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# Anticipation and Adaptation: Cost-Sharing Responses and Mortality Among the Oldest-Old

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## Abstract

Understanding how vulnerable populations respond to healthcare price changes is critical as demographic pressures mount globally. We examine a Japanese reform that doubled coinsurance rates from 10% to 20% for higher-income enrollees aged 75+, using novel data linking national health insurance claims to individual tax records for essentially the entire population aged 75+ over 26 months. Near-perfect compliance at the income eligibility threshold enables a clean difference-in-differences event study with near-experimental treatment assignment. We document three behavioral phases: a last-minute anticipatory surge (+5.9% in costs) concentrated in the final notification month, consistent with deadline salience; an immediate decrease (−6.3%) upon implementation; and persistent modest reductions (~3%) thereafter. Patients adjust almost entirely through visit frequency rather than treatment intensity. Responses follow a gradient of service discretionarity, with dental care showing the sharpest reductions and hospital admissions declining in a pattern consistent with deferral of elective procedures. Expenditure caps shield the most medically intensive patients from the reform’s financial impact. Despite utilization reductions, 15-month cumulative mortality is lower among those facing higher coinsurance, though statistically insignificant. Price elasticities (−0.03 to −0.06) are notably smaller than estimates for younger elderly populations. These results support value-based insurance design and demonstrate that carefully calibrated cost-sharing, paired with effective safety nets, need not compromise health outcomes among the oldest-old.

**Keywords:** cost-sharing; healthcare utilization; oldest-old; value-based insurance design

**JEL Codes:** I11; I13; J14; H51

# 1 Introduction

Understanding how individuals anticipate and adapt to price changes has long been central to economics, with evidence spanning retail purchasing (Hendel and Nevo, 2004, 2006), housing transactions (Best and Kleven, 2018), durable goods programs (Mian and Sufi, 2012), and fiscal policy reforms (Poterba, 1988; Goolsbee, 2000). Healthcare offers a particularly well-suited setting for studying these mechanisms: the deferability of care varies sharply across service types, from life-threatening emergencies that cannot wait to elective procedures that patients can readily reschedule, providing rich within-domain variation in how much room individuals have to time their consumption around anticipated price changes (Manning et al., 1987; Chandra et al., 2010; Duarte, 2012; Einav et al., 2018). Yet the existing literature has focused overwhelmingly on working-age and younger elderly populations, leaving open three questions that bear directly on the growing fiscal challenge of aging societies: how the oldest-old—the fastest-growing demographic globally and the heaviest users of healthcare—respond to cost increases, which margins of adjustment they employ, and whether the resulting reductions in care harm their health.

Healthcare cost-sharing theory, established by Arrow (1963) and Grossman (1972), demonstrates how price signals influence consumption decisions. The RAND Health Insurance Experiment found healthcare demand price elasticities of approximately  $-0.2$  (Manning et al., 1987; Newhouse, 1993), with subsequent research revealing substantial heterogeneity across populations and contexts (Aron-Dine et al., 2013). Building on this foundation, the literature has documented three strategic behavioral patterns: (i) anticipatory behavior where individuals strategically time healthcare consumption around policy changes (Card et al., 2008; Einav et al., 2015; Alpert, 2016), (ii) adaptation pathways as individuals adjust to new cost regimes following implementation (Brot-Goldberg et al., 2017; Cabral, 2017), and (iii) systematic heterogeneity across service types, with essential services showing lower price elasticity than discretionary care (Chandra et al., 2010; Duarte, 2012). This last pattern also provides empirical support for value-based insurance design, which holds that cost-sharing should be calibrated to the clinical value of each service rather than applied uniformly.<sup>1</sup> (Chernew et al., 2007; Fendrick et al., 2012; Baicker and Levy, 2015; Gruber et al., 2020).

These strands of research, however, still leave a coherent set of open questions. First, existing studies face two interrelated methodological limitations that prevent a complete picture of behavioral responses. Most examine either anticipatory behavior (Einav et al., 2015) or post-implementation adaptation (Brot-Goldberg et al., 2017) in isolation, capturing only part of the behavioral arc—yet the full trajectory carries distinct implications: a sharp change that gradually attenuates suggests

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<sup>1</sup>Value-based insurance design principles recognize that medical services differ in their contribution to health, with clinical benefits that vary by patient, timing, and context (Chernew et al., 2007; Fendrick et al., 2012). Inpatient and emergency care show low price sensitivity owing to their urgent nature (Ellis and McGuire, 2007; Chandra et al., 2010), dental care demonstrates markedly higher responsiveness (Manning et al., 1987; Newhouse, 1993), and pharmaceutical utilization reveals strategic patient responses to coverage changes (Einav et al., 2015; Abaluck et al., 2018).

a temporary adjustment; a persistent stable shift implies a genuine new utilization equilibrium; and a progressively deepening response implies long-run adjustment remains incomplete. Clean identification compounds this challenge, as eligibility criteria are often measured with error in existing cost-sharing studies (Bound et al., 2001), making it difficult to isolate behavioral responses from correlated individual characteristics. Second, the oldest-old represent the fastest-growing yet least-studied demographic in this literature. Unlike younger cohorts who retain flexibility to substitute or delay care, the oldest-old face a qualitatively different clinical profile: frailty affects one in four, multimorbidity affects 70% of those over 80, and emergency utilization rises sharply with age (Fried et al., 2001; Clegg et al., 2013; Salive, 2013; Gruneir et al., 2011), all of which constrain the scope for discretionary reduction without health consequences. Whether and how this population responds to cost increases, and on which margins, remains incompletely understood. Third, direct evidence that cost-sharing harms health—the question that ultimately motivates the policy debate—remains scarce across the literature (Rice and Matsuoka, 2004; Chandra et al., 2010), and is especially absent where the stakes are highest: aging societies facing acute fiscal pressure (Ministry of Health, Labour and Welfare, 2022; Eurostat, 2024; OECD, 2023) are expanding cost-sharing precisely for populations whose health may be most vulnerable to such changes.

Japan's healthcare system provides an ideal context to address these gaps. Its comprehensive universal coverage, standardized fee schedules, and transparent cost-sharing structure minimizes the provider and pricing variation that complicates identification in many other settings (Iizuka and Shigeoka, 2022). Its rapidly aging population further makes insights from this context directly relevant to other societies facing similar fiscal pressures. The existing Japanese literature has extensively examined behavioral responses to cost-sharing *reductions* (Ando and Takaku, 2016; Shigeoka, 2014; Fukushima et al., 2016; Nishi et al., 2012), but responses to *increases* remain far less studied, despite evidence of potential asymmetry (Iizuka and Shigeoka, 2023). The closest antecedent to our study is Komura and Bessho (2025), who examine coinsurance rate increases for patients aged 70–74 and find persistent utilization reductions without adverse health impacts. Their analysis, however, relies on semi-aggregated data, which precludes the individual-level temporal analysis needed to trace the complete behavioral arc from anticipation through adaptation, and uses indirect proxies for health stock rather than direct evidence, leaving open whether utilization reductions translate into adverse health outcomes.

We contribute to this literature by examining a policy implemented in Japan in October 2022 that doubled coinsurance rates from 10% to 20% for individuals aged 75 and above (75+) with income above specified thresholds. We leverage a novel dataset linking national health insurance claims to individual tax records for essentially the *entire* population aged 75+. Crucially, eligibility is determined by each individual's income in the *previous* fiscal year, reported well before the policy was announced, rendering income manipulation in anticipation of the reform essentially impossible. Near-

perfect compliance at the eligibility cutoff further confirms this exogeneity: the probability of paying the 20% rate jumps from near zero to 1.0 at the threshold, so that treatment assignment within the local neighborhood of the cutoff approximates random assignment. Furthermore, affected individuals were notified of their new rates two months before implementation, creating a well-defined anticipation window that allows us to separately identify anticipatory and post-implementation behavioral responses within a difference-in-differences event study framework. By tracing behavioral responses over 26 months and directly examining 15-month mortality outcomes, we provide a comprehensive analysis of how the oldest-old navigate increased healthcare cost-sharing, what margins of adjustment they use, and whether the resulting care reductions compromise their health.

We document three sequential behavioral phases and a previously underexplored margin of adjustment. First, a significant anticipatory last-minute surge (+5.9% in costs, +2.3% in utilization rates) concentrated entirely in the final month before implementation, with no detectable response in earlier months—consistent with deadline salience rather than gradual intertemporal optimization. Second, an immediate decrease (−6.3% in costs, −3.0% in utilization rates) upon policy change. Third, persistent but modest reductions (~3% in costs, ~1% in utilization rates) throughout the subsequent year. The implied price elasticities (−0.03 to −0.06) are notably smaller than previous estimates for younger elderly populations (Shigeoka, 2014; Fukushima et al., 2016), reflecting the more essential nature of care for the oldest-old (Clegg et al., 2013; Dent et al., 2019) and the fact that the immediate response—more than twice the steady-state reduction—likely inflates elasticity estimates from studies with shorter observation windows. A decomposition of the intensive margin reveals the dominant adjustment mechanism: patients reduce visit frequency rather than economize on treatment intensity within each visit, so cost reductions are driven almost entirely by fewer appointments rather than cheaper care per visit.

Responses vary systematically across service types, following a gradient aligned with the discretionary nature of each service. Dental care—the most discretionary category—exhibits the sharpest extensive margin response: visit rates surge in the final pre-implementation month and fall substantially afterward, while costs conditional on utilization show little persistent change, implying that patients adjust the visit-or-not decision rather than the scope of treatment within each visit. Outpatient and pharmaceutical services occupy an intermediate position, with persistent reductions across both margins that stabilize over the post-implementation period, indicating a new utilization equilibrium. Hospital admissions, by contrast, follow a qualitatively different trajectory: admission rates decline progressively before partially recovering, while inpatient costs conditional on admission remain essentially unchanged throughout, consistent with deferral of elective or semi-elective procedures rather than foregone acute care.

A parallel gradient emerges along another dimension: patients' underlying medical needs. Among tertiles defined by pre-reform utilization frequency, the margin of adjustment shifts with medical

need: lower-need patients curtail visit frequency most sharply, while higher-need patients, with limited room to reduce visits further, show larger reductions in cost intensity. The highest-need group, patients covered by Japan’s High-Cost Medical Expense Benefit (*Kōryō*, see section 2), presents a striking contrast: despite having the greatest medical needs in the sample, they are the least price-sensitive of all. This is precisely what the institutional design predicts—because the *Kōryō* cap binds independently of the coinsurance rate, doubling the rate leaves their effective out-of-pocket costs largely unchanged. The safety net thus shields the most medically intensive patients from the financial impact of the reform at precisely the point where unmet need would be most consequential.

Crucially, despite reductions in both outpatient visits and hospital admissions over the 15-month post-implementation period, we find no evidence that the increased coinsurance rate adversely affected survival. The point estimate indicates that 15-month mortality is *lower* among those assigned the higher coinsurance rate by 0.2 percentage points (2.2% relative to a baseline mortality rate of approximately 8%), though statistically insignificant. The direction of the evidence nonetheless supports the key welfare result: healthcare reductions induced by cost-sharing were concentrated in lower-value utilization that did not compromise survival, directly supporting value-based insurance design principles (Chernew et al., 2007; Fendrick et al., 2012).

These findings speak to three broader questions. For the economics of consumer behavior, they demonstrate that even populations with predominantly essential needs retain capacity for forward-looking optimization. For health economics, they provide new evidence that cost-sharing can function as a value filter: the oldest-old, a population often assumed to have limited discretionary care, in fact engage in meaningful self-triage that concentrates reductions on lower-value utilization while protecting essential care. For aging policy, they show that carefully calibrated cost-sharing increases, when paired with effective safety nets, can achieve fiscal sustainability without compromising health outcomes—offering a replicable model for aging societies navigating the tension between budgetary pressures and health protection.

## 2 Institutional Background

### 2.1 Japan’s Healthcare System for the Oldest-Old Population

Japan’s universal health insurance system offers an ideal setting for studying cost-sharing responses among the oldest-old. The system covers all residents through mandatory enrollment with standardized fee schedules, eliminating the provider and pricing variation that complicates identification in many other settings (Ikegami et al., 2011). Cost-sharing is structured around transparent age-based coinsurance tiers, with income serving as the sole determinant of differences within each age group—creating the clean variation for causal inference that we exploit below.

The coinsurance rate structure follows a graduated age-based reduction: 30% for individuals under age 70, decreasing to 20% at age 70, and further to 10% at age 75. Since 2006, individuals with income comparable to working-age earners pay 30% regardless of age, preserving income-based equity (Ministry of Health, Labour and Welfare, 2022); prior to the 2022 reform that is the focus of our study, the oldest-old (75+) therefore faced either 10% or 30% depending solely on their income level. In 2008, Japan established a dedicated financing system for this age group—the Late-Stage Elderly Healthcare System (LSEH)—to address their unique healthcare needs and enhance fiscal sustainability through a separate financing pool with its own intergenerational funding structure.<sup>2</sup> This restructuring affected the financing mechanism only; coinsurance rates for the oldest-old remained unchanged. By 2022, LSEH covered approximately 18.52 million individuals—about 98.6% of Japan’s oldest-old population—with approximately 93% paying the 10% rate and only the highest-income 7% paying 30% (Ministry of Health, Labour and Welfare, 2022).

LSEH also incorporates an important expenditure safeguard: the High-Cost Medical Expense Benefit—known in Japanese as *Kōryō*—which caps monthly out-of-pocket expenditures at 57,600 JPY regardless of the coinsurance rate.<sup>3</sup> This cap plays a central role in shielding the most medically intensive patients from the financial impact of the 2022 reform—a feature we return to in detail below.

## 2.2 The 2022 Cost-Sharing Reform

Japan’s demographic transformation created acute fiscal pressure on LSEH as the ratio of working-age contributors to elderly beneficiaries declined. In response, an additional tier of income-based cost-sharing was introduced in October 2022, increasing the coinsurance rate from 10% to 20% for middle-income LSEH enrollees while maintaining the existing 10% and 30% rates for lower- and higher-income groups (Ministry of Health, Labour and Welfare, 2022). This created a new three-tier structure in which approximately 73% of enrollees continued to pay 10%, approximately 20% moved to the new 20% tier, and 7% maintained the pre-existing 30% rate.

Eligibility for the increased rate required two conditions: classification as an ordinary income earner with individual annual taxable income exceeding 0.28 million JPY, and total household income exceeding thresholds of 2 million JPY for single-person households and 3.2 million JPY for multi-person households.<sup>4</sup> Figure A.1 illustrates the complete eligibility determination process. These thresholds generate sharp discontinuities in cost-sharing: individuals just above faced a doubling of their coinsurance rate while those just below maintained their original rate. Importantly, the income

<sup>2</sup>LSEH operates under an intergenerational financing model with 10% funded by enrollee premiums, 40% by working-age contributions, and 50% by government subsidies.

<sup>3</sup>The *Kōryō* benefit applies across Japan’s healthcare system and is not specific to LSEH. The cap amount varies by income tier; 57,600 JPY corresponds to the ordinary income category relevant to our analytical sample. Within the analytical sample, *Kōryō*-eligible individuals constitute 2% of the population, with no significant difference between treatment and control groups (Table 1).

<sup>4</sup>The policy applied these thresholds separately by household type, as the income pooling available to multi-person households means a given threshold implies different per-capita burdens across household compositions.

measure encompasses pension income and adjusted other income from the *previous* fiscal year—a backward-looking rule that is central to our identification strategy, since eligibility is determined by income already earned and reported before the policy was announced, making manipulation of treatment status essentially impossible. Note, however, that coinsurance status is reassessed annually based on the prior year’s income, with updates typically implemented in July or August; some individuals may therefore experience a change in status partway through our panel. We verify in Section 4.5 that this potential implications of treatment status does not drive our results.

To mitigate the burden of the rate doubling, the reform incorporated two safeguards. For outpatient care, increases in monthly cost-sharing were capped at 3,000 JPY—a transitional relief measure in effect until September 2025, covering the entire study period.<sup>5</sup> As Appendix D shows, however, this cap meaningfully softened the price increase only for a small share of high-utilization patients: for the median treated outpatient user, the actual-to-nominal price increase ratio was 98.2%, indicating that the cap left the effective price increase nearly intact for most patients. In addition, the *Kōryō* cap continued to bind for the highest-spending enrollees independently of the coinsurance rate, shielding the most medically intensive patients from sharp increases in out-of-pocket costs. As we show in Section 5.4, this combination of safeguards effectively protected high-expenditure patients from the financial impact of the reform—an important feature for interpreting its distributional effects.

Reflecting the combined effect of both safeguards, realized out-of-pocket burden relative to baseline income (Figure D.1) rose sharply by 57–60% upon implementation and stabilized at 44–56% in steady state, confirming that the reform generated a substantial and economically meaningful financial shock for most affected individuals despite the protective measures in place.

Figure 1 Here

The implementation process followed a structured timeline, illustrated in Figure 1. We index all months relative to October 2022 ( $k = 0$ ), the implementation month; notifications of new coinsurance rates were issued in August 2022 ( $k = -2$ ), two months before implementation. This timing creates three well-defined periods: a pre-notification period ( $k \leq -3$ ), a two-month anticipation window ( $k \in \{-2, -1\}$ ) during which affected individuals knew their new rates but had not yet faced them, and a post-implementation period ( $k \geq 0$ ). Combined with the essentially perfect compliance at income thresholds (see Figure 2 below), this temporal structure allows us to separately identify anticipatory and post-implementation behavioral responses within a difference-in-differences event study framework, as we describe in Section 4.

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<sup>5</sup>For example, if an individual’s monthly outpatient expenses were 50,000 JPY, the coinsurance payment would normally increase from 5,000 JPY (10%) to 10,000 JPY (20%), but the relief measure would limit the actual increase to 3,000 JPY, resulting in an actual payment of 8,000 JPY.

### 3 Data and Measurements

We employ the Medical Claims Data with Income Tax Information for the Oldest-Old in Japan (MCD-Tx), a novel administrative dataset developed by the Health Insurance Bureau of the Ministry of Health, Labour and Welfare (MHLW). MCD-Tx directly links two comprehensive databases: national health insurance claims and individual income tax records for the population aged 75+. This linkage enables precise measurement of each individual's income-based eligibility for cost-sharing rates—a key advantage over studies relying on self-reported income or administrative proxies (Bound et al., 2001).

The dataset contains monthly observations from November 2021 to December 2023 (26 months) for all LSEH enrollees, spanning the full reform period from well before notification through 15 months post-implementation. Crucially, it includes all enrollees regardless of whether they utilized healthcare services in a given month, avoiding the selection bias common in claims-based studies that observe only active service users. For each individual, the data provide monthly healthcare utilization metrics (total costs and service frequency), income by source (pension, salary, business, and other income), demographic characteristics (residential municipality, sex, and age), and service utilization by disease category.

#### 3.1 Analytical Sample

Our analytical sample covers all LSEH enrollees across both single-person and multi-person households, with income-based eligibility determined following the exact formula specified by MHLW (see Figure A.1).<sup>6</sup> To ensure comparability between treatment and control groups, we restrict the sample to individuals with income within 0.5 million JPY of the applicable threshold (i.e., 1.5–2.5 million JPY for single-person households; 2.7–3.7 million JPY for multi-person households). To pool both household types in a single specification, we normalize income relative to the applicable thresholds, so that the cutoff is at zero for all observations. The resulting sample comprises 1,549,144 individuals (34% single-person, 66% multi-person households), contributing more than 40 million person-month observations.

Table 1 Here

Table 1 (columns 1 and 2) compares the full LSEH population to the analytical sample. The analytical sample contains a higher share of males (57% vs. 40%) and a lower share of single-person households (34% vs. 58%), reflecting the income and household composition of individuals near the middle-income threshold. Baseline healthcare utilization rates, however, are broadly similar across the two groups, suggesting that care consumption patterns near the threshold are representative

<sup>6</sup>For single-person households, the income threshold is 2 million JPY; for multi-person households, 3.2 million JPY. In both cases, the income measure is pension income plus adjusted other income from the previous fiscal year.

of the broader population’s service-use behavior. Our estimates should therefore be interpreted as local average treatment effects at the income threshold—relevant to middle-income enrollees near the eligibility cutoff—rather than as population-average effects.

### 3.2 Treatment and Control

Treatment status is determined by whether an individual’s normalized income exceeds the applicable household-specific thresholds as of October 2022. The treatment group comprises individuals whose coinsurance rate increased from 10% to 20%; the control group comprises those who maintained the 10% rate. Within our bandwidth, the treatment group accounts for approximately 80% of the analytical sample (Table 1), reflecting the rightward skew of the income distribution near the threshold (see Figure B.1).

Figure 2 Here

Figure 2 confirms near-perfect compliance at the thresholds: the probability of paying the 20% coinsurance rate jumps from near zero to exactly 1.0 immediately above the respective cutoff, with no anomalous bins or partial compliance. This sharp discontinuity confirms that treatment assignment is exogenously determined by the institutional income formula rather than individual choice.

