is 46 976.

Given these findings, we interpret the increase in unhealthy-goods spending as suggestive but not conclusive. The effect is economically meaningful and appears consistently across levels of aggregation. Nonetheless, its statistical significance is attenuated once we correct for the large number of tests. This limitation should be borne in mind when interpreting behavioral spillovers of the policy.

Appendix G Additional Results

Table G1: Donut Difference-in-Discontinuities

Outcome	Pre	Post	Diff.
Med. Expenditures	2.629	-7.955**	-10.584*
	(2.405)	(2.785)	(4.287)
bandwidth	3.28	2.73	
p-value	0.276	0.004	0.014
Med. Budget Share	0.010	-0.011**	-0.022**
	(0.006)	(0.003)	(0.009)
bandwidth	2.26	3.47	
p-value	0.098	0.001	0.024
Catastrophic Spending	0.055	-0.052*	-0.107*
	(0.032)	(0.023)	(0.043)
bandwidth	2.49	3.26	
p-value	0.079	0.021	0.022
Poverty	-0.035	-0.045*	-0.010
	(0.034)	(0.023)	(0.052)
bandwidth	2.34	3.74	
p-value	0.288	0.047	0.836

Notes: This table reports the difference-in-discontinuities estimates from the specification excluding 3 weeks around the 75th birthday for four outcomes: Spending, Budget Share, Catastrophic Spending, and Poverty. Standard errors (in parentheses) are clustered on the household level. BW is the average bandwidth across 1000 bootstrap iterations. The p-values are the smallest significance levels for which confidence intervals does not include 0. Sample size is 46 777. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

Table G2: Robustness: Difference-in-Discontinuities by Kernel and Bandwidth Selection

Panel A: Baseline - Triangular Kernel — MSE symmetric

	Pre			Post			Difference	
Outcome	Estimate	BW	p	Estimate	BW	p	Estimate	p
Med. Expenditures	2.633 (2.373)	3.12	0.268	-5.723* (2.354)	3.41	0.015	-8.356* (3.822)	0.034
Med. Budget Share	0.010 (0.006)	2.30	0.087	-0.009** (0.003)	3.98	0.004	-0.020* (0.008)	0.017
Catas. Spending	0.057 (0.032)	2.43	0.073	-0.041* (0.021)	3.57	0.049	-0.098* (0.042)	0.025
Poverty	-0.028 (0.031)	2.53	0.361	-0.041 (0.021)	3.95	0.052	-0.013 (0.045)	0.758

Panel B: Uniform Kernel — MSE symmetric

	Pre			Post			Difference	
Outcome	Estimate	BW	p	Estimate	BW	p	Estimate	p
Med. Expenditures	2.489 (2.415)	2.58	0.396	-6.209** (2.377)	2.70	0.007	-8.698* (3.688)	0.025
Med. Budget Share	0.010 (0.005)	1.78	0.081	-0.009* (0.003)	2.87	0.011	-0.019** (0.007)	0.008
Catas. Spending	0.057 (0.030)	2.09	0.056	-0.041* (0.021)	2.99	0.049	-0.098** (0.038)	0.008
Poverty	-0.008 (0.030)	2.31	0.696	-0.046* (0.020)	3.46	0.033	-0.038 (0.042)	0.373

Panel C: Epanechnikov Kernel — MSE symmetric

	Pre			Post			Difference	
Outcome	Estimate	BW	p	Estimate	BW	p	Estimate	p
Med. Expenditures	2.327 (2.402)	2.89	0.334	-5.736* (2.234)	3.39	0.010	-8.063* (3.777)	0.034
Med. Budget Share	0.010 (0.006)	2.15	0.094	-0.009** (0.003)	3.68	0.004	-0.019* (0.008)	0.011
Catas. Spending	0.058 (0.032)	2.26	0.066	-0.041* (0.020)	3.63	0.041	-0.099* (0.043)	0.032
Poverty	-0.023 (0.031)	2.36	0.443	-0.042* (0.021)	3.76	0.044	-0.019 (0.046)	0.716

