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Wait-for-Discount: Strategic Deferral, Health Deterioration, and the Hidden Costs of Cost-Sharing Design

Masato Oikawa Rong Fu

Akira Kawamura Haruko Noguchi

Waseda INstitute of Political EConomy
Waseda University
Tokyo, Japan

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Masato Oikawa * Rong Fu † Akira Kawamura ‡ Haruko Noguchi §

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Abstract

When patients anticipate a reduction in their healthcare copayment, do they wait for the lower price before seeking care—and if so, at what cost? We answer this question by exploiting Japan's age-70 copayment threshold—a sharp and fully anticipated price reduction determined solely by date of birth—using administrative claims from Japan's National Database of Health Insurance Claims tracking over 5.7 million individuals. We document significant wait-for-discount behavior within universal health insurance. Using a dynamic difference-in-differences event study design, we show that standard regression discontinuity estimates overstate long-run price elasticity by conflating intertemporal substitution with structural demand: the immediate elasticity of −0.200 reflects a transitory spike driven by pent-up demand, while the steady-state elasticity stabilizes at −0.088 once deferred demand is absorbed. The welfare consequences are severe and concentrated among the relatively healthy: heterogeneity analysis reveals a behavioral decoupling in which healthy individuals strategically time elective procedures, exhibiting a 5% drop in admissions pre-threshold and an 8.5% spike post-threshold. Crucially, deferral triggers a deterioration mechanism: inpatient expenditures escalate steadily in subsequent months, peaking at a 4.3% surge in months seven through nine—reflecting the progression of conditions left unmanaged during deferral rather than a simple release of pent-up demand. Cost-benefit analysis reveals a severe targeting inefficiency: for every 1 yen saved by a patient through deferral, the social insurance system incurs approximately 47 yen in downstream social costs—a ratio that represents a strict lower bound on the true social cost and that standard static policy evaluations miss entirely.

Keywords: Strategic Care Deferral, Wait-for-discount, Intertemporal Substitution, Offset Effect, Price Elasticity, Targeting Inefficiency, Dynamic Inefficiency, Administrative Claims Data (NDB)
JEL Classification Codes: I11, I13, I18, D12, H51

*Corresponding author. Faculty of Education and Integrated Arts and Sciences, Waseda University, 1-6-1 Nishi-Waseda, Shinjuku-ku, Tokyo 169-8050 Japan, and Waseda Institute of Social & Human Capital Studies (WISH), Tokyo, Japan Email: m.oikawa@waseda.jp

†Faculty of Commerce, Waseda University, 1-6-1 Nishi-Waseda, Tokyo, Japan and WISH, Tokyo, Japan, Email: nataliefu@waseda.jp

‡Faculty of Human Sciences, Waseda University, Saitama, Japan, and WISH, Tokyo, Japan, Email: akawamura@waseda.jp

§Faculty of Political Science and Economics, Waseda University, Tokyo, Japan, and WISH, Tokyo, Japan, Email: h.noguchi@waseda.jp

1 Introduction

People wait for sales. When a price reduction is anticipated, forward-looking agents delay purchases to take advantage of the lower future price; when a price increase looms, they accelerate. This pattern of intertemporal substitution is well documented across retail markets (Hendel and Nevo, 2004, 2006a,b; Agarwal et al., 2017), durable goods (Mian and Sufi, 2012), corporate investment (House and Shapiro, 2008), housing markets (Best and Kleven, 2018; Best et al., 2020), and household consumption (Cashin and Unayama, 2016). In each of these settings, strategic timing is individually rational even when it generates aggregate welfare losses. In healthcare, however, the welfare costs of this intertemporal substitution are qualitatively different. When patients delay medical care to wait for a lower copayment, they are not merely deferring a consumption decision—they are deferring the maintenance of their own health. Conditions that go untreated can progress; routine care postponed can become acute care required (e.g., Card et al., 2008). The individual saving may be modest; the downstream cost, to both health and the public purse, may be far larger.

This paper documents this dynamic inside one of the world's most comprehensive universal health insurance systems. Japan provides continuous coverage to all citizens, with nationally standardized fees and free choice of provider—a system explicitly designed to remove financial barriers to care (Ikegami et al., 2011). Yet we show that even within this setting, the structure of cost-sharing rules generates a systematic behavioral response with large unintended consequences. At age 70, Japanese patients face a sharp, fully anticipated reduction in their copayment rate, an exogenous change determined solely by date of birth. Using the universe of administrative claims from Japan's National Database of Health Insurance Claims (NDB), tracking over 5.7 million individuals monthly across a 30-month window around their 70th birthday, we document that individuals strategically defer non-urgent care before this threshold, wait for the lower price to take effect, and subsequently experience health deterioration that generates inpatient costs far exceeding the savings from deferral. For every 1 yen saved through strategic deferral, the social insurance system incurs approximately 47 yen in additional downstream costs—a stark illustration of how the gap between individual rationality and collective welfare can open up from seemingly straightforward insurance design choices.

The behavioral pattern we document—which we term wait-for-discount behavior—is conceptually distinct from the well-studied wait-for-Medicare phenomenon in the United States, where individuals postpone care until gaining insurance coverage at age 65 (Card et al., 2008; McWilliams et al., 2007)—a binary transition from uninsured to insured. Our setting involves deferral among continuously insured individuals responding to a purely marginal price change. That this behavior is observable within a universal system, among patients who face no access barriers whatsoever, suggests that the structure of cost-sharing rules—not merely the presence or absence of coverage—shapes healthcare utilization in ways that carry significant welfare consequences.

The paper's primary contribution is identifying and quantifying a deterioration mechanism that connects pre-threshold deferral to downstream health shocks. The downstream consequences of this deferral are not uniform across the population. They are concentrated among relatively healthy individuals whose care is sufficiently discretionary to allow strategic timing, and who exhibit sharp swings in utilization around the threshold that trace the clinical pathway from deferred outpatient consultation to scheduled inpatient admission. More strikingly, this group shows a lagged deterioration in health, manifesting as a significant increase in inpatient expenditures months after the threshold, that cannot be attributed to the simple release of pent-up demand. It instead reflects the progression of conditions left unmanaged during the pre-threshold deferral period. Among individuals with greater baseline health needs, whose utilization is governed by clinical necessity and who are largely insulated from marginal price changes by institutional safety nets, no such deterioration is observed. This heterogeneity exposes a targeting inefficiency at the heart of the policy.

The targeting inefficiency runs in a direction that is precisely the opposite of what the policy intends. The copayment reduction at age 70 is intended primarily to improve access for high-need elderly individuals. Yet it is the relatively healthy—those with the most discretionary care to defer—who respond most strongly and bear the largest downstream health consequences. The policy thus disproportionately subsidizes intertemporal timing optimization among those it was not designed to help, while inadvertently creating conditions for health deterioration among that same group. The scale of this misallocation is captured by a straightforward cost-benefit calculation: deferring one month of routine outpatient care saves the marginal patient approximately 833 yen, while the implied excess inpatient costs from the resulting health deterioration amount to approximately 39,380 yen per episode—a social cost ratio of 47 to 1 that represents a strict lower bound, since it abstracts entirely from unpriced welfare losses including suffering, lost productivity, and potential mortality.

A secondary but important contribution concerns measurement. Beyond its substantive findings, this paper offers a reinterpretation of the existing empirical literature on the price elasticity of healthcare demand. Regression discontinuity designs at eligibility thresholds are widely used to estimate this elasticity, treating the post-threshold surge in utilization as structural price responsiveness. Our longitudinal framework reveals that when thresholds are anticipated, this approach conflates two distinct phenomena: the release of pent-up demand accumulated during strategic pre-threshold deferral, and the genuine long-run structural response to lower prices. Rather than representing contradictory estimates, the immediate elasticity of -0.200 of Shigeoka (2014) and the steady-state elasticity of -0.06 of Komura and Bessho (2025) both emerge naturally from our framework as different points in the same adjustment process—a reconciliation that clarifies rather than challenges the existing literature. The validity of this framework rests on a parallel trends assumption that we formally test in Section 5, where pre-treatment coefficients are shown to be statistically indistinguishable from zero in the period well before the threshold. Combining our analysis with Fu et al. (2025), who document anticipatory stockpiling before a copayment increase in the same system, establishes a symmetric picture of intertemporal substitution: patients stockpile before price increases and defer before price decreases. This symmetry, structurally unobservable without universal coverage, suggests the dynamic we identify is a general feature of how forward-looking agents respond to anticipated insurance price changes, rather than a peculiarity of direction or magnitude.

Our findings speak to a design principle with broad relevance for aging societies. Sharp discontinuities in copayment rates at statutory thresholds create concentrated intertemporal incentives whose social costs—operating through the deterioration mechanism we identify—substantially exceed their private benefits and are missed by standard policy evaluations focused on immediate utilization responses. More gradual transitions in cost-sharing, alongside targeted protections for preventive and chronic care approaching the eligibility threshold, could preserve the access benefits of lower copayments while mitigating these dynamic inefficiencies. Any insurance system that reduces cost-sharing at a statutory age threshold faces the same design tension, and the fiscal stakes, given the externalities we document, are likely first-order wherever such thresholds exist. As populations age and health systems face growing fiscal pressure, correcting these dynamic inefficiencies is not a refinement but a first-order policy priority. The findings connect to several strands of literature in ways that sharpen each connection. We build on the empirical literature on health insurance design and price elasticity of demand, drawing on the RAND Health Insurance Experiment (Manning et al., 1987) and subsequent quasi-experimental work (Finkelstein et al., 2012; Goodman-Bacon, 2018; Kondo and Shigeoka, 2013; Card et al., 2009; Finkelstein and McKnight, 2008), and we dynamically reconcile findings from the Japanese context (Nishi et al., 2012; Shigeoka, 2014; Fukushima et al., 2016; Ando and Takaku, 2016; Komura and Bessho, 2025). We contribute to the literature on anticipatory responses to insurance changes (Card et al., 2008; Alpert, 2016; Kaplan and Zhang, 2017) by establishing wait-for-discount behavior within a universal system—extending the anticipatory response literature from the binary coverage context to the continuous price context, where the welfare stakes prove substantially larger. Finally, we contribute to the literature on offset effects and the health costs of cost-sharing (Chandra et al., 2010, 2024) by identifying a deterioration mechanism operating through strategic deferral of routine maintenance—distinct from direct rationing under higher prices—with implications for how the welfare costs of cost-sharing design are evaluated.

The remainder of this paper proceeds as follows. Section 2 provides institutional background on the Japanese health insurance system. Section 3 presents the data for the analysis. Section 4 describes our identification strategy. Section 5 summarizes results, and heterogeneity findings. Section 6 discusses the interpretation of price elasticity estimates, the heterogeneity of behavioral responses, and policy implications. Finally, Section 7 concludes.

2 Institutional Background

2.1 The Japanese Health Insurance System

Japan maintains a universal health insurance system that provides comprehensive coverage to all citizens. Under this system, patients have free choice of provider, meaning they can seek care at any medical institution—from small clinics to large general hospitals—without a referral from a gatekeeper. Medical fees are standardized nationally through a fee schedule determined by the government, and providers are reimbursed on a fee-for-service basis. Three features of this system are particularly relevant for our analysis. First, the combination of free provider choice and standardized fees means that utilization decisions are driven primarily by patient demand rather than by supply-side rationing or price variation across providers. Second, while the system ensures high accessibility, patients are required to pay a portion of their medical costs at the point of service. For individuals under the age of 70, the standard copayment rate is 30% of the total medical expenditure. Third, Japan's "High-Cost Medical Expense Benefit System" (*Kogaku Ryoyo-hi Seido*) imposes a monthly cap on out-of-pocket expenditures based on age and income, a safety net that, as we discuss in Section 5.2, plays a crucial role in explaining the low price elasticity of inpatient care, since the effective marginal price often drops to zero for high-cost treatments.

2.2 Copayment Changes at Age 70 and the 2014 Reform

Upon reaching the age of 70, the copayment rate is reduced, with the specific rate determined by birth date and income level. Historically, the statutory copayment rate for those aged 70 to 74 was 20%. However, through a special provisional measure, this rate had been kept at 10% for many years. To ensure the long-term fiscal sustainability of the healthcare system amidst a rapidly aging population, the Japanese government implemented a reform in April 2014 to phase out this special measure.

Under the 2014 reform, the copayment rate for individuals aged 70–74 was raised from 10% to 20%. Crucially, the government introduced a grandfathering clause to mitigate the impact: the 10% rate was maintained for those who had already reached age 70 by April 1, 2014. For those who reached age 70 on or after April 2, 2014, the new 20% rate was applied.

This policy change created a sharp discontinuity based on an individual's date of birth. Those born on or before April 1, 1944 (the treatment group), experienced a substantial reduction in their copayment rate from 30% to 10% upon turning 70. In contrast, those born on or after April 2, 1944, experienced a smaller reduction from 30% to 20% at the same age threshold. While we refer to this latter cohort as the "control group" for analytical convenience, it is important to note that they are essentially a partially treated arm affected by the 2014 reform. Since both groups faced the same 30% pre-treatment rate until age 69, this setting allows for a difference-in-differences approach to identify the behavioral impact of the additional 10 percentage point (or one-third) net price reduction.

Two features of this institutional setting are central to our identification strategy. First, the reform was announced well in advance and eligibility for the 10% versus 20% copayment rate was determined solely by an individual's date of birth—a factor entirely outside their control. This exogeneity ensures that any differences in healthcare utilization between the two groups around the age 70 threshold can be attributed to the difference in their copayment rates rather than to selection. Second, the sharp and fully anticipated nature of the price reduction at a known calendar date creates precisely the conditions under which forward-looking patients have both the incentive and the information to adjust the timing of their healthcare utilization—making this setting ideal for identifying wait-for-discount behavior and its downstream consequences. We describe the identification strategy exploiting these two features in detail in Section 4.

3 Data

3.1 National medical claim database

The primary data source for this study is the National Database of Health Insurance Claims and Specific Health Checkups of Japan (NDB), managed by the Ministry of Health, Labour and Welfare (MHLW). The NDB is one of the most comprehensive administrative health databases in the world, accumulating approximately 1.943 billion claims as of 2017,[1] covering nearly all medical insurance claims (receipts) in Japan, including medical and pharmacy records—a scale and completeness that is essential for identifying the dynamic behavioral responses documented in this paper. Our dataset covers the period from April 2009 to March 2017.

The NDB provides detailed individual-level demographic information, including month and year of birth and gender. To construct a longitudinal panel dataset, we utilize an anonymized person-specific identifier known as "ID2". The ID2 is generated based on a combination of the individual's name, date of birth, and sex. A change in name due to marriage or divorce would result in the issuance of a different ID2 for the same person. However, according to the Vital Statistics of Japan, the demographic transition rates for the relevant age groups are extremely low.[2] The risk of attrition due to name changes is therefore statistically negligible, and the ID2 thus serves as a highly reliable identifier for tracking individuals across the age-70 threshold.

Based on these records, we aggregate healthcare expenditures for each individual on a monthly basis. Specifically, we sum the total medical points recorded in the claims and convert them into

[1] Ministry of Health, Labour and Welfare, Insurance Bureau, Medical and Long-term Care Integration Policy Division, 'The 21st Expert Committee on the Provision of Anonymous Medical Information (Document 3, June 12, 2024),' available at https://www.mhlw.go.jp/content/12400000/001262623.pdf (accessed April 1, 2026).

[2] Specifically, in 2014, the divorce rates per 1,000 population are 0.89 for males aged 65–69 and 0.56 for those aged 70–74 (0.52 and 0.28 for females, respectively). Similarly, marriage rates are negligible; first-marriage rates are 0.06 and 0.02 for males and 0.02 and 0.01 for females across the same age groups, while remarriage rates are also low at 0.56 and 0.34 for males and 0.27 and 0.16 for females.

monetary values (1 point = 10 JPY). This individual-month panel structure enables us to capture the dynamic responses in healthcare-seeking behavior, including the precise timing of care deferral and subsequent increases in utilization.

3.2 Sample Restriction

Our analysis focuses on individuals born between April 1942 and March 1946, covering the ages from 68 years and 6 months to 71 years. To ensure the validity of our comparison, we excluded high-income earners who remained at the 30% copayment rate beyond age 70. Unlike the general elderly population, who experience a reduction to 10% or 20%, these individuals possess systematically higher income levels. Using this group as a control would likely introduce selection bias, as their healthcare-seeking behavior and underlying health investments may differ significantly due to their greater financial resources. By excluding them, we ensure that our treatment and control groups are more socioeconomically homogeneous, thereby isolating the behavioral response to the price reduction from any income-driven selection effects.

Our analysis distinguishes between two margins of healthcare utilization. The intensive margin captures variation in expenditures among active users, while the extensive margin captures the binary decision to seek care at all—including months with zero utilization. Each margin requires a different sample construction, as described below.

Following these procedures, the number of patients with any monthly expenditures is 7,531,071 (120,951,951 claim cases) for outpatient visits and 785,571 (2,468,939 claim cases) for inpatient care. For the estimation of the intensive margin, we use data from individuals with multiple records, yielding a final estimation sample of 6,753,900 patients (120,174,780 claim cases) for outpatient visits and 384,489 patients (2,067,857 claim cases) for inpatient care. A detailed account of the step-by-step sample restriction procedure is provided in Appendix A.

3.3 Balanced Panel Construction for Extensive Margin Analysis

To rigorously evaluate the extensive margin—the fundamental decision to seek care—we constructed a balanced panel dataset. Raw claims data only generate records when a patient utilizes a medical service, so months with zero utilization are absent by construction. The balanced panel addresses this by explicitly imputing zeros, making it possible to distinguish genuine behavioral responses—such as strategic deferral—from the absence of a record.

To isolate genuine behavioral responses from sample attrition (e.g., mortality or insurer changes), we imposed a strict inclusion criterion for this panel: individuals must have utilized medical services (either outpatient or inpatient) at least once before the policy threshold (prior to age 70 and 1 month) and at least once after the threshold. This requirement ensures that the individuals remained alive and consistently enrolled in the observable insurance system throughout the transition period.