### 3.3 Healthcare Utilization Outcomes

We examine multiple outcome measures capturing different dimensions of healthcare utilization response. Our two primary outcomes correspond to distinct margins of consumption: healthcare utilization rates—binary indicators of whether an individual used any healthcare services in a given month—capture the extensive margin, while total monthly healthcare costs, measured in 1,000 JPY and log-transformed, capture the intensive margin.<sup>7</sup> We further decompose the intensive margin into total days of healthcare service use and costs per day of service, distinguishing two mechanisms of adjustment: reducing visit frequency versus reducing treatment intensity conditional on a visit.

Beyond these aggregate measures, we disaggregate utilization and costs into four service categories: inpatient (hospital admissions), outpatient (clinic and hospital visits for non-emergency care), dental, and pharmaceutical services, spanning a gradient from the least discretionary (inpatient) to the most discretionary (dental). This allows us to test whether patients direct reductions toward lower-value, more discretionary care.

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<sup>7</sup>We also present results using raw (level) values; results are robust (see Figure C.6).

### 3.4 Mortality Outcome

Beyond utilization outcomes, we examine 15-month cumulative mortality (October 2022 through December 2023) as a direct measure of health consequences. We proxy mortality using sample attrition, i.e., the event that an individual permanently exits the dataset. In the LSEH system, attrition can arise from three sources: death, transition to public assistance (which moves an individual out of LSEH), or relocation across prefectures accompanied by a change in individual identifiers. Two features of our setting suggest that attrition predominantly reflects mortality. First, our sample is restricted to individuals near the middle-income threshold, for whom transition to public assistance is essentially ruled out. Second, inter-prefectural migration among the elderly is rare, at approximately 0.3% annually for ages 75–79 and lower still at older ages.<sup>8</sup>

Appendix E further validates this proxy along three complementary dimensions. First, attrition counts capture 80–96% of official national death counts across age groups, consistent with LSEH’s 98.6% population coverage net of the small residual non-mortality exit routes. Second, healthcare utilization accelerates sharply in the months preceding attrition. Specifically, hospitalization probability more than triples in the final eleven months, while outpatient visits decline monotonically over the same window. This is a pattern consistent with terminal transfer from ambulatory to inpatient care rather than administrative exit. Third, disease-specific trajectories for the three leading causes of elderly death in Japan—cancer, circulatory disease, and respiratory disease—all exhibit accelerating inpatient intensity approaching exit, confirming that the aggregate pattern reflects genuine end-of-life disease progression. Together, these checks support sample attrition as a valid proxy for mortality.

## 4 Identification Strategy

### 4.1 Main Specifications

Our identification strategy exploits two complementary sources of variation within a difference-in-differences (DD) event study framework: clean treatment assignment at the income threshold, arising from the near-perfect compliance documented in Figure 2, and temporal variation from policy implementation. The DD event study leverages this clean assignment while additionally capturing temporal dynamics, including anticipatory behavior, that a static cross-sectional comparison would miss.

We compare individuals with normalized incomes just above and below zero (the thresholds),

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<sup>8</sup>Statistics Bureau of Japan (2023) records 18,391 inter-prefectural moves among individuals aged 75–79 in 2022.

before and after  $k = 0$  (October 2022), within a bandwidth of 0.5 million JPY. Specifically, we estimate:

$$y_{imt} = \alpha_0 + \sum_{k \neq -3} \delta_k T_i \times \mathbf{1}\{t = k\} + \theta_i + \phi_t + \eta_{mt} + u_{it} \quad (1)$$

for individual  $i$  in municipality  $m$  and month  $t$ . The dependent variable  $y_{imt}$  represents measures of healthcare utilization (binary indicators) or costs (log-transformed values). The treatment indicator  $T_i$  equals one if the individual's coinsurance rate is 20% at  $k = 0$  and zero if it remains at 10%. The terms  $\mathbf{1}\{t = k\}$  are time indicators, with  $k = -3$  (July 2022) as the reference period—the last month before individuals were *notified* of their new coinsurance rates (Figure 1), providing a clean baseline unaffected by anticipatory behavior and preserving the full temporal structure needed to separately identify anticipatory and post-implementation responses. We include individual fixed effects ( $\theta_i$ ) to absorb time-invariant characteristics, time fixed effects ( $\phi_t$ ) to absorb secular trends and seasonality, and municipality-time fixed effects ( $\eta_{mt}$ ) to absorb local factors. Standard errors are clustered at the individual level.

The coefficients of interest,  $\delta_k$ , capture the differential effect of treatment in each month relative to  $k = -3$ , tracing the complete behavioral arc from pre-notification ( $k = -11$  through  $k = -3$ ) through the anticipation window to post-implementation adjustment:  $k = -2$  (August 2022) and  $k = -1$  (September 2022) are the two notification months,  $k = 0$  marks implementation (October 2022), and positive values denote subsequent post-implementation months.

For binary outcomes, we transform estimated coefficients to percentage changes relative to the pre-treatment mean of the treatment group: percentage change =  $(\delta_k / \bar{y}_{\text{pre}}) \times 100$ , where  $\bar{y}_{\text{pre}}$  is the treatment group mean from  $k = -11$  to  $k = -3$ , so that percentage changes are expressed relative to the treated individuals' own pre-reform baseline. For log-transformed costs, coefficients directly approximate percentage changes. Equation (1) estimates average treatment effects; we next examine heterogeneity across service types and baseline medical needs.

## 4.2 Heterogeneity Analysis

We examine behavioral heterogeneity along two dimensions. First, for service-type heterogeneity, we estimate equation (1) separately for inpatient, outpatient, dental, and pharmaceutical services, examining both the extensive and intensive margins. These four categories span a gradient from the least discretionary (inpatient) to the most discretionary (dental), allowing us to test whether patients direct reductions toward lower-value care.

Second, we examine whether price sensitivity varies with patients' underlying medical needs—a dimension conceptually distinct from service-type discretionarity. Whereas discretionarity is an intrinsic property of a service category, medical needs reflect individual patients' health status and

care dependency.<sup>9</sup> We classify individuals into four groups based on pre-notification utilization patterns ( $k = -11$  to  $k = -3$ ): individuals who were *Kōryō*-eligible at least once during this period form the “*Kōryō*” group,<sup>10</sup> and the remaining patients are divided into three tertiles—low (T1), middle (T2), and high (T3)—based on average outpatient visit days prior to  $k = -3$ .

The *Kōryō* group warrants particular attention for two reasons. First, because the benefit cap binds independently of the coinsurance rate (see Section 2), this group’s effective out-of-pocket costs remain largely unchanged by the reform, providing a natural falsification test: we should observe little or no behavioral response among them if our estimated effects elsewhere truly reflect price incentives rather than other confounds. Second, the *Kōryō* group represents the most medically intensive patients in our sample, so whether the existing safety net shields them from adverse consequences of the coinsurance increase is of direct relevance to benefit design.

### 4.3 Mortality Analysis

Beyond these utilization margins, we examine whether cost-sharing affects health directly by estimating a cross-sectional regression discontinuity design (RDD) using cumulative mortality over the 15-month post-implementation period ( $k = 0$  through  $k = 14$ ) as the outcome. This complements the event study by providing a static estimate of the mortality effect that does not rely on the parallel trends assumption. Specifically, we estimate:

$$D_i = \alpha_0 + \tau \cdot T_i + f(X_i) + \varepsilon_i \quad (2)$$

where  $D_i$  is an indicator for mortality over the 15-month post-implementation window,  $T_i = \mathbf{1}\{X_i \geq 0\}$  is the treatment indicator, and  $X_i$  denotes normalized income (income relative to the applicable household-specific threshold). The function  $f(\cdot)$  is estimated using local linear regression separately on each side of the cutoff, with the optimal bandwidth selected by Calonico et al. (2014). We report the conventional point estimate alongside robust bias-corrected standard errors. The coefficient of interest  $\tau$  identifies the local average treatment effect at the threshold under the assumption that potential outcomes vary smoothly through the cutoff.

### 4.4 Identification Validity

The validity of both the DD event study and the cross-sectional RDD rests on a common identifying assumption: potential outcomes vary smoothly through the income threshold, which in turn requires

<sup>9</sup>As a concrete illustration, a high-need patient’s dental visit can remain discretionary, and a low-need patient’s emergency admission can remain non-discretionary.

<sup>10</sup>An individual is classified as *Kōryō*-eligible in a given month if their total healthcare costs exceed 576,000 JPY—the level at which the 10% coinsurance rate would generate out-of-pocket costs equal to the *Kōryō* monthly cap of 57,600 JPY ( $576,000 \times 0.10 = 57,600$ ). At this cost level or above, the cap binds regardless of whether the coinsurance rate is 10% or 20%, leaving effective out-of-pocket costs unchanged by the reform.

that individuals cannot precisely manipulate their income around the threshold. This is virtually impossible by institutional design as the relevant income reporting period had concluded well before the policy announcement. We verify this with two tests. First, Figure B.1 shows no evidence of bunching below the threshold in the income density; the distribution is smooth through the threshold for the full sample and for single-person and multi-person households separately. Second, we show covariate balance across five demographic and socioeconomic characteristics at the threshold: age, gender, single-person household ratio, working ratio, and pension amount. As shown in Figure B.2, all five covariates display smooth distributions without detectable discontinuities at the cutoff, supporting quasi-random assignment near the threshold.<sup>11</sup>

For the DD event study (Equation 1) specifically, a second identifying assumption is parallel trends in the absence of the policy change. The main results demonstrate that pre-notification estimates are statistically insignificant and clustered around zero across all outcomes, supporting this assumption. As additional validation that does not rely on parallel trends, we estimate cross-sectional RDDs separately for individual months (Figure B.3); the direction, magnitude, and statistical significance of these estimates are consistent with the main event-study findings throughout.

Finally, as validation of the price transmission mechanism underlying our interpretation, Figure D.1 confirms that the sharp rise in realized out-of-pocket burden (see Section 2) translated into a genuine and exogenously timed financial shock for affected individuals—consistent with interpreting the estimated behavioral responses as reflecting real price incentives. The absence of differential pre-trend further corroborates the parallel-trends assumption underlying the DD event study.

## 4.5 Robustness Checks

We conduct five additional checks addressing potential threats to our main estimates, all of which leave our main findings unchanged. First, we re-estimate with an alternative reference period, using  $k = -4$  as the omitted baseline instead of  $k = -3$ ; estimates are nearly identical throughout (Figure C.1).

Second, because coinsurance rates are reassessed annually based on updated income, some individuals—particularly those whose pension income declined—may have reverted from the 20% to the 10% rate in the final months of our panel; the reverse is unlikely in this elderly, largely pension-dependent population, since pension income rarely rises. If anything, this would attenuate our late-period estimates toward zero, since some individuals coded as treated would in fact face the

<sup>11</sup>We note that the single-person household ratio exhibits a pronounced dip in the region just below the threshold before recovering above it. This pattern is unlikely to reflect strategic income manipulation: as shown in Figure B.1, there is no evidence of bunching immediately below the cutoff in the overall income density. Instead, the non-monotone pattern in household composition likely reflects exogenous institutional features of Japan’s pension benefit structure and tax deduction system (e.g., the basic income deduction), which tend to concentrate non-single-person households in the income range just above 2 million JPY. Crucially, there is no detectable *discontinuity* in the single-person household ratio at the cutoff itself, satisfying the key requirement for identification.

lower rate. To verify this does not drive our results, we exclude individuals whose coinsurance status changed following the annual reassessment, which affected approximately 16% of individuals observed in the month of the update; excluding them leaves the main results essentially unchanged (Figure C.2).

Third, and related, we guard against post-implementation income manipulation—whereby individuals just above the threshold might selectively reduce their income in subsequent years to escape the higher rate—by examining the income distribution and income changes roughly a year after implementation; we find no bunching below the threshold and no discontinuity in income changes at the cutoff, providing no evidence of strategic manipulation (Figures C.3 and C.4).

Fourth, we conduct placebo threshold tests at income levels 0.5 million JPY below and above the actual cutoff, where no reform-induced rate change occurred; neither placebo produces the anticipatory surge, implementation drop, or persistent decline observed at the true threshold (Figure C.5).

Fifth, for the intensive margin analysis, we compare log-specification estimates against raw-level estimates; both yield consistent patterns (Figure C.6).

## 5 Results

### 5.1 Descriptive Statistics

Table 1 (columns 3–4) presents descriptive statistics for the analytical sample at  $k = -3$  (July 2022), the reference month. The population is predominantly male (57%) with a mean age of 81.65 years; 34% live in single-person households. Healthcare utilization is high: 83% of individuals accessed any services, with outpatient care most commonly used (78%), followed by pharmaceutical (64%) and dental services (22%). Average monthly healthcare costs total 60,590 JPY, with inpatient services accounting for the largest share (25,420 JPY) despite being utilized by only 4% of individuals—reflecting the high per-episode cost of hospital admissions. The *Kōryō* share is 2% with no difference between treatment and control groups.

Treatment and control groups differ modestly on observable characteristics by construction: the treatment group is slightly older (81.69 vs. 81.47 years), more likely to live alone (37% vs. 24%), and has higher income (3.03 vs. 2.76 million JPY). These between-group differences are absorbed by individual fixed effects in our main specification, which identifies treatment effects from within-person variation over time.

### 5.2 Effects on Healthcare Utilization and Costs

Figure 3 Here

Figure 3 presents event study estimates of the effect of the increased coinsurance rate on healthcare utilization and costs over the 26-month observation period, with  $k = 0$  marking implementation and  $k = -3$  the omitted reference period. Across both panels, pre-notification estimates ( $k \leq -3$ ) are statistically insignificant and clustered tightly around zero, consistent with the parallel trends assumption.

Panel (a) reveals three sequential patterns in healthcare utilization rates. First, a statistically significant anticipatory surge at  $k = -1$  shows a 2.3% increase among the treatment group. Notably, the coefficient at  $k = -2$ —the first notification month—is indistinguishable from zero, indicating that the anticipatory response was concentrated entirely in the final month before implementation rather than spread across the two-month notification window. This concentration is consistent with deadline salience rather than gradual forward-looking optimization: patients appear to have responded not upon receiving their notification letters, but only under the immediate pressure of an impending price change (Laibson, 1997; O’Donoghue and Rabin, 1999).<sup>12</sup> Second, at  $k = 0$ , utilization falls sharply by 3.0%. Third, the post-implementation period ( $k = 1$  through  $k = 14$ ) shows persistent negative effects ranging from  $-0.6\%$  to  $-1.2\%$ , consistent with a sustained reduction of approximately 1%.

Panel (b) examines log healthcare costs and reveals a similar temporal structure with larger magnitudes. The anticipatory increase at  $k = -1$  is 5.9%, while the coefficient at  $k = -2$  is again near zero, reinforcing the last-minute character of the surge. At  $k = 0$ , costs fall immediately by 6.3%, followed by a persistent steady-state decline of approximately 3% throughout the post-implementation period, with point estimates ranging from  $-2.7\%$  to  $-4.4\%$ .<sup>13</sup> The roughly 3:1 ratio of intensive to extensive margin effects in steady state indicates that the cost response is driven more by reductions in the amount of care consumed than by the decision of whether to seek care at all. These findings yield price elasticity estimates of  $-0.03$  to  $-0.06$ ,<sup>14</sup> substantially smaller than the  $-0.1$  to  $-0.3$  range documented for younger elderly populations (Shigeoka, 2014; Fukushima et al., 2016), reflecting the more essential nature of healthcare needs among the oldest-old.

<sup>12</sup>The asymmetry between  $k = -2$  and  $k = -1$  could reflect several mechanisms. Present-biased preferences predict precisely this pattern: individuals systematically underweight future consequences until they become imminent, then respond sharply as the deadline arrives (O’Donoghue and Rabin, 1999). Demand-side inertia may also play a role, as patients may require the salience of an imminent price change—one month away—to overcome scheduling frictions. Provider-side constraints may further compress feasible reorganization into the final month: booking substantially extra appointments two months in advance is difficult in Japan’s busy outpatient system, whereas accelerating already-planned visits within a single month is more feasible (Komura and Bessho, 2025). Our data do not allow us to disentangle these demand-side and supply-side explanations, but the concentration of the strategic response at  $k = -1$  is itself informative: it implies that the effective window for anticipatory care-seeking is considerably shorter than the formal notification period, with direct implications for how advance notification periods should be designed in future reforms.

<sup>13</sup>Figure B.3 and Table B.1 present cross-sectional RDD estimates for four key months, corroborating the event-study findings. Each panel shows binned averages (dots) and local linear fits (solid lines) estimated separately on each side of the cutoff. The bandwidth is selected by `rdrobust` with a local linear specification. At  $k = -3$  (Panel a), the discontinuity is  $+0.4\%$  and statistically insignificant, confirming no pre-existing differences. At  $k = -1$  (Panel b), a positive discontinuity of  $+3.6\%$  corroborates the last-minute surge. At  $k = 0$  (Panel c), the discontinuity reverses sharply to  $-5.0\%$ , and by  $k = 5$  (Panel d) it has partially attenuated to  $-3.0\%$ , consistent with the steady-state decline. All three post-baseline estimates are significant at the 1% level.

<sup>14</sup>Appendix D provides a more detailed analysis accounting for the outpatient relief cap and *Kōryō* benefit; the median treated outpatient user faced an actual-to-nominal price increase ratio of 98.2%, confirming that the price signal reached most patients in full.

Figure 4 Here

The disproportionately large intensive margin response raises an important question about the underlying mechanism. Figure 4 decomposes the intensive margin into days of service use and costs per day. Days of service use and total costs move almost identically throughout the post-implementation period, both declining by approximately 3% in steady state, while costs per day shows only a minimal decline of 0.5–1%. Patients thus adjust almost entirely by reducing visit frequency—attending fewer appointments—rather than by economizing on treatment intensity within each visit. We examine this visit-frequency mechanism further across service types in the next section.

### 5.3 Heterogeneity Across Service Types

Figures 5 and 6 Here

We examine heterogeneity across four service categories: inpatient, outpatient, dental, and pharmaceutical. Given inpatient care’s distinct nature—lower utilization rates, higher per-episode costs, and correspondingly wider confidence intervals—we present its results separately in Figure 5, while Figure 6 displays the remaining three service types together to facilitate comparison.

**Inpatient care.** Hospital admission rates (Panel a) show a progressive decline over the post-implementation period, deepening from approximately  $-2.5\%$  in the first months to around  $-5\%$  to  $-7\%$  by months 8–10 before partially recovering. Among those admitted, however, inpatient costs (Panel b) show no systematic change throughout, fluctuating around zero. This divergence—declining admission rates alongside stable per-admission costs—is consistent with deferral of elective or semi-elective episodes: if patients are postponing admissions rather than forgoing acute care, the composition and intensity of care within each admitted episode would remain unchanged, as we observe. Diagnosis-level evidence from Appendix F further supports this interpretation: the post-implementation decline is concentrated among elective and deferrable diagnoses such as cataract and arthritis, averaging approximately 14–15% below pre-treatment levels, while non-deferrable acute-sensitive diagnoses (ischemic heart disease, stroke) show no comparable systematic decline—though the latter estimates carry wide confidence intervals given the rarity of these events. We return to the health consequences of these reductions in Section 5.5, where the mortality evidence provides further context.

**Dental care.** Dental services exhibit the most pronounced extensive margin response among all service types. Dental visit rates surge by approximately 6–7% at  $k = -1$  before falling by a similar magnitude post-implementation, with a persistent steady-state reduction of 3–5%—much larger than

the aggregate 1% reduction in overall utilization rates. This pattern reflects the discretionary nature of dental care: unlike outpatient or inpatient services, some dental visits—cleanings, check-ups, elective restorations—can be postponed or brought forward with minimal health risk, making the visit-or-not decision more responsive to price signals. The intensive margin tells a contrasting story: monthly dental costs conditional on utilization show only modest immediate changes and revert toward zero in steady state—markedly weaker than the aggregate 3% intensive margin reduction—implying that when patients do attend, the scope of treatment is largely unchanged. Dental care is thus the most discretionary at the visit margin but not at the within-visit treatment margin.

**Outpatient care and pharmaceutical services.** Outpatient and pharmaceutical services exhibit an intermediate response pattern. Both show anticipatory behavior at  $k = -1$ , immediate reductions at  $k = 0$ , and persistent reductions of approximately 2–3% in both the extensive and intensive margins throughout the post-implementation period. Given that outpatient care is by far the most prevalent service type—utilized by 78% of individuals in the pre-reform period (Table 1)—these reductions in both margins account for the bulk of the aggregate utilization and cost declines documented in the main results. The stability of both margins also implies a genuine new utilization equilibrium rather than a temporary adjustment.