Panel D: Triangular Kernel — MSE asymmetric

	Pre			Post			Difference	
Outcome	Estimate	BW	p	Estimate	BW	p	Estimate	p
Med. Expenditures	2.679 (2.352)	3.42	0.249	-5.953* (2.333)	3.41	0.010	-8.632* (3.870)	0.028
Med. Budget Share	0.012* (0.006)	3.05	0.047	-0.008* (0.004)	3.40	0.020	-0.020* (0.008)	0.014
Catas. Spending	0.059 (0.031)	3.15	0.057	-0.041* (0.021)	4.38	0.046	-0.100* (0.040)	0.012
Poverty	-0.014 (0.030)	2.33	0.648	-0.038 (0.023)	2.93	0.103	-0.024 (0.041)	0.554

Panel E: Triangular Kernel — CER symmetric

	Pre			Post			Difference	
Outcome	Estimate	BW	p	Estimate	BW	p	Estimate	p
Med. Expenditures	3.650 (2.648)	1.91	0.168	-6.042* (2.701)	2.05	0.025	-9.693* (4.193)	0.020
Med. Budget Share	0.012 (0.007)	1.41	0.089	-0.008* (0.004)	2.38	0.028	-0.020* (0.009)	0.019
Catas. Spending	0.064 (0.037)	1.49	0.087	-0.044 (0.024)	2.14	0.062	-0.108* (0.045)	0.019
Poverty	-0.045 (0.036)	1.54	0.201	-0.032 (0.023)	2.37	0.170	0.013 (0.053)	0.803

Panel F: Triangular Kernel — CER asymmetric

	Pre			Post			Difference	
Outcome	Estimate	BW	p	Estimate	BW	p	Estimate	p
Med. Expenditures	3.435 (2.618)	2.09	0.184	-6.127* (2.676)	2.04	0.022	-9.562* (4.333)	0.028
Med. Budget Share	0.011 (0.007)	1.86	0.105	-0.008 (0.004)	2.04	0.051	-0.019* (0.009)	0.034
Catas. Spending	0.060 (0.036)	1.93	0.089	-0.039 (0.023)	2.63	0.098	-0.100* (0.046)	0.037
Poverty	-0.026 (0.034)	1.42	0.440	-0.024 (0.026)	1.76	0.356	0.002 (0.045)	0.967

Notes: Table reports pre- and post-policy RD estimates and their difference. Standard errors in parentheses. BW denotes average bandwidths across iterations and cutoffs. P-value for difference comes from bootstrap (1000 iterations) and is defined as the smallest significance level for which confidence interval does not include 0. MSE symmetric stands for Mean Squared Error Optimal Bandwidth - identical on both sides of the cutoff. MSE asymmetric is also MSE optimal but allows for different bandwiths across cutoffs. CER stands for Coverage Error Rate Optimal Bandwidth, with CER symmetric being one bandwith on both sides, and CER asymmetric being two different bandwiths. Sample size is 46 976. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

Table G3: Difference-in-Discontinuities Estimates: Unweighted

	Pre			Post			Difference	
Outcome	Estimate	BW	p	Estimate	BW	p	Estimate	p
Med. Expenditures	2.512 (2.335)	3.18	0.284	-5.036* (2.085)	3.70	0.016	-7.548* (3.614)	0.036
Med. Budget Share	0.008 (0.005)	2.37	0.112	-0.009** (0.003)	4.16	0.004	-0.017* (0.006)	0.013
Catas. Spending	0.045 (0.029)	2.59	0.130	-0.039 (0.022)	3.33	0.070	-0.084* (0.039)	0.039
Poverty	-0.048 (0.032)	2.20	0.132	-0.045* (0.021)	4.06	0.032	0.003 (0.048)	0.954

Notes: Table reports pre- and post-policy RD estimates and their difference. Standard errors in parentheses. BW denotes average bandwidths across iterations and cutoffs. P-value for difference comes from bootstrap (1000 iterations) and is defined as the smallest significance level for which confidence interval does not include 0. Observations are not weighted. Sample size is 46 976. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