For individuals meeting this criterion, we expanded their records across the entire 30-month observation period and assigned a value of zero for both medical expenditures and visit indicators in months without any claims. This procedure yields a final balanced panel estimation sample of 5,777,540 unique individuals, resulting in 179,103,740 individual-month observations. Note that this balanced panel is a more stringently defined subset compared to the intensive margin sample for outpatient visits (6,753,900 individuals), as the latter only required multiple periods of utilization without mandating that they straddle the policy threshold.

3.4 Subgroup Definition by Prior Health Status

To examine the heterogeneity of price responses, we categorize individuals based on their prior health status. We define two subgroups based on an individual's hospital admission history during the one-year period from age 67 years 0 months to 67 years 11 months. Individuals with no recorded inpatient medical expenditures during this period are classified into the "Group without Prior Admissions" and are considered relatively healthy. Conversely, those who had at least one recorded inpatient medical expenditure during the same period are assigned to the "Group with Prior Admissions," representing a population with chronic medical needs or a history of significant health shocks. The choice of age 67 as the reference period is deliberate: it is sufficiently far from the age-70 threshold to ensure that the health status classification is not contaminated by the anticipatory behaviors or the policy-induced changes in utilization that begin to emerge in the months approaching age 70, while remaining close enough to reflect each individual's health status at the time of the policy change.

4 Identification Strategy

Our identification strategy exploits the sharp, fully anticipated, and exogenously determined reduction in copayment rates at age 70 to estimate the causal effect of cost-sharing on healthcare utilization. The key source of variation is the grandfathering clause introduced by the 2014 reform, which created a discontinuity in the magnitude of the copayment reduction across birth cohorts: individuals born on or before April 1, 1944 experienced a reduction from 30% to 10%, while those born on or after April 2, 1944 experienced a smaller reduction from 30% to 20%. Since both groups faced the same pre-reform copayment rate and the assignment to each group was determined solely by date of birth, any difference in their utilization trajectories around the age-70 threshold can be attributed to the difference in the magnitude of their copayment reductions. We implement this comparison using a dynamic difference-in-differences event study design that traces the full trajectory of utilization in the months before and after the threshold, allowing us to separately identify anticipatory deferral, the immediate post-threshold surge, and the long-run structural response to lower prices.

4.1 Estimation Model

To identify the causal effect of the copayment rate reduction on healthcare utilization, we employ a dynamic event study design that exploits the exogenous variation in the copayment rate at age 70. Our identification strategy compares individuals who experience a reduction from 30% to 10% (the treatment group) with those who experience a reduction from 30% to 20% (the control group), based on their date of birth.

The estimation equation is specified as follows:

$$y_{ibmt} = \alpha + \sum_{\substack{k=-18 \\ k\neq-12}}^{12} [\beta_k(treat_i \times \mathbb{1}\{m = k\})] + \theta_i + \phi_m + \eta_t + \rho_b + u_{ibmt}, \tag{1}$$

where i, b, m, and t index individuals, birth cohorts, age in months, and calendar months, respectively. The dependent variable, y_{ibmt}, represents healthcare utilization measures, such as monthly medical expenditures. The variable $treat_i$ is a dummy variable equal to one if individual i was born on or before April 1, 1944 (the treatment group), and zero otherwise (the control group). The terms $\mathbb{1}\{m = k\}$ are indicator variables for the age in months relative to the threshold.

A central design feature of our specification is the choice of reference period. The reference age is set at 69 years and 0 months ($k = -12$), placing the baseline 12 months before the copayment reduction takes effect. This choice is deliberate and consequential; because the policy change at age 70 is perfectly predictable, individuals may strategically postpone medical consultations in the months immediately preceding the threshold to take advantage of the lower prices. Setting the reference period at 12 months prior to the threshold—well before anticipatory behavior is likely to begin—allows us to capture this pre-threshold deferral as a measurable deviation from the baseline utilization level, rather than having it absorbed into the reference period itself. This is a key advantage of our dynamic design over static regression discontinuity approaches, which cannot distinguish anticipatory deferral from structural price responsiveness.

The parameters θ_i, ϕ_m, η_t, and ρ_b represent fixed effects for individuals, birth cohorts, age in months, and calendar months, respectively. These fixed effects account for time-invariant individual heterogeneity, age-specific utilization patterns, seasonal fluctuations, and cohort-specific characteristics. The coefficients of primary interest are β_k, which identify the month-by-month differences in healthcare utilization between the treatment and control groups at each relative age k, relative to the baseline level at 69 years and 0 months.

4.2 Identification Assumption and Validation

The validity of our difference-in-differences approach relies on the parallel trends assumption, which posits that in the absence of the copayment reduction, the healthcare utilization of the treatment and control groups would have followed the same trajectory. This assumption is well motivated by the institutional setting: since treatment assignment is determined solely by date of birth, there is no reason to expect systematic differences in the health trajectories of individuals born just before versus just after April 1, 1944, other than through the copayment reduction itself.

We formally test this assumption by examining the estimated coefficients for the pre-treatment period ($k < -1$). If the parallel trends assumption holds, the coefficients β_k should be statistically indistinguishable from zero during the months prior to the age-70 threshold, except for the window where anticipatory behaviors are expected. We present formal evidence in support of this assumption in Section 5, where the pre-treatment coefficients are shown to be statistically indistinguishable from zero in the period well before the threshold, with deviations emerging only in the months immediately preceding age 70 in a pattern consistent with strategic care deferral rather than a violation of parallel trends.

5 Results

5.1 Descriptive statistics

Table 1 presents the summary statistics of age and the female ratio for patients aged 68.5 to 71 years. The mean age of the analysis sample is nearly identical between outpatient (69.77) and inpatient (69.78) users (Columns (2) and (4)). While the female ratio is approximately 50% for both groups, the outpatient sample has a slightly higher proportion of females, whereas the inpatient sample contains a higher proportion of males (Columns (2) and (4)).

Before presenting the main results, we address a potential concern arising from the fact that the birth years of the analysis sample largely coincide with World War II. During this conflict, Japan experienced a significant decline in the birth rate, likely driven by factors such as fertility postponement and the separation of couples. This trend was particularly pronounced during the final stages of the war, when the military situation deteriorated sharply. Since the treatment status is determined by birth timing, the dramatic shrinkage of these birth cohorts may introduce estimation bias. To explore this possibility, Figure 1 presents the number of outpatients from FY2009 to FY2016 by birth month to assess this concern.

Panel (b) of Figure 1 depicts the percentage change in the number of outpatients relative to the previous year, adjusting for the inherent seasonality of births shown in Panel (a).[3] Among the treated cohorts (between the left blue line and the red line), the number of outpatients is nearly identical to that of the previous year's cohorts. In contrast, for the control cohorts (between the red line and the right blue line), the number of outpatients is lower than in the previous year across most birth cohorts, with a maximum decrease of 28%. This substantial decline in the number of patients primarily reflects the shrinkage of the birth cohorts themselves. It is therefore important to examine whether such a dramatic change in cohort size is associated with differences in the intensive

[3] In Japan, births tend to concentrate in January each year.

margin, such as medical expenditure per patient.

Table 2 addresses this concern directly. Despite the significant differences in the absolute number of patients across cohorts, we find no systematic disparities in the level of healthcare expenditures. In the mean, monthly healthcare expenditures are almost identical between the treated and control cohorts for both outpatient (5.3% difference) and inpatient (4.4% difference) services. Furthermore, the distribution of expenditures shows a consistent pattern across most percentiles. For outpatient visits, the expenditures of the treated cohort are slightly lower than those of the control cohort across the entire distribution, with the difference in the median being only 2.6%. Regarding inpatient care, while the treated group shows higher values in the lower tail of the distribution (e.g., 36.8% at the 5th percentile), these disparities diminish at higher percentiles. Overall, the absence of large or systematic gaps across the distribution suggests that the dramatic shrinkage in cohort size did not lead to a substantial selection bias in terms of healthcare needs.[4]

Figure 2a plots the log of monthly outpatient expenditures by birth-month cohort. The markers represent the average expenditure for each month of age, while the solid lines indicate quadratic polynomial fits estimated separately before and after Month 0 (Age 70). To account for the mid-month transition of the copayment rate, the data point at exactly age 70 is excluded from the quadratic polynomial fits estimation.

The figure reveals a sharp discontinuous jump in outpatient demand at the age-70 threshold. Specifically, for the treated cohort (highlighted by blue circles), the log of medical expenditures increases from 10.58 at Month -1 (age 69 years and 11 months) to 10.68 at Month 1 (age 70 years and 1 month). This 0.10 log-point increase corresponds to an approximately 10.5% rise in outpatient expenditures ($e^{0.10} - 1 \approx 0.105$) following the 20-percentage-point reduction in the copayment rate (from 30% to 10%). Notably, this magnitude of response is highly consistent with the findings of Shigeoka (2014), who documented a similar increase in outpatient utilization among the elderly in Japan following a reduction in cost-sharing.

In contrast, the control cohort shows a more modest response. For this group (highlighted by red circles), the log of expenditures increases from 10.65 to 10.71 over the same window. This 0.06 log-point jump represents a roughly 6.2% increase, consistent with the smaller 10-percentage-point reduction in their copayment rate. The nearly identical levels and trends of the fitted lines prior to age 70 confirm that the two cohorts are highly comparable, ensuring that the divergence after the threshold is driven by the disparity in the cost-sharing policy.

Figure 2b presents the log of monthly inpatient expenditures. Following the same empirical specification as the outpatient analysis, we plot the cohort averages along with quadratic polynomial fits estimated separately before and after the age-70 threshold, excluding the data point at age 70.

In contrast to the results for outpatient services, there is no discernible discontinuous jump in inpatient expenditures for either cohort. Despite the substantial reduction in the nominal out-of-pocket cost-sharing rate, the expenditures remain remarkably stable. Specifically, for the treated

[4] See Appendix D for further visual evidence of the continuity in healthcare expenditures across birth cohorts.

cohort, the log of expenditures is virtually identical before and after the threshold, remaining at 14.69 at both Month -1 (age 69 years and 11 months) and Month 1 (age 70 years and 1 month). Similarly, the control cohort shows negligible change, with the log value moving slightly from 14.58 to 14.56. The event study analysis presented below provides a more rigorous examination of these patterns.

These results suggest that inpatient demand in this age group is highly inelastic with respect to price. One primary reason is the urgent medical necessity typically associated with inpatient care, which leaves limited room for behavioral adjustments. Furthermore, a crucial institutional factor is Japan's "High-Cost Medical Expense Benefit System" (*kogaku-ryoyo-hi-seido*), which imposes a monthly ceiling on out-of-pocket payments. Since inpatient care often incurs significant costs, many patients likely reach this monthly cap regardless of their nominal copayment rate. Consequently, the effective price of additional medical services remains unchanged at the margin, effectively neutralizing the institutional price shock at age 70.

5.2 Event Study Results

We begin by confirming the validity of the parallel trends assumption. As shown in Panel (a) of Figure 3, the estimated coefficients for the outpatient intensive margin are consistently close to zero and statistically insignificant during the period from Month -18 to Month -6, confirming the absence of pre-existing systematic differences between the treatment and control groups. The downward deviation that emerges from approximately Month -5 onward is consistent with the onset of strategic care deferral rather than a violation of parallel trends—a pattern we examine in detail below. The same conclusion holds for the inpatient margin, as shown in Panel (b) of Figure 3.

The event study results for outpatient expenditures (intensive margin), presented in Panel (a) of Figure 3, provide two further key insights. We observe a significant downward trend as individuals approach the age-70 threshold. Specifically, the coefficient reaches -0.02 in Month 0 ($p < 0.01$), representing a 2% reduction in healthcare expenditures. This pattern is consistent with care deferral; anticipating the copayment reduction from Month 1, patients and physicians likely postponed non-urgent treatments to take advantage of the lower prices in the following month.

Further, a sharp jump occurs at Month 1, where the coefficient surges to 0.07 ($p < 0.01$), representing an approximately 7.25% ($e^{0.07} - 1 \approx 0.0725$) increase in expenditures. This spike reflects the realization of pent-up demand. Following this peak, the effect eventually stabilizes at around 3% by Month 12, indicating a sustained structural increase in outpatient utilization.

In contrast, the event study results for inpatient expenditures (intensive margin), presented in Panel (b) of Figure 3, show no significant price response. The coefficients remain near zero throughout the entire observation period. This rigidity is attributed to standardized clinical pathways and the "High-Cost Medical Expense Benefit System" (*kogaku-ryoyo-hi-seido*), which caps monthly out-of-pocket payments. Since inpatient episodes are generally costly, most patients reach this monthly ceiling regardless of their nominal copayment rate (10%, 20%, or 30%), thereby neutralizing marginal price incentives for adjusting treatment intensity.

To further unpack these behavioral mechanisms, we investigate the extensive margin—the probability of any medical utilization. Unlike the intensive margin, which captures adjustments in treatment intensity among active users, the extensive margin reflects the fundamental decision to visit a healthcare provider. For elderly individuals, this decision represents a pivotal choice that may be strategically timed in response to policy thresholds.

Our analysis of the extensive margin (Figure 4) reveals striking asymmetries in both the timing and nature of behavioral responses.[5] For outpatient services, we identify a notable discrepancy in timing: while the intensive margin begins to exhibit a gradual decline as early as Month -5, the extensive margin—the actual decision to visit the clinic—only shows a significant reduction starting around Month -1. This suggests a pre-consultation strategic behavior; while patients and physicians may prune treatment intensity (e.g., delaying non-essential tests) months in advance, the binary and potentially riskier decision to forego a visit entirely is deferred until the final months before the price reduction.

Even more striking is the contrast in inpatient care. While treatment intensity remains unresponsive, the extensive margin reveals a strategic care deferral in Month 0. Although this estimate is significant at the 10% level ($p = 0.082$), the point estimate indicates a substantial 2.36% reduction in the probability of admission just before the threshold. This indicates that while clinical necessity dictates the intensity of inpatient treatment once admitted, the timing of entry remains a flexible and strategically managed choice, particularly for elective procedures.

Furthermore, we identify a distinct one-month lag in the rebound of inpatient utilization compared to outpatient services. While the probability of an outpatient visit peaks immediately in Month 1, the peak for inpatient admissions occurs in Month 2. This delay reflects the clinical referral pathway: a patient waits for the copayment reduction in Month 1 to seek an initial outpatient consultation, after which a subsequent, scheduled admission is finalized for Month 2. This lag may also be amplified by supply-side capacity constraints, as hospitals manage the surge of pent-up demand from Month 0 within limited bed and operating room availability.

In sum, these event study results provide nuanced longitudinal evidence of how price incentives shape healthcare utilization among the elderly, revealing a distinct behavioral decoupling between the intensity of care and the decision to visit. While the reduction in the copayment rate leads to a significant and persistent expansion in outpatient utilization across both margins, the response

[5] The figure presents the proportionate change and 95% confidence intervals for the probability of any outpatient visit (Panel (a)) and inpatient admission (Panel (b)). The analysis is conducted using a balanced panel of individuals to ensure consistent tracking of utilization behavior and to account for sample attrition. The proportionate change is calculated by dividing the estimated coefficients by the pre-treatment mean of the dependent variable for the treatment group. While these results are presented in terms of proportionate changes for intuitive interpretation, the corresponding raw estimates (β_k) and their confidence intervals are reported in Appendix B.1.

in inpatient care is characterized by a striking duality. Our findings demonstrate that inpatient demand remains remarkably price-inelastic on the intensive margin, governed by standardized clinical pathways and institutional safety nets—most notably the High-Cost Medical Expense Benefit System—that effectively neutralize marginal price shocks for costly treatments.

However, by shifting the focus to the extensive margin, we uncover a hidden layer of strategic behavior even in hospital care. The substantial 2.36% reduction in admission probability in Month 0 suggests that while patients and physicians cannot easily manipulate the clinical content of a stay, they actively coordinate to optimize the timing of elective procedures to coincide with lower out-of-pocket costs. Furthermore, the identified one-month lag between outpatient (Month 1) and inpatient (Month 2) peaks vividly illustrates the institutional referral pathway and supply-side capacity constraints inherent in the Japanese healthcare system. Collectively, these findings underscore that for the elderly, the decision to visit is not merely a medical necessity but a strategically timed economic choice, mediated by the complex interplay between health risks, institutional design, and clinical pathways.[6]

5.3 Heterogeneity by Prior Health Status

To further explore the mechanisms underlying the observed price responsiveness, we conduct a subgroup analysis based on individuals' prior medical history. Specifically, we divide the sample into two groups according to whether they had any inpatient admissions at the age of 67. This allows us to examine whether the impact of the cost-sharing reduction differs by underlying health status or baseline medical needs.

To unpack the behavioral dynamics, we first examine the extensive margin (the probability of medical utilization). The analysis of the extensive margin (Figure 5) reveals striking differences in anticipatory behavior and short-term price responsiveness.[7] For outpatient services, the group without prior hospital admissions (the relatively healthy group) exhibits a distinct care deferral, with the probability of a visit dropping by approximately 1.1% in Month 0, followed by a sharp 2.3% surge in Month 1. In contrast, the group with prior admissions (the high-need group) shows a muted response, with a much smaller long-run price elasticity.

This behavioral divergence is even more pronounced in the extensive margin of inpatient care. For the healthy group, the probability of admission drops by approximately 5% in Month 0, followed

[6] We conducted robustness checks by re-estimating the model with Month 6 as the base period and narrowing the sample window. Findings remained robust across both service types (see Appendix Figures B.6, B.8, B.3, and B.5).