Taken together, these patterns reveal a clear gradient of price responsiveness aligned with service discretionarity: dental care shows the largest visit-frequency reductions but no persistent change in within-visit costs; outpatient and pharmaceutical care show moderate, stable reductions across both margins; and inpatient care shows progressively deepening admission rate declines concentrated in deferrable diagnoses, alongside unchanged per-admission costs. As in the aggregate results, visit frequency is the dominant margin of adjustment across service types, with costs conditional on attendance remaining broadly stable where separable—most clearly for inpatient care and dental care. We next examine whether this pattern varies further with patients’ underlying medical needs, and whether the existing safety net shields the most vulnerable.

## 5.4 Heterogeneity by Underlying Medical Needs

Figure 7 Here

Figure 7 presents results for the four groups defined in Section 4.2, ordered by increasing medical needs. Panel (a) reports extensive margin responses alongside each group’s baseline utilization rate; Panel (b) reports intensive margin responses alongside baseline average monthly costs.

Among the non-*Kōryō* groups, price sensitivity follows opposite gradients across the two margins. At the extensive margin, sensitivity varies *inversely* with medical needs—lowest-need users (T1, baseline utilization rate 56%) exhibit the largest anticipatory surge (+6.7% at  $k = -1$ ) and the

sharpest immediate reduction ( $-5.3\%$  at  $k = 0$ ), while highest-need users (T3, baseline 99%) show minimal fluctuation. This reflects a mechanical ceiling on extensive margin adjustment, as patients who already use services in nearly every month have little room to reduce visit frequency further regardless of price incentives (Chandra et al., 2010). At the intensive margin, the gradient reverses: higher medical needs are associated with larger cost reductions in both the immediate and steady-state periods, with T3 showing the largest swings. Having exhausted the extensive margin as an adjustment channel, high-need patients express their price response through whatever intensive margin flexibility remains, while low-need patients who can freely reduce visit frequency have less need to economize within each visit.

The *Kōryō* group stands in sharp contrast to all three non-*Kōryō* tertiles. Despite being the highest-need group in the sample—with average monthly costs of JPY 317,070, more than five times those of T3—they are the *least* price-sensitive across both margins. Effects on visit rates are small and mostly statistically insignificant throughout the post-implementation period, and no statistically significant reduction in costs is observed from  $k = 1$  onward. This null result is precisely what the institutional design predicts: patients whose monthly expenses regularly exceed the *Kōryō* cap face only negligible changes in effective out-of-pocket costs when the coinsurance rate doubles, because the cap binds independently of the rate. The *Kōryō* benefit thus functions as intended—shielding the most medically vulnerable patients from the financial consequences of the reform at precisely the point where unmet need would be most consequential. The absence of behavioral response among this group also confirms the falsification test described in Section 4.2: our estimated effects elsewhere reflect genuine price incentives rather than other confounds.

The distributional implications are therefore more benign than a uniform cost-sharing increase might suggest. Utilization reductions are concentrated among patients with sufficient room to adjust their care-seeking behavior, while the existing safety net shields those for whom foregone care would be most consequential.

## 5.5 Effects on 15-Month Cumulative Mortality

Figure 8 Here

The utilization reductions documented above—particularly the progressive decline in hospital admissions—raise a central policy concern: does the foregone care adversely affect health? We address this question directly by examining 15-month cumulative mortality.

Figure 8 presents RDD estimates of the effect of the increased coinsurance rate on 15-month cumulative mortality ( $k = 0$  through  $k = 14$ ). Panel (a) plots the raw relationship between normalized income and mortality across the full income distribution; a negative income–mortality gradient is

apparent overall, with no obvious discontinuity near the threshold.<sup>15</sup> Panel (b) presents the formal RDD estimate. Despite the reductions in hospital admissions and outpatient visits documented above, we find no evidence that the increased coinsurance rate adversely affected survival: the point estimate indicates that 15-month mortality is *lower* among individuals just above the threshold by 0.2 percentage points—a 2.2% relative reduction against a baseline mortality rate of approximately 8%—though the estimate does not reach conventional levels of statistical significance.<sup>16</sup>

The direction of this evidence is consistent with the value-based insurance design hypothesis that cost-sharing reduces lower-value utilization without compromising health. Three strands of evidence converge on this interpretation. First, diagnosis-level evidence from Appendix F shows that the admission decline is concentrated in elective and deferrable procedures rather than acute conditions, suggesting that the marginal admissions foregone were clinically deferrable. Second, the largest utilization reductions occurred in dental care—highly elective at the visit margin—with the progressive deepening and partial recovery of inpatient admission rates consistent with deferral rather than permanent foregone care. Third, *Kōryō*-eligible patients—those with the most intensive and presumably most essential healthcare needs—experienced no significant utilization reductions, implying that the safety net successfully ring-fenced the care most critical to survival. Taken together, these patterns suggest that the oldest-old engage in meaningful self-triage, concentrating reductions on care that can safely be deferred while protecting what cannot.

The 15-month observation window nonetheless warrants caution: the progressive deepening of hospital admission reductions raises the possibility of adverse effects that lie beyond our data, and longer-term follow-up remains an important avenue for future research.

## 6 Discussion and Conclusion

This study provides a comprehensive analysis of how the oldest-old respond to increased healthcare cost-sharing, tracing the complete behavioral arc from anticipation through adaptation over 26 months and directly evaluating health consequences. Using data covering essentially the entire population aged 75+ in Japan, we examine the 2022 reform that doubled coinsurance rates from 10% to 20% for individuals with income above specified thresholds. Our findings yield three principal contributions: documenting a last-minute anticipatory surge driven by deadline salience rather than gradual forward-looking optimization; identifying visit frequency rather than treatment intensity as the dominant margin of adjustment, with heterogeneous adaptation paths across service types

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<sup>15</sup>The greater dispersion in mortality among lower-income individuals likely reflects the compounding of multiple disadvantages at the lower end of the income distribution: greater heterogeneity in health status, living conditions, social isolation, and access to informal care networks (Liang et al., 2002). These individuals fall outside our analytical bandwidth and are therefore unaffected by the reform examined here; this systematic heterogeneity nonetheless represents an important avenue for future research.

<sup>16</sup>See Table C.1 for the point estimate with statistical inference.

and medical need groups; and providing direct evidence that carefully calibrated cost-sharing, when paired with effective safety nets, can achieve fiscal objectives without adverse mortality consequences among the oldest-old.

We first document a last-minute anticipatory surge concentrated entirely in the final month before implementation, with no detectable response in the earlier notification month—a pattern that holds uniformly across service types and medical need groups. This concentration is consistent with a deadline effect (Laibson, 1997; O’Donoghue and Rabin, 1999): patients respond to the immediate salience of an impending price change rather than engaging in gradual intertemporal optimization upon receiving written notification two months earlier. This contrasts with the classic anticipatory response literature, which has typically found responses spread across notification windows (Einav et al., 2015). The implication is that the effective anticipation window for this population is considerably shorter than the formal notification period, reflecting both demand-side inertia and supply-side scheduling constraints. That even the oldest-old engage in strategic healthcare timing challenges any assumption that essential-need populations are behaviorally unresponsive to cost signals.

Post-implementation adaptation unfolds in three sequential phases: a sharp immediate reduction, a period of partial attenuation, and convergence to a persistent new equilibrium stable throughout our 14-month post-implementation window. The immediate response is more than twice the magnitude of the steady-state reduction—a gap with an important methodological implication for the broader literature. Prior studies have documented price elasticities for elderly populations substantially larger than our estimates (Shigeoka, 2014; Fukushima et al., 2016; Ando and Takaku, 2016; Nishi et al., 2012). While part of this difference reflects the more essential nature of healthcare needs among the oldest-old (Clegg et al., 2013; Dent et al., 2019), our results suggest an additional explanation: studies with shorter observation windows may capture the immediate response rather than the steady-state equilibrium, conflating the two and potentially overstating long-run price sensitivity. Our post-implementation window is, to our knowledge, among the longest in this literature, and the stability of steady-state estimates supports their interpretation as mid- to long-run elasticities.

Understanding how the steady-state equilibrium is achieved requires examining the underlying mechanism of adjustment. A decomposition of the intensive margin reveals that patients adapt almost entirely by reducing visit frequency rather than by economizing on treatment intensity within each visit. The relevant margin for cost-sharing policy among the oldest-old thus appears to be the visit-or-not decision rather than the treatment-intensity decision.

This visit-frequency mechanism manifests differently across service types depending on how much discretion patients have over the visit decision itself, producing three qualitatively distinct adaptation trajectories. Dental care, the most discretionary category, shows a large anticipatory surge followed by a substantial persistent reduction in visit rates, while costs conditional on attendance revert toward zero—patients decide whether to attend, but once there, receive essentially unchanged

care. Hospital admissions follow a distinct trajectory of progressive deepening before partial recovery, with costs conditional on admission unchanged throughout, consistent with deferral of elective procedures rather than foregone acute care and corroborated by the diagnosis-level evidence in Appendix F. Outpatient and pharmaceutical services occupy an intermediate position, with moderate stable reductions across both margins implying a genuine new utilization equilibrium—here the visit-frequency interpretation is supported by the aggregate decomposition, though direct per-visit cost data are not available at the service-type level.

Heterogeneity by medical needs reveals a complementary dimension. Among non-*Kōryō* patients, price sensitivity follows opposite gradients across margins: lowest-need patients reduce visit frequency most sharply, while highest-need patients—facing a ceiling on extensive margin adjustment—express their response through the intensive margin instead. The *Kōryō*-eligible group, the most medically intensive in our sample, shows no sustained behavioral response across either margin, precisely because the expenditure cap binds independently of the coinsurance rate and leaves their effective out-of-pocket costs largely unchanged. The two gradients together imply that reductions are systematically concentrated among patients with lower medical needs consuming more discretionary services—a pattern that, combined with the null mortality finding, supports the interpretation that foregone care was lower-value care.

Finally, our central welfare result is that carefully calibrated cost-sharing, paired with effective safety nets, need not compromise health outcomes among the oldest-old. Despite reductions in outpatient visits and hospital admissions, the point estimate for 15-month cumulative mortality is actually *lower* among those assigned the higher coinsurance rate—albeit insignificantly so. The direction of this evidence, together with the service-type and medical needs gradients described above, is consistent with the value-based insurance design hypothesis (Chernew et al., 2007; Fendrick et al., 2012): the oldest-old engage in meaningful self-triage, concentrating reductions on discretionary and deferrable care while protecting what cannot safely be deferred. Rather than passive recipients with fixed consumption patterns, they emerge as sophisticated decision-makers—albeit with a short effective response window—capable of navigating price incentives without compromising survival.

Our study has several important limitations. First, the 15-month post-implementation window may not capture longer-term consequences of sustained care reductions; the progressive deepening of hospital admission reductions is particularly worth monitoring with extended follow-up. Second, while mortality is the most consequential health outcome available to us, we cannot rule out adverse effects on health status, functional capacity, or quality of life that do not manifest as mortality within this period—an important gap given that the oldest-old bear disproportionate burdens of functional decline. Third, our analysis focuses on a relatively affluent subset near the income threshold, and responses may differ among lower-income populations (Finkelstein et al., 2019). Fourth, we cannot fully distinguish demand-side from supply-side forces driving the visit-frequency mechanism, an

important avenue for future research.

Our findings nonetheless speak to a broader tension between fiscal sustainability and health protection (Rice and Matsuoka, 2004). The conventional worry is that coinsurance rate increases reduce essential care and harm vulnerable populations. Our evidence suggests a more nuanced picture: when safety nets shield the highest-need patients and the broader population retains sufficient discretionary care to absorb visit-frequency reductions, cost-sharing can achieve fiscal objectives without measurable health costs—offering a replicable model for aging societies navigating demographic pressure on healthcare systems.

Our findings also carry several implications for the design of cost-sharing among the oldest-old. The *Kōryō* experience suggests that binding expenditure caps are a critical complement to coinsurance rate increases: by shielding the highest-need patients from the financial consequences of reform at precisely the point where unmet need would be most consequential, such safety nets allow cost-sharing to achieve fiscal objectives without compromising the care most essential to health. The dominance of the visit-frequency margin also has practical relevance for instrument choice—flat per-visit fees may achieve similar utilization goals more directly than proportional coinsurance rates, which in principle target treatment intensity but appear largely ineffective at that margin among the oldest-old. Finally, the concentration of the anticipatory response in a single month rather than across the two-month notification window suggests that policymakers should not assume that longer advance notice translates into smoother behavioral adjustment; notification periods may need to be either considerably longer or accompanied by targeted provider outreach to prevent last-minute scheduling surges.

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Table 1: Descriptive Statistics One Month Prior to Policy Notification ( $k = -3$ , July 2022)

|  | (1)<br>Full<br>Sample | (2)<br>Analytical<br>Sample | (3)<br>Control    | (4)<br>Treatment  | (5)<br>Difference |
|--|-----------------------|-----------------------------|-------------------|-------------------|-------------------|
| <i>A. Demographic characteristics</i>  |                       |                             |                   |                   |                   |
| Male                                   | 0.40<br>(0.49)        | 0.57<br>(0.50)              | 0.51<br>(0.50)    | 0.58<br>(0.49)    | 0.07***           |
| Age                                    | 82.41<br>(5.91)       | 81.65<br>(4.91)             | 81.47<br>(4.87)   | 81.69<br>(4.92)   | 0.22***           |
| Single-person households               | 0.58<br>(0.49)        | 0.34<br>(0.47)              | 0.24<br>(0.43)    | 0.37<br>(0.48)    | 0.13***           |
| Household income (million JPY)         | 2.47<br>(4.85)        | 2.97<br>(0.59)              | 2.76<br>(0.52)    | 3.03<br>(0.60)    | 0.27***           |
| <i>B. Healthcare utilization rates</i> |                       |                             |                   |                   |                   |
| Healthcare utilization rate            | 0.84<br>(0.36)        | 0.83<br>(0.37)              | 0.82<br>(0.38)    | 0.84<br>(0.37)    | 0.01***           |
| Inpatient                              | 0.05<br>(0.23)        | 0.04<br>(0.19)              | 0.04<br>(0.19)    | 0.04<br>(0.19)    | 0.00**            |
| Outpatient                             | 0.78<br>(0.41)        | 0.78<br>(0.41)              | 0.77<br>(0.42)    | 0.78<br>(0.41)    | 0.01***           |
| Dental                                 | 0.21<br>(0.41)        | 0.22<br>(0.42)              | 0.20<br>(0.40)    | 0.23<br>(0.42)    | 0.03***           |
| Pharmaceutical                         | 0.65<br>(0.48)        | 0.64<br>(0.48)              | 0.63<br>(0.48)    | 0.65<br>(0.48)    | 0.01***           |
| <i>C. Healthcare costs (1,000 JPY)</i> |                       |                             |                   |                   |                   |
| Healthcare costs                       | 74.45<br>(216.62)     | 60.59<br>(191.60)           | 58.42<br>(187.05) | 61.12<br>(192.69) | 2.70***           |
| Inpatient                              | 36.13<br>(202.80)     | 25.42<br>(177.94)           | 24.81<br>(174.58) | 25.57<br>(178.75) | 0.76**            |
| Outpatient                             | 22.50<br>(63.06)      | 20.20<br>(56.56)            | 19.30<br>(52.78)  | 20.41<br>(57.44)  | 1.11***           |
| Dental                                 | 3.10<br>(12.10)       | 3.30<br>(12.12)             | 3.04<br>(11.16)   | 3.36<br>(12.35)   | 0.32***           |
| Pharmaceutical                         | 11.90<br>(34.47)      | 11.18<br>(32.81)            | 10.84<br>(31.36)  | 11.27<br>(33.15)  | 0.43***           |
| <i>Kōryō</i> share                     | 0.03<br>(0.17)        | 0.02<br>(0.14)              | 0.02<br>(0.14)    | 0.02<br>(0.14)    | 0.00              |
| Number of individuals                  | 18,653,796            | 1,549,144                   | 304,210           | 1,244,934         | 1,549,144         |

*Notes:* This table presents summary statistics at  $k = -3$  (July 2022), one month before policy notification. Column (1) reports the full LSEH population with ordinary income certification. Columns (2)–(5) report the analytical sample, restricted to individuals within the 0.5 million JPY bandwidth around the applicable household-income threshold (2 million JPY for single-person households; 3.2 million JPY for multi-person households), with income normalized relative to the applicable threshold. The analytical sample skews toward male and multi-person households relative to the full population, while baseline healthcare utilization rates are broadly comparable across samples, supporting the generalizability of the estimated effects within the analytical sample (see Section 5.2 for further discussion). The control group includes individuals with normalized income below zero (10% coinsurance rate); the treatment group includes those above (20% coinsurance rate). Healthcare expenditures are in thousands of JPY. Standard deviations in parentheses. Statistical significance of differences: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ .

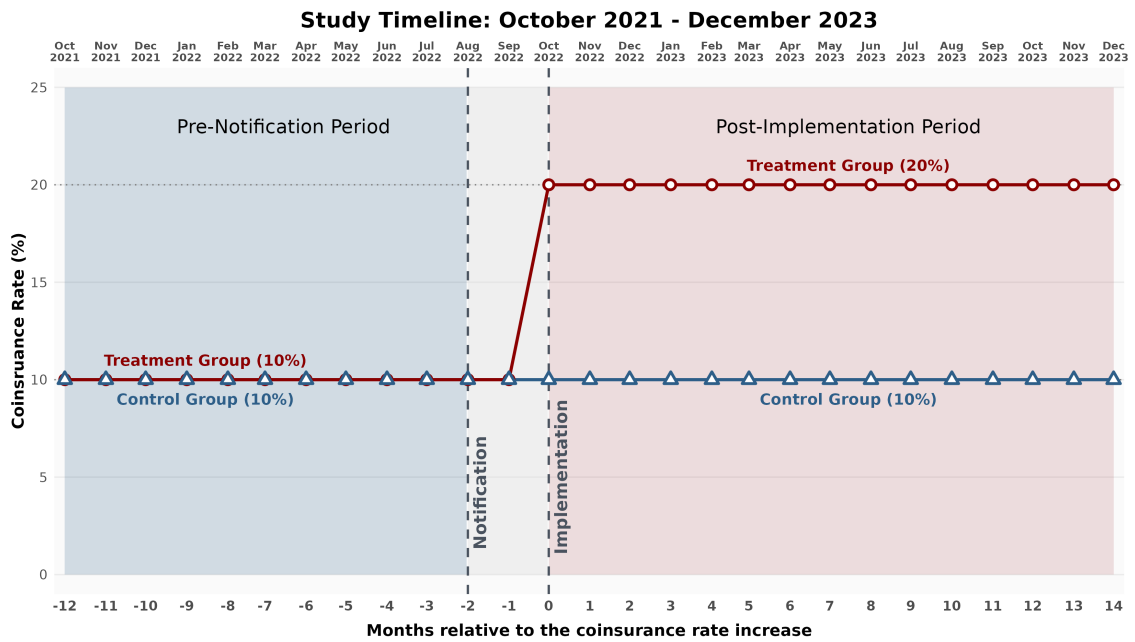


Figure 1: Study Timeline: Coinsurance Rates for Treatment and Control Groups

*Notes:* This figure illustrates the study timeline and coinsurance rate changes. The treatment group experienced an increase from 10% to 20% after  $k = 0$  (October 2022, implementation), while the control group maintained 10% throughout. The timeline divides into three phases: Pre-Notification ( $k \leq -3$ , October 2021–July 2022), Notification and Anticipation ( $k \in \{-2, -1\}$ , August–September 2022), and Post-Implementation ( $k \geq 0$ , October 2022 onward).  $k = -3$  denotes July 2022, the omitted reference period;  $k = -2$  (August 2022) and  $k = -1$  (September 2022) are the two notification months;  $k = 0$  (October 2022) marks implementation.