Table G4: Parametric Difference-in-Discontinuities

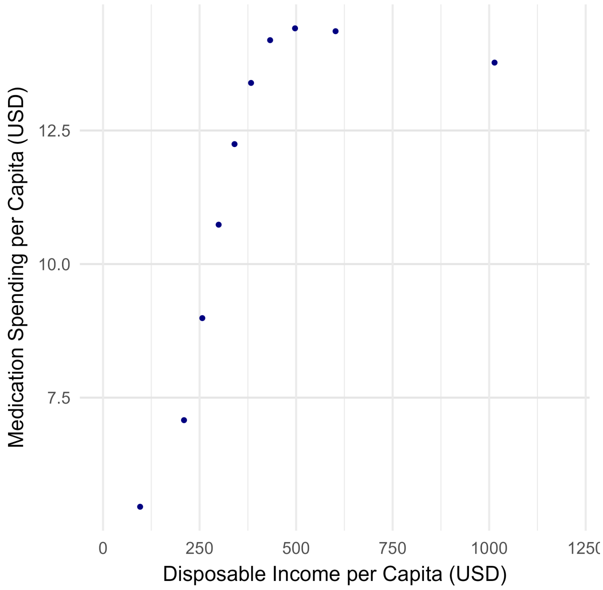
BW	**Poly. Order**	**Med. Exp.**	**Med. Budget Share**	**Catastrophic Exp.**	**Poverty**	**Obs.**
2	1	-6.020* (2.980)	-0.013* (0.005)	-0.074* (0.030)	-0.041 (0.031)	9,877
2	2	-9.317* (4.553)	-0.017* (0.008)	-0.091 (0.047)	-0.001 (0.047)	9,877
2	3	-4.288 (6.019)	-0.014 (0.011)	-0.025 (0.066)	0.034 (0.063)	9,877
3	1	-6.715** (2.496)	-0.011** (0.004)	-0.057* (0.025)	-0.016 (0.026)	13,943
3	2	-6.434 (3.686)	-0.016* (0.006)	-0.092* (0.038)	-0.036 (0.038)	13,943
3	3	-8.880 (4.761)	-0.014 (0.009)	-0.073 (0.052)	0.010 (0.050)	13,943
5	1	-3.973* (1.974)	-0.007* (0.003)	-0.055** (0.020)	-0.030 (0.021)	22,670
5	2	-6.879* (2.944)	-0.015** (0.005)	-0.068* (0.030)	-0.026 (0.031)	22,670
5	3	-8.994* (3.939)	-0.017* (0.007)	-0.090* (0.041)	-0.016 (0.041)	22,670
Mean at age 74		35.747	0.054	0.158	0.187	

Notes: Each row reports results of a parametric difference-in-discontinuities estimates from a different specification. All are based on the equation:

$$Y_{it} = \alpha + \sum_{k=0}^{p} \left[\beta_k x^k + \gamma_k D\, x^k + \delta_k \text{Post}\, x^k + \theta_k (D \times \text{Post})\, x^k\right] + \varepsilon_{it},$$

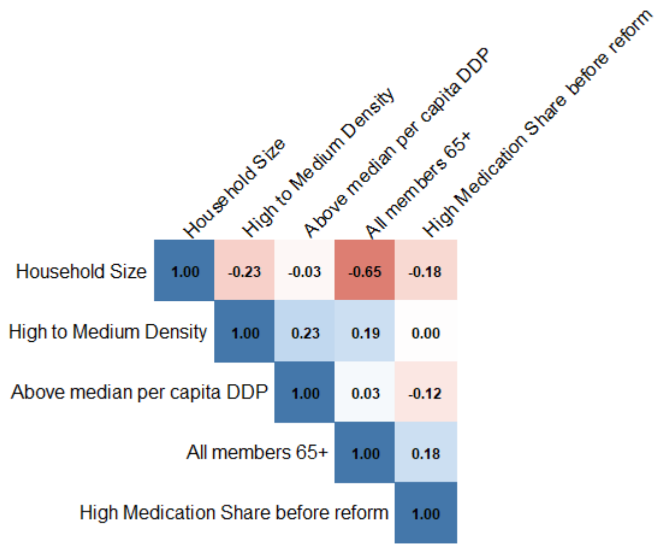
where $x = a - 75$, $D = \mathbf{1}\{a \geq 75\}$, and $\text{Post} = \mathbf{1}\{t \geq t_0\}$. The coefficient of interest is the difference-in-discontinuities at the cutoff, $\tau = \theta_0$. Specifications differ by bandwidth length (symmetric) in years (BW) near the cutoff and polynomial order (p). "Poly. Order" denotes the global polynomial order $p \in \{1, 2, 3\}$ in the running variable ($a - 75$). For each specification, the table reports $\widehat{\theta}_0$ for all main outcomes with cluster-robust standard errors (in parentheses) clustered at the household level. The last row reports mean outcomes at age 74 (for BW = 5) to aid scale interpretation. Sample size is 46 976. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