[7] The figure presents the proportionate change and 95% confidence intervals for the probability of any outpatient visit (Panel (a)) and inpatient admission (Panel (b)). The analysis is conducted using a balanced panel of individuals to ensure consistent tracking of utilization behavior. The proportionate change is calculated by dividing the estimated coefficients by the pre-treatment mean of the dependent variable for the treatment group. Refer to Figure 3 for details on the horizontal axis and reference month. While these results are presented in terms of proportionate changes for intuitive interpretation, the corresponding raw estimates (β_k) and their confidence intervals are reported in Appendix B.1. The sample is stratified into individuals without prior hospital admission at age 67 (red diamonds) and with prior hospital admission (blue squares).

by a substantial 8.5% spike in Month 2. This extreme fluctuation suggests that for healthier individuals, inpatient care may encompass a larger proportion of deferrable treatments whose timing can be strategically manipulated. Conversely, the high-need group exhibits virtually no anticipatory deferral or subsequent spike, underscoring that their medical utilization is dictated largely by strict clinical necessity rather than nominal price changes.

Building on these differences in the fundamental decision to visit, we turn to the intensive margin (expenditures) to evaluate the financial implications.

Panel (a) of Figure 6 presents the event study results for outpatient expenditures. The analysis reveals striking differences in the behavioral responses between the two groups. First, we observe a distinct difference in anticipatory behavior leading up to the age-70 threshold. For the group without prior admissions, there is a significant downward trend in expenditures starting from Month -5, reaching a notable low at Month 0 (approx. -2%). This indicates that relatively healthy individuals, whose medical needs may be less urgent, are more likely to exhibit care deferral by postponing visits until the lower copayment rate takes effect. In contrast, the group with prior admissions shows almost no evidence of such anticipatory behavior, suggesting that their medical demand is governed by necessity rather than strategic timing.

Second, the short-term and long-term price responsiveness follow diverging paths. Immediately following the policy change at Month 1, the healthy group shows a sharp, significant jump (approx. 7%), representing the realization of pent-up demand. However, this effect gradually diminishes and stabilizes at a lower level. For the group with prior admissions, the initial jump at Month 1 is more modest, yet the coefficients remain remarkably persistent through the end of the observation period (Months 11 and 12). This implies that lower out-of-pocket costs may facilitate more consistent monitoring for chronic conditions in high-need groups.

Regarding inpatient expenditures, presented in Panel (b) of Figure 6, the estimates are generally less precise due to the infrequent nature of inpatient utilization. However, we observe a suggestive upward trend for the healthy group starting from approximately Month 5. By Months 9 and 10, the point estimates for this group increase to over 0.05, whereas the coefficients for the prior-admission group remain relatively flat and close to zero.

To rigorously test this suggestive divergence and address the limited statistical power of monthly-level models in capturing rare inpatient events, we aggregated the post-treatment period into three-month intervals and conducted a Difference-in-Differences (DID) analysis. The results reveal a striking temporal trajectory. For the healthy group, rather than an immediate post-reform spike, inpatient expenditures exhibit a steady, statistically significant escalation: rising by a mere 0.3% in Months 1–3, climbing to 2.8% ($p < 0.10$) in Months 4–6, peaking at a 4.3% surge ($p < 0.05$) in Months 7–9, and remaining elevated at 3.6% ($p < 0.05$) in Months 10–12. In stark contrast, the group with prior admissions shows a persistently flat trajectory near zero throughout the entire post-reform year. This escalating, delayed pattern among the healthy group is critical for our identification of the underlying mechanism. The temporal distance of this effect from the policy change—and its complete absence among the high-need group—rules out a simple release of pent-up demand for elective procedures, which would have manifested immediately in the first quarter. It instead provides strong evidence for a deterioration mechanism: the strategic deferral of routine outpatient maintenance allowed untreated conditions to progress, ultimately culminating in acute health shocks that necessitated intensive, high-cost inpatient care months later.[8]

In summary, the subgroup analysis demonstrates that the response to cost-sharing reductions is significantly moderated by baseline health status, exposing a targeting inefficiency in uniform age-based policies. For relatively healthy individuals, the price response is characterized by strategic care deferral and timing optimization, which inadvertently leads to a significant long-term rise in inpatient costs due to health deterioration. In contrast, individuals with higher baseline needs show more persistent outpatient utilization without such adverse inpatient shocks, as their clinical necessity limits short-term strategic behavior. This suggests the copayment reduction disproportionately generates unintended health risks among the healthy, rather than purely expanding access for the sick.

6 Discussion

The results presented above speak to three broader questions: how to interpret the existing empirical literature on price elasticity, what the aggregate and heterogeneous responses reveal about the nature of healthcare demand, and what the findings imply for the design of cost-sharing policy.

A large empirical literature has used regression discontinuity designs at insurance eligibility thresholds to estimate the price elasticity of healthcare demand. Our longitudinal framework reveals a systematic limitation of this approach when thresholds are anticipated: the immediate post-threshold surge in utilization conflates the release of pent-up demand—accumulated during months of strategic pre-threshold deferral—with the structural long-run response to lower prices. The two are quantitatively distinct. The immediate elasticity of -0.200 reconciles with Shigeoka (2014), while the steady-state elasticity of -0.088 reconciles with Komura and Bessho (2025)—and our framework shows that both are correct, capturing different phases of the same dynamic transition: the immediate elasticity is inflated by the release of pent-up demand accumulated during months of strategic pre-threshold deferral, while the steady-state elasticity reflects the genuine long-run structural response to lower prices once that stock of deferred demand has been fully absorbed. These are not contradictory estimates of the same parameter but rather different points along the same adjustment path. Static designs that focus on the immediate post-threshold window will systematically overstate long-run price sensitivity wherever thresholds are anticipated—a concern that extends to any setting where eligibility cutoffs are known in advance.

The symmetry established by combining our findings with Fu et al. (2025) strengthens this interpretation. Patients stockpile before price increases and defer before price decreases—a complete

[8] Appendix C provides the full results and graphical evidence of this DID estimation.

picture of intertemporal substitution around insurance price changes that is only observable within a universal system. This symmetry suggests the dynamic is a general feature of how forward-looking agents respond to anticipated changes in insurance prices, not a product of the specific direction or magnitude of the price change studied here. Taken together, these findings suggest that a substantial share of the variation in elasticity estimates across studies may reflect differences in the degree to which thresholds are anticipated, rather than genuine differences in structural price sensitivity.

The aggregate utilization response conceals a fundamental heterogeneity in the nature of healthcare demand. Among relatively healthy individuals, demand is largely discretionary—encompassing elective procedures, routine screenings, and non-urgent consultations whose timing can be strategically managed. For this group, the anticipated price reduction creates a strong intertemporal incentive, generating sharp pre-threshold deferral, a large immediate post-threshold surge, and a long-run elasticity nearly twice that of the high-need group. The one-month lag between outpatient and inpatient peaks is particularly informative: it traces the clinical referral pathway with precision, confirming that strategic timing operates through a deliberate sequencing of consultation and admission rather than through isolated decisions. Among individuals with prior admissions, clinical necessity governs utilization throughout. Japan's High-Cost Medical Expense Benefit System further insulates this group from marginal price incentives by capping monthly out-of-pocket payments, rendering the copayment reduction largely irrelevant for costly inpatient care. The result is near-zero strategic deferral and a subdued long-run price response.

This decoupling carries a broader implication. Aggregate elasticity estimates blend discretionary and non-discretionary demand that respond to price through qualitatively different mechanisms. For policy design, disaggregation by the discretionary content of care—proxied here by prior health status—is more informative than aggregate estimates alone.

The most consequential finding is the lagged increase in inpatient expenditures among relatively healthy individuals in the months following the threshold. The temporal distance of this effect from the policy change—and its complete absence among the high-need group—rules out pent-up demand as an explanation. It instead reflects a deterioration mechanism: the pre-threshold deferral of routine outpatient maintenance allowed conditions to progress, ultimately necessitating more intensive inpatient care months later.

This mechanism is conceptually distinct from the offset effects documented by Chandra et al. (2010, 2024), which arise when patients forgo necessary care under higher cost-sharing. Our deterioration is generated by strategic optimization in anticipation of lower future prices—a channel that operates even as copayments are falling. The welfare implication is correspondingly different and has been largely absent from prior discussions of cost-sharing design: even a well-intentioned reduction in cost-sharing can carry hidden health costs if it is structured in a way that incentivizes deferral of routine maintenance beforehand.

A simple back-of-the-envelope calculation illustrates the scale of this dynamic inefficiency. Because our DID framework identifies the behavioral response to an additional 10-percentage-point reduction in the copayment rate (the difference between the 10% treatment and 20% control rates), the relevant marginal saving that drives this deferral is strikingly modest. Deferring one month of routine outpatient care saves the patient only about 833 JPY.[9] Against this, the implied excess inpatient costs driven by the delayed health shocks are staggering. Applying the peak 4.3% surge observed during Months 7–9 to the median inpatient expenditure (915,804 JPY) translates to an excess cost of approximately 39,380 JPY per episode. Consequently, for every 1 yen a patient saves on the margin through this strategic deferral, the system ultimately incurs over 47 yen in subsequent intensive care, a burden borne almost entirely by the social insurance pool. This 47:1 ratio should be read as a strict lower bound: it captures only the direct financial externality at the peak of deterioration, completely abstracting from unpriced welfare losses such as prolonged physical suffering, reduced labor productivity, and potential mortality. These figures make clear that standard static evaluations of cost-sharing completely miss the first-order fiscal externalities generated by maintenance deferral.

Turning to policy implications, our findings expose a targeting inefficiency in uniform age-based cost-sharing policy. The copayment reduction at age 70 is intended to improve access for high-need elderly individuals — yet this group is already largely protected by institutional safety nets and responds with low price elasticity and negligible strategic behavior. The relatively healthy respond most strongly and bear the largest downstream health consequences. The policy thus subsidizes intertemporal timing optimization among those it was not designed to help while inadvertently generating health deterioration among that same group.

Two directions for reform follow from our analysis. First, replacing the sharp discontinuity in copayment rates at age 70 with a more gradual transition would spread intertemporal incentives across multiple years, attenuating the concentration of strategic deferral at a single threshold. The social benefit of such smoothing, given the externalities we document, could be substantial. Second, targeted elimination of copayments for preventive screenings and chronic disease monitoring in the years approaching the threshold would address the deterioration mechanism directly, severing the link between price incentives and maintenance deferral without sacrificing the broader access benefits of reduced cost-sharing.

Two limitations of our analysis are worth noting. First, we do not directly observe clinical health outcomes such as mortality or disease progression—the deterioration mechanism is inferred from inpatient expenditure patterns, which serve as a proxy for health shocks. Second, while we stratify by prior hospitalization, the behavioral response likely varies across specific disease categories; future work examining disease-specific responses could sharpen the policy recommendations further.

More broadly, our findings point to a general limitation of standard insurance policy evaluation: focusing on the immediate utilization response to a price change misses the dynamic incentives created by anticipated thresholds and their downstream consequences for health. These dynamic effects are a first-order consideration for the design of cost-sharing rules in aging societies, where

[9] 10% of the median monthly outpatient expenditure of 8,330 JPY.

the fiscal sustainability of universal health systems increasingly depends on getting the details right.

7 Conclusion

This paper documents a deterioration mechanism connecting strategic care deferral to downstream health shocks within a universal health insurance system. Exploiting Japan's age-70 copayment threshold—a fully anticipated, exogenous price reduction—we show that relatively healthy individuals defer non-urgent care before the threshold, allow conditions to progress during the deferral period, and subsequently require intensive inpatient care at social costs approximately 47 times the private savings that motivated the deferral. The policy generates its largest unintended consequences precisely among those it was not designed to affect, while high-need individuals—the intended beneficiaries—are largely insulated by institutional safety nets and exhibit neither strategic deferral nor downstream deterioration.

These findings carry two broader implications. First, standard regression discontinuity estimates of price elasticity at anticipated thresholds conflate intertemporal substitution with structural demand, systematically overstating long-run price sensitivity. Our dynamic event study resolves this by showing that both values—the immediate elasticity of -0.200 (consistent with Shigeoka (2014)) and the steady-state elasticity (reported as -0.06 by Komura and Bessho (2025) and estimated at -0.088 in our analysis)—emerge naturally from the same adjustment process: the immediate elasticity is inflated by pent-up demand, while the steady-state elasticity reflects the genuine long-run price response once deferred demand is absorbed. Second, the welfare costs of cost-sharing design extend beyond the direct rationing effects studied in the existing literature: even a well-intentioned reduction in copayments can generate hidden health costs if structured in a way that concentrates intertemporal incentives at a single threshold.

Universal health insurance represents one of the most consequential policy achievements of the modern welfare state. Our findings do not challenge its value—they underscore that its benefits depend critically on design. The structure of cost-sharing rules, not just their generosity, determines whether a well-intentioned policy achieves its goals or generates unintended consequences that undermine them. The gap between individual rationality and collective welfare that we document is not an argument against universal coverage—it is a precise and actionable diagnosis of how coverage can be made to work better.

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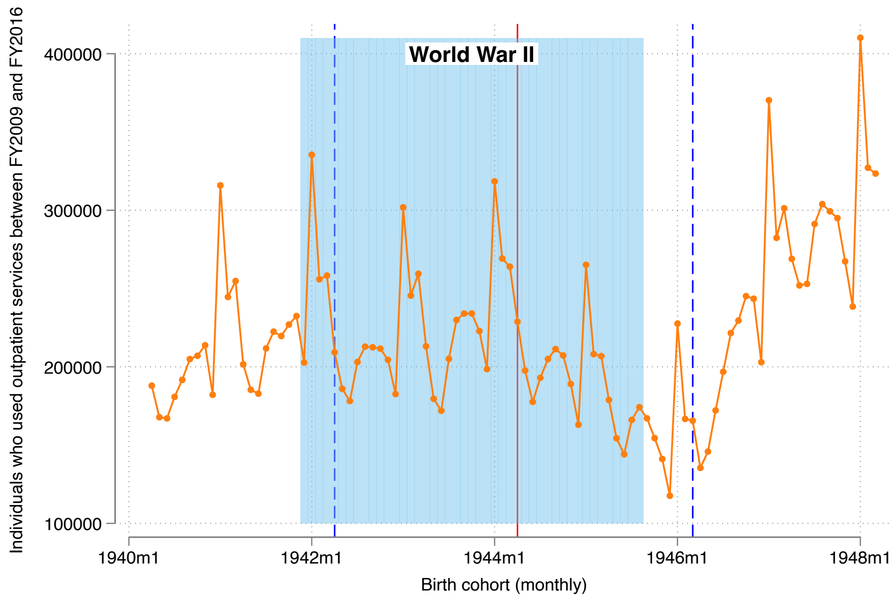
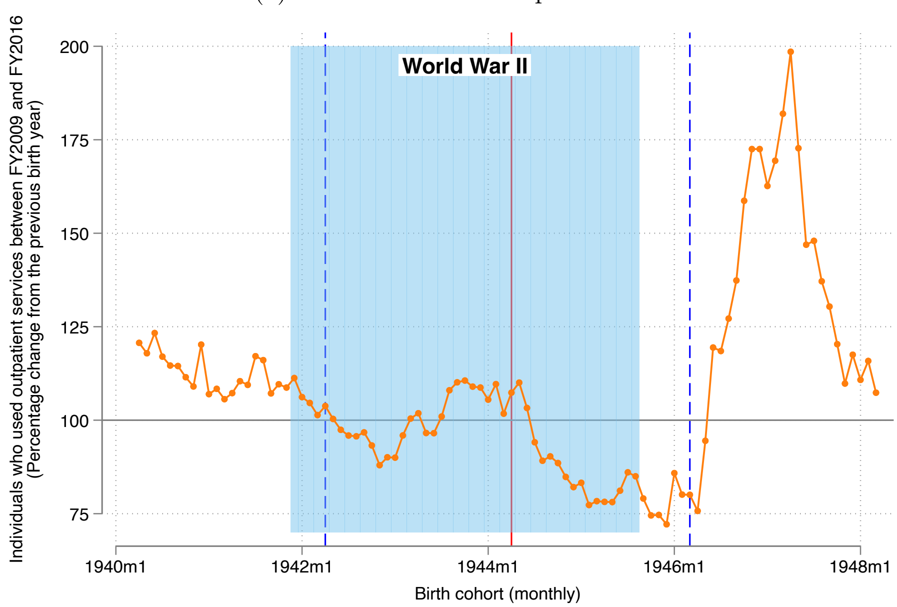
Declaration of Generative AI in the writing process: During the preparation of this manuscript, the authors used Gemini (Google) and Claude (Anthropic) to refine the English language and improve the overall readability of the work. This assistance was limited to stylistic and linguistic adjustments and did not involve the generation of original research ideas or data analysis. Following these services, the authors reviewed and edited the content and take full responsibility for the final version of the publication.