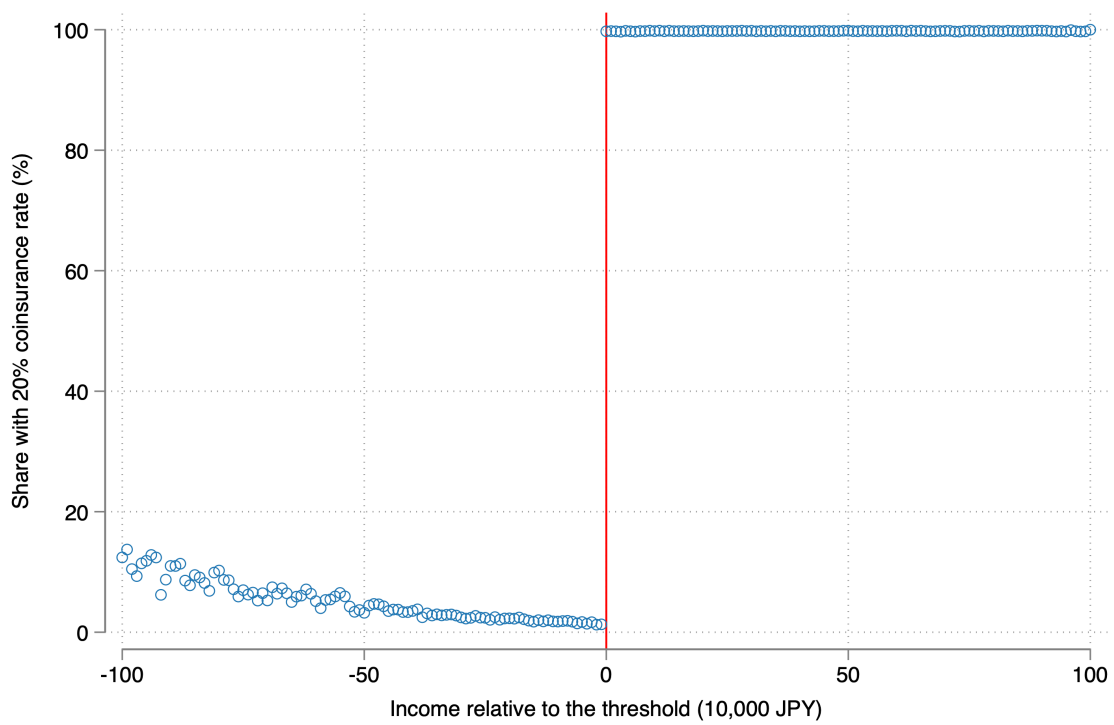
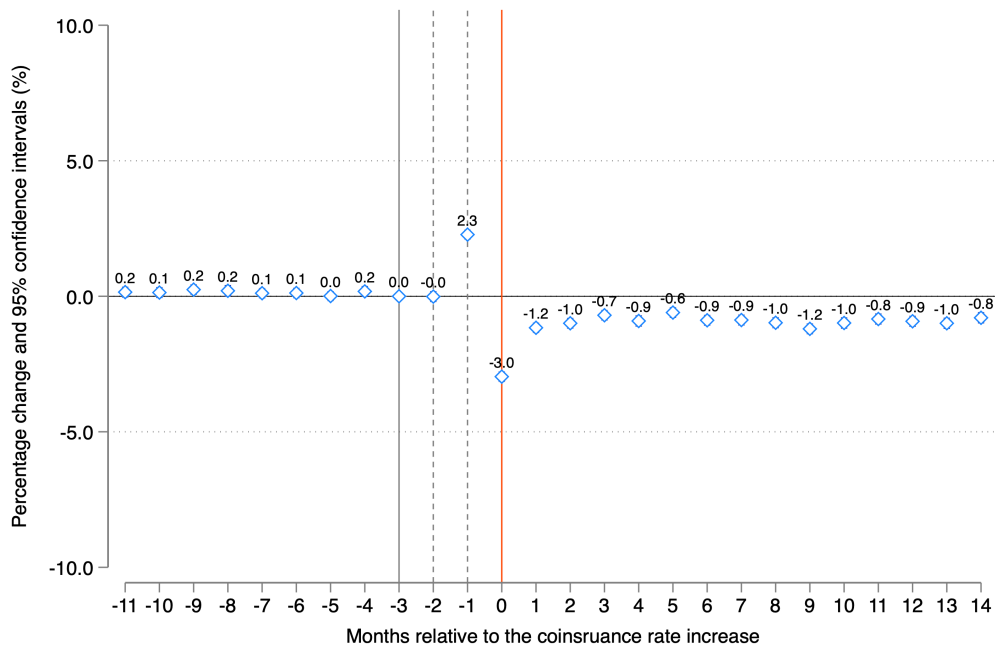
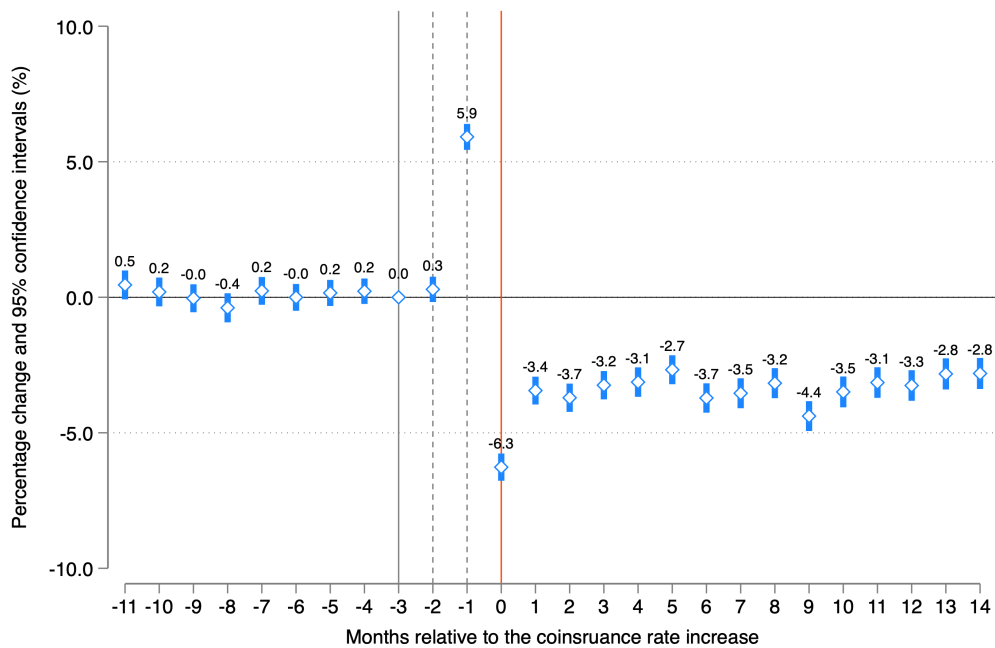


Figure 2: Treatment Assignment: Coinsurance Rate by Normalized Income

Notes: This figure illustrates the sharp discontinuity in coinsurance rate assignment at the income threshold as of  $k = 0$  (October 2022). The  $x$ -axis shows annual income relative to the applicable threshold (2 million JPY for single-person; 3.2 million JPY for multi-person households), in million JPY. The  $y$ -axis represents the proportion assigned the 20% coinsurance rate. The jump from near-zero to essentially 1.0 at the threshold confirms near-perfect compliance and reflects the correct implementation of the income formula for both household types.



(a) Healthcare utilization rate (extensive margin)



(b) Healthcare costs, log (intensive margin)

Figure 3: Effects on Healthcare Utilization and Costs

Notes: Event study estimates of the effect of increased coinsurance rates (from 10% to 20%) on overall healthcare consumption among individuals aged 75+ with normalized income within 0.5 million JPY of the applicable threshold. Panel (a) shows utilization rates (extensive margin); Panel (b) shows log expenditures (intensive margin). Diamonds are point estimates; vertical bars show 95% confidence intervals. The solid black vertical line marks  $k = -3$  (July 2022, the omitted reference period). Two dashed vertical lines indicate  $k = -2$  (August 2022) and  $k = -1$  (September 2022), spanning the anticipation period. The solid red vertical line marks  $k = 0$  (October 2022, implementation). Estimates are expressed as percentage changes relative to the treatment group's pre-treatment mean for binary outcomes and as approximate percentage changes for log-transformed expenditure outcomes. All models include individual, month, and municipality-by-month fixed effects. Standard errors are clustered at the individual level.

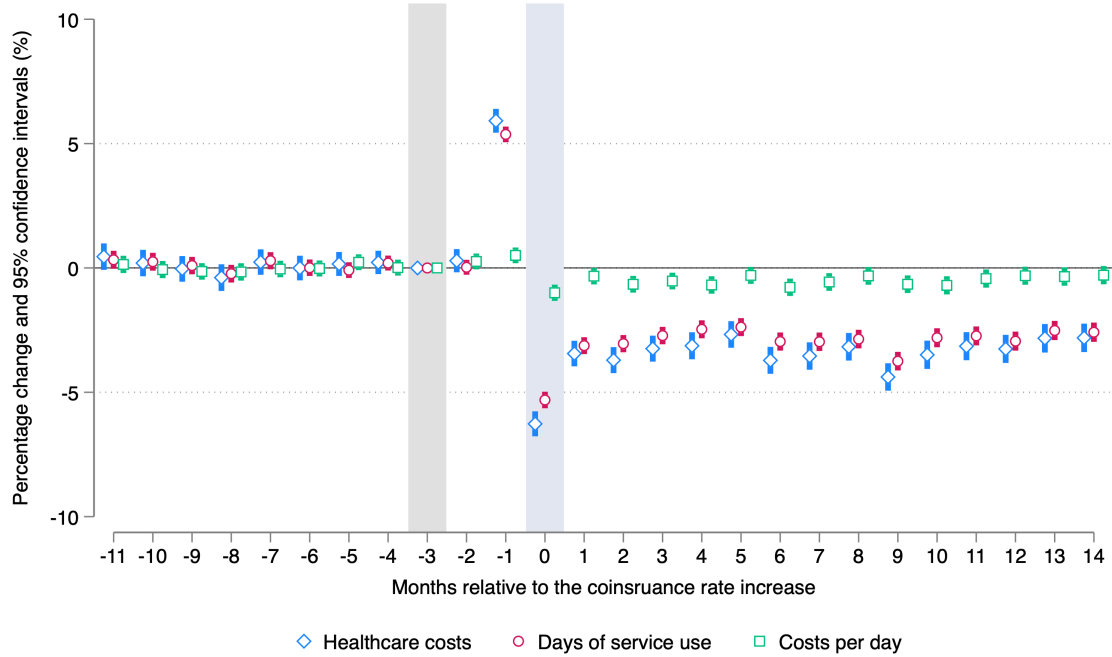
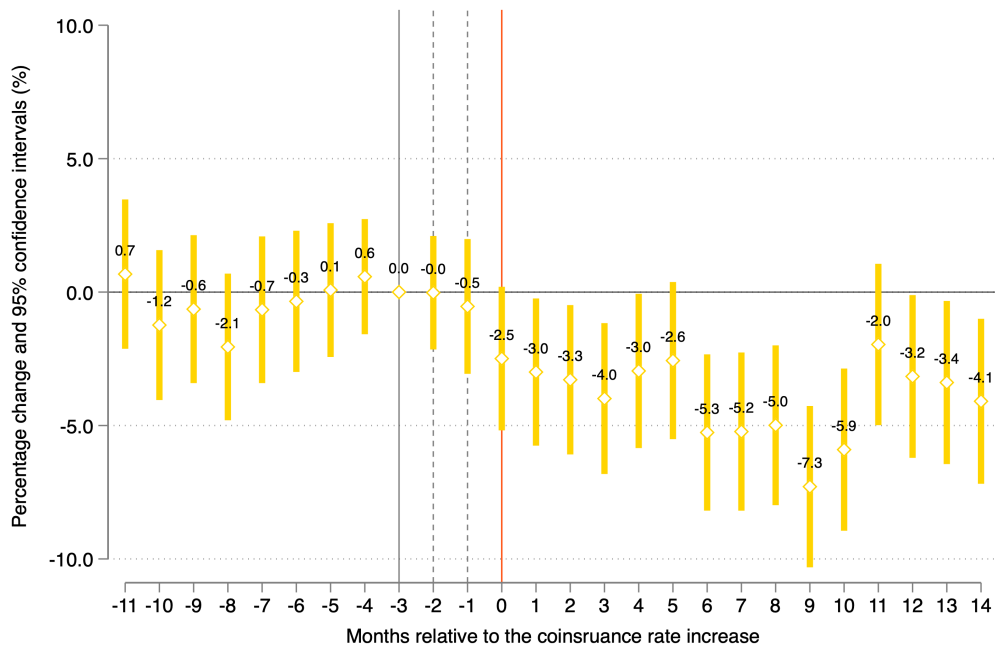
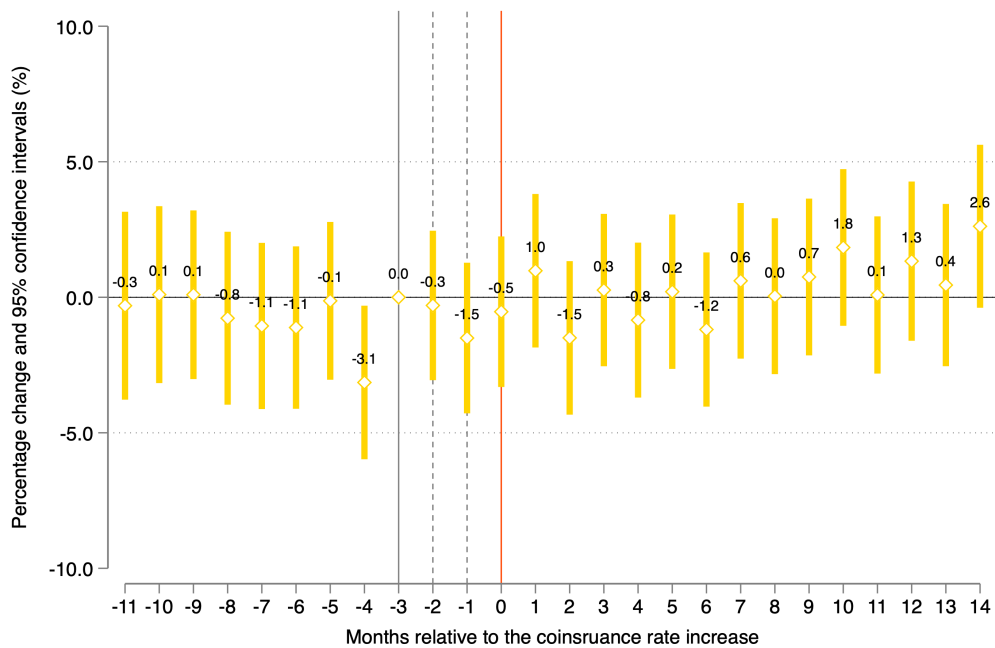


Figure 4: Decomposition of the Intensive Margin: Total Costs, Days of Service, and Costs per Day

*Notes:* This figure decomposes the intensive margin into three components: total medical expenditure (blue diamonds), days of healthcare service use (red circles), and expenditure per day (green squares). All series are expressed as percentage changes relative to pre-treatment means. The gray shaded region marks  $k = -3$  (July 2022, the omitted reference period); the light blue shaded region marks  $k = 0$  (October 2022, implementation); the unshaded region between them ( $k \in \{-2, -1\}$ ) constitutes the anticipation period. In the anticipation period, total expenditure and days of service both surge at  $k = -1$ , while expenditure per day shows little change, indicating that the anticipatory response operates through visit frequency rather than treatment intensity. Post-implementation, expenditure and days of service track each other closely, both declining by approximately 3% in steady state, while expenditure per day shows only a 0.5–1% decline—confirming that the dominant adjustment mechanism throughout is visit frequency reduction rather than within-visit treatment intensity reduction. All models include individual, month, and municipality-by-month fixed effects. Standard errors are clustered at the individual level.



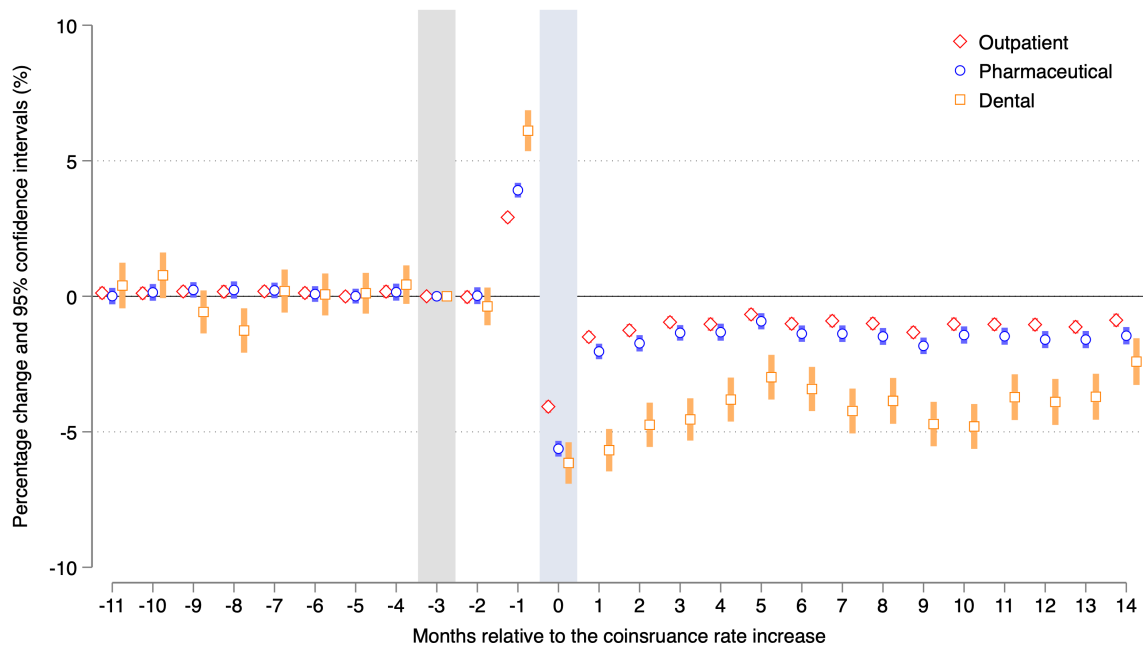
(a) Hospital admission rate (extensive margin)



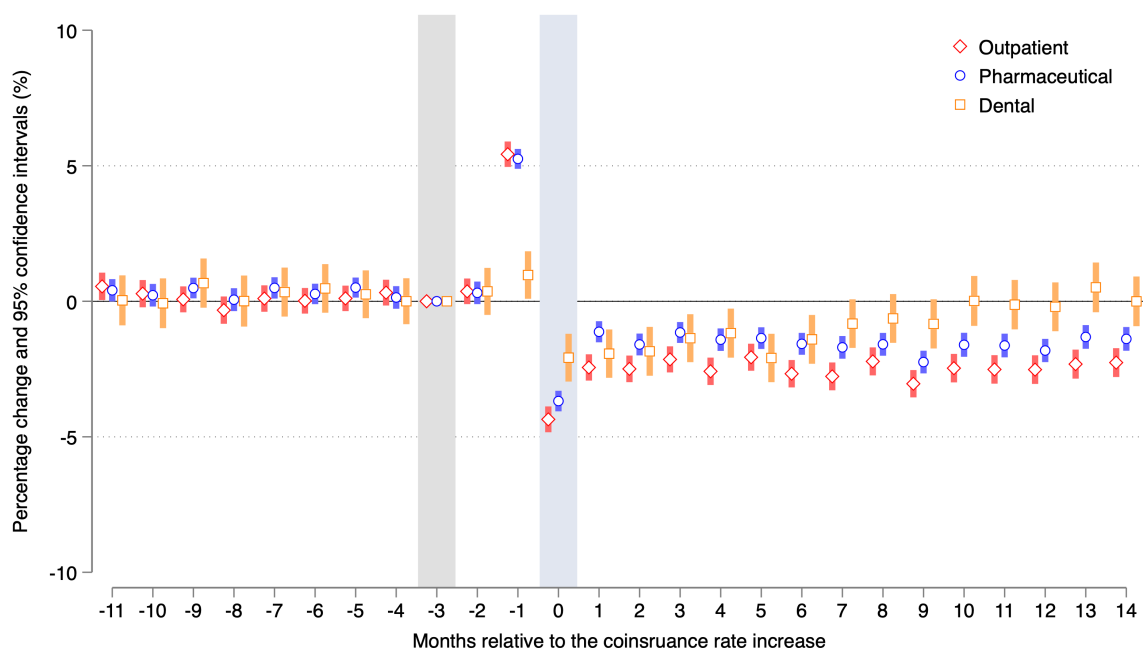
(b) Inpatient costs, log (intensive margin)

Figure 5: Heterogeneous Responses for Inpatient Care

Notes: Event study estimates for inpatient care. Panel (a) shows hospital admission rates; Panel (b) shows log inpatient expenditures conditional on admission. The solid black vertical line marks  $k = -3$  (July 2022, the omitted reference period). Two dashed vertical lines indicate  $k = -2$  (August 2022) and  $k = -1$  (September 2022), spanning the anticipation period. The solid red vertical line marks  $k = 0$  (October 2022, implementation). The divergence—declining admission rates alongside stable per-admission costs—is consistent with deferral of elective or semi-elective admissions rather than foregone acute care. All models include individual, month, and municipality-by-month fixed effects. Standard errors are clustered at the individual level.