Figure G1: Binsreg of Medication Spending and Income



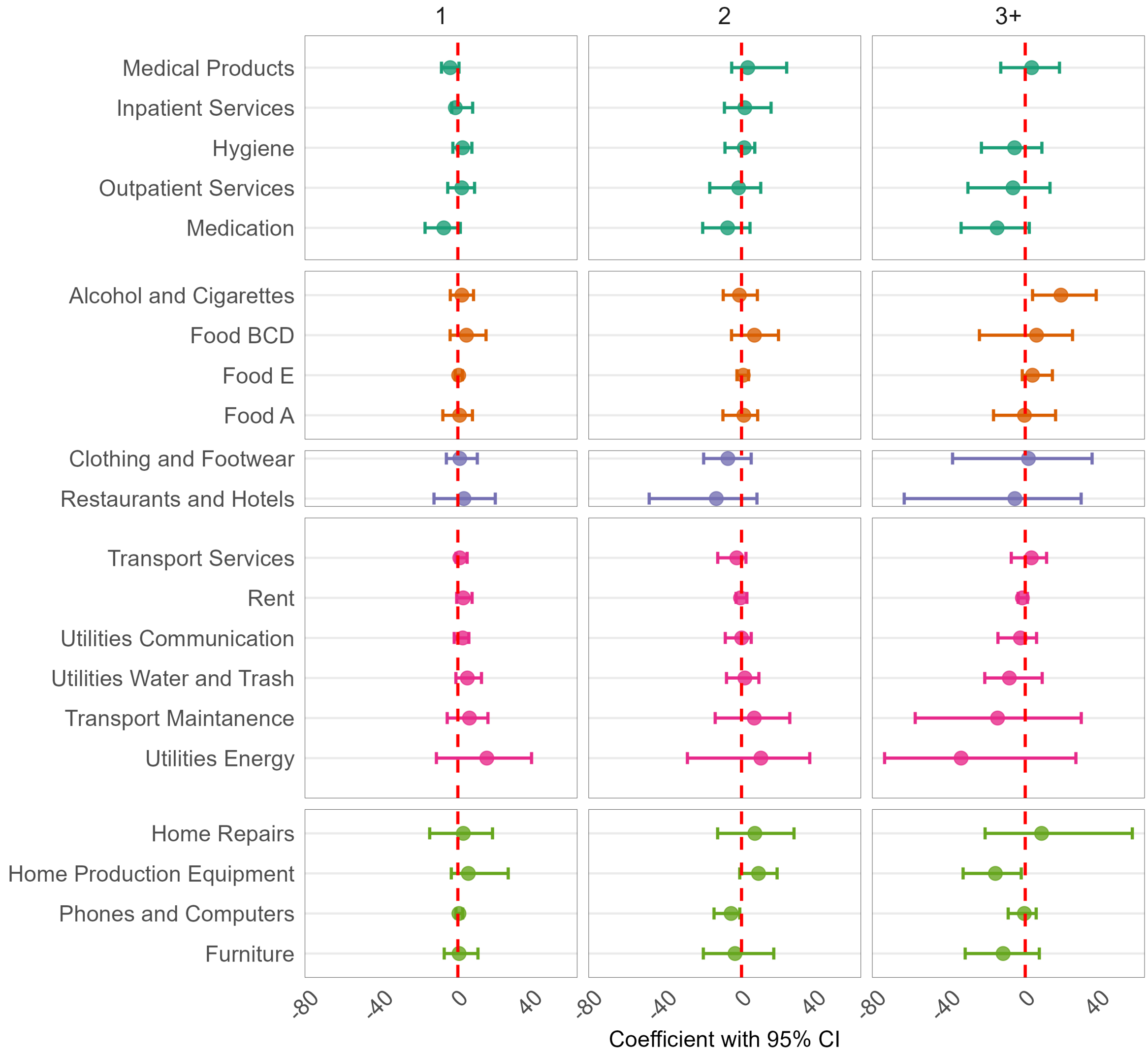
Note: This figure presents binscatter regression (with 10 bins representing deciles) of per capita spending on medication (USD) on per capita disposable income (USD) in the main estimation sample (46 976 observations).