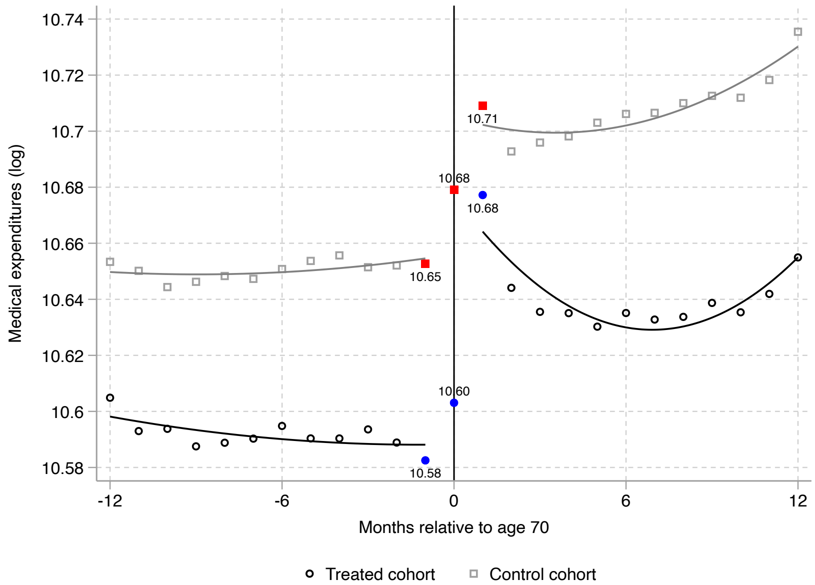
(a) Absolute number of patients



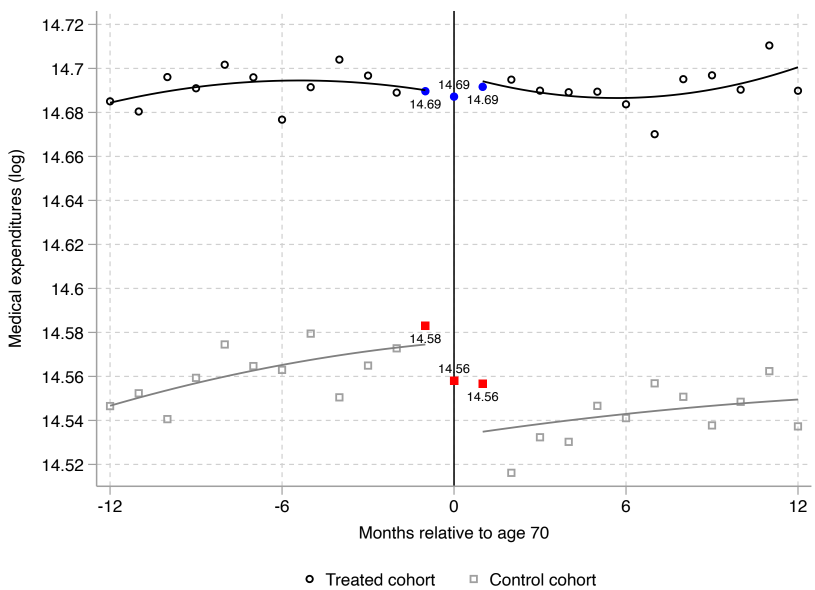
(b) Percentage change from the previous birth year (%)

Figure 1: Individuals Who Used Outpatient Services between FY2009 and FY2016, by Birth Cohort

Note: The figure displays the number of patients utilizing outpatient services by birth cohort (month of birth). Panel (a) presents the absolute number of patients, while Panel (b) depicts the percentage change in the number of outpatient users from the corresponding cohort of the previous year, adjusted for the inherent seasonality of births. The red vertical line indicates the policy cutoff birth cohort of April 1944. The two blue vertical lines represent the boundaries of the analytical sample: April 1942 and March 1946. The light blue shaded area indicates birth cohorts born during the period corresponding to World War II.



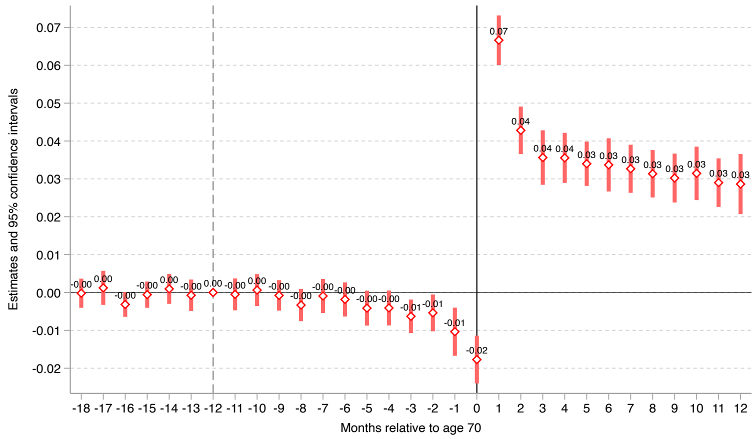
(a) Outpatient services



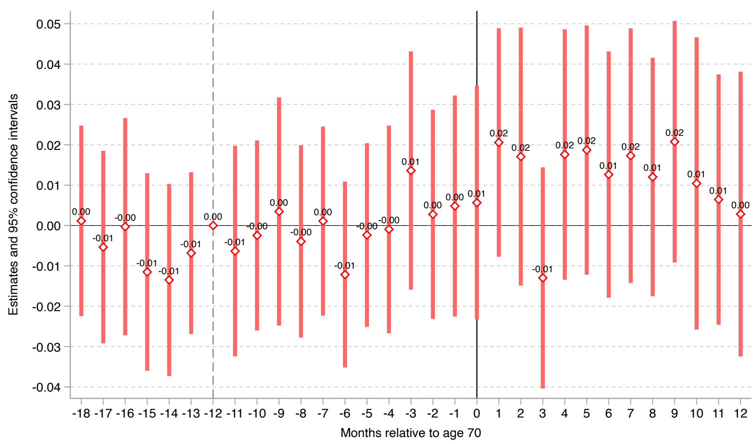
(b) Inpatient care

Figure 2: Monthly Healthcare Expenditures by Age and Treatment Status (Logged Values)

Note: The figure plots the average log of monthly healthcare expenditures by age (in months) for each birth-month cohort, separately for Panel(a) outpatient services and Panel(b) inpatient care. The circles represent the treated cohort (born between April 1942 and March 1944), and the squares represent the control cohort (born between April 1944 and March 1946). The solid lines indicate quadratic polynomial fits estimated separately for the periods before and after the age-70 threshold. The data point at exactly age 70 is excluded from the quadratic polynomial fits to account for the mid-month transition of the copayment rate, whereby the reduction applies from the birth month for those born on the first day of the month and from the following month for all others. The blue and red circles highlight the period from Month -1 (Age 69 years and 11 months) to Month 1 (70 years and 1 month) to illustrate the immediate impact of the policy change. The vertical dashed line indicates the age-70 threshold (Month 0).



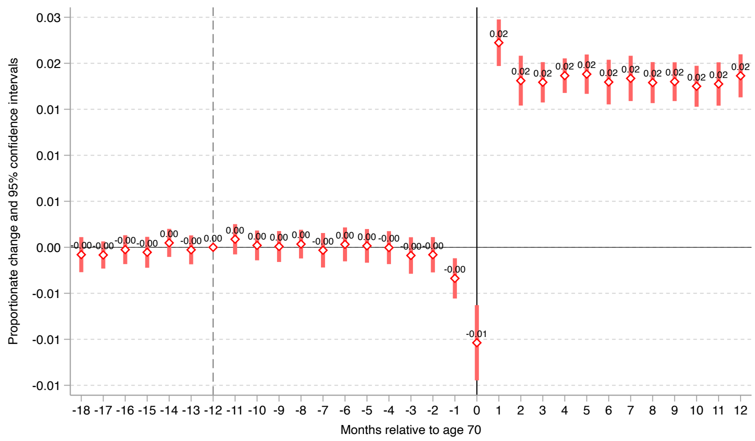
(a) Outpatient services



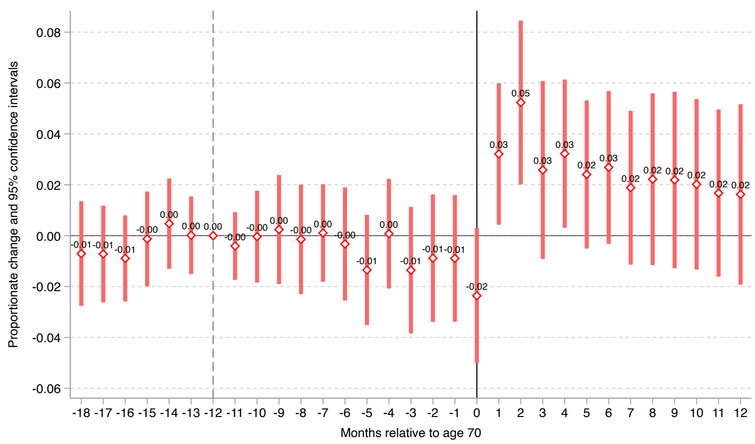
(b) Inpatient care

Figure 3: Results of Event Study Model: Intensive Margin

Note: The figure presents the coefficient estimates and 95% confidence intervals from the event study analysis for outpatient expenditures (Panel (a)) and inpatient expenditures (Panel (b)). The dependent variable is the log of monthly healthcare expenditures. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level.



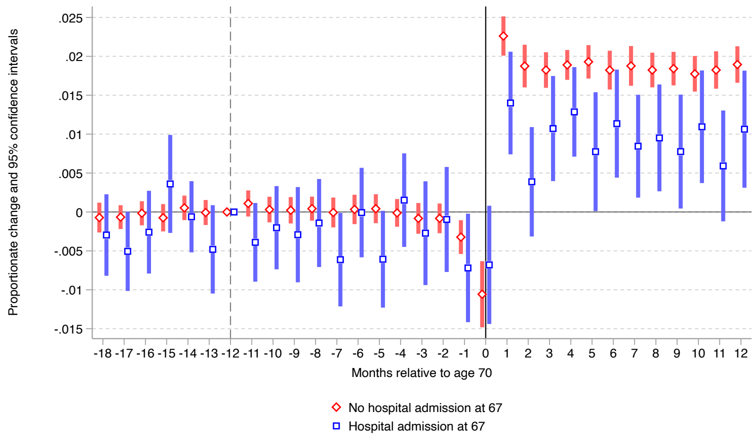
(a) Office visit



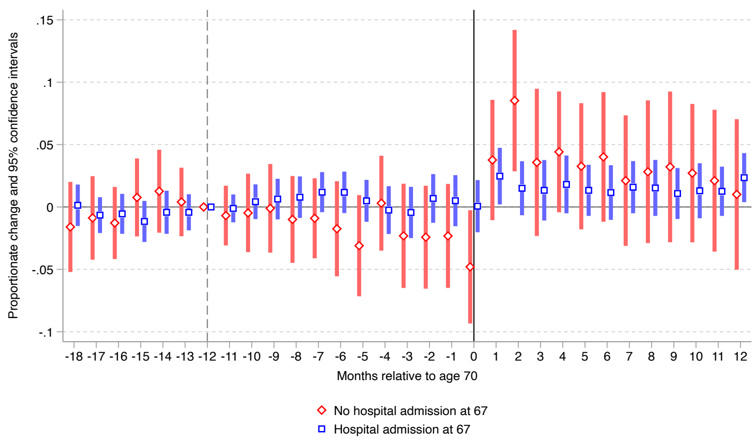
(b) Hospital admission

Figure 4: Results of Event Study Model: Extensive Margin

Note: The figure presents the proportionate change and 95% confidence intervals for the probability of any outpatient visit (Panel (a)) and inpatient admission (Panel (b)). The analysis is conducted using a balanced panel of individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The proportionate change is calculated by dividing the estimated coefficients by the pre-treatment mean of the dependent variable for the treatment group. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. The corresponding raw coefficient estimates (β_k) and their confidence intervals are reported in Appendix B.1.



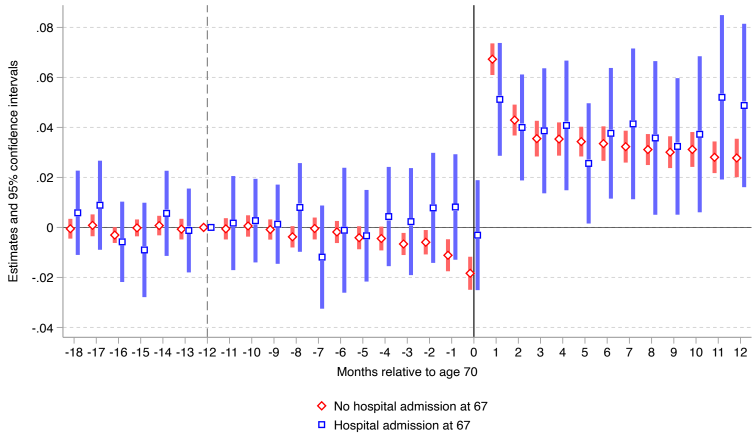
(a) Office visit



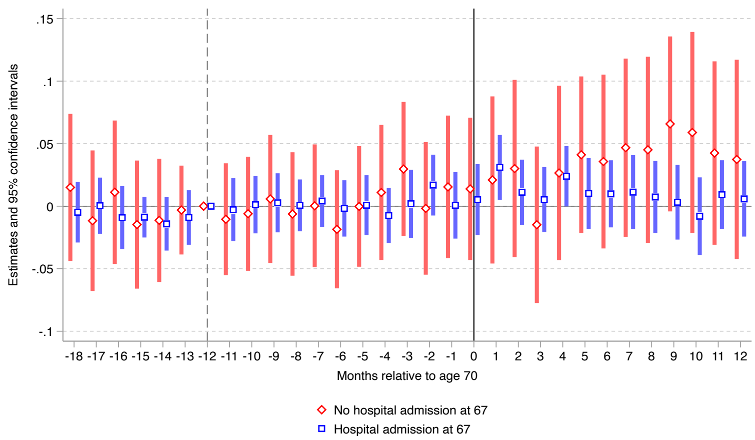
(b) Hospital admission

Figure 5: Results of Event Study Model: Extensive Margin by Prior Health Status

Note: The figure presents the proportionate change and 95% confidence intervals for the probability of any outpatient visit (Panel (a)) and inpatient admission (Panel (b)). The analysis is conducted using a balanced panel of individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The proportionate change is calculated by dividing the estimated coefficients by the pre-treatment mean of the dependent variable for the treatment group. The sample is stratified into two groups based on hospital admission history during the one-year period from age 67 years 0 months to 67 years 11 months: individuals without prior hospital admissions (red diamonds) and individuals with at least one prior hospital admission (blue squares). The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. The corresponding raw coefficient estimates (β_k) and their confidence intervals are reported in Appendix B.1.



(a) Outpatient services



(b) Inpatient care

Figure 6: Results of Event Study Model: Intensive Margin by Prior Health Status

Note: The figure presents the coefficient estimates and 95% confidence intervals from the event study analysis for outpatient expenditures (Panel (a)) and inpatient expenditures (Panel (b)), stratified by prior health status. The dependent variable is the log of monthly healthcare expenditures. The sample is stratified into two groups based on hospital admission history during the one-year period from age 67 years 0 months to 67 years 11 months: individuals without prior hospital admissions (red diamonds) and individuals with at least one prior hospital admission (blue squares). The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level.

Table 1: Summary Statistics (Patients Aged between 68 Years and 6 Months and 71 Years)

	Outpatient visit		Inpatient care	
	(1)	(2)	(3)	(4)
	Whole	Apr.1942–Mar.1946	Whole	Apr.1942–Mar.1946
Age in year	69.74	69.77	69.76	69.78
Female ratio	0.55	0.56	0.46	0.46
Number of observations (patients × monthly date)	261,208,224	120,951,952	5,323,906	2,471,886

Note: The unit of observation is the individual month. The table reports the average age in years and the female ratio for patients aged between 68 years and 6 months and 71 years, separately for outpatient visits (Columns (1) and (2)) and inpatient care (Columns (3) and (4)), where Columns (2) and (4) restrict the sample to individuals born between April 1942 and March 1946—the analytical sample used in the main analysis—for outpatient visits and inpatient care, respectively.

Table 2: Healthcare Expenditures between the Treated and Control cohort

	Outpatient visit				Inpatient care			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treated	Control	Difference	%Δ	Treated	Control	Difference	%Δ
Mean	38,882	40,996	-2,114	-5.3	2,400,003	2,296,533	103,470	4.4
Percentiles								
5th	676	760	-84	-11.7	109,191	75,283	33,908	36.8
10th	1,404	1,504	-100	-6.9	205,316	167,736	37,580	20.1
25th	3,052	3,374	-322	-10.0	352,320	317,790	34,530	10.3
Median	8,330	8,550	-220	-2.6	915,804	860,409	55,394	6.2
75th	26,300	27,301	-1,001	-3.7	2,180,032	2,061,505	118,527	5.6
90th	70,669	73,102	-2,433	-3.4	5,666,693	5,501,186	165,507	3.0
95th	134,610	140,391	-5,781	-4.2	9,306,171	9,118,874	187,297	2.0

Note: All values are reported in Japanese Yen (JPY). The sample consists of monthly healthcare expenditures for individuals born between April 1942 and March 1946, measured at ages 68.5 to 69 years—the pre-threshold period before any anticipatory behavioral responses are expected to emerge. Columns (1) and (5) report mean and percentile statistics for the treated cohort, while Columns (2) and (6) report the corresponding statistics for the control cohort, separately for outpatient visits and inpatient care. Columns (3) and (7) present the absolute differences between the treated and control cohorts. Columns (4) and (8) report the normalized percentage differences for each statistic, calculated as $(T - C)/[(T + C)/2]$, where T and C denote the treated and control cohort values, respectively.

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Appendix

A Details of the Sample Restriction Procedure

We construct our analytical sample through a systematic six-step restriction process using the National Database of Health Insurance Claims (NDB). As illustrated in Figure A.1, we initially extract 3,051,278,961 claim cases (87,388,538 patients) for outpatient visits and 95,965,723 claim cases (20,034,820 patients) for inpatient care for individuals aged 55 and older between FY2009 and FY2016.

To focus on the behavioral response to the age-70 copayment transition, we first restrict the sample to individuals born between April 1942 and March 1946 (Step 2). We then limit our observation window to the period between age 68.5 and 71 years (Step 3), resulting in 128,216,543 claim cases for outpatient and 2,491,430 inpatient claim cases.

A critical step for our identification strategy is the exclusion of high-income earners who maintain a 30% copayment rate beyond age 70. Unlike the general elderly population who experience a reduction to 10% or 20%, these individuals possess systematically higher income levels. Using this group as a control would likely introduce selection bias, as their healthcare-seeking behavior and underlying health investments may differ significantly due to their greater financial resources. By excluding them, we ensure that our treatment and control groups are more socioeconomically homogeneous, thereby isolating the behavioral response to the price reduction from any income-driven selection effects.

Finally, to implement our event study framework, we restrict the sample to patients with multiple data records (Step 6). This yields a final analytical sample of 120,174,780 claim cases from 6,753,900 outpatient individuals and 2,067,857 claim cases from 384,489 inpatient individuals. This longitudinal structure is essential for capturing the dynamic utilization patterns around the age-70 threshold.