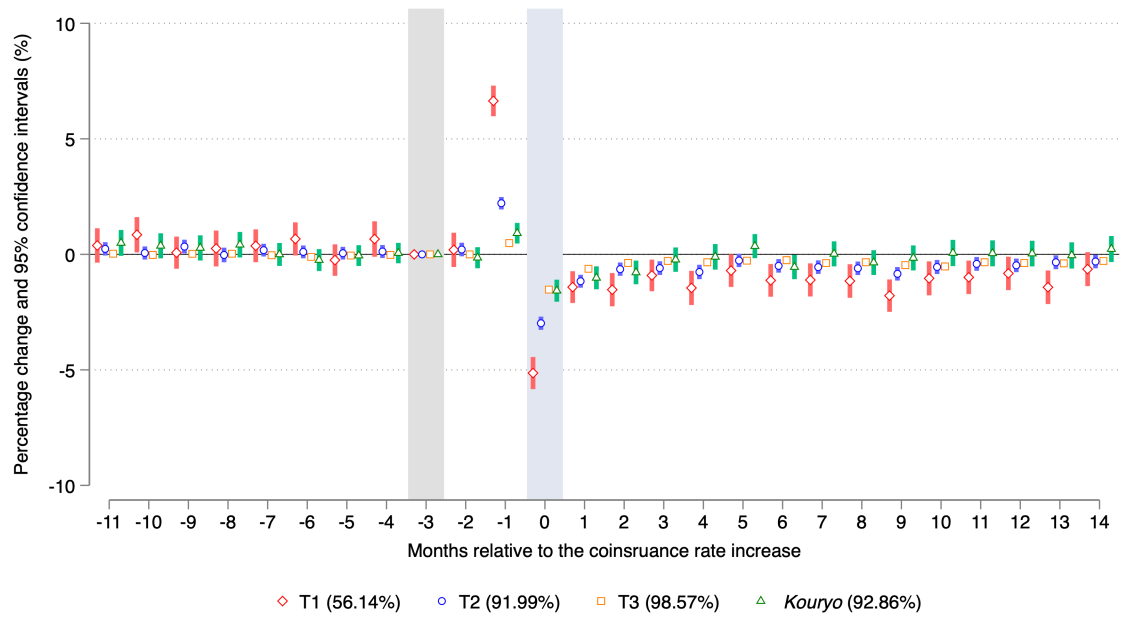
(a) Utilization rates (extensive margin)



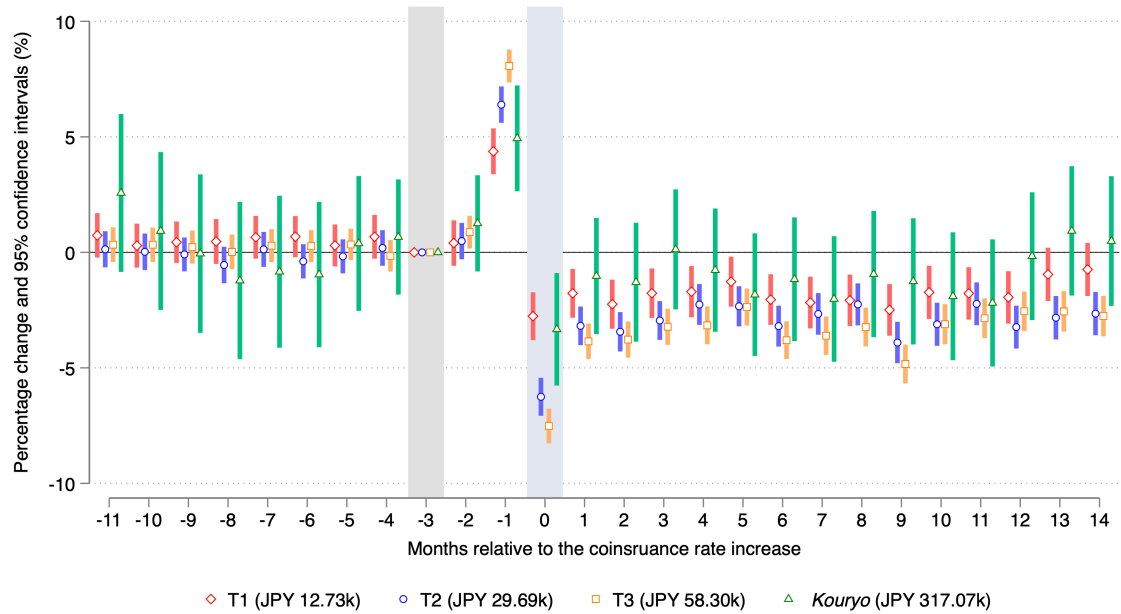
(b) Costs, log (intensive margin)

Figure 6: Heterogeneous Responses for Outpatient, Pharmaceutical, and Dental Care

Notes: Event study estimates for outpatient (red diamonds), pharmaceutical (blue circles), and dental (orange squares) services. Panel (a) shows utilization rates (extensive margin); Panel (b) shows log expenditures (intensive margin). The gray shaded region marks  $k = -3$  (July 2022, the omitted reference period, pre-notification); the light blue shaded region marks  $k = 0$  (October 2022, implementation); the unshaded region between them ( $k \in \{-2, -1\}$ ) constitutes the anticipation period. All models include individual, month, and municipality-by-month fixed effects. Standard errors are clustered at the individual level.



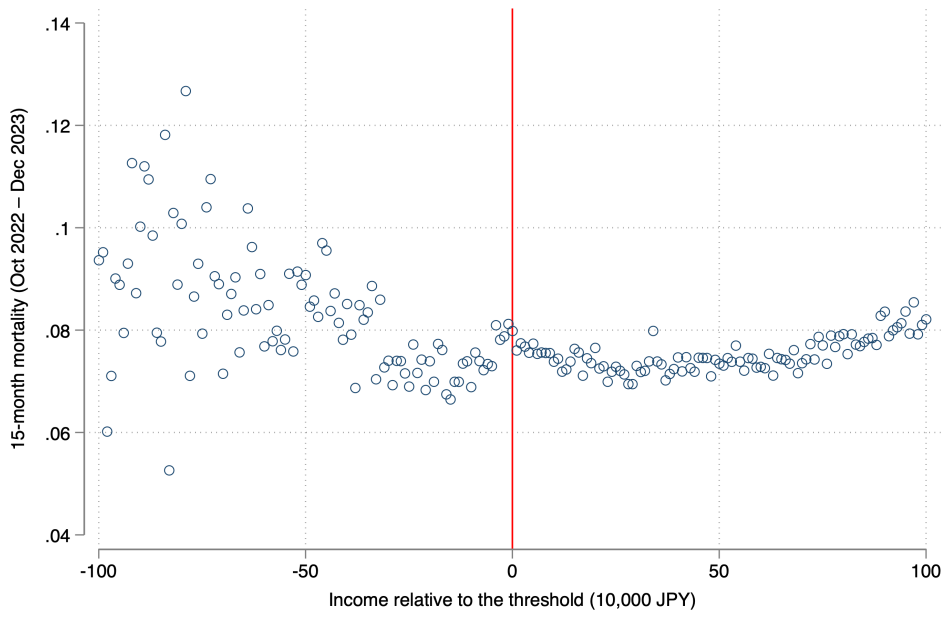
(a) Healthcare utilization rate (extensive margin) by pre-treatment utilization group



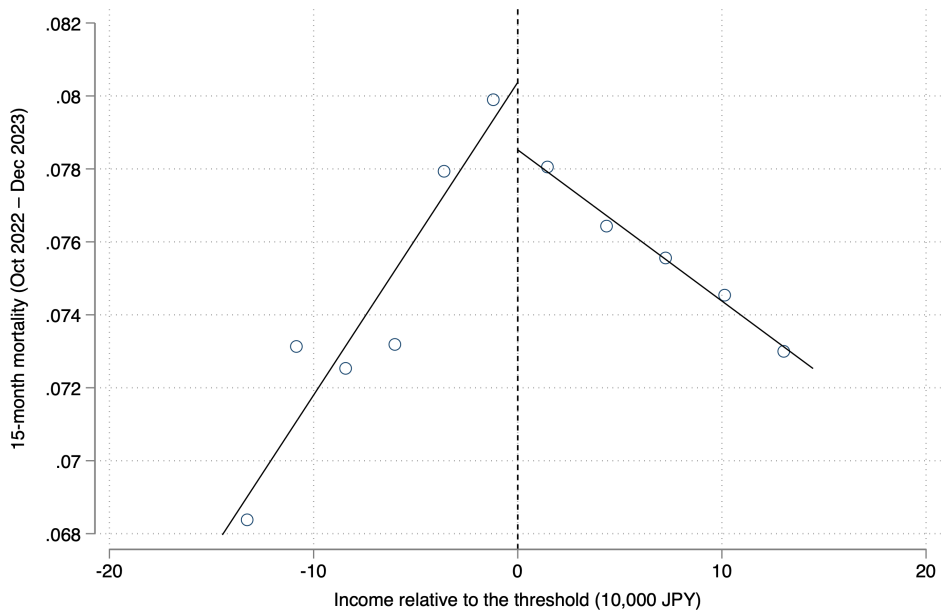
(b) Healthcare costs, log (intensive margin) by pre-treatment utilization group

Figure 7: Heterogeneous Responses by Medical Needs

Notes: Event study estimates by pre-treatment utilization group. Red diamonds: T1 (low utilization, mean expenditure JPY 12,730, baseline utilization rate 56.14%); blue circles: T2 (middle, JPY 29,690, 91.99%); orange squares: T3 (high, JPY 58,300, 98.57%); green triangles: *Kōryō* (High-Cost Medical Expense Benefit eligible, JPY 317,070, 92.86%). The gray shaded region marks  $k = -3$  (July 2022, the omitted reference period, pre-notification); the light blue shaded region marks  $k = 0$  (October 2022, implementation); the unshaded region between them ( $k \in \{-2, -1\}$ ) constitutes the anticipation period. Subsamples are constructed as follows: (1) observations with monthly medical expenditures  $\geq$  JPY 576,000 prior to  $k = -3$  (July 2022) are classified as *Kōryō*-eligible; (2) patients eligible at least once form the *Kōryō* group; (3) the remaining patients are divided into three tertiles (T1, T2, T3) based on average outpatient visit days prior to  $k = -3$ . All models include individual, month, and municipality-by-month fixed effects. Standard errors are clustered at the individual level.



(a) Normalized income and 15-month mortality (full sample)



(b) RDD estimate of 15-month cumulative mortality

Figure 8: Effects on 15-Month Cumulative Mortality

Notes: Panel (a) plots the relationship between normalized income and 15-month mortality (probability of sample attrition from  $k = 0$  to  $k = 14$ , October 2022 to December 2023) across the full income distribution; this descriptive plot is not restricted to the RDD bandwidth. Panel (b) presents the regression discontinuity estimate within the optimal bandwidth selected by `rdrobust`. Dots represent binned averages; solid lines are local linear fits estimated separately on each side of the cutoff (dashed vertical line). Mortality is proxied by sample attrition, which arises primarily from death given that inter-prefectural migration among individuals aged 75+ is rare (approximately 0.3% annually; [Statistics Bureau of Japan 2023](#)) and transition to public assistance is unlikely near the income threshold. See Section E for details.

# **Anticipation and Adaptation: Cost-Sharing Responses and Mortality Among the Oldest-Old**

## **Online Appendix**

Rong Fu, Masato Oikawa, Akira Kawamura, Haruko Noguchi

## A Coinsurance Rate Determination Process

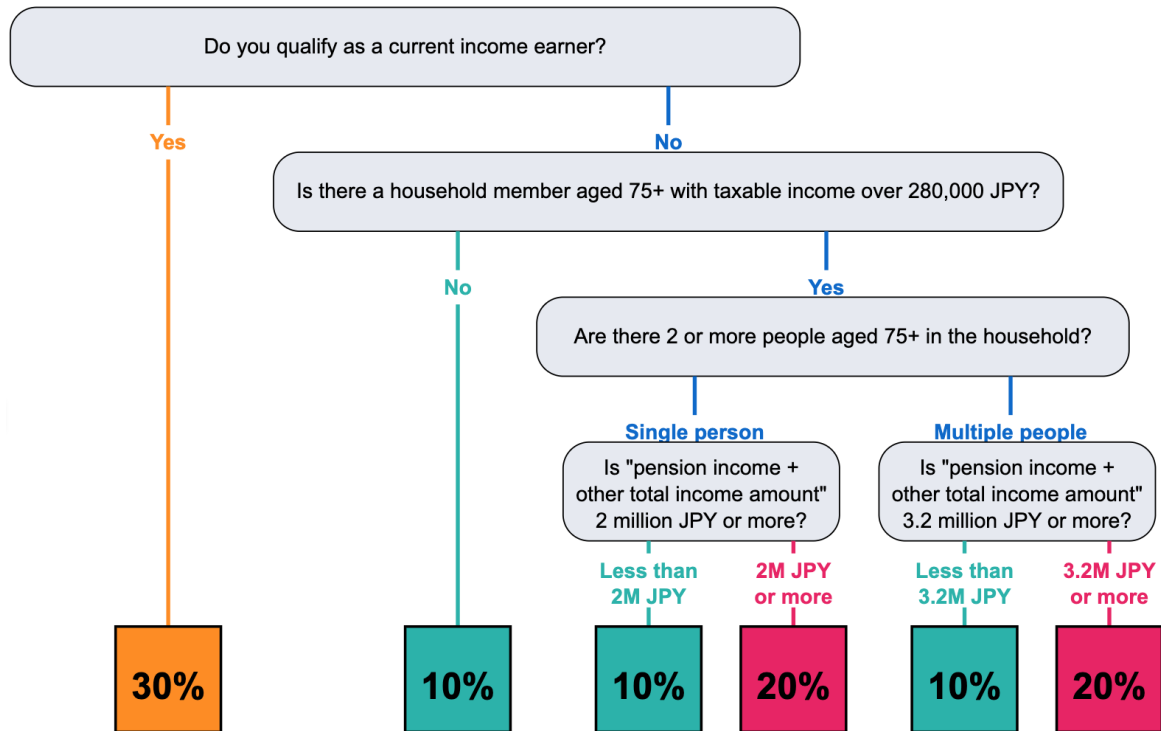


Figure A.1: Coinsurance Rate Determination Process for Population Aged 75+

*Notes:* This flowchart illustrates the decision process used to determine coinsurance rates under Japan’s Medical Care System for the Elderly in the Latter Stage of Life. The process begins by identifying current income earners (who pay 30%), then assesses household members aged 75+ with taxable income exceeding 280,000 JPY. For qualifying elderly individuals, rates depend on household composition (single vs. multiple) and total income thresholds (2 million JPY for single households, 3.2 million JPY for multiple-person households). Individuals exceeding these thresholds pay a 20% coinsurance rate, while those below pay 10%. The income calculation uses:  $AOI = SUMI + \max(ANSI, 0) - MIRPP$ , where SUMI aggregates business, agricultural, real estate, interest, dividend, miscellaneous, capital gains, and forest income; and ANSI combines net salary income with an adjusted income deduction amount.

## B Identification Validity

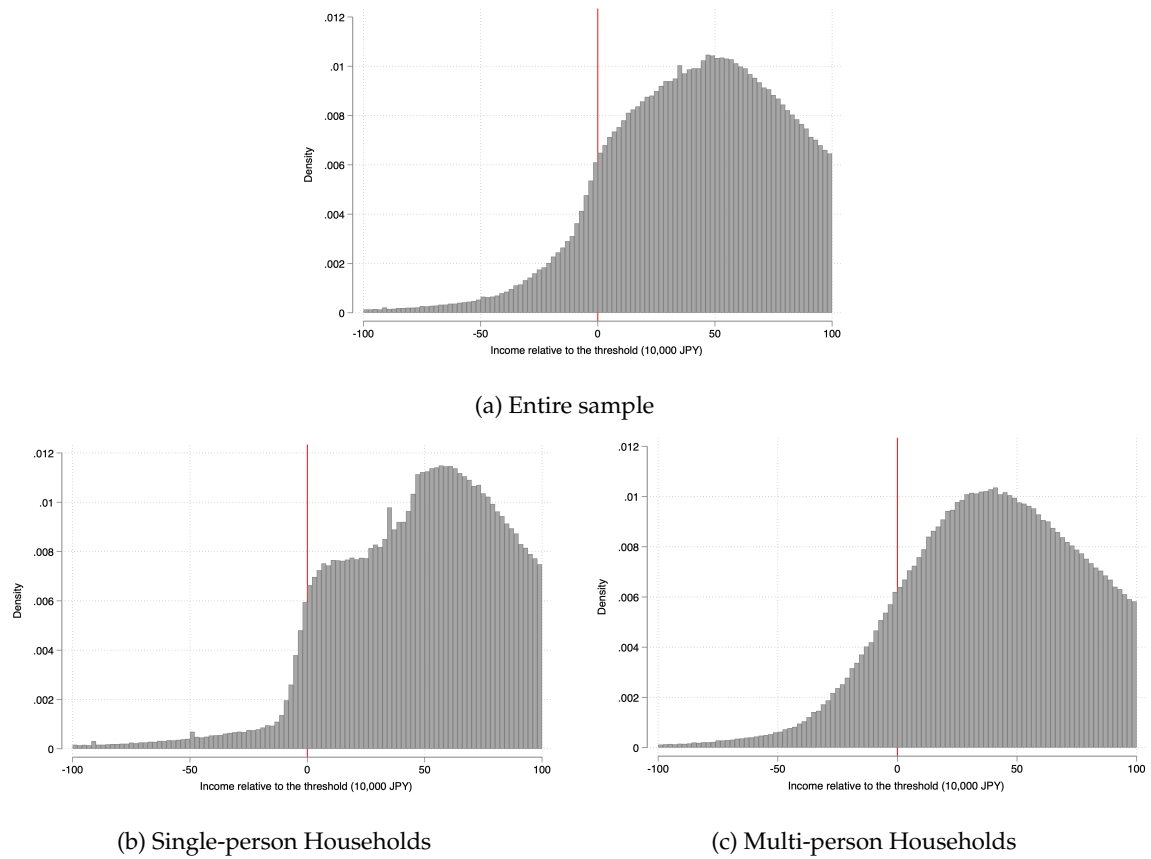


Figure B.1: Distribution of the Running Variable around the Income Threshold

*Notes:* This figure plots the distribution of normalized income around the cutoff (defined as zero). The three panels show the full sample, single-person households, and multi-person households, respectively. There is no evidence of bunching immediately below the threshold, consistent with the absence of income manipulation. The smooth density through the cutoff supports the validity of the regression discontinuity design.

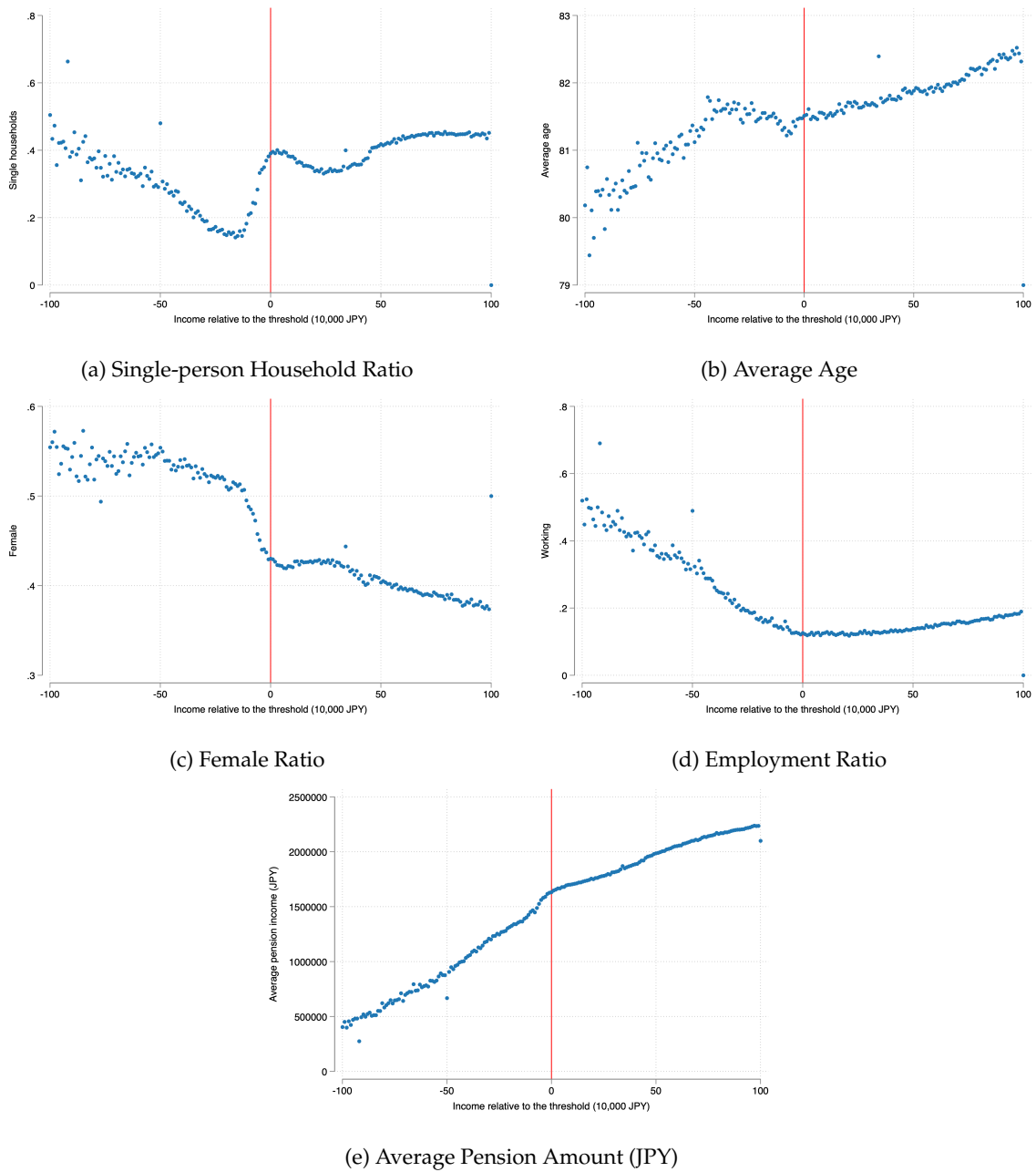


Figure B.2: Continuity of Observable Characteristics around the Cutoff

*Notes:* This figure plots the mean values of five observable characteristics against normalized income, with zero corresponding to the eligibility cutoff. Each dot represents a binned average. All five covariates—single-person household ratio, average age, female ratio, employment ratio, and average pension amount—display smooth distributions without detectable discontinuities at the cutoff, supporting quasi-random assignment near the threshold.