Figure G2: Correlation among Heterogeneity Variables



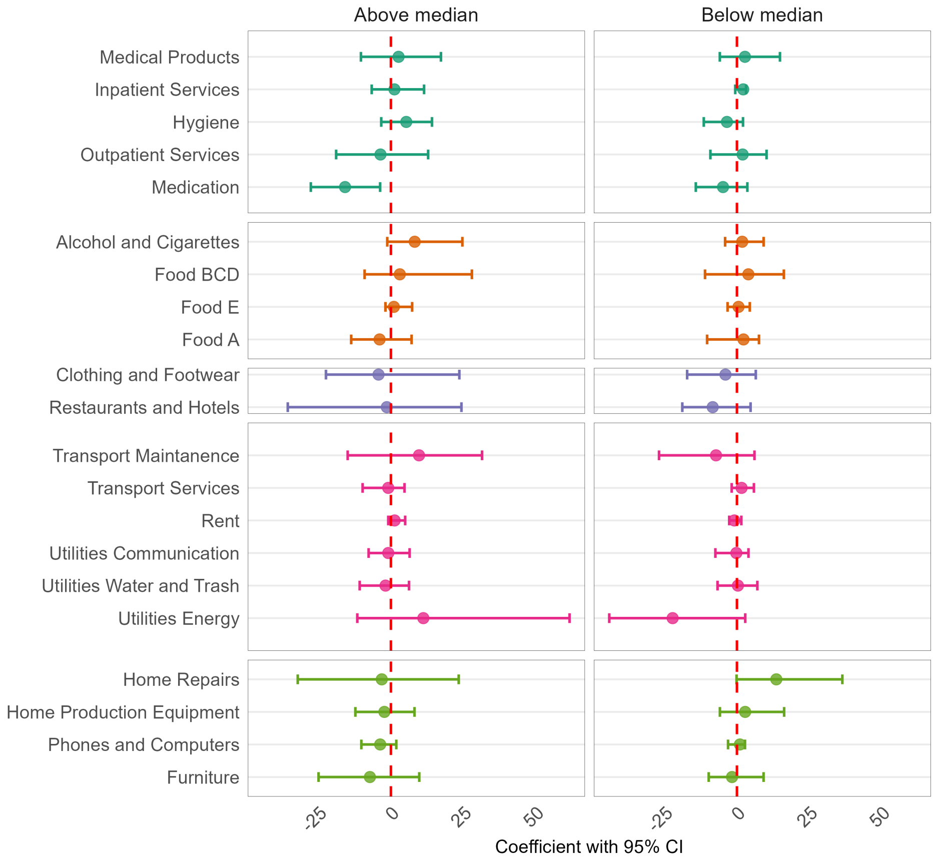
Note: This figure presents correlations between variables used for the heterogeneity analysis. Dark blue indicates strong positive correlation, dark red strong negative correlation and white zero correlation.