B Other Event Study Results

B.1 Raw Event Study Estimates for the Extensive Margin Analysis

This appendix provides the raw coefficient estimates and their associated 95% confidence intervals from the event study model for the extensive margin analysis (Figures B.1 and B.2). While the main text reports the results in terms of proportionate changes (relative to the pre-treatment mean) to facilitate interpretation of the economic magnitude, the figures below present the underlying point estimates (β_k) in levels. These coefficients represent the month-specific changes in the probability of seeking care for the treatment group relative to the control group, baseline-adjusted to the reference month (Month -12). The statistical precision shown here underscores the robustness of the strategic timing and the subsequent surges in utilization documented in Section 5.

B.2 Robustness Checks

In this appendix, we present supplementary analyses to verify the robustness of our main event study results presented in Section 5.2. Specifically, we examine the sensitivity of our estimates to two critical modeling choices: the selection of the reference period and the bandwidth of the sample cohorts.

B.2.1 Sensitivity to Reference Period

In our baseline event study specification, we set Month -12 (the month of turning age 69) as the reference period, normalizing the coefficient for this month to zero. This choice effectively measures the utilization levels relative to a baseline 12 months before the copayment reduction takes effect. To ensure that our findings—particularly the pre-trend stability and the sharp changes around age 70—are not artifacts of this specific normalization, we re-estimated the model using Month -6 (age 69 and 6 months) as an alternative reference period.

Figures B.3, B.4, and B.5 compare the estimates from the baseline specification (red diamonds) with those from the alternative specification (blue squares). As shown in the figures, the two series exhibit virtually identical patterns. Most importantly, the key behavioral responses identified in our main analysis—the significant dip at Month 0 (indicative of care deferral) and the sharp surge at Month 1 (indicative of pent-up demand)—remain quantitatively stable and statistically significant regardless of the reference period. This consistency confirms that the observed dynamics are driven by the policy change at age 70 rather than by the choice of the reference month.

B.2.2 Sensitivity to Sample Cohort Selection

Our identification strategy relies on the sharp discontinuity in policy application across birth cohorts. The baseline analysis utilizes a sample of individuals born between April 1942 and March 1946 (a 4-year window centered around the policy cutoff). A potential concern with using a relatively wide window is that cohort-specific shocks or broader macroeconomic trends unrelated to the policy might influence the estimates.

To address this concern, we conducted a sensitivity analysis by narrowing the sample window around the policy cutoff. Specifically, we re-estimated the model using two restricted samples: (i) a narrower window comprising individuals born between January 1943 and July 1945, and (ii) a highly restricted window comprising those born between April 1943 and March 1945 (a 2-year window centered on the cutoff).

Figures B.6, B.7, and B.8 present the results. The estimates from the restricted samples (blue squares and green X-marks) closely track the baseline estimates (red diamonds) throughout the entire observation period. The magnitudes of the coefficients at the critical months (Month 0 and Month 1) show minimal variation across specifications. The stability of the estimates, even when excluding cohorts further from the cutoff, provides strong evidence that our results are identified by the discontinuity at the policy threshold and are not driven by unobserved trends specific to certain birth cohorts.

B.3 Tables of Event Study Results

Tables B.1, B.2, and B.4 provide the numerical point estimates and standard errors underlying the event study results visualized in the figures. Tables B.3 and B.5 provide the numerical point estimates and standard errors for the proportionate change results for the extensive margin analysis. The proportionate change is calculated by dividing the estimated coefficients by the pre-treatment mean of the dependent variable for the treatment group.

C Difference in Differences Analysis for Inpatient Care

To address the infrequent nature of inpatient utilization, we complement our monthly event study with a Difference-in-Differences (DID) analysis. Monthly-level event study models often encounter difficulties in capturing structural shifts in inpatient care due to the high variance and sparsity of such events at a granular time scale. By partitioning the post-threshold period into four distinct three-month intervals (Months 1-3, 4-6, 7-9, and 10-12), we effectively reduce statistical noise and obtain more stable estimates of the dynamic behavioral response.

This aggregation is particularly instrumental in capturing the precise trajectory of the deterioration mechanism. As illustrated in Figure C.1,[1] the post-reform response for the healthy subgroup (no prior hospital admission) is initially modest and statistically insignificant (0.3% in Months 1-3). However, this is followed by a sharp and statistically significant escalation: expenditures rise by 2.8% in Months 4–6 ($p < 0.10$), peak at a 4.3% surge during Months 7–9 ($p < 0.05$), and remain elevated at 3.6% in Months 10–12 ($p < 0.05$). In stark contrast, the group with prior admissions exhibits a persistently flat trajectory near zero across all periods. This DID framework confirms that the delayed escalation in inpatient expenditures among the healthy group is not a transient monthly fluctuation, but a robust structural consequence of prior strategic deferral—consistent with the deterioration mechanism identified in Section 5: conditions left unmanaged during the pre-threshold deferral period progress to the point of requiring intensive inpatient care in the months that follow.

D Monthly Healthcare Expenditures by Age and Birth Cohort (Logged Values)

This appendix provides a comprehensive discussion of the dynamic patterns observed in the descriptive plots for log-transformed outpatient and inpatient expenditures (Figures D.1 and D.2). The

[1] Table C.1 provides the numerical point estimates and standard errors underlying the event study results visualized in the figures.

analysis covers a 25-month window spanning from Month -12 (Age 69) to Month 12 (Age 71), with the central policy intervention occurring at Month 0 (Age 70). The following interpretation is based on the visual patterns of the point estimates across the birth cohorts, with particular attention to the mid-month transition at the age-70 threshold.

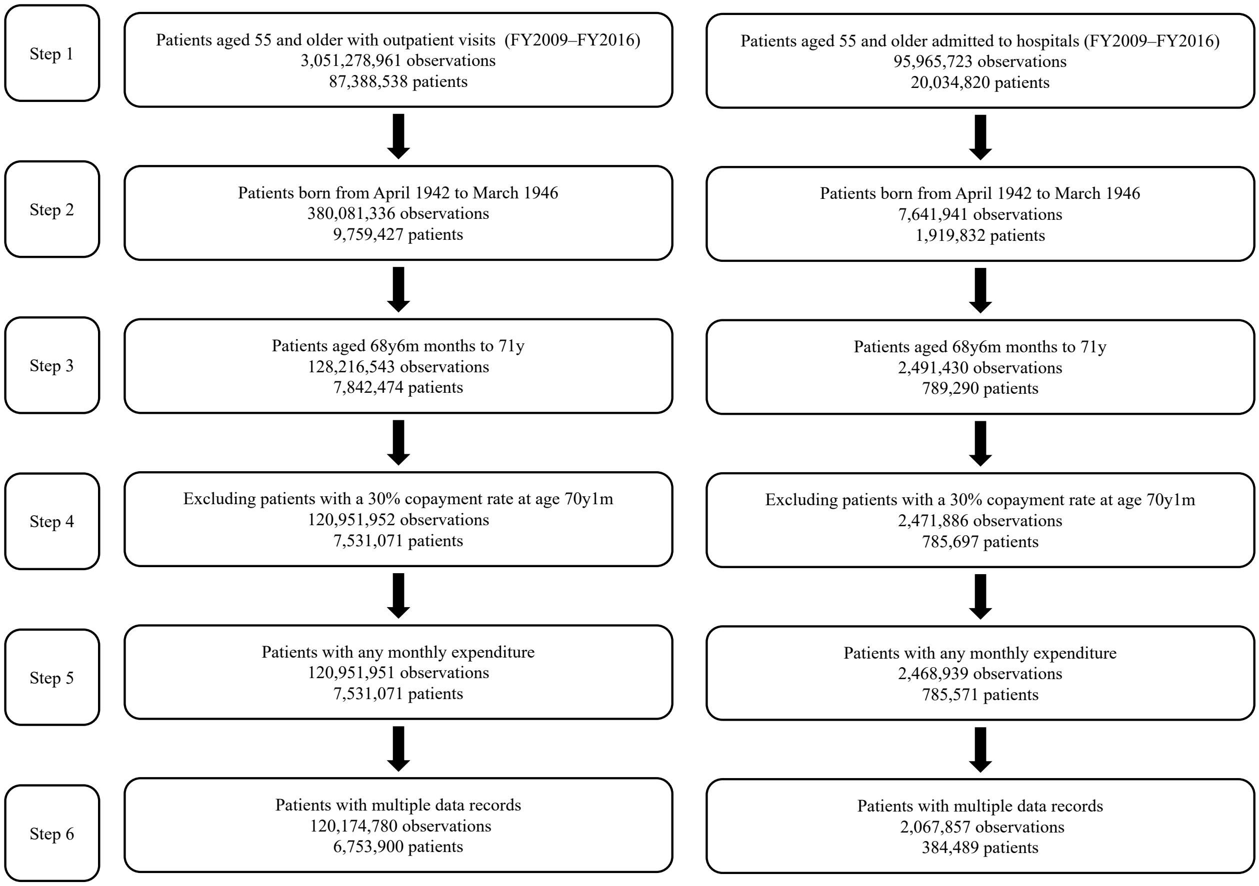
The baseline period, spanning from Month -12 to Month -11, serves as a crucial check for the baseline comparability between the cohorts. During these initial months, both the treatment and control cohorts were subject to a uniform 30% copayment rate. The plots for both outpatient and inpatient expenditures exhibit a high degree of smoothness at the cutoff, with no discernible discontinuities in the point estimates. This visual evidence suggests that there are no systematic differences in underlying health status or baseline medical demand between the cohorts, thereby supporting the identification assumption that any subsequent divergence is attributable to the policy change.

A critical feature of this analysis is the treatment of Month 0, the month in which individuals turn 70. Because the transition to the lower copayment rate occurs on the day following an individual's 70th birthday (or on the birthday itself if born on the first of the month), the specific copayment rate within this month is determined by the day of birth. This results in a mixture of pre-reform (30%) and post-reform (10% for the treatment group and 20% for the control group) rates within the same month. It is important to note that this mixing rule is applied consistently across both the treatment and control cohorts based on their respective birth dates. To properly disentangle these mixing effects and identify the true policy impact, this paper employs a dynamic event study design. Rather than excluding the transition month, the event study framework accounts for the heterogeneous timing of the copayment change and provides a more refined estimation of the treatment effects.

In the outpatient sector, the point estimates show a noticeable downward shift in the months immediately preceding the threshold, particularly as they approach Month 0. This pattern is indicative of strategic care deferral, consistent with individuals who postponed non-urgent medical visits to take advantage of the significantly lower copayment rates. Upon the implementation of the new rates in Month 1 (Age 70 years and 1 month), which is the first full month under the new regime, a clear gap opens between the two groups. The 20%-copayment group exhibits lower expenditures than the 10%-group, providing qualitative evidence of the price elasticity of outpatient demand.

By the time the cohorts reach Month 12, 11 months after the copayment reduction, the large gaps observed during the transition period largely disappear, and the expenditures for both groups converge toward a steady state. This convergence reflects two forces operating simultaneously: the absorption of the initial backlog of elective procedures whose timing was strategically deferred, and the influence of the High-Cost Medical Expense Benefit System. Since many treatments, especially inpatient care, reach the monthly out-of-pocket cap, the marginal price difference between 10% and 20% copayment becomes negligible, leading to similar utilization levels in the long run.

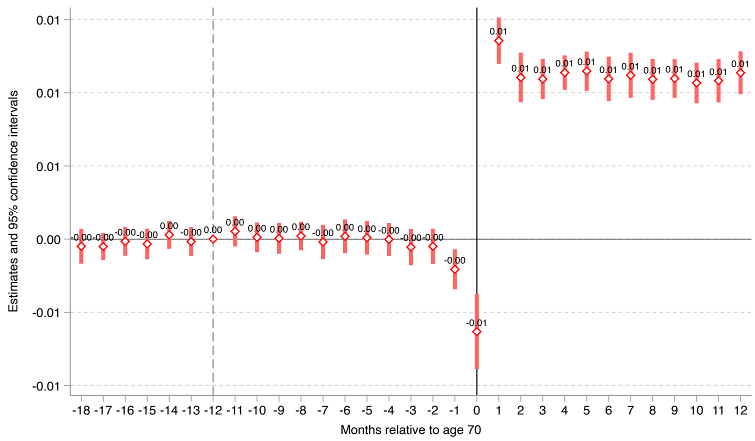
Importantly, the results from this full sample analysis provide further insights into aggregate pattern of the deterioration mechanism documented in Section 5. While the 20%-copayment group demonstrated a clear reduction in outpatient utilization initially, there is no evidence of a subsequent increase in inpatient expenditures for this group by the Month 12 mark. This lack of a compensatory spike in hospitalizations suggests that, for the aggregate population, the copayment structure did not lead to health deterioration severe enough to necessitate additional inpatient care. This is consistent with the finding in Section 5.3 that the deterioration mechanism is concentrated among the relatively healthy subgroup rather than the aggregate population—a pattern that aggregate descriptive plots such as these are not designed to detect, and that requires the subgroup event study analysis presented in the main text to identify. The event study analysis, by controlling for individual-level factors and disentangling the transition effects, further reinforces these findings by providing statistically grounded evidence of the long-term impacts across both healthcare margins.



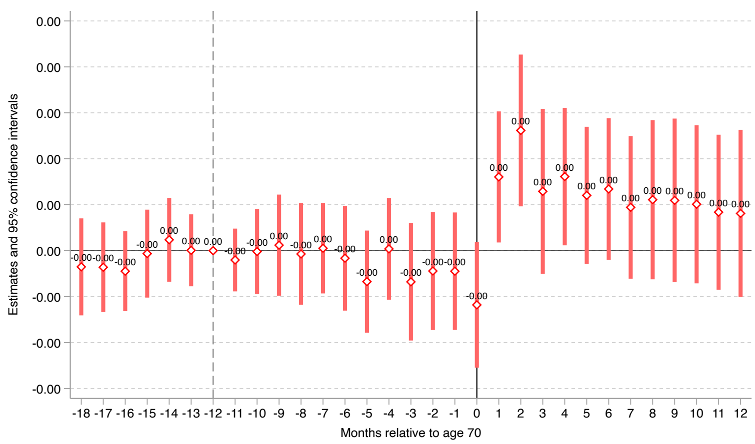
(a) Outpatient visits (b) Inpatient cares

Figure A.1: Sample Restriction Procedure

Note: This figure illustrates the step-by-step sample restriction process from the National Database of Health Insurance Claims (NDB), separately for (a) outpatient visits and (b) inpatient care. Step 1 defines the initial study population of individuals aged 55 and older from FY2009 to FY2016. Step 2 restricts the sample to the analytical birth cohorts (April 1942–March 1946) to focus on individuals experiencing the age-70 rate transition. Step 3 limits the observation window to the period around age-70 threshold (age 68.5 to 71). Step 4 excludes high-income earners who maintain a 30% copayment rate beyond age 70 years and 1 month, since their healthcare-seeking behavior and underlying health investments may differ systematically from the general elderly population due to their greater financial resources, thereby isolating the treated and control groups whose cost-sharing burden decreases at age 70. Step 5 retains individuals with any monthly expenditure, ensuring that the sample consists of active users of the healthcare system. The final sample in Step 6 consists of individuals with multiple data records, allowing for the dynamic event study analysis of utilization patterns around the age-70 threshold.



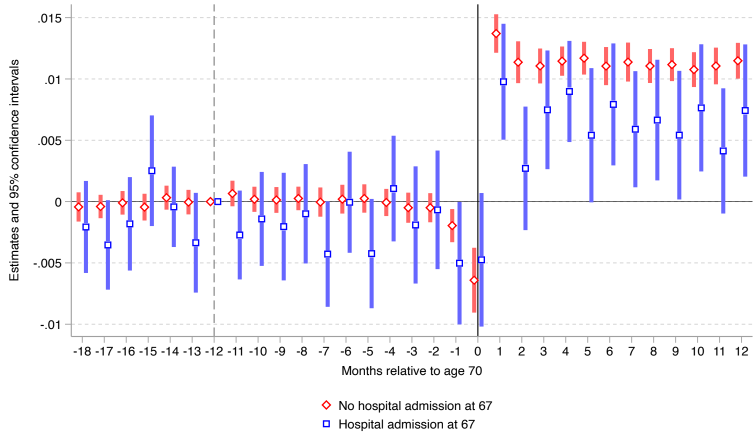
(a) Office visit



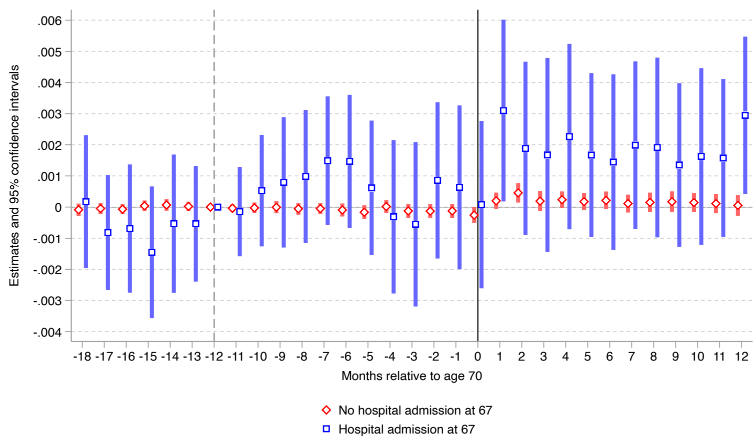
(b) Hospital admission

Figure B.1: Estimated Coefficients of Event Study Model: Extensive Margin

Note: The figure presents the raw coefficient estimates (β_k) and 95% confidence intervals for the probability of any outpatient visit (Panel (a)) and inpatient admission (Panel (b)). These are the underlying point estimates in levels corresponding to the proportionate changes presented in Figure 4 of the main text. The analysis is conducted using the balanced panel of 5,777,540 individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The vertical axis represents the month-specific change in the probability of seeking care for the treatment group relative to the control group, baseline-adjusted to the reference period. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level.