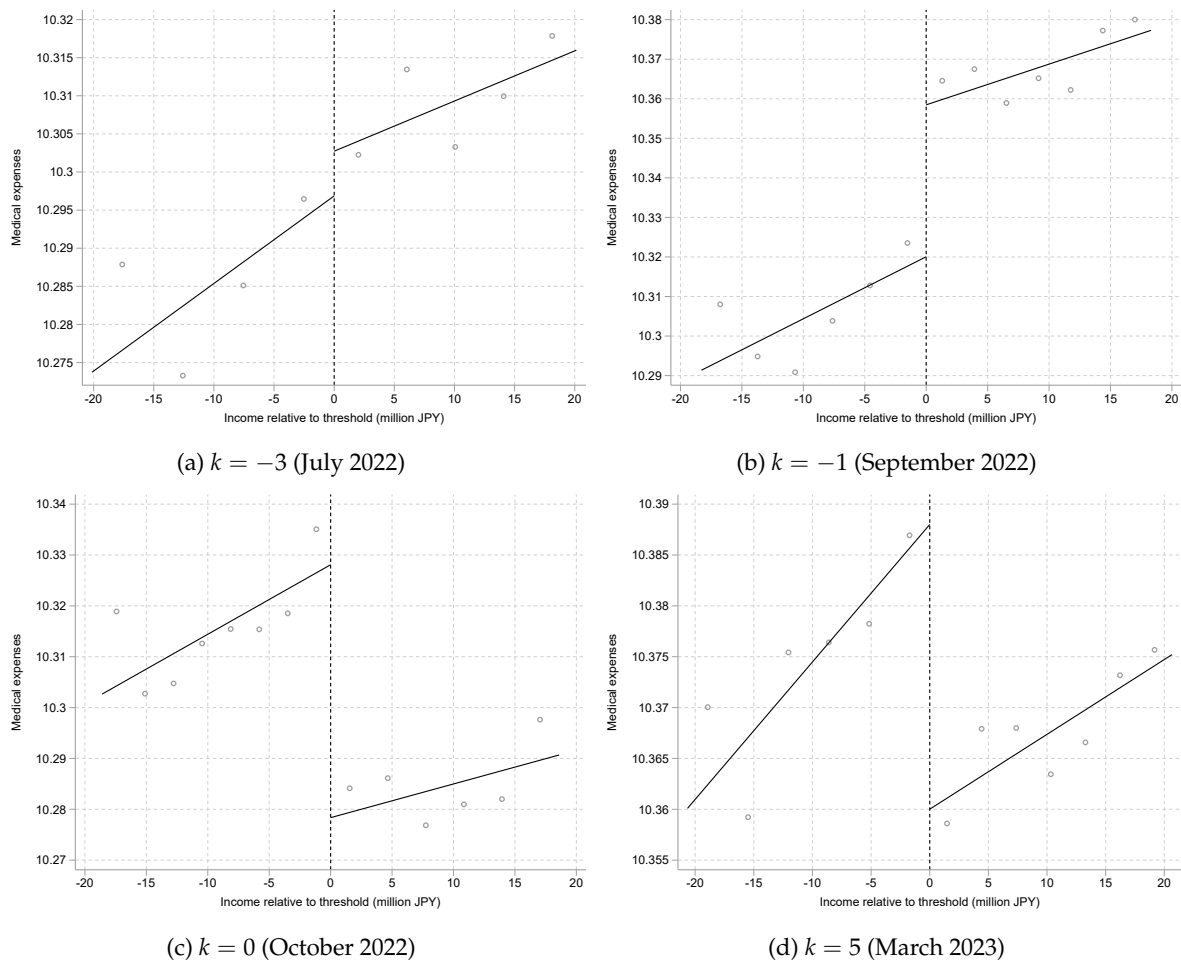


Figure B.3: Cross-Sectional RDD Estimates by Month: Total Healthcare Costs

*Notes:* Cross-sectional RDD estimates for total healthcare costs (log) in four key months. Each panel plots binned averages (dots) and local linear fits (solid lines) estimated separately on each side of the income threshold (dashed vertical line). The bandwidth is selected by `rdrobust` with a local linear specification. Estimated discontinuities:  $k = -3$  (July 2022, pre-notification baseline): +0.4% (statistically insignificant, consistent with no pre-existing difference);  $k = -1$  (September 2022, anticipation): +3.6% (last-minute surge);  $k = 0$  (October 2022, implementation): -5.0%;  $k = 5$  (March 2023, steady state): -3.0%. These estimates are free of the DD parallel-trends assumption and independently corroborate the event-study findings.

Table B.1: Cross-Sectional RDD Estimates by Month with Statistical Inference: Total Healthcare Costs

|                    | (1)<br>Jul. 2022 | (2)<br>Sep. 2022    | (3)<br>Oct. 2022     | (4)<br>Mar. 2023     |
|--------------------|------------------|---------------------|----------------------|----------------------|
| RD Estimates       | 0.004<br>(0.007) | 0.036***<br>(0.008) | -0.050***<br>(0.008) | -0.030***<br>(0.008) |
| Observations left  | 173,723          | 166,874             | 170,199              | 178,046              |
| Observations right | 358,905          | 334,212             | 326,769              | 370,826              |
| Bandwidth left     | 20.134           | 18.294              | 18.596               | 20.648               |
| Bandwidth right    | 20.134           | 18.294              | 18.596               | 20.648               |

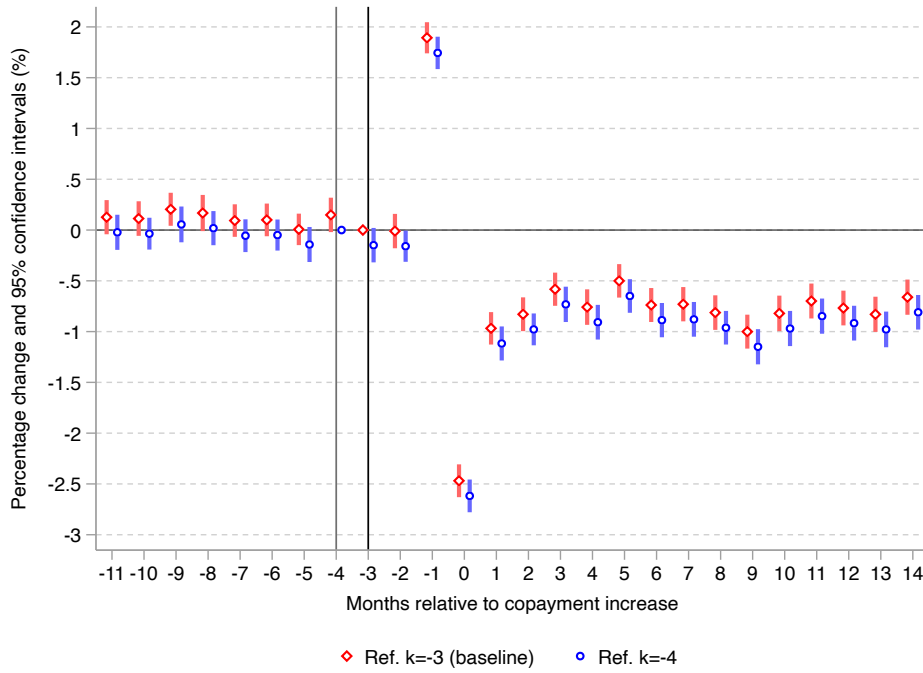
*Notes:* This table reports conventional local linear regression discontinuity estimates of the effect of the increase in the coinsurance rate on monthly medical expenditures. The running variable is income normalized relative to the applicable household-type-specific income threshold: 2 million JPY for single-person households and 3.2 million JPY for multi-person households. Individuals below the cutoff remained subject to the 10% coinsurance rate, while those above the cutoff became subject to the 20% coinsurance rate. The models are estimated separately on each side of the cutoff, with the optimal bandwidth selected by `rdrobust`. Robust bias-corrected standard errors are reported in parentheses. Medical expenditures are measured in Japanese yen and include total insurer-recorded healthcare spending during the corresponding month. Statistical significance: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

## C Robustness Checks

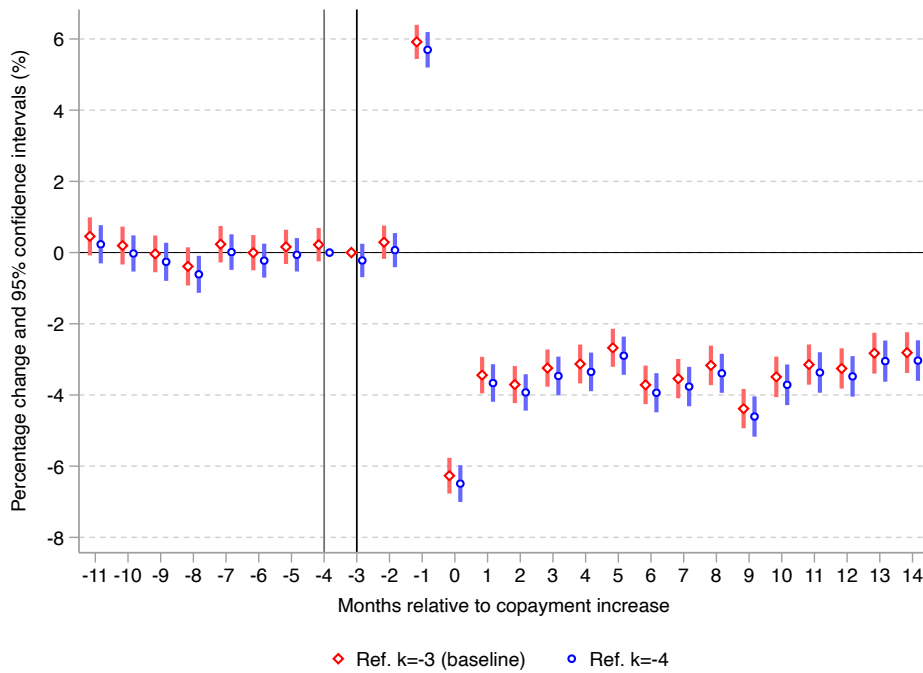
Table C.1: Regression Discontinuity Estimates for 15-Month Mortality

|                    | (1)               |
|--------------------|-------------------|
| RD Estimate        | -0.002<br>(0.002) |
| Observations left  | 176,788           |
| Observations right | 306,615           |
| Bandwidth left     | 14.468            |
| Bandwidth right    | 14.468            |

*Notes:* This table reports conventional local linear regression discontinuity estimates of the effect of the increase in the coinsurance rate on 15-month mortality, proxied by sample attrition from  $k = 0$  to  $k = 14$  (October 2022 to December 2023). The running variable is income normalized relative to the applicable household-type-specific income threshold: 2 million JPY for single-person households and 3.2 million JPY for multi-person households. Individuals below the cutoff remained subject to the 10% coinsurance rate, while those above the cutoff became subject to the 20% coinsurance rate. The models are estimated separately on each side of the cutoff, with the optimal bandwidth selected by `rdrobust`. Robust bias-corrected standard errors are reported in parentheses. Mortality is proxied by sample attrition, which arises primarily from death given that inter-prefectural migration among individuals aged 75+ is rare, approximately 0.3% annually; [Statistics Bureau of Japan 2023](#), and transition to public assistance is unlikely near the income threshold. See Section 3.4 for validation. Statistical significance: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .



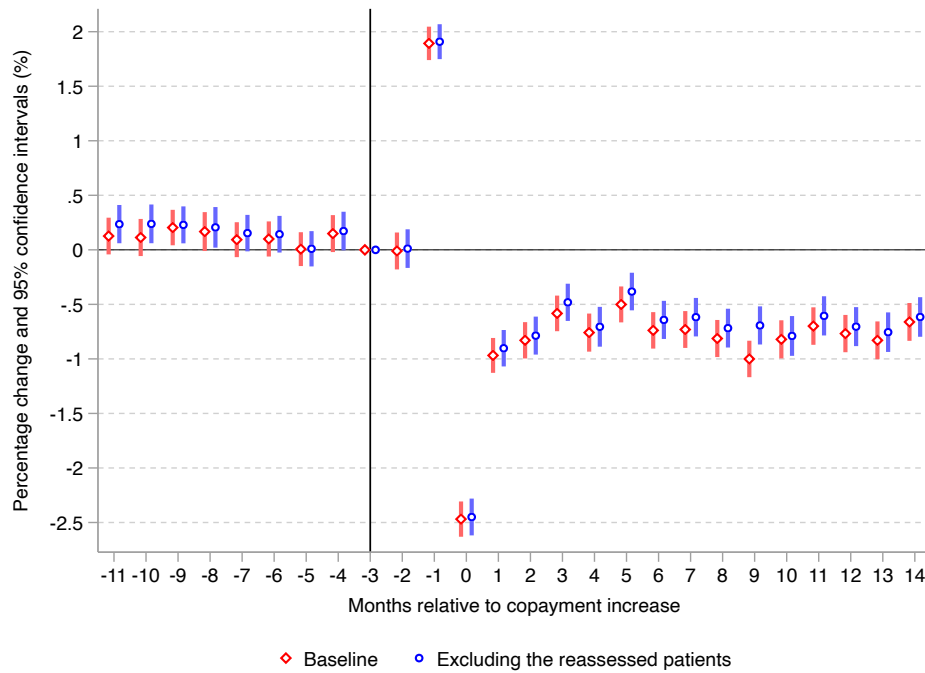
(a) Healthcare utilization rate (extensive margin)



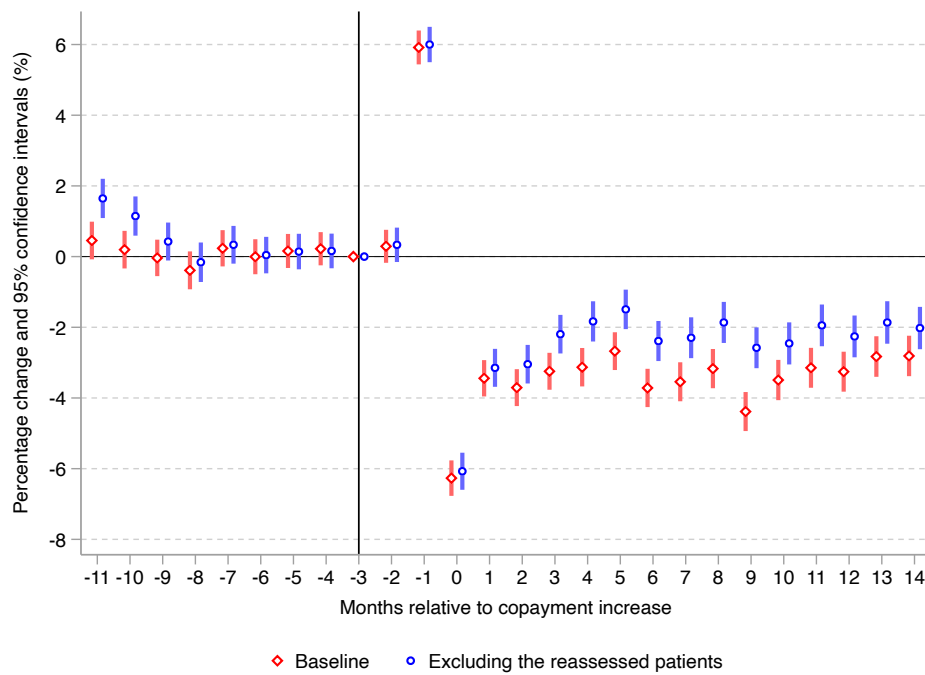
(b) Healthcare costs, log (intensive margin)

Figure C.1: Robustness: Alternative Reference Period

Notes: This figure compares the baseline event-study estimates, which use  $k = -3$  (July 2022) as the omitted reference period, with estimates that instead use  $k = -4$  (June 2022) as the omitted reference period. Panel (a) reports estimates for healthcare utilization rates (extensive margin), and Panel (b) reports estimates for log healthcare costs (intensive margin). The specification is otherwise identical to the main specification in equation (1), including individual, month, and municipality-by-month fixed effects. Estimates are expressed as percentage changes relative to the treatment group's pre-treatment mean for binary outcomes and as approximate percentage changes for log-transformed expenditure outcomes. Standard errors are clustered at the individual level. The similarity between the two series indicates that the main findings are not sensitive to the choice of omitted pre-notification reference month.



(a) Healthcare utilization rate (extensive margin)



(b) Healthcare costs, log (intensive margin)

Figure C.2: Robustness: Excluding Individuals with Post-Implementation Changes in Coinsurance Status

Notes: This figure compares the baseline event-study estimates with estimates that exclude individuals whose coinsurance status ever changed after the October 2022 implementation. Among individuals observed in July 2023, 16.85% experienced a change in coinsurance status at some point after implementation, with most first changes occurring in August 2023. Panel (a) reports estimates for healthcare utilization rates (extensive margin), and Panel (b) reports estimates for log healthcare costs (intensive margin). The specification is otherwise identical to the main specification in equation (1), including individual, month, and municipality-by-month fixed effects. Estimates are expressed as percentage changes relative to the treatment group's pre-treatment mean for binary outcomes and as approximate percentage changes for log-transformed expenditure outcomes. Standard errors are clustered at the individual level. The results remain similar after excluding these individuals, indicating that the main estimates are not driven by subsequent changes in coinsurance status.