Figure G3: Diff-in-Disc Heterogeneity: Household Size



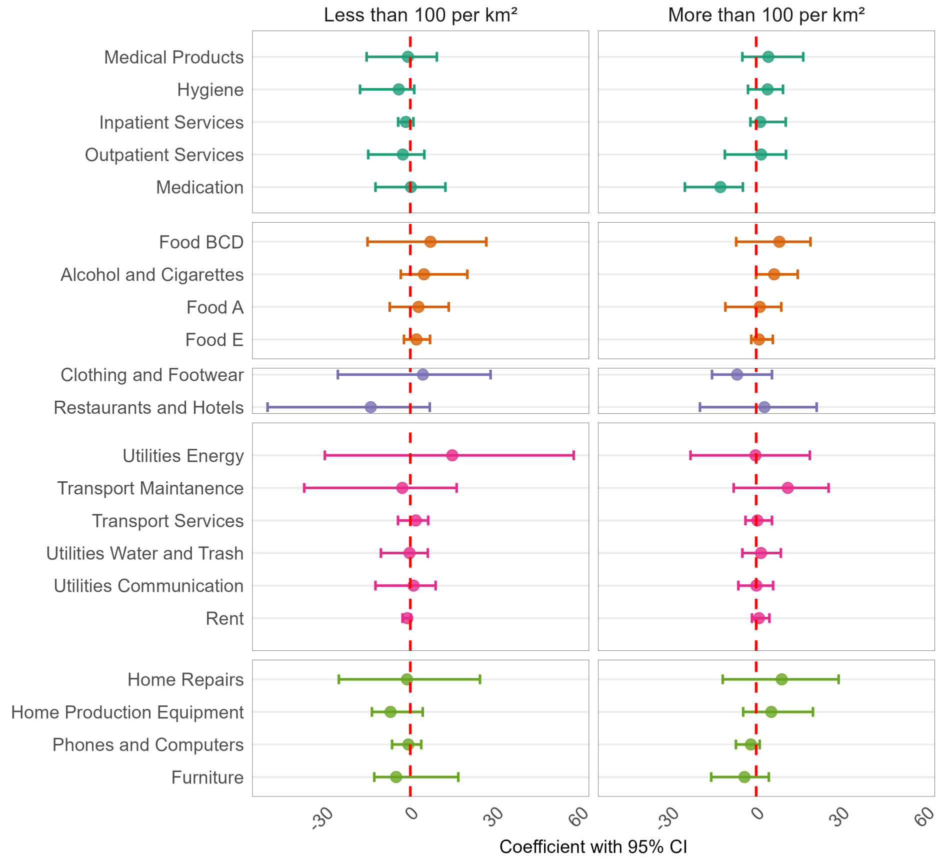
Note: This figure presents difference-in-discontinuity estimates for the alternative outcomes split by household size. Regressions are weighted by survey weights. 95% confidence intervals are based on 1000 bootstrap iterations. Sample size: for n=1: 14 821, for n=2: 20 775, for n=3+: 11 380.

Figure G4: Diff-in-Disc Heterogeneity: Income



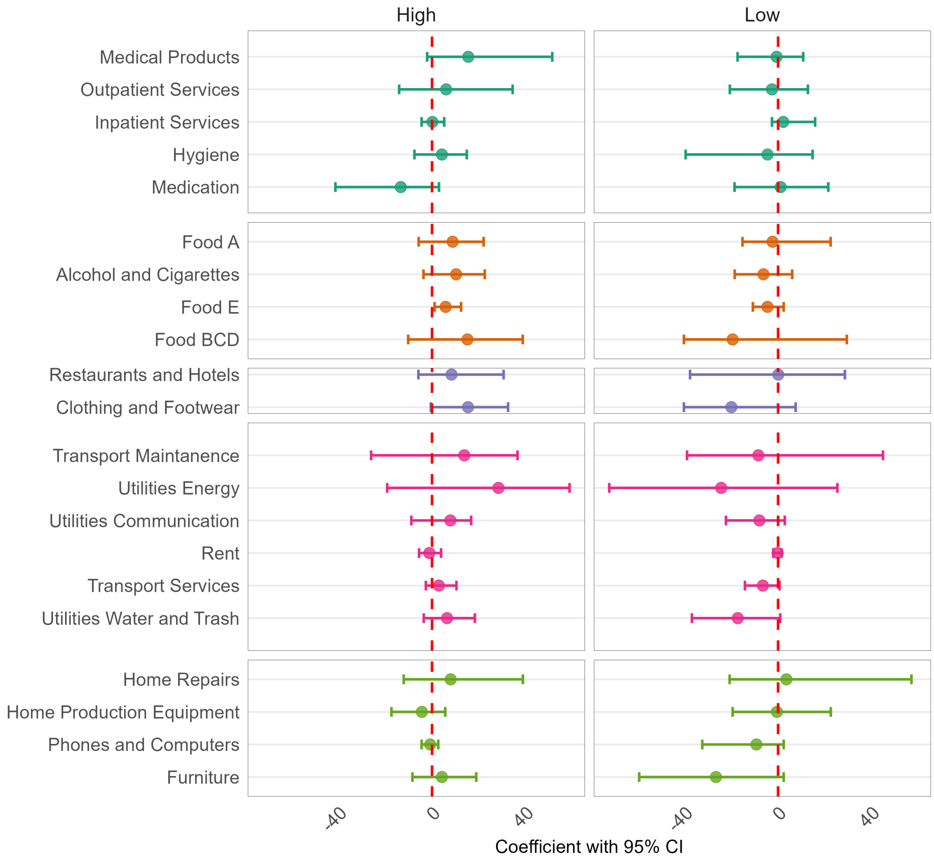
Note: This figure presents difference-in-discontinuity estimates for the alternative outcomes split by per capita disposable income. Regressions are weighted by survey weights. 95% confidence intervals are based on 1000 bootstrap iterations. Sample size: below median: 23 474, above median: 23 502.