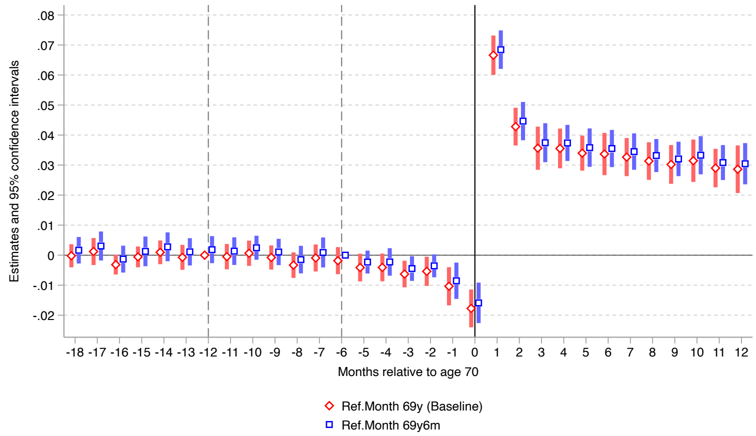
(a) Office visit



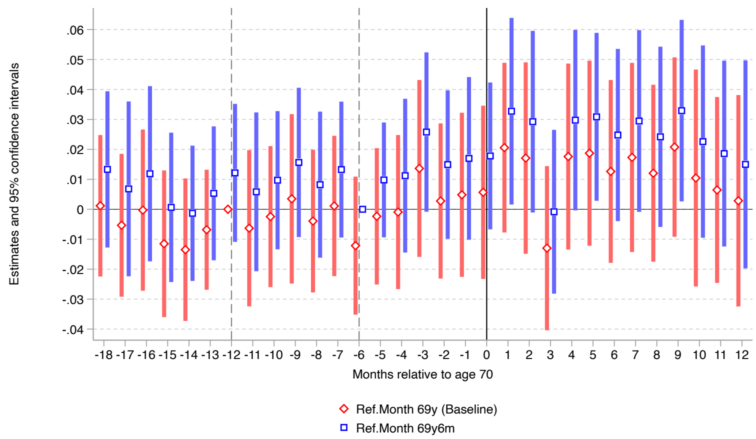
(b) Hospital admission

Figure B.2: Estimated Coefficients of Event Study Model: Extensive Margin by Prior Health Status

Note: The figure presents the raw coefficient estimates (β_k) and 95% confidence intervals for the extensive margin, stratified by prior health status. Panel (a) shows the probability of any outpatient visit, and Panel (b) shows the probability of any inpatient admission. The analysis is conducted using the balanced panel of 5,777,540 individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The red diamonds represent the "Without Prior Admission" group (relatively healthy individual with no recorded inpatient expenditures during the one-year period from age 67 years 0 months to 67 years 11 months), and the blue squares represent the "With Prior Admission" group (individuals with at least one recorded inpatient expenditure during the same period). These coefficients serve as the basis for the corresponding heterogeneous proportionate changes presented in Figure 5 of the main text. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level.



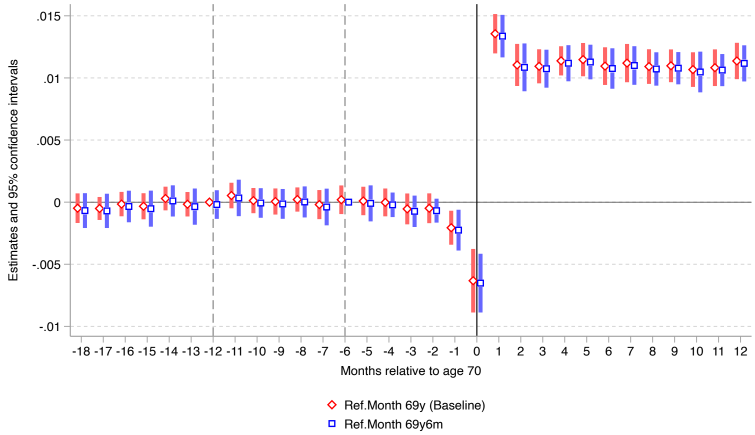
(a) Outpatient services



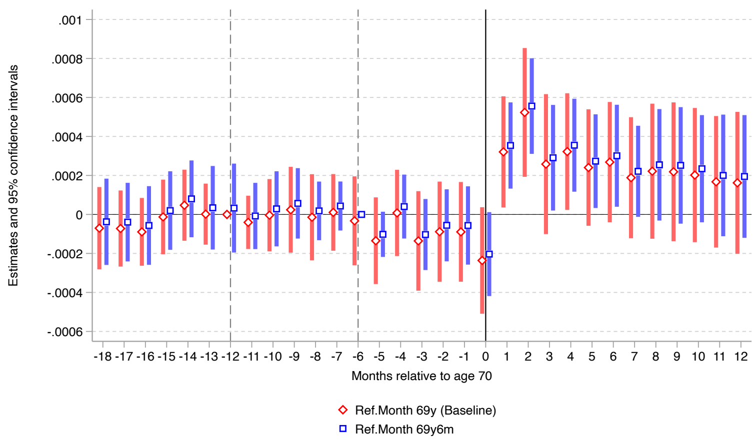
(b) Inpatient care

Figure B.3: Robustness Checks: Sensitivity to Reference Period (Intensive margin)

Note: The figure presents the coefficient estimates and 95% confidence intervals from the event study analysis for outpatient expenditures (Panel(a)) and inpatient expenditures (Panel(b)). The dependent variable is the log of monthly healthcare expenditures. The red diamonds indicate the baseline specification using Month -12 as the reference period. The blue squares indicate the alternative specification using Month -6 as the reference period. The two specifications are otherwise identical, and the close correspondence between the red and blue series across all months confirms that the main results are not sensitive to the choice of reference period. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold. The gray vertical dashed lines indicate the two reference periods (Month -12 for the baseline specification and Month -6 for the alternative specification), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level.



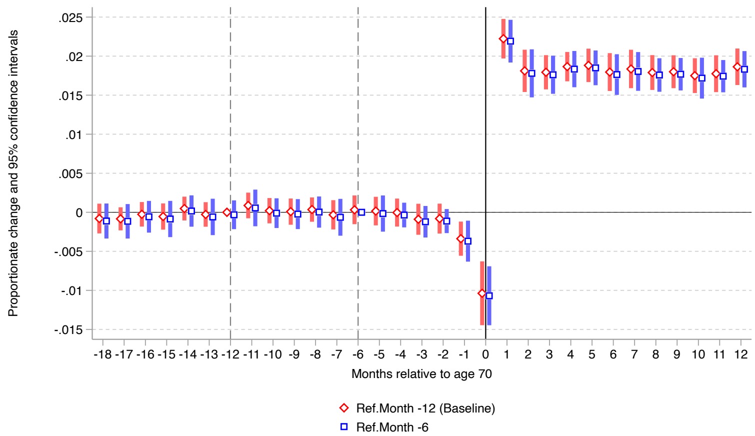
(a) Office visit



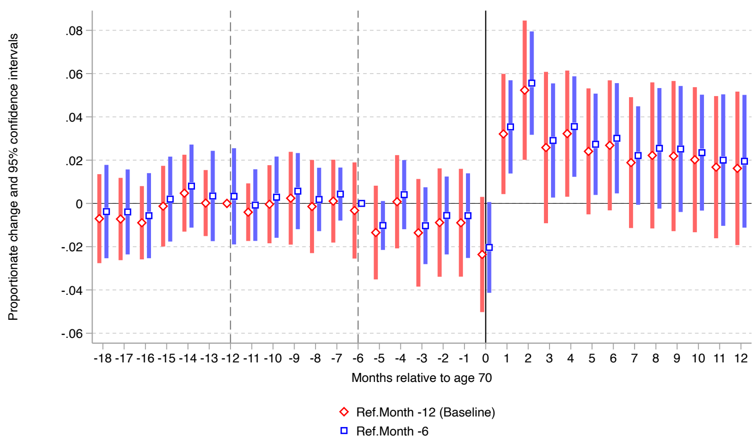
(b) Hospital admission

Figure B.4: Robustness Checks: Sensitivity to Reference Period (Extensive Margin)

Note: The figure presents the raw coefficient estimates (β_k) and 95% confidence intervals from the event study analysis for the extensive margin analysis, specifically the probability of any outpatient visit (Panel(a)) and inpatient admission (Panel(b)). The analysis is conducted using the balanced panel of 5,777,540 individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The red diamonds indicate the baseline specification using Month -12 as the reference period. The blue squares indicate the alternative specification using Month -6 as the reference period. The two specifications are otherwise identical, and the close correspondence between the red and blue series across all months confirms that the extensive margin results are not sensitive to the choice of reference period. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold. The gray vertical dashed lines indicate the two reference periods (Month -12 for the baseline specification and Month -6 for the alternative specification), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level.



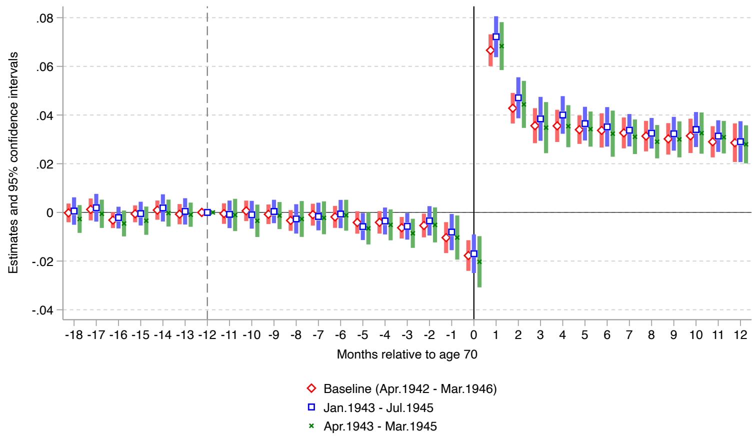
(a) Office visit



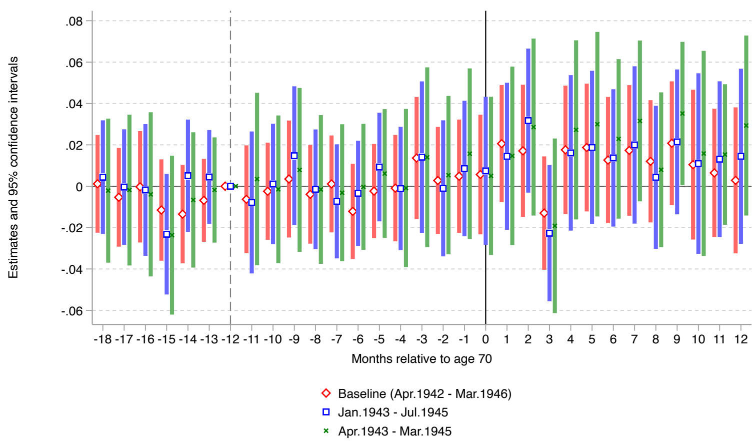
(b) Hospital admission

Figure B.5: Robustness Checks: Sensitivity to Reference Period (Extensive Margin, Proportionate Change)

Note: The figure presents the proportionate change and 95% confidence intervals from the event study analysis for the extensive margin, specifically the probability of any outpatient visit (Panel (a)) and inpatient admission (Panel (b)). The analysis is conducted using the balanced panel of 5,777,540 individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The proportionate change is calculated by dividing the estimated coefficients by the pre-treatment mean of the dependent variable for the treatment group. The red diamonds indicate the baseline specification using Month -12 as the reference period. The blue squares indicate the alternative specification using Month -6 as the reference period. The two specifications are otherwise identical, and the close correspondence between the red and blue series across all months confirms that the extensive margin results are not sensitive to the choice of reference period. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold. The gray vertical dashed lines indicate the two reference periods (Month -12 for the baseline specification and Month -6 for the alternative specification), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. The corresponding raw coefficient estimates (β_k) and their confidence intervals are reported in Figure B.4.



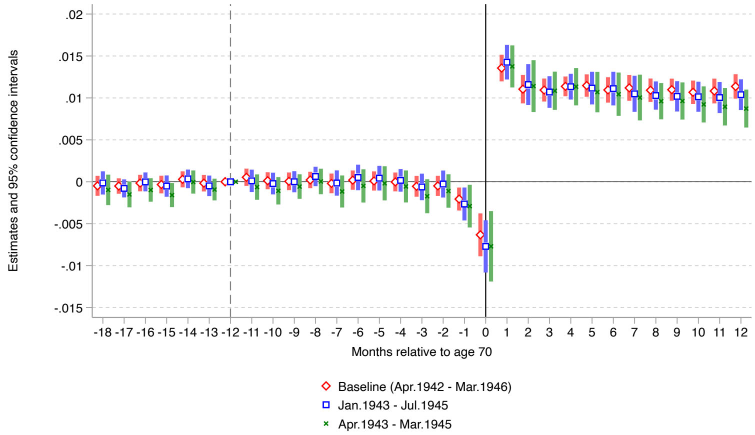
(a) Outpatient services



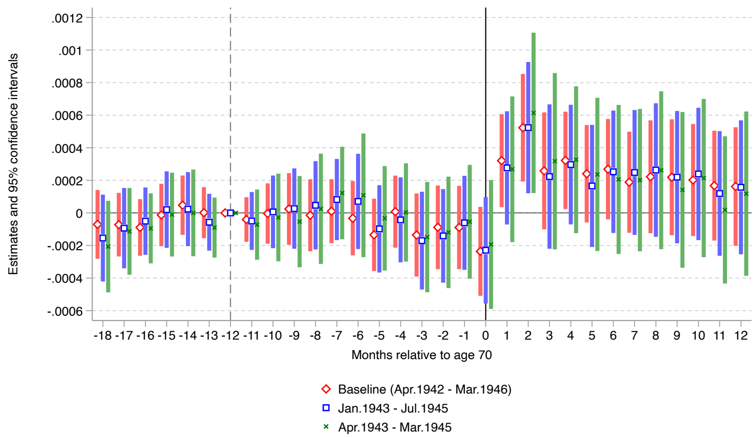
(b) Inpatient care

Figure B.6: Robustness Checks: Sensitivity to Sample Cohort Selection (Intensive Margin)

Note: The figure presents the coefficient estimates and 95% confidence intervals from the event study analysis for outpatient expenditures (Panel (a)) and inpatient expenditures (Panel (b)). The dependent variable is the log of monthly healthcare expenditures. The red diamonds indicate the baseline sample of individuals born between April 1942 and March 1946 (a 4-year window centered around the policy cutoff). The blue squares indicate the narrower sample restricted to individuals born between January 1943 and July 1945. The green X-marks indicate the most restrictive sample comprising individuals born between April 1943 and March 1945 (a 2-year window centered on the cutoff). The close correspondence between the three series across all months—and in particular at Month 0 and Month 1, where the key behavioral responses are concentrated—confirms that the main results are not sensitive to the choice of sample cohort bandwidth. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level.



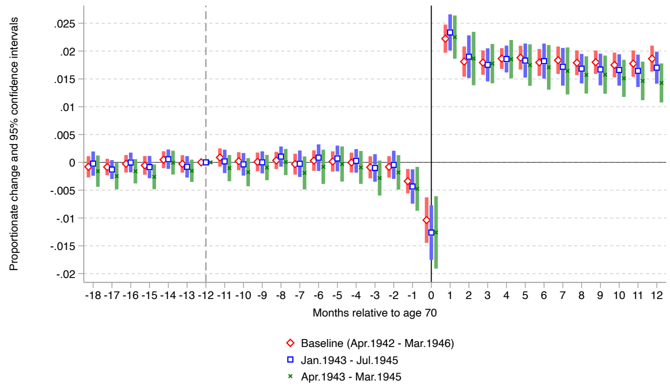
(a) Outpatient services



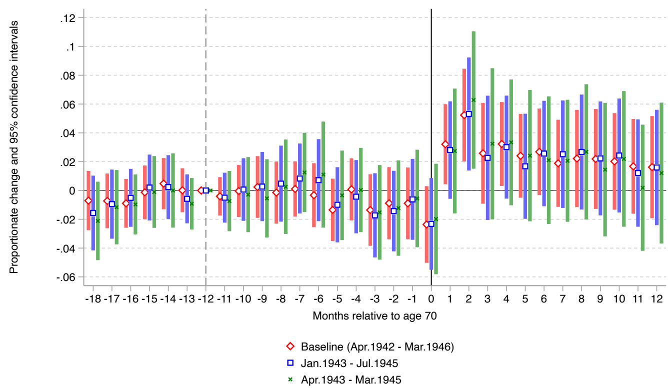
(b) Inpatient care

Figure B.7: Robustness Checks: Sensitivity to Sample Cohort Selection (Extensive Margin)

Note: The figure presents the raw coefficient estimates (β_k) and 95% confidence intervals from the event study analysis for the extensive margin, specifically the probability of any outpatient visit (Panel (a)) and inpatient admission (Panel (b)). The analysis is conducted using the balanced panel of 5,777,540 individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The red diamonds indicate the baseline sample of individuals born between April 1942 and March 1946 (a 4-year window centered around the policy cutoff). The blue squares indicate the sample restricted to individuals born between January 1943 and July 1945. The green X-marks indicate the most restrictive sample comprising individuals born between April 1943 and March 1945 (a 2-year window centered on the cutoff). The close correspondence between the three series across all months—and in particular at Month 0 and Month 1, where the key behavioral responses are concentrated—confirms that the extensive margin results are not sensitive to the choice of sample cohort bandwidth. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level.