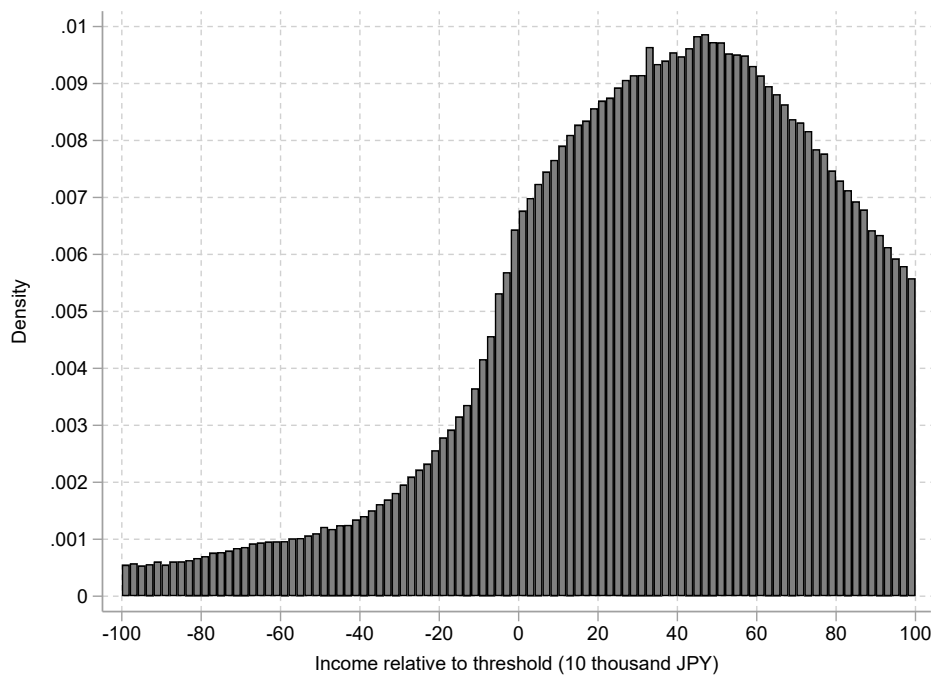
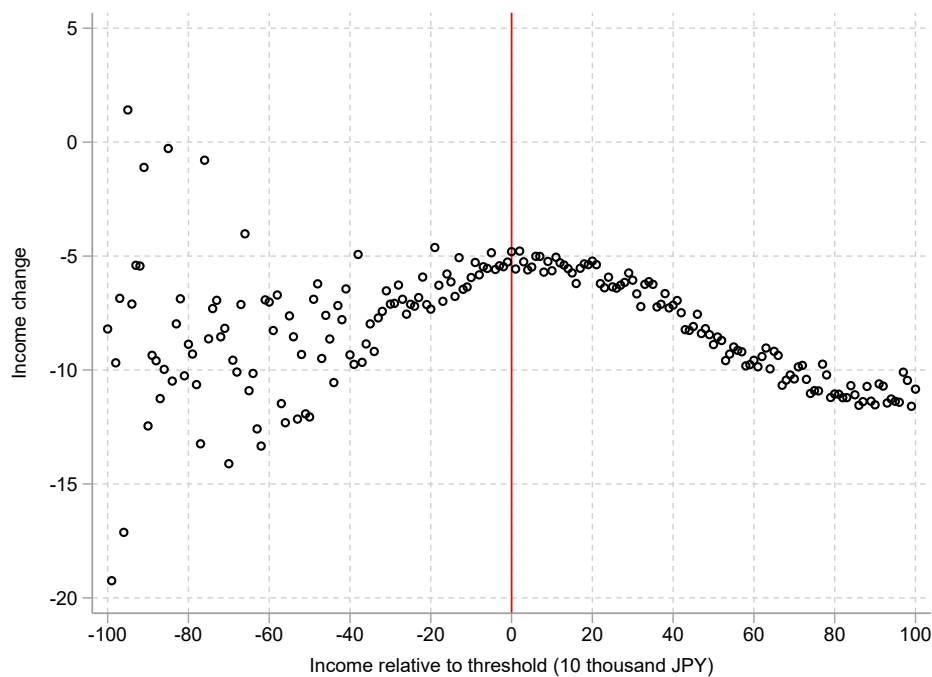
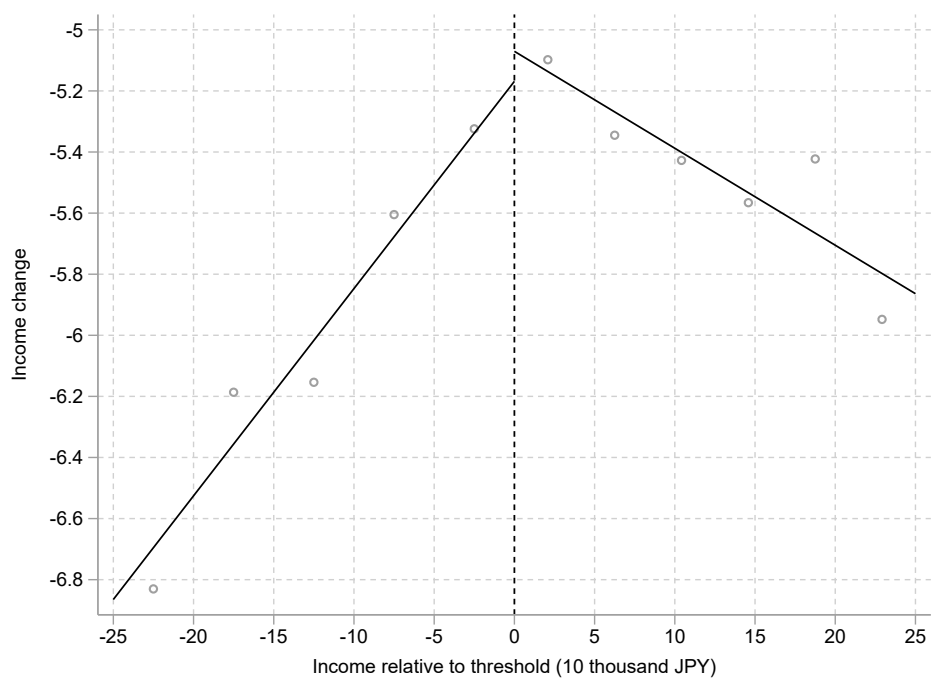


Figure C.3: Post-Period Income Distribution around the Threshold

*Notes:* This figure plots the distribution of normalized income in August 2023 around the applicable household-income threshold. Income is measured relative to the household-specific threshold and reported in units of 10,000 JPY. The sample is restricted to individuals with baseline normalized income within 2 million JPY of the threshold as of October 2022. If individuals adjusted income to avoid the higher coinsurance rate, bunching would be expected just below the threshold. The absence of visible bunching below the cutoff provides no evidence of selective post-implementation income manipulation around the threshold.



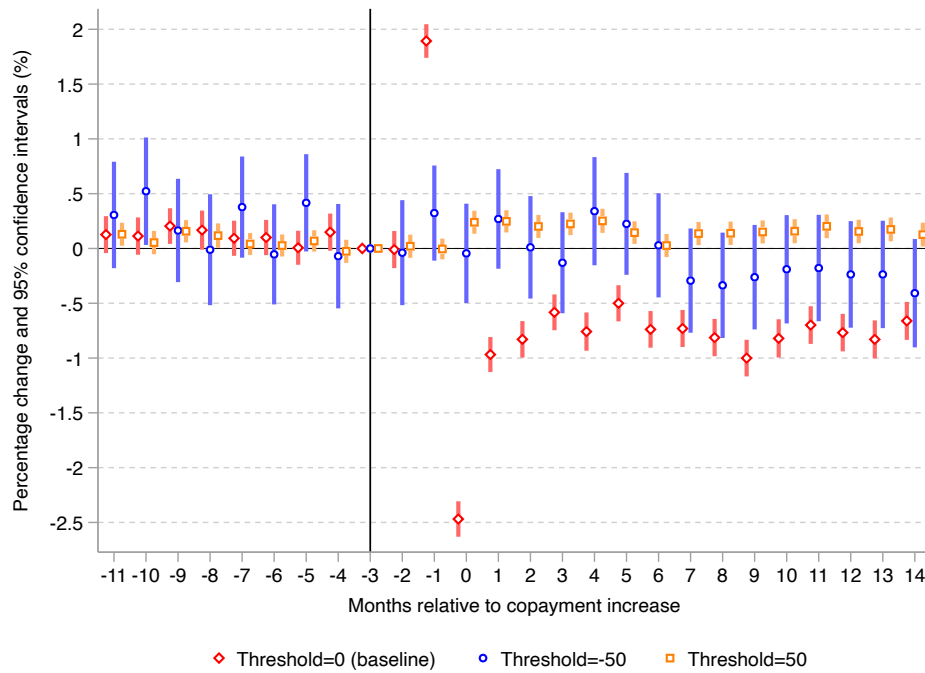
(a) Income change by baseline normalized income



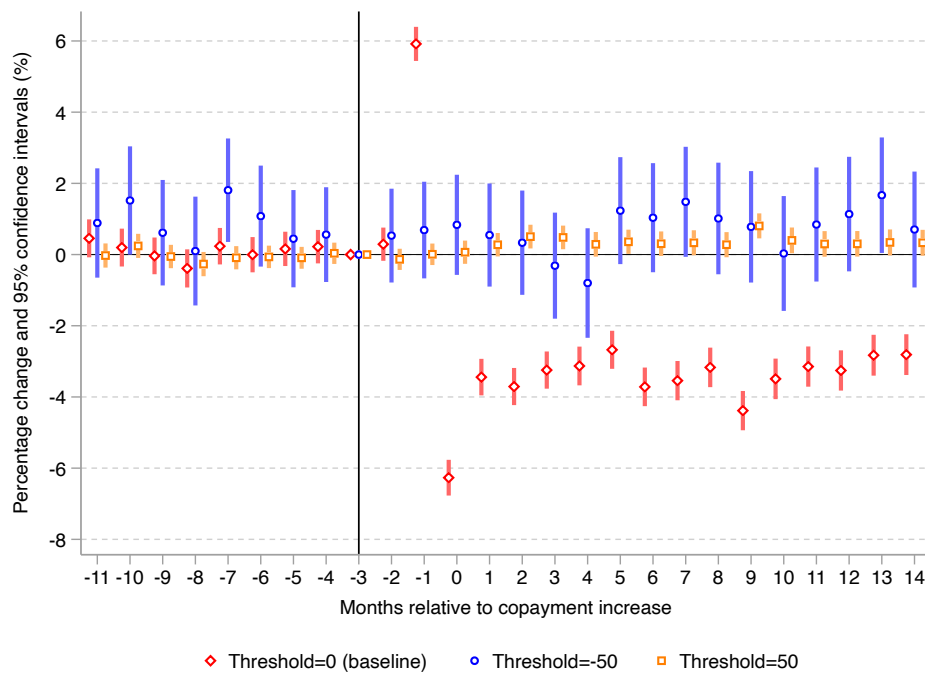
(b) RDD estimate for income change

Figure C.4: Post-Implementation Income Changes around the Threshold

*Notes:* This figure examines whether individuals near the income threshold selectively changed their income after the October 2022 implementation. The sample is restricted to individuals with baseline normalized income within 2 million JPY of the applicable household-income threshold as of October 2022. Panel (a) plots the change in normalized income between October 2022 and August 2023 against baseline normalized income. Panel (b) presents a regression discontinuity plot of income change at the baseline threshold, using baseline normalized income as the running variable. Income is measured relative to the applicable household-specific threshold and reported in units of 10,000 JPY. If individuals just above the cutoff selectively reduced their income after implementation to avoid the higher coinsurance rate, income changes would display a negative discontinuity at the cutoff. The absence of such a pattern provides no evidence of selective post-implementation income reduction among individuals just above the threshold.



(a) Healthcare utilization rate (extensive margin)



(b) Healthcare costs, log (intensive margin)

Figure C.5: Robustness: Placebo Income Cutoffs

*Notes:* This figure reports placebo cutoff checks using artificial income thresholds at which no policy-induced change in coinsurance rates occurred. The baseline estimates use the actual threshold normalized to zero and the main analytical sample around this cutoff. The placebo estimates use artificial thresholds located 0.5 million JPY below and above the actual cutoff. For the lower placebo cutoff, the sample is restricted to individuals with normalized income between  $-0.99$  and  $-0.01$  million JPY, and treatment is redefined as being above the placebo cutoff at  $-0.5$  million JPY. For the upper placebo cutoff, the sample is restricted to individuals with normalized income between  $0.01$  and  $0.99$  million JPY, and treatment is redefined as being above the placebo cutoff at  $0.5$  million JPY. These restrictions ensure that each placebo comparison is conducted within regions where the actual coinsurance rate does not change at the placebo threshold. Panel (a) reports estimates for healthcare utilization rates (extensive margin), and Panel (b) reports estimates for log healthcare costs (intensive margin). Each placebo specification preserves the same event-study structure as in equation (1). All models include individual, month, and municipality-by-month fixed effects. Standard errors are clustered at the individual level. The absence of systematic effects at the placebo cutoffs supports the interpretation that the baseline estimates are driven by the actual policy-induced discontinuity in coinsurance rates rather than by smooth income-related trends.

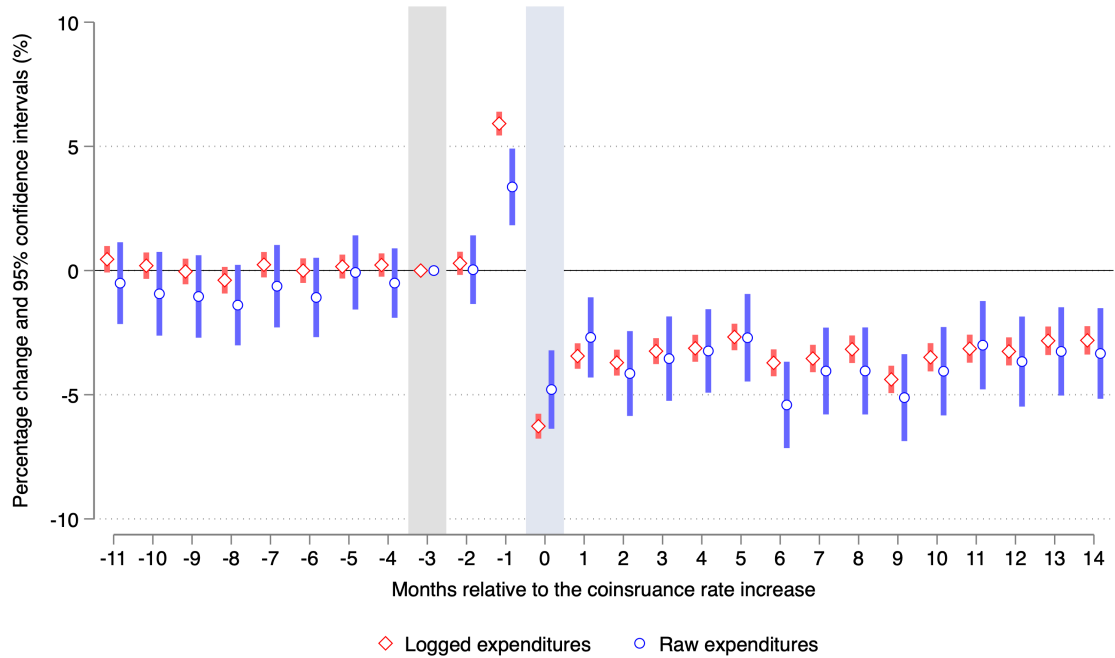


Figure C.6: Robustness: Log Specification vs. Raw Level Values

Notes: Comparison of event study estimates from the log specification (red diamonds) and raw-level specification expressed as percentage changes relative to the pre-treatment mean (blue circles). The gray shaded region marks  $k = -3$  (July 2022, the omitted reference period, pre-notification); the light blue shaded region marks  $k = 0$  (October 2022, implementation); the unshaded region between them ( $k \in \{-2, -1\}$ ) constitutes the anticipation period. Both specifications produce consistent patterns. The log specification yields tighter confidence intervals; the raw-level specification reveals a slightly negative pre-trend that the log transformation eliminates, supporting the log specification as the preferred approach.

## D Out-of-Pocket Cost Analysis

This appendix documents two complementary dimensions of the reform’s financial impact on patients. First, it quantifies how the temporary outpatient relief measure attenuated the effective price increase faced by patients. Second, it examines the temporal pattern of out-of-pocket burden relative to patients’ baseline income.

### D.1 Actual Price Increase after the Outpatient Relief Measure

The nominal coinsurance rate increase from 10% to 20% implies a 100% increase in the patient price for a given level of healthcare utilization. For outpatient care, however, the reform included a temporary relief measure that capped the monthly increase in out-of-pocket payments at 3,000 JPY. This safeguard implies that the effective increase in outpatient cost-sharing was smaller than the nominal 10-percentage-point increase for patients with sufficiently high monthly outpatient spending.

To quantify the magnitude of this attenuation, we use pre-policy outpatient utilization among individuals in the treatment group. Abstracting from the high-cost medical expense benefit, we define the nominal monthly increase in outpatient out-of-pocket payments as

$$\Delta_{it}^{\text{nom}} = 0.1 \times M_{it}^{\text{out}},$$

where ( $M_{it}^{\text{out}}$ ) denotes total outpatient medical expenditure for individual (i) in month (t). The actual monthly increase after the relief measure is

$$\Delta_{it}^{\text{act}} = \min \Delta_{it}^{\text{nom}}, 3000.$$

Because the cap operates at the person-month level, we apply this calculation before aggregating across months or individuals.

Table D.1 summarizes the implied attenuation of the outpatient price increase. Among treated outpatient-months in the pre-policy period, the 3,000 JPY cap would have been binding in 20.6% of months. At the individual level, 53.0% of treated outpatient users would have experienced a binding cap at least once. The effective monthly price increase, measured as the ratio of average actual to nominal increases, was 61.0% of the nominal increase. The distribution, however, is highly skewed: the median individual-level actual-to-nominal ratio was 98.2%. This contrast indicates that the cap had little effect for the median patient but substantially attenuated the price increase for high-utilization patients in the upper tail of outpatient spending.

Table D.1: Outpatient Relief Cap and Effective Price Increase

| Measure  | Estimate | Interpretation                                      |
|--|----------|---|
| Binding cap among treated outpatient-months      | 20.6%    | Share of treated months with outpatient use         |
| Treated outpatient users ever facing binding cap | 53.0%    | Share of treated individuals with outpatient use    |
| Mean actual-to-nominal ratio                     | 61.0%    | Ratio of average monthly actual to nominal increase |
| Median individual actual-to-nominal ratio        | 98.2%    | Median of individual-level actual-to-nominal ratios |

*Notes:* This table summarizes the implied effect of the outpatient transitional relief measure among treated individuals in the pre-policy period. The sample is restricted to individuals within the ( $\pm$ )500,000 JPY income bandwidth around the applicable household-income threshold and to months before August 2022. The nominal monthly increase is calculated as 10% of total outpatient medical expenditure. The actual monthly increase applies the statutory 3,000 JPY cap at the person-month level. The calculations abstract from the high-cost medical expense benefit. The mean actual-to-nominal ratio is computed as the ratio of average monthly actual to nominal increases. The median actual-to-nominal ratio is computed from individual-level ratios.

These results imply that elasticities based on the nominal doubling of the coinsurance rate are conservative in absolute value for outpatient care, because the effective outpatient price increase was smaller than the full nominal increase. At the same time, the attenuation does not overturn the basic interpretation of the estimates. Even after the relief measure, the effective outpatient price increase remained substantial on an aggregate amount basis. Thus, the reduced-form estimates should be interpreted as responses to the policy-induced effective cost-sharing increase, inclusive of the outpatient relief measure, rather than as responses to an unconstrained full doubling of outpatient out-of-pocket prices.

## D.2 Out-of-Pocket Burden Relative to Income

Figure D.1 presents event-study estimates for out-of-pocket spending as a share of baseline monthly income, using the same difference-in-differences specification as the main analysis. This exercise complements the mechanical calculations above by showing how patients' realized out-of-pocket burden evolved around implementation.

Three features of the figure are notable. First, prior to notification ( $k \leq -3$ ), the differential in out-of-pocket burden between treatment and control groups is close to zero, consistent with the parallel-trends assumption underlying the main analysis and suggesting no pre-existing differential trend in financial exposure.

Second, at implementation ( $k = 0$ ), out-of-pocket burden rises sharply by approximately 57–60% relative to baseline monthly income. This increase is smaller than a literal doubling of out-of-pocket payments, consistent with the outpatient relief cap and other institutional safeguards, but it remains large. The result confirms that the reform generated an immediate and economically meaningful

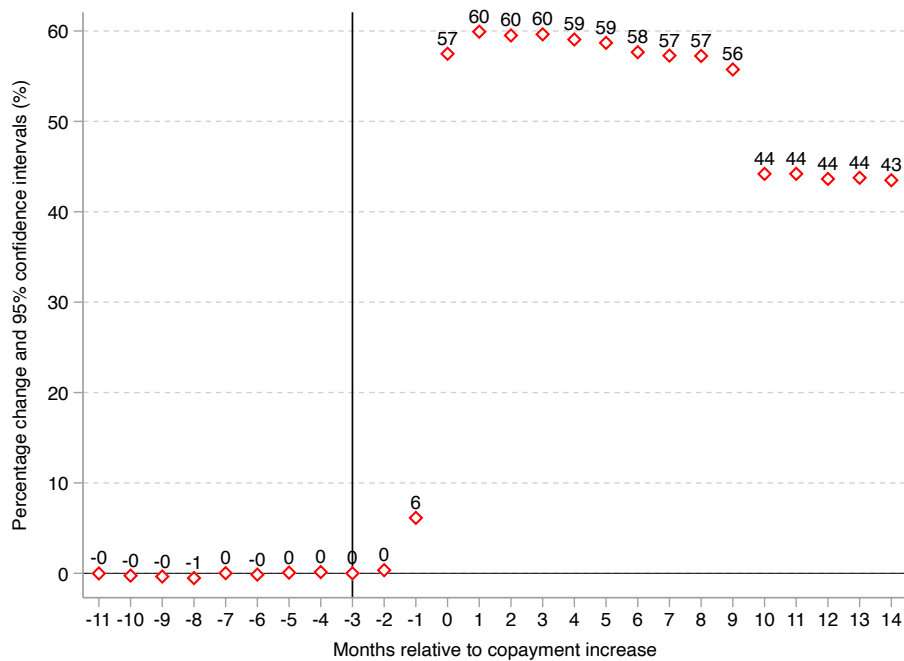


Figure D.1: Effect on Out-of-Pocket Burden Relative to Baseline Income

Notes: This figure presents event-study estimates for out-of-pocket medical spending relative to baseline monthly income. The outcome is  $\log(\text{out-of-pocket spending}/\text{baseline monthly income})$ , where baseline monthly income equals annual household income at  $(k = -3)$  divided by 12. Observations with zero out-of-pocket spending are excluded. The solid black vertical line marks  $(k = -3)$  (July 2022, the omitted reference period), and  $(k = 0)$  denotes October 2022 (implementation). Estimates are approximate percentage changes. All models include individual, month, and municipality-by-month fixed effects. Standard errors are clustered at the individual level.

increase in patients' financial exposure.

Third, the burden attenuates modestly over the post-implementation period, stabilizing at approximately 44–56% above baseline in later months. This partial attenuation is mechanically consistent with the utilization reductions documented in the main analysis: as patients reduce healthcare use in response to higher cost sharing, realized out-of-pocket spending falls relative to what would have occurred under unchanged utilization. The fact that the burden remains substantially elevated throughout the post-implementation period supports the interpretation of steady-state utilization reductions as responses to a persistent increase in effective patient prices rather than as a purely transitory adjustment.