Figure G5: Diff-in-Disc Heterogeneity: Population Density



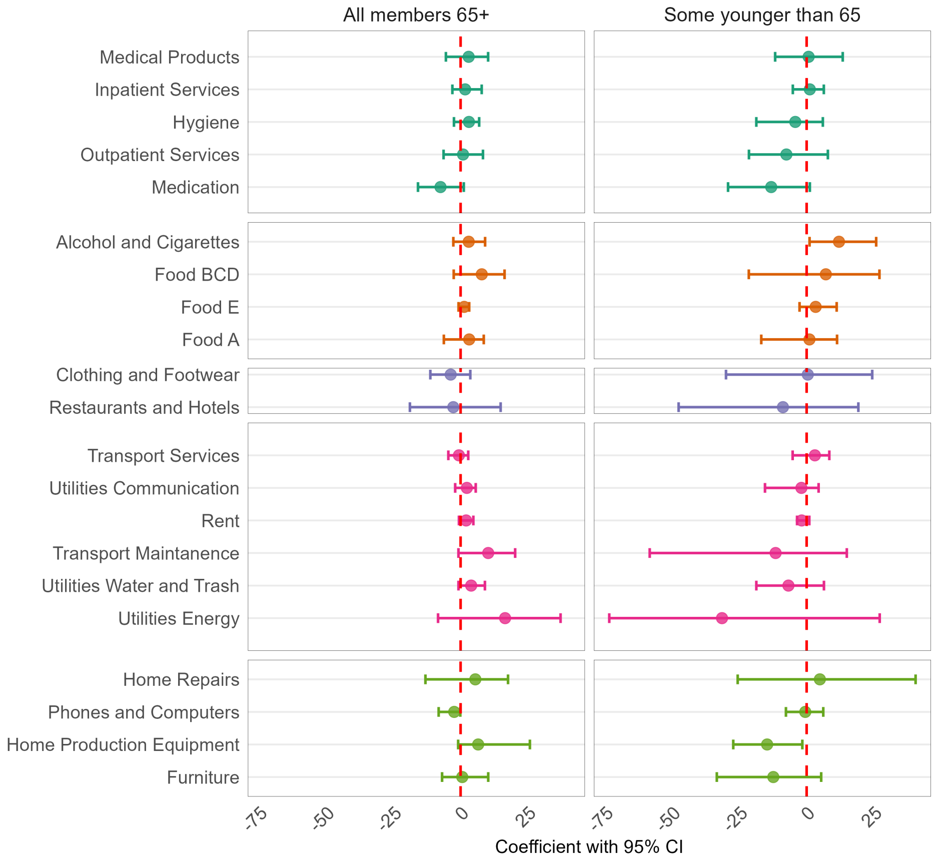
Note: This figure presents difference-in-discontinuity estimates for the alternative outcomes by the population density of the location. It is divided into two groups: below vs above 100 inhabitants per km squared. Regressions are weighted by survey weights. 95% confidence intervals are based on 1000 bootstrap iterations. Sample size: low density: 20 571, high density: 26 405.

Figure G6: Diff-in-Disc Heterogeneity: Previous Medication Budget Share



Note: This figure presents difference-in-discontinuity estimates for the alternative outcomes split (along median) by the pre-policy budget share of the household dedicated to medication . Regressions are weighted by survey weights. 95% confidence intervals are based on 1000 bootstrap iterations. Only households observed prior to the policy are present. Sample size: for below median (*Low*): 11 689, for above median (*High*): 11 777.

Figure G7: Diff-in-Disc Heterogeneity: Household Composition



Note: This figure presents difference-in-discontinuity estimates from the main specification for the alternative outcomes. Regressions are weighted by survey weights. 95% confidence intervals are based on 1000 bootstrap iterations. Sample size: households with only members above 65: 27 033, households with at least one member below 65: 19 943.