(a) Outpatient services



(b) Inpatient care

Figure B.8: Robustness Checks: Sensitivity to Sample Cohort Selection (Extensive Margin, Proportionate Change)

Note: The figure presents the proportionate change and 95% confidence intervals from the event study analysis for the extensive margin analysis, specifically the probability of any outpatient visit (Panel (a)) and inpatient admission (Panel (b)). The analysis is conducted using the balanced panel of 5,777,540 individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The proportionate change is calculated by dividing the estimated coefficients by the pre-treatment mean of the dependent variable for the treatment group. The red diamonds indicate the baseline sample of individuals born between April 1942 and March 1946 (a 4-year window centered around the policy cutoff). The blue squares indicate the narrower sample restricted to individuals born between January 1943 and July 1945. The green X-marks indicate the most restrictive sample comprising individuals born between April 1943 and March 1945 (a 2-year window centered on the cutoff). The close correspondence between the three series across all months—and in particular at Month 0 and Month 1, where the key behavioral responses are concentrated—confirms that the extensive margin results are not sensitive to the choice of sample cohort bandwidth. The horizontal axis represents the number of months relative to age 70, where Month 0 denotes the age-70 threshold, and Month -12 serves as the reference period. The gray vertical dashed line indicates the reference period (Month -12), and the black vertical dashed line indicates the age-70 threshold (Month 0). Month 0 serves as a transition month in which the treatment status is mixed across individuals: the reduction in the copayment rate applies from the birth month for those born on the first day of the month, but from the following month for all others. Consequently, the full institutional impact of the policy change is captured starting from Month 1. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. The corresponding raw coefficient estimates (β_k) and their confidence intervals are reported in Figure B.7.

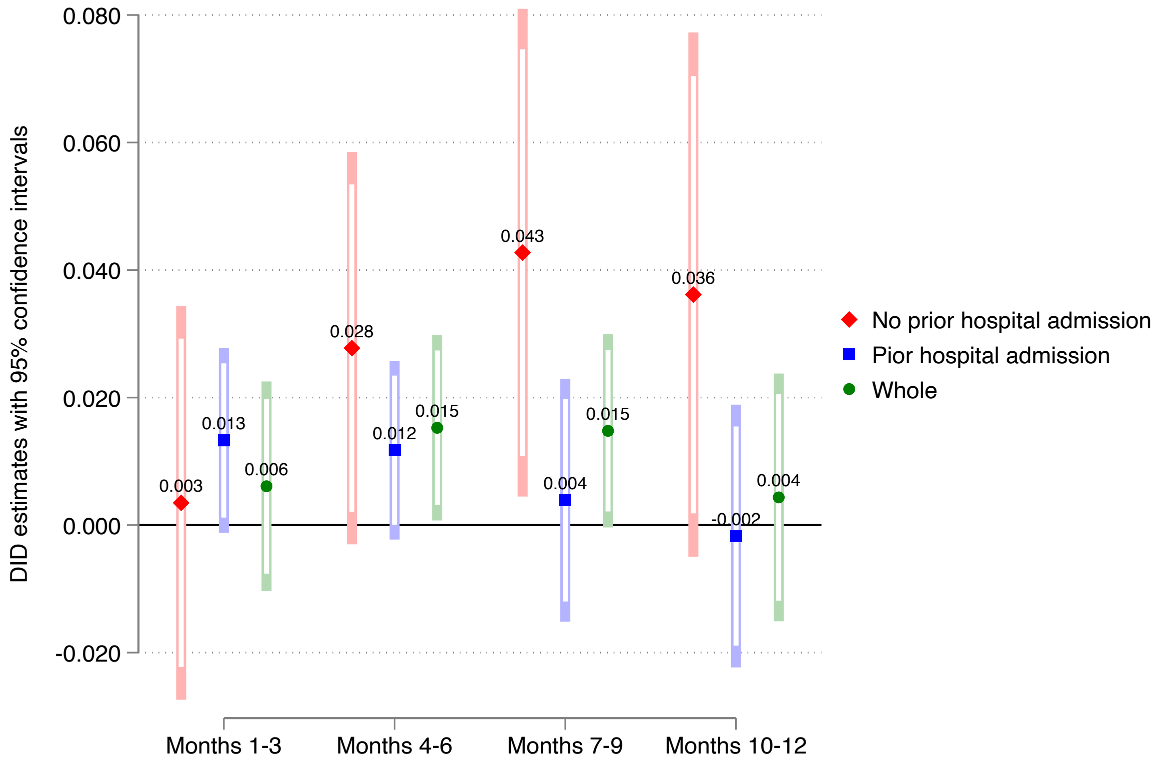
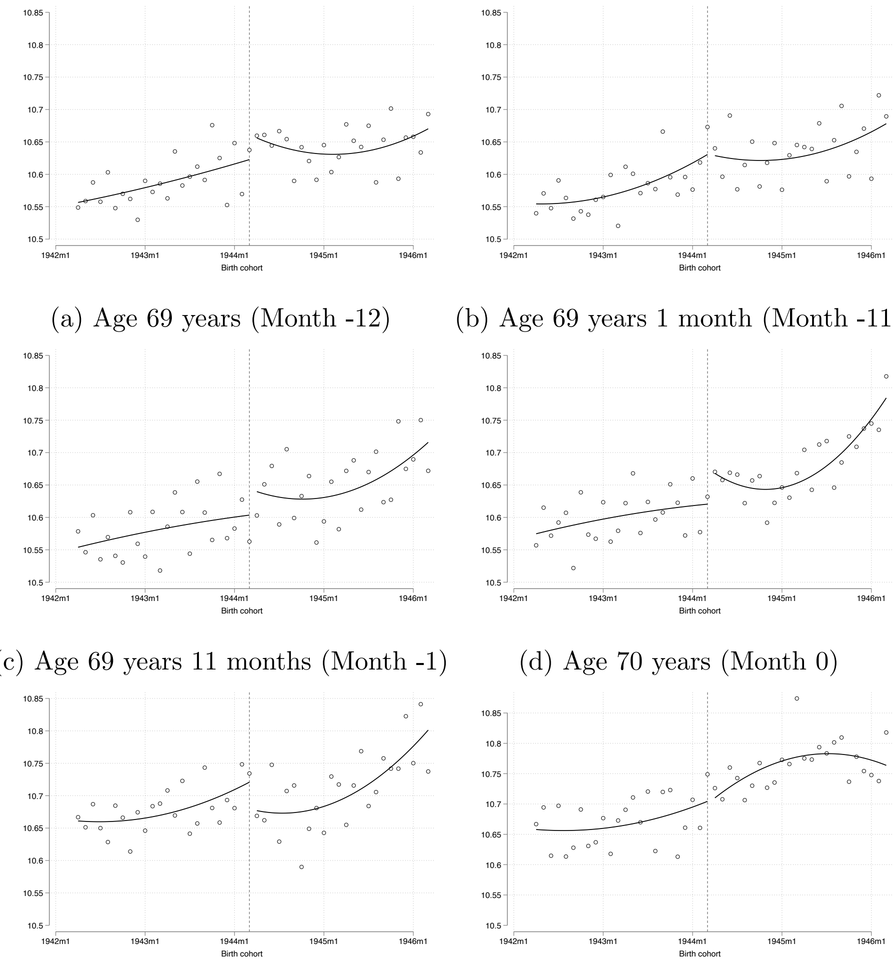


Figure C.1: DID Estimates of Inpatient Expenditures by Prior Hospitalization Status at Age 67

Note: This figure presents the difference-in-differences (DID) estimates of log monthly inpatient expenditures following the copayment rate reduction at age 70. The dependent variable is the log of monthly inpatient expenditures. The sample is stratified into three groups based on hospital admission history during the one-year period from age 67 years 0 months to 67 years 11 months: the full sample ("Whole"), individuals with no prior hospital admissions ("No prior hospital admission"), and individuals with at least one prior hospital admission ("Prior hospital admission"). The four intervals on the horizontal axis represent distinct three-month periods (Months 1-3, Months 4-6, Months 7-9, and Months 10-12) following the age-70 threshold. The two-tiered error bars represent the 90% (inner white) and 95% (outer colored) confidence intervals. The escalating trajectory for the group without prior hospital admissions—rising from 0.3% in Months 1–3 to a peak of 4.3% in Months 7–9 and remaining elevated at 3.6% in Months 10–12—provides evidence of the deterioration mechanism identified in Section 5.3. The persistently flat trajectory for the group with prior hospital admissions across all four intervals serves as a placebo check, confirming that the escalation among the healthy group reflects the consequences of strategic deferral rather than a general post-reform trend. All models include individual, birth cohort, age-in-months, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. Table C.1 provides the numerical point estimates and standard errors underlying the event study results visualized in the figures.



(a) Age 69 years (Month -12)

(b) Age 69 years 1 month (Month -11)

(c) Age 69 years 11 months (Month -1)

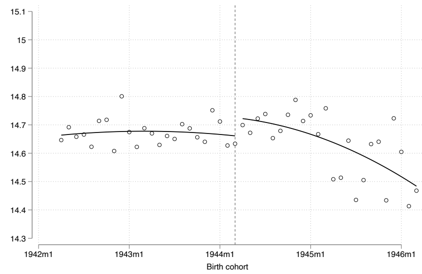
(d) Age 70 years (Month 0)

(e) Age 70 years 1 month (Month 1)

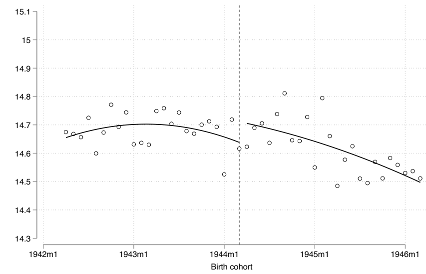
(f) Age 71 years (Month 12)

Figure D.1: Descriptive Plots of Log Outpatient Expenditures by Birth Cohort

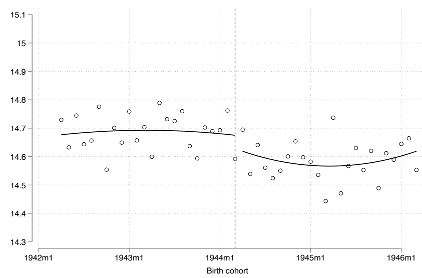
Note: The figures display the log-transformed monthly outpatient expenditures across birth cohorts at six different points in time, ranging from Month -12 (age 69 years) to Month 12 (age 71 years) , with each panel corresponding to a distinct month relative to the age-70 threshold: Panel (a) Month -12, Panel (b) Month -11, Panel (c) Month -1, Panel (d) Month 0, Panel (e) Month 1, and Panel (f) Month 12. The vertical dashed line denotes the policy cutoff birth cohort (April 1944). Individuals to the left of the cutoff transition to a 10% copayment rate at age 70 (the treated cohort), while those to the right transition to a 20% copayment rate (the control cohort). Each circle represents the sample average of log monthly outpatient expenditures within a monthly birth cohort bin. The solid lines indicate quadratic polynomial fitted values estimated separately on each side of the cutoff. The absence of any discontinuity at the cutoff in Panels (a) and (b)—the pre-threshold baseline months—supports the identification assumption that the two cohorts are comparable in terms of underlying health status and baseline medical demand prior to the policy change. In Month 0 (Age 70), the actual copayment rate is a mixture of pre- and post-reform rates due to the mid-month transition rule based on exact birth dates, whereby the reduction applies from the birth month for those born on the first day of the month and from the following month for all others.



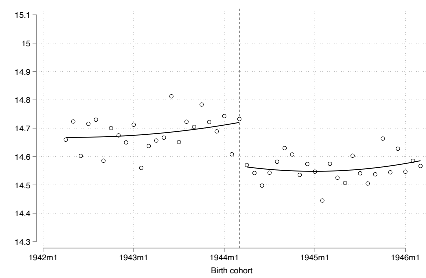
(a) Age 69 years (Month -12)



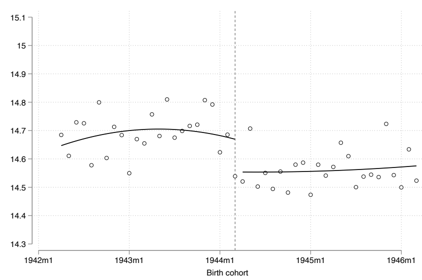
(b) Age 69 years 1 month (Month -11)



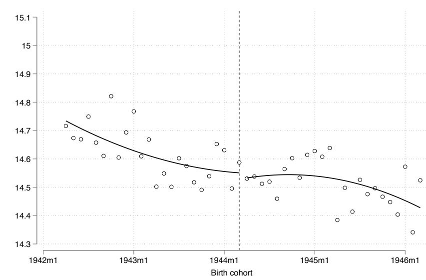
(c) Age 69 years 11 months (Month -1)



(d) Age 70 years (Month 0)



(e) Age 70 years 1 month (Month 1)



(f) Age 71 years (Month 12)

Figure D.2: Descriptive Plots of Log Inpatient Expenditures by Birth Cohort

Note: The figures display the log-transformed monthly inpatient expenditures across birth cohorts at six different points in time, ranging from Month -12 (age 69 years) to Month 12 (age 71 years), with each panel corresponding to a distinct month relative to the age-70 threshold: Panel (a) Month -12, Panel (b) Month -11, Panel (c) Month -1, Panel (d) Month 0, Panel (e) Month 1, and Panel (f) Month 12. The vertical dashed line denotes the policy cutoff birth cohort (April 1944). Individuals to the left of the cutoff transition to a 10% copayment rate at age 70 (the treated cohort), while those to the right transition to a 20% copayment rate (the control cohort). Each circle represents the sample average of log monthly inpatient expenditures within a monthly birth cohort bin. The solid lines indicate quadratic polynomial fitted values estimated separately on each side of the cutoff. The absence of any discontinuity at the cutoff in Panels (a) and (b)—the pre-threshold baseline months—supports the identification assumption that the two cohorts are comparable in terms of underlying health status and baseline medical demand prior to the policy change. Consistent with the descriptive results presented in Section 5.1, no discontinuous jump is visible at the cutoff in any panel, reflecting the price inelasticity of inpatient demand documented in the main text and the role of the High-Cost Medical Expense Benefit System in neutralizing marginal price incentives for costly inpatient treatments. In Month 0 (Age 70), the actual copayment rate is a mixture of pre- and post-reform rates due to the mid-month transition rule based on exact birth dates, whereby the reduction applies from the birth month for those born on the first day of the month and from the following month for all others.

Table B.1: Results of Event Study Models (Intensive Margin)