## E Validation of Sample Attrition as a Mortality Proxy

This appendix provides detailed evidence supporting our use of sample attrition as a proxy for mortality (Section 3.4). We present four complementary strands of evidence: (i) a comparison of attrition counts against official prefecture-level death records, (ii) healthcare utilization trajectories in the months preceding attrition, and (iii) disease composition among those who subsequently attrit.

### E.1 Comparison with Official Death Records

Table E.1 compares, for calendar year 2022, the number of individuals who permanently exit our dataset against official national death counts by age group and sex published by the Ministry of Health, Labour and Welfare.<sup>17</sup> The LSEH system covers approximately 98.6% of Japan’s population aged 75 and above, so in the absence of non-mortality attrition one would expect our counts to equal roughly 99% of the official figures.

Table E.1: Comparison of Dataset Attrition with Official Death Counts, 2022

| Age group        | Official Deaths (A) |                |                | Our Attrition (B) |                |                | Ratio B/A    |
|------------------|---------------------|----------------|----------------|-------------------|----------------|----------------|--------------|
|                  | Total               | Male           | Female         | Total             | Male           | Female         |              |
| 75–79            | 170,192             | 109,955        | 60,237         | 163,278           | 96,855         | 66,423         | 0.959        |
| 80–84            | 247,487             | 144,728        | 102,759        | 198,206           | 113,000        | 85,206         | 0.801        |
| 85–89            | 320,647             | 159,604        | 161,043        | 248,737           | 122,464        | 126,273        | 0.776        |
| 90–94            | 295,883             | 110,745        | 185,138        | 224,817           | 83,694         | 141,123        | 0.760        |
| 95–99            | 155,165             | 38,692         | 116,473        | 115,609           | 28,799         | 86,810         | 0.745        |
| 100+             | 40,183              | 5,608          | 34,575         | 29,113            | 4,157          | 24,956         | 0.725        |
| <b>Total 75+</b> | <b>1,229,557</b>    | <b>569,332</b> | <b>660,225</b> | <b>979,760</b>    | <b>448,969</b> | <b>530,791</b> | <b>0.797</b> |

Notes: “Official Deaths” are from Japan’s *Vital Statistics 2022* (Vital Statistics of Japan 2022), restricted to deaths of individuals aged 75 and above. “Our Attrition” counts individuals who permanently exit the MCD-Tx dataset during 2022. As described in the text, new entrants at age 75 are excluded from the attrition count for the 75–79 group because their first appearance in the data may reflect relocation rather than entry into LSEH. The ratio B/A ranges from 0.725 to 0.959 across age groups, consistent with LSEH covering approximately 98.6% of the 75+ population and a small share of exits reflecting non-mortality attrition.

Several patterns emerge. First, across all age groups, our attrition counts are a substantial fraction of the official death figures—ranging from 80% to 96%—consistent with attrition being driven primarily by mortality rather than administrative exit. Second, the ratio of our count to the official count is highest for the 75–79 group (overall ratio 0.96), and then stabilizes in the 0.72–0.80 range for older cohorts. The somewhat lower ratios at older ages likely reflect two factors: (a) individuals aged 80 and above who transitioned from regular health insurance into LSEH at age 75 but subsequently shifted to public assistance (*seikatsu hogo*), a non-mortality exit route that is rare but not zero at older

<sup>17</sup>Official counts are drawn from the *Vital Statistics of Japan 2022* (Vital Statistics of Japan 2022). The sample is not restricted to the analytical bandwidth. Note that the 75–79 age group is not directly comparable: individuals first appearing in the dataset at age 75 cannot be distinguished from those who relocated into a new prefecture at that age, so we exclude new entrants at age 75 from the attrition count for this group (see Section E.4 for details on the relocation argument). The remaining five age groups are unaffected by this issue.

ages; and (b) inter-prefectural moves accompanied by a change in individual identifiers, which are more common in the 75–79 group precisely because that group contains new LSEH entrants for whom no prior within-dataset trajectory is observed. Third, male and female ratios track closely within each age group, indicating no systematic sex-specific bias in the attrition measure. Taken together, the high and consistent ratios confirm that sample attrition in our dataset is a valid, though not perfect, proxy for mortality.

## E.2 Healthcare Utilization Trajectories Prior to Attrition

Tables E.2 and E.3 report the mean utilization of individuals in the months leading up to their final appearance in the dataset, indexed by months relative to the exit month ( $attr = 0$ ). The sample is not restricted to the analytical bandwidth.

**Inpatient care.** Table E.2 reports the probability of any inpatient admission (extensive margin) and the mean number of inpatient days (intensive margin) for months  $attr = -11$  through  $attr = 0$ .

Table E.2: Inpatient Utilization Trajectories Prior to Sample Exit

| Months before exit<br>( $attr$ ) | Inpatient probability<br>(extensive margin) | Mean inpatient days<br>(intensive margin) |
|----------------------------------|---|---|
| -11                              | 0.125                                       | 2.70                                      |
| -10                              | 0.130                                       | 2.85                                      |
| -9                               | 0.133                                       | 2.95                                      |
| -8                               | 0.138                                       | 3.10                                      |
| -7                               | 0.143                                       | 3.25                                      |
| -6                               | 0.152                                       | 3.50                                      |
| -5                               | 0.163                                       | 4.25                                      |
| -4                               | 0.183                                       | 4.70                                      |
| -3                               | 0.208                                       | 5.40                                      |
| -2                               | 0.262                                       | 6.25                                      |
| -1                               | 0.333                                       | 7.90                                      |
| 0                                | 0.422                                       | 9.80                                      |
| Change (0 vs. -11)               | +29.7 pp                                    | +7.10 days                                |

*Notes:* Means are computed across all individuals who subsequently exit the dataset (not restricted to the analytical bandwidth), indexed by months relative to their final observed month ( $attr = 0$ ). Both the probability of any inpatient admission and mean inpatient days increase monotonically and accelerate in the final three months, consistent with end-of-life hospitalization. “pp” denotes percentage points.

Both series increase monotonically and accelerate markedly in the final months. The probability of hospitalization rises from 12.5% eleven months before attrition to 42.2% in the final month—a 3.4-fold increase. The steepest acceleration occurs in the last four months: admission probability roughly doubles from 20.8% at  $attr = -3$  to 42.2% at  $attr = 0$ . Mean inpatient days follow a similar pattern, rising from 2.70 days at  $attr = -11$  to 9.80 days at  $attr = 0$ , with the majority of the increase concentrated in the final three months. This accelerating end-of-life escalation in inpatient

care is inconsistent with administrative relocation—which would generate no systematic change in utilization before exit—and is precisely the pattern expected as individuals enter terminal inpatient care.

**Outpatient care.** Table E.3 presents analogous trajectories for outpatient services. The outpatient visit probability declines monotonically from 77.5% at  $attr = -11$  to 65.0% at  $attr = 0$ , with the pace of decline accelerating in the final two months. This pattern is consistent with a progressive shift from ambulatory outpatient care toward inpatient hospitalization as health deteriorates: individuals who are admitted to hospital cease attending outpatient clinics, generating a mechanical negative correlation between the two series. The sharp drop in outpatient probability from 71.0% at  $attr = -1$  to 65.0% at  $attr = 0$  mirrors precisely the sharp jump in inpatient admission probability (33.3% to 42.2%) over the same interval, consistent with a transfer of care setting.

Table E.3: Outpatient Utilization Trajectories Prior to Sample Exit

| Months before exit<br>( <i>attr</i> ) | Outpatient probability<br>(extensive margin) | Mean outpatient days<br>(intensive margin) |
|---------------------------------------|--|--|
| -11                                   | 0.775  | 2.167                                      |
| -10                                   | 0.775  | 2.188                                      |
| -9                                    | 0.770  | 2.150                                      |
| -8                                    | 0.766  | 2.083                                      |
| -7                                    | 0.763  | 2.110                                      |
| -6                                    | 0.760  | 2.128                                      |
| -5                                    | 0.756  | 2.130                                      |
| -4                                    | 0.750  | 2.145                                      |
| -3                                    | 0.745  | 2.148                                      |
| -2                                    | 0.735  | 2.155                                      |
| -1                                    | 0.710  | 2.163                                      |
| 0                                     | 0.650  | 2.210                                      |
| Change (0 vs. -11)                    | -12.5 pp                                     | +0.043 days                                |

*Notes:* Means are computed across all individuals who subsequently exit the dataset, indexed by months relative to their final observed month ( $attr = 0$ ). The outpatient visit probability declines monotonically, consistent with a progressive shift from ambulatory care to inpatient hospitalization. Mean outpatient days exhibit non-monotone variation (range: 2.08–2.21), reflecting competing forces: the mechanical reduction from hospitalization and increased specialist outpatient contact among those not yet admitted. The final-month drop in outpatient probability (from 0.710 to 0.650) coincides with the final-month surge in inpatient probability (from 0.333 to 0.422) shown in Table E.2. “pp” denotes percentage points.

Mean outpatient days exhibit a less smooth trajectory, reflecting the competing forces of declining ambulatory contact and increased specialist outpatient visits among those not yet admitted. The small variation in mean outpatient days (ranging from 2.08 to 2.21 across the 12-month window) relative to the large variation in mean inpatient days (2.70 to 9.80) underscores that it is the inpatient margin that dominates the end-of-life care transition.

### E.3 Disease-Specific Trajectories Prior to Attrition

Table E.4 traces disease-specific utilization trajectories for the three leading causes of death among the Japanese elderly: cancer/neoplasms, circulatory diseases, and respiratory diseases. For each disease, we report mean inpatient days and outpatient visit probability by months prior to exit.

Table E.4: Disease-Specific Utilization Trajectories Prior to Sample Exit

| <i>attr</i> | Cancer    |            | Circulatory |            | Respiratory |            |
|-------------|-----------|------------|-------------|------------|-------------|------------|
|             | Inp. days | Out. prob. | Inp. days   | Out. prob. | Inp. days   | Out. prob. |
| -11         | 0.175     | 0.050      | 0.650       | 0.410      | 0.200       | 0.037      |
| -10         | 0.185     | 0.053      | 0.680       | 0.415      | 0.218       | 0.038      |
| -9          | 0.195     | 0.058      | 0.720       | 0.407      | 0.240       | 0.038      |
| -8          | 0.210     | 0.063      | 0.760       | 0.403      | 0.265       | 0.038      |
| -7          | 0.230     | 0.068      | 0.820       | 0.408      | 0.295       | 0.039      |
| -6          | 0.270     | 0.073      | 0.900       | 0.410      | 0.340       | 0.040      |
| -5          | 0.330     | 0.080      | 1.000       | 0.412      | 0.400       | 0.041      |
| -4          | 0.430     | 0.093      | 1.160       | 0.415      | 0.500       | 0.042      |
| -3          | 0.600     | 0.105      | 1.350       | 0.420      | 0.680       | 0.043      |
| -2          | 0.830     | 0.118      | 1.620       | 0.430      | 0.900       | 0.044      |
| -1          | 1.100     | 0.138      | 1.920       | 0.448      | 1.120       | 0.047      |
| 0           | 1.430     | 0.158      | 2.140       | 0.450      | 1.350       | 0.051      |
| Change      | +1.255    | +0.108     | +1.490      | +0.040     | +1.150      | +0.014     |

*Notes:* “Inp. days” denotes mean days of inpatient care for the indicated disease category. “Out. prob.” denotes the probability of any outpatient visit with the indicated primary diagnosis. Values are read from graphical output and are approximate to two decimal places. All three disease categories show monotonically increasing inpatient intensity approaching exit, confirming end-of-life disease escalation across the leading causes of elderly mortality in Japan (Ministry of Health, Labour and Welfare, 2022).

All three disease categories exhibit accelerating inpatient intensity in the approach to exit, confirming that the aggregate pattern in Table E.2 reflects genuine end-of-life disease progression across multiple major diagnostic categories. Cancer inpatient days rise from 0.175 eleven months before attrition to 1.430 in the final month—an 8.2-fold increase. Respiratory disease days rise most steeply in absolute terms in the final three months (from 0.680 to 1.350 days), consistent with acute respiratory failure as a proximate cause of death. Circulatory disease inpatient days rise from 0.650 to 2.140 days, with the steepest acceleration in the final four months.

The outpatient trajectories differ by disease. Cancer and respiratory outpatient probabilities rise monotonically toward exit, reflecting increasing specialist contact for disease management prior to terminal admission. Circulatory disease outpatient probability is more variable, exhibiting a slight U-shape before rising sharply in the final two months, consistent with the heterogeneity of circulatory conditions (ranging from acute cardiac events to chronic heart failure management).

These disease-specific patterns are what one would expect if sample attrition reflects mortality from the leading causes of elderly death in Japan, providing direct diagnostic confirmation of the mortality interpretation.

## F Hospital Admission Probabilities by Selected Diagnosis Group

To assess whether the aggregate inpatient decline documented in Section 5.3 is concentrated in diagnoses with greater scope for scheduling or admission-margin adjustments, we classify selected principal inpatient diagnoses into three groups: non-deferrable acute-sensitive diagnoses (ischemic heart disease, subarachnoid hemorrhage, intracerebral hemorrhage, and cerebral infarction), urgent but margin-sensitive diagnoses (pneumonia and fracture), and elective or deferrable proxies (cataract and arthrosis).<sup>18</sup>

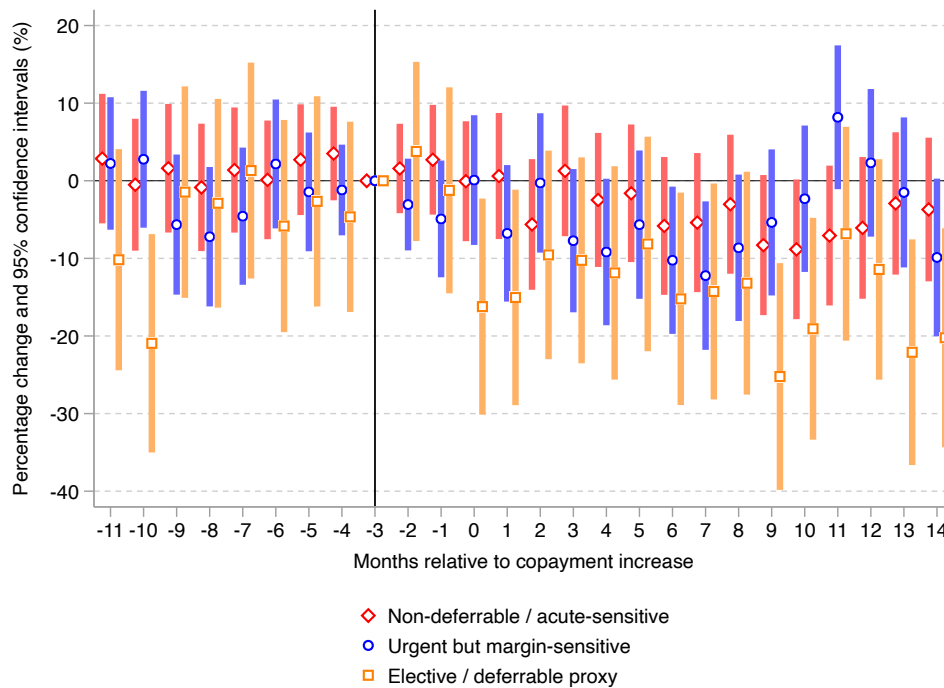


Figure F.1: Effects on Hospital Admissions by Selected Diagnosis Group

*Notes:* This figure presents event-study estimates for inpatient admission probabilities by selected principal diagnosis groups. The outcomes are binary indicators for whether an individual had an inpatient admission in the corresponding diagnosis group in a given month. Estimates are expressed as percentages of the category-specific pre-treatment mean admission probability. For each diagnosis group, event-study coefficients and confidence intervals are divided by the corresponding pre-treatment mean and multiplied by 100. The non-deferrable acute-sensitive group includes ischemic heart disease, subarachnoid hemorrhage, intracerebral hemorrhage, and cerebral infarction. The urgent but margin-sensitive group includes pneumonia and fracture. The elective or deferrable proxy includes cataract and arthrosis. All models use the same difference-in-differences specification as the main analysis, with individual, month, and municipality-by-month fixed effects. Standard errors are clustered at the individual level.

Figure F.1 presents event-study estimates for monthly inpatient admission probabilities in each

<sup>18</sup>These groups are not intended to provide an exhaustive decomposition of all inpatient admissions; rather, they capture selected diagnosis groups with qualitatively different degrees of plausible scope for scheduling or admission-margin adjustment. The non-deferrable acute-sensitive group comprises conditions requiring immediate intervention where delay would directly compromise outcomes (ischemic heart disease, stroke subtypes). The urgent but margin-sensitive group comprises conditions that are clinically urgent but where the admission decision may involve some discretion at the margin (pneumonia, fracture). The elective or deferrable proxy comprises conditions for which scheduled procedures are standard practice and timing is largely at the patient's and provider's discretion (cataract, arthrosis). These classifications are consistent with standard clinical categorizations used in the health economics literature.

Table F.1: Period-Average Effects on Inpatient Admissions by Selected Diagnosis Group

|                                  | Non-deferrable / acute-sensitive coefficients |       | Urgent but margin-sensitive coefficients |       | Elective / deferrable proxy coefficients |       |
|----------------------------------|---|-------|--|-------|--|-------|
|                                  |   | se    |  | se    |  | se    |
| Pre-period average, k=-11 to -1  | 1.5   | (2.8) | -2.1                                     | (3.0) | -4.5                                     | (5.1) |
| Early pre-period, k=-11 to -4    | 1.4   | (3.1) | -1.6                                     | (3.3) | -5.9                                     | (5.3) |
| Immediate pre-period, k=-2 to -1 | 2.1   | (2.9) | -4.0                                     | (3.1) | 1.3                                      | (5.7) |
| Post-period average, k=0 to 14   | -3.9  | (3.4) | -4.6                                     | (3.5) | -14.6                                    | (5.3) |
| Immediate post-period, k=0 to 2  | -1.7  | (3.6) | -2.3                                     | (3.9) | -13.6                                    | (6.1) |
| Middle post-period, k=3 to 8     | -2.8  | (3.8) | -8.9                                     | (3.9) | -12.2                                    | (5.7) |
| Late post-period, k=9 to 14      | -6.1  | (3.9) | -1.4                                     | (3.9) | -17.5                                    | (5.8) |
| Pre-treatment mean               | 0.0038  |       | 0.0039                                   |       | 0.0016                                   |       |

*Notes:* This table summarizes period-average effects for selected inpatient diagnosis groups. Estimates are expressed as percentages of the category-specific pre-treatment mean admission probability. Each estimate is calculated as the average event-study coefficient over the indicated period, divided by the corresponding pre-treatment mean and multiplied by 100. Standard errors, reported in parentheses, are computed using the variance-covariance matrix of the event-study estimates and therefore account for covariance across monthly coefficients. The omitted reference month is  $k = -3$ . The non-deferrable acute-sensitive group includes ischemic heart disease, subarachnoid hemorrhage, intracerebral hemorrhage, and cerebral infarction. The urgent but margin-sensitive group includes pneumonia and fracture. The elective or deferrable proxy includes cataract and arthrosis.

group, scaled by the category-specific pre-treatment mean to make magnitudes comparable across groups with different baseline admission rates. Because diagnosis-specific admissions are rare events, monthly estimates carry wide confidence intervals and should be interpreted with appropriate caution. Table F.1 summarizes these estimates as period averages.<sup>19</sup>

The pre-period estimates are imprecise across all three groups, reflecting the rarity of these diagnosis-specific admissions. In the post-implementation period, however, a clear gradient emerges across groups. Non-deferrable acute-sensitive diagnoses show no systematic decline, with a post-period average of  $-3.9\%$  that is statistically indistinguishable from zero. Urgent but margin-sensitive diagnoses show a similarly modest and noisy post-period average of  $-4.6\%$ . By contrast, the elective or deferrable proxy exhibits a substantially larger and more persistent decline, averaging approximately  $-14$  to  $-15\%$  below the category-specific pre-treatment mean throughout the post-period, with the reduction deepening over time. While the wide confidence intervals for the non-deferrable group preclude strong claims about the precise magnitude of any effect on acute admissions, the contrast between groups is consistent with the interpretation advanced in the main text: the aggregate inpatient decline primarily reflects adjustments in admissions with greater scope for scheduling decisions, rather than a broad reduction in clearly unavoidable acute care.

<sup>19</sup>For diagnosis group  $d$  and period  $P$ , the period-average effect is computed as  $100 \times |P|^{-1} \sum_{k \in P} \hat{\beta}_{dk} / \bar{Y}_{pre}^d$ , where  $\hat{\beta}_{dk}$  is the event-study coefficient for group  $d$  in relative month  $k$ , and  $\bar{Y}_{pre}^d$  is the category-specific pre-treatment mean. Standard errors are computed using the variance-covariance matrix of the event-study estimates, accounting for covariance across monthly coefficients. The omitted reference month is  $k = -3$ .