Table G5: Difference Estimates by Heterogeneity Group in Single-Member Households

Heterogeneity Group	**Medication**	**Alcohol & Cigarettes**	**Unhealthy Products**	**Food E**	**N**
Baseline	−7.549 (4.654) [0.098]	2.147 (3.543) [0.514]	2.803 (3.655) [0.424]	0.440 (1.102) [0.672]	14821
Pop. density >100/km^2	−11.477 (5.982) [0.054]	3.493 (4.581) [0.421]	4.421 (4.836) [0.364]	0.371 (1.419) [0.772]	9916
Pop. density <100/km^2	−1.607 (7.136) [0.817]	−0.100 (4.558) [0.984]	1.215 (5.141) [0.810]	1.757 (1.896) [0.369]	4905
Above-median income	−19.548 (7.743) [0.015]*	2.282 (6.867) [0.734]	2.465 (7.423) [0.713]	−0.607 (1.900) [0.751]	7409
Below-median income	1.663 (5.612) [0.777]	0.700 (2.791) [0.788]	1.959 (2.996) [0.513]	1.328 (1.301) [0.313]	7412
Low prior spending	0.035 (10.595) [0.996]	−3.453 (6.642) [0.590]	−1.694 (8.379) [0.815]	0.556 (3.014) [0.849]	3659
High prior spending	−8.189 (16.443) [0.593]	−4.193 (6.005) [0.423]	−3.753 (6.199) [0.529]	−0.118 (2.904) [0.962]	3641
No higher education	−5.682 (4.851) [0.230]	0.945 (3.421) [0.773]	0.827 (3.949) [0.828]	0.138 (1.104) [0.913]	12769
Higher education	−27.420 (14.445) [0.059]	6.553 (11.778) [0.573]	10.598 (12.232) [0.375]	3.251 (3.566) [0.372]	2052

Notes: Entries report non-parametric difference-in-discontinuities estimates for single-member households. Each coefficient corresponds to the estimated post–pre discontinuity difference at age 75 for the specified heterogeneity group. Bootstrap standard errors are shown in parentheses, and p-values in brackets. Unhealthy Products is a sum of Alcohol and Cigarettes and Food Categories E. Sample size is indicated in the last column. Stars denote significance: * $p < 0.05$, ** $p < 0.01$.

G.1 Food Categories

We follow the food classification developed by Gromadzki (2024). In his analysis, Gromadzki assigns each detailed food and beverage category a nutritional rating from A (healthiest) to E (least healthy) based on the Nutri-Score system created by Santé Publique France. The Nutri-Score aggregates multiple nutritional attributes: it increases with higher content of fruits, vegetables, nuts, legumes, dietary fiber, protein, and healthier oils (rapeseed, walnut, and olive oil), and it decreases with higher energy density, sugar content, saturated fat, and salt per 100 grams or 100 milliliters. Nutritional scores are obtained using product-level data from OpenFoodFacts.

We adopt his classification in full. The list below reproduces the exact mapping between food categories and their Nutri-Score ratings used in our analysis.

- **Food A**: rice; wheat flour; other flours; groats and cereal grains; bread; other bakery products; pasta and pasta products; other cereal products; curd/white cheese; eggs; citrus fruit; bananas; apples; berries; stone fruit; other fruit; frozen fruit; dried fruit and nuts; fruit preserves; lettuce; cabbage; brassicas/cauliflowers; tomatoes; cucumbers; carrots; beets; onions; other vegetables and mushrooms; frozen vegetables and mushrooms; potatoes; other tuber vegetables and products; baby food.

- **Food BCD**: offal and offal products; fresh/chilled fish; frozen fish; fresh whole milk; low-fat fresh milk; yogurt; sauerkraut; other preserved vegetables and mushrooms; potato products; artificial sweeteners; tea; cocoa and drinking chocolate powder; vegetable and vegetable-fruit juices; breakfast cereals; chickens; other poultry; poultry cold cuts; fresh/chilled seafood; frozen seafood; dried/smoked/salted fish and seafood; other fish/seafood products; dairy beverages and other dairy products; margarine and other vegetable fats; sauces and seasonings; salt; spices and culinary herbs; fruit juices; pizza and other dough-based semi-products; beef; veal; pork; lamb/mutton and goat; other meats; cold cuts (non-poultry); mixed ground meat; other processed meats; ripened and processed cheeses; cream; olive oil; other edible oils; chips; sugar; jams and marmalades; honey; ice cream; coffee.

- **Food E**: condensed and powdered milk; butter; other animal fats; chocolate (bars/tablets); confectionery; other non-alcoholic beverages not elsewhere classified.