	Monthly expenditure on outpatient services (log)					Monthly expenditure on inpatient care (log)				
		Prior Admission		Birth cohort			Prior Admission		Birth cohort	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Whole	Without	With	Apr.1943 - Mar.1945	Jan.1943 - Jul.1945	Whole	Without	With	Apr.1943 - Mar.1945	Jan.1943 - Jul.1945
Treated cohort × age dummy										
-18	-0.00020	-0.00053	0.00585	-0.00271	0.00056	0.00116	0.01501	-0.00484	-0.00213	0.00437
	(0.00191)	(0.00197)	(0.00838)	(0.00276)	(0.00276)	(0.01174)	(0.02923)	(0.01203)	(0.01683)	(0.01345)
-17	0.00122	0.00083	0.00887	-0.00058	0.00192	-0.00535	-0.01163	0.00039	-0.00185	-0.00040
	(0.00224)	(0.00217)	(0.00885)	(0.00281)	(0.00279)	(0.01186)	(0.02793)	(0.01116)	(0.01762)	(0.01367)
-16	-0.00316*	-0.00305*	-0.00577	-0.00455*	-0.00213	-0.00029	0.01116	-0.00920	-0.00394	-0.00180
	(0.00163)	(0.00158)	(0.00799)	(0.00257)	(0.00218)	(0.01338)	(0.02850)	(0.01254)	(0.01918)	(0.01558)
-15	-0.00058	-0.00019	-0.00901	-0.00340	-0.00049	-0.01151	-0.01473	-0.00879	-0.02364	-0.02319
	(0.00172)	(0.00168)	(0.00938)	(0.00281)	(0.00239)	(0.01217)	(0.02546)	(0.00807)	(0.01856)	(0.01427)
-14	0.00095	0.00073	0.00563	-0.00027	0.00185	-0.01349	-0.01133	-0.01415	-0.00663	0.00509
	(0.00197)	(0.00194)	(0.00846)	(0.00267)	(0.00274)	(0.01182)	(0.02450)	(0.01061)	(0.01581)	(0.01329)
-13	-0.00073	-0.00069	-0.00123	-0.00094	0.00042	-0.00683	-0.00310	-0.00906	-0.00182	0.00446
	(0.00206)	(0.00205)	(0.00834)	(0.00240)	(0.00265)	(0.00996)	(0.01766)	(0.01083)	(0.01230)	(0.01112)
-11	-0.00049	-0.00056	0.00172	-0.00105	-0.00079	-0.00632	-0.01048	-0.00285	0.00347	-0.00785
	(0.00209)	(0.00212)	(0.00935)	(0.00320)	(0.00277)	(0.01296)	(0.02225)	(0.01252)	(0.02015)	(0.01680)
-10	0.00063	0.00057	0.00271	-0.00344	-0.00096	-0.00245	-0.00605	0.00117	-0.00149	0.00107
	(0.00209)	(0.00212)	(0.00831)	(0.00322)	(0.00279)	(0.01170)	(0.02267)	(0.01138)	(0.01725)	(0.01427)
-9	-0.00078	-0.00085	0.00128	-0.00129	0.00037	0.00348	0.00578	0.00268	0.00786	0.01475
	(0.00200)	(0.00201)	(0.00788)	(0.00269)	(0.00236)	(0.01405)	(0.02544)	(0.01173)	(0.01916)	(0.01644)
-8	-0.00332	-0.00376*	0.00798	-0.00270	-0.00266	-0.00394	-0.00624	0.00063	-0.00157	-0.00148
	(0.00211)	(0.00212)	(0.00882)	(0.00356)	(0.00295)	(0.01184)	(0.02453)	(0.01031)	(0.01738)	(0.01417)
-7	-0.00093	-0.00043	-0.01188	-0.00222	-0.00169	0.00110	0.00027	0.00414	-0.00316	-0.00733
	(0.00223)	(0.00218)	(0.01026)	(0.00326)	(0.00278)	(0.01164)	(0.02441)	(0.01024)	(0.01600)	(0.01350)
-6	-0.00184	-0.00184	-0.00114	-0.00117	-0.00066	-0.01215	-0.01852	-0.00180	-0.00032	-0.00341
	(0.00224)	(0.00220)	(0.01242)	(0.00307)	(0.00284)	(0.01145)	(0.02347)	(0.01119)	(0.01472)	(0.01246)
-5	-0.00413*	-0.00414*	-0.00334	-0.00654*	-0.00583**	-0.00235	-0.00022	0.00075	0.00613	0.00927
	(0.00229)	(0.00229)	(0.00911)	(0.00320)	(0.00271)	(0.01131)	(0.02396)	(0.01191)	(0.01505)	(0.01286)
-4	-0.00409*	-0.00444*	0.00435	-0.00511	-0.00353	-0.00093	0.01097	-0.00748	-0.00087	-0.00112
	(0.00229)	(0.00237)	(0.00987)	(0.00307)	(0.00270)	(0.01278)	(0.02685)	(0.01094)	(0.01848)	(0.01461)
-3	-0.00629***	-0.00663***	0.00232	-0.00859***	-0.00573**	0.01363	0.02969	0.00194	0.01400	0.01406
	(0.00220)	(0.00220)	(0.01064)	(0.00289)	(0.00267)	(0.01467)	(0.02666)	(0.01352)	(0.02102)	(0.01793)
-2	-0.00539**	-0.00593**	0.00782	-0.00514	-0.00346	0.00277	-0.00177	0.01687	0.00535	-0.00104
	(0.00242)	(0.00243)	(0.01094)	(0.00348)	(0.00295)	(0.01287)	(0.02634)	(0.01209)	(0.01851)	(0.01610)
-1	-0.01036***	-0.01114***	0.00816	-0.01032**	-0.00808**	0.00483	0.01538	0.00066	0.01573	0.00855
	(0.00316)	(0.00319)	(0.01049)	(0.00439)	(0.00364)	(0.01362)	(0.02836)	(0.01322)	(0.01993)	(0.01605)
0	-0.01773***	-0.01834***	-0.00311	-0.02028***	-0.01698***	0.00566	0.01381	0.00520	0.00499	0.00744
	(0.00313)	(0.00329)	(0.01093)	(0.00508)	(0.00389)	(0.01438)	(0.02830)	(0.01410)	(0.01848)	(0.01753)
1	0.06661***	0.06728***	0.05121***	0.06832***	0.07218***	0.02058	0.02097	0.03103**	0.01467	0.01446
	(0.00325)	(0.00315)	(0.01122)	(0.00476)	(0.00411)	(0.01408)	(0.03321)	(0.01287)	(0.02086)	(0.01741)
2	0.04281***	0.04294***	0.03998***	0.04438***	0.04710***	0.01709	0.03008	0.01116	0.02860	0.03173*
	(0.00312)	(0.00307)	(0.01053)	(0.00467)	(0.00412)	(0.01589)	(0.03524)	(0.01295)	(0.02068)	(0.01705)
3	0.03563***	0.03551***	0.03864***	0.03483***	0.03844***	-0.01298	-0.01487	0.00524	-0.01913	-0.02270
	(0.00357)	(0.00355)	(0.01243)	(0.00507)	(0.00441)	(0.01362)	(0.03110)	(0.01293)	(0.02040)	(0.01613)
4	0.03555***	0.03535***	0.04079***	0.03542***	0.04004***	0.01760	0.02652	0.02392*	0.02724	0.01611
	(0.00328)	(0.00331)	(0.01289)	(0.00417)	(0.00379)	(0.01543)	(0.03467)	(0.01198)	(0.02093)	(0.01840)
5	0.03399***	0.03432***	0.02560**	0.03423***	0.03648***	0.01871	0.04109	0.01016	0.02999	0.01874
	(0.00290)	(0.00296)	(0.01196)	(0.00349)	(0.00338)	(0.01536)	(0.03115)	(0.01403)	(0.02157)	(0.01815)
6	0.03369***	0.03352***	0.03764***	0.03234***	0.03516***	0.01263	0.03569	0.00985	0.02289	0.01369
	(0.00349)	(0.00343)	(0.01299)	(0.00458)	(0.00397)	(0.01517)	(0.03454)	(0.01333)	(0.01864)	(0.01626)
7	0.03267***	0.03230***	0.04141***	0.03109***	0.03377***	0.01731	0.04677	0.01125	0.03156	0.01996
	(0.00316)	(0.00317)	(0.01499)	(0.00344)	(0.00326)	(0.01569)	(0.03540)	(0.01468)	(0.01879)	(0.01862)
8	0.03135***	0.03114***	0.03578**	0.02905***	0.03253***	0.01203	0.04506	0.00735	0.00797	0.00430
	(0.00311)	(0.00308)	(0.01528)	(0.00331)	(0.00307)	(0.01468)	(0.03699)	(0.01432)	(0.01808)	(0.01694)
9	0.03022***	0.03009***	0.03239**	0.03001***	0.03232***	0.02077	0.06577*	0.00314	0.03515**	0.02143
	(0.00321)	(0.00316)	(0.01358)	(0.00357)	(0.00340)	(0.01488)	(0.03477)	(0.01484)	(0.01674)	(0.01715)
10	0.03145***	0.03118***	0.03725**	0.03258***	0.03404***	0.01044	0.05891	-0.00802	0.01582	0.01097
	(0.00351)	(0.00347)	(0.01551)	(0.00410)	(0.00352)	(0.01800)	(0.03997)	(0.01544)	(0.02398)	(0.02136)
11	0.02901***	0.02805***	0.05203***	0.03092***	0.03136***	0.00644	0.04249	0.00919	0.01532	0.01307
	(0.00318)	(0.00315)	(0.01635)	(0.00323)	(0.00318)	(0.01542)	(0.03645)	(0.01367)	(0.01642)	(0.01844)
12	0.02862***	0.02777***	0.04877***	0.02796***	0.02906***	0.00284	0.03736	0.00582	0.02935	0.01447
	(0.00394)	(0.00382)	(0.01625)	(0.00378)	(0.00411)	(0.01754)	(0.03962)	(0.01497)	(0.02103)	(0.02073)
# observations	120174780	115325738	4849042	63947738	81805206	2067857	1179052	888805	1089588	1402169
# patients	6753900	6528650	225250	3595759	4599004	384489	310186	74303	203340	260654

The unit of observation is the individual-month. The dependent variables are the log of monthly healthcare expenditures, separately for outpatient visits (Columns (1)-(5)) and inpatient care (Columns (6)-(10)). "Prior Admission" refers to hospital admission history during the one-year period from age 67 years 0 months to 67 years 11 months. "Without Prior Admission" indicates the relatively healthy group, and "With Prior Admission" indicates the prior-admission group. The reference period is Month -12 (age 69 years and 0 months). All models include individual fixed effects, age-in-months fixed effects, birth cohort fixed effects, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. Significance levels: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table B.2: Results of Event Study Models (Extensive Margin)

	Office visit					Hospital admission				
		Prior Admission		Birth cohort			Prior Admission		Birth cohort	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Whole	Without	With	Apr.1943 - Mar.1945	Jan.1943 - Jul.1945	Whole	Without	With	Apr.1943 - Mar.1945	Jan.1943 - Jul.1945
Treated cohort × age dummy										
-18	-0.00049	-0.00044	-0.00207	-0.00097	-0.00014	-0.00007	-0.00009	0.00017	-0.00021	-0.00015
	(0.00059)	(0.00059)	(0.00187)	(0.00088)	(0.00068)	(0.00010)	(0.00010)	(0.00106)	(0.00014)	(0.00013)
-17	-0.00051	-0.00040	-0.00353*	-0.00151*	-0.00079	-0.00007	-0.00005	-0.00082	-0.00011	-0.00009
	(0.00046)	(0.00047)	(0.00181)	(0.00074)	(0.00053)	(0.00010)	(0.00009)	(0.00092)	(0.00013)	(0.00012)
-16	-0.00016	-0.00010	-0.00181	-0.00098	-0.00001	-0.00009	-0.00007	-0.00069	-0.00009	-0.00005
	(0.00049)	(0.00048)	(0.00189)	(0.00068)	(0.00055)	(0.00009)	(0.00008)	(0.00102)	(0.00010)	(0.00010)
-15	-0.00033	-0.00045	0.00252	-0.00159**	-0.00052	-0.00001	0.00004	-0.00145	-0.00001	0.00002
	(0.00052)	(0.00054)	(0.00224)	(0.00069)	(0.00062)	(0.00010)	(0.00009)	(0.00105)	(0.00012)	(0.00012)
-14	0.00029	0.00032	-0.00043	-0.00003	0.00035	0.00005	0.00007	-0.00053	-0.00000	0.00002
	(0.00047)	(0.00049)	(0.00163)	(0.00067)	(0.00054)	(0.00009)	(0.00009)	(0.00110)	(0.00013)	(0.00011)
-13	-0.00017	-0.00004	-0.00335	-0.00093	-0.00049	0.00000	0.00002	-0.00053	-0.00009	-0.00006
	(0.00049)	(0.00050)	(0.00202)	(0.00062)	(0.00060)	(0.00008)	(0.00008)	(0.00092)	(0.00009)	(0.00009)
-11	0.00053	0.00066	-0.00273	-0.00063	0.00011	-0.00004	-0.00004	-0.00014	-0.00007	-0.00005
	(0.00051)	(0.00052)	(0.00180)	(0.00073)	(0.00065)	(0.00007)	(0.00007)	(0.00071)	(0.00010)	(0.00009)
-10	0.00012	0.00019	-0.00141	-0.00108	-0.00022	-0.00000	-0.00003	0.00053	-0.00003	0.00001
	(0.00050)	(0.00051)	(0.00190)	(0.00079)	(0.00063)	(0.00009)	(0.00009)	(0.00089)	(0.00013)	(0.00011)
-9	0.00005	0.00013	-0.00204	-0.00057	0.00000	0.00002	-0.00001	0.00079	-0.00005	0.00003
	(0.00052)	(0.00052)	(0.00218)	(0.00071)	(0.00062)	(0.00011)	(0.00010)	(0.00104)	(0.00014)	(0.00012)
-8	0.00021	0.00026	-0.00099	0.00004	0.00063	-0.00001	-0.00005	0.00098	0.00003	0.00005
	(0.00048)	(0.00048)	(0.00201)	(0.00073)	(0.00057)	(0.00011)	(0.00010)	(0.00106)	(0.00016)	(0.00013)
-7	-0.00020	-0.00004	-0.00428*	-0.00116	-0.00015	0.00001	-0.00005	0.00149	0.00012	0.00008
	(0.00058)	(0.00059)	(0.00214)	(0.00092)	(0.00074)	(0.00010)	(0.00009)	(0.00103)	(0.00014)	(0.00012)
-6	0.00019	0.00020	-0.00005	-0.00049	0.00051	-0.00003	-0.00009	0.00147	0.00011	0.00007
	(0.00057)	(0.00058)	(0.00205)	(0.00096)	(0.00074)	(0.00011)	(0.00010)	(0.00106)	(0.00018)	(0.00014)
-5	0.00009	0.00025	-0.00424*	-0.00018	0.00043	-0.00013	-0.00017	0.00062	-0.00003	-0.00010
	(0.00057)	(0.00058)	(0.00222)	(0.00098)	(0.00072)	(0.00011)	(0.00011)	(0.00107)	(0.00016)	(0.00013)
-4	-0.00002	-0.00007	0.00106	-0.00056	0.00016	0.00001	0.00002	-0.00031	0.00000	-0.00004
	(0.00056)	(0.00055)	(0.00214)	(0.00092)	(0.00066)	(0.00011)	(0.00010)	(0.00122)	(0.00015)	(0.00013)
-3	-0.00054	-0.00050	-0.00190	-0.00172*	-0.00062	-0.00014	-0.00012	-0.00055	-0.00015	-0.00017
	(0.00062)	(0.00061)	(0.00238)	(0.00098)	(0.00077)	(0.00013)	(0.00011)	(0.00131)	(0.00016)	(0.00015)
-2	-0.00049	-0.00050	-0.00067	-0.00110	-0.00027	-0.00009	-0.00013	0.00086	-0.00012	-0.00014
	(0.00060)	(0.00059)	(0.00240)	(0.00097)	(0.00079)	(0.00013)	(0.00011)	(0.00125)	(0.00017)	(0.00014)
-1	-0.00206***	-0.00196***	-0.00502**	-0.00291**	-0.00265***	-0.00009	-0.00012	0.00063	-0.00005	-0.00006
	(0.00068)	(0.00067)	(0.00248)	(0.00123)	(0.00096)	(0.00013)	(0.00011)	(0.00131)	(0.00017)	(0.00014)
0	-0.00633***	-0.00641***	-0.00475*	-0.00770***	-0.00771***	-0.00024*	-0.00026**	0.00008	-0.00019	-0.00023
	(0.00127)	(0.00131)	(0.00271)	(0.00203)	(0.00153)	(0.00014)	(0.00012)	(0.00134)	(0.00019)	(0.00016)
1	0.01356***	0.01371***	0.00977***	0.01376***	0.01426***	0.00032**	0.00020	0.00310**	0.00027	0.00028
	(0.00079)	(0.00078)	(0.00235)	(0.00121)	(0.00101)	(0.00014)	(0.00013)	(0.00145)	(0.00022)	(0.00017)
2	0.01104***	0.01136***	0.00271	0.01141***	0.01159***	0.00052***	0.00046***	0.00188	0.00061**	0.00052**
	(0.00084)	(0.00085)	(0.00250)	(0.00150)	(0.00119)	(0.00016)	(0.00015)	(0.00138)	(0.00024)	(0.00020)
3	0.01094***	0.01106***	0.00748***	0.01085***	0.01070***	0.00026	0.00019	0.00168	0.00032	0.00022
	(0.00068)	(0.00071)	(0.00241)	(0.00110)	(0.00093)	(0.00018)	(0.00016)	(0.00155)	(0.00026)	(0.00022)
4	0.01138***	0.01145***	0.00898***	0.01133***	0.01134***	0.00032**	0.00024*	0.00226	0.00033	0.00030
	(0.00058)	(0.00059)	(0.00205)	(0.00107)	(0.00075)	(0.00015)	(0.00013)	(0.00148)	(0.00022)	(0.00018)
5	0.01147***	0.01170***	0.00541*	0.01069***	0.01117***	0.00024	0.00017	0.00167	0.00024	0.00017
	(0.00067)	(0.00066)	(0.00272)	(0.00116)	(0.00095)	(0.00015)	(0.00014)	(0.00131)	(0.00023)	(0.00018)
6	0.01095***	0.01105***	0.00792***	0.01044***	0.01111***	0.00027*	0.00021	0.00145	0.00021	0.00025
	(0.00075)	(0.00077)	(0.00247)	(0.00125)	(0.00098)	(0.00015)	(0.00014)	(0.00140)	(0.00022)	(0.00018)
7	0.01119***	0.01138***	0.00590**	0.01004***	0.01048***	0.00019	0.00011	0.00199	0.00020	0.00025
	(0.00077)	(0.00079)	(0.00235)	(0.00132)	(0.00105)	(0.00015)	(0.00014)	(0.00134)	(0.00021)	(0.00019)
8	0.01091***	0.01106***	0.00665***	0.00961***	0.01028***	0.00022	0.00015	0.00191	0.00026	0.00026
	(0.00069)	(0.00069)	(0.00244)	(0.00104)	(0.00083)	(0.00017)	(0.00016)	(0.00143)	(0.00023)	(0.00020)
9	0.01098***	0.01117***	0.00542**	0.00963***	0.01019***	0.00022	0.00017	0.00135	0.00014	0.00022
	(0.00066)	(0.00067)	(0.00261)	(0.00107)	(0.00089)	(0.00018)	(0.00016)	(0.00131)	(0.00023)	(0.00020)
10	0.01067***	0.01076***	0.00764***	0.00923***	0.01014***	0.00020	0.00015	0.00163	0.00021	0.00024
	(0.00069)	(0.00071)	(0.00258)	(0.00104)	(0.00088)	(0.00017)	(0.00015)	(0.00141)	(0.00023)	(0.00020)
11	0.01082***	0.01106***	0.00413	0.00895***	0.01004***	0.00017	0.00011	0.00158	0.00002	0.00012
	(0.00073)	(0.00074)	(0.00254)	(0.00108)	(0.00091)	(0.00017)	(0.00016)	(0.00126)	(0.00022)	(0.00019)
12	0.01136***	0.01149***	0.00743***	0.00873***	0.01038***	0.00016	0.00005	0.00295**	0.00012	0.00016
	(0.00073)	(0.00073)	(0.00268)	(0.00109)	(0.00090)	(0.00018)	(0.00016)	(0.00126)	(0.00024)	(0.00020)
# observations	179103740	172321622	6782118	95285630	121922690	179103740	172321622	6782118	95285630	121922690
# patients	5777540	5558762	218778	3073730	3932990	5777540	5558762	218778	3073730	3932990
Mean before Month 0 (treated)	0.610	0.606	0.698	0.611	0.610	0.010	0.005	0.125	0.010	0.010
Mean before Month 0 (control)	0.615	0.612	0.697	0.616	0.616	0.010	0.005	0.127	0.010	0.010

Note: The unit of observation is the individual-month. The dependent variables are the log of monthly healthcare expenditures, separately for outpatient visits (Columns (1)-(5)) and inpatient care (Columns (6)-(10)). "Prior Admission" refers to hospital admission history during the one-year period from age 67 years 0 months to 67 years 11 months. "Without Prior Admission" indicates the relatively healthy group, and "With Prior Admission" indicates the prior-admission group. The reference period is Month -12 (age 69 years and 0 months). All models include individual fixed effects, age-in-months fixed effects, birth cohort fixed effects, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. Significance levels: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table B.3: Results of Event Study Models (Extensive Margin, Proportionate Change)

	Office visit					Hospital admission				
		Prior Admission		Birth cohort			Prior Admission		Birth cohort	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Whole	Without	With	Apr.1943 - Mar.1945	Jan.1943 - Jul.1945	Whole	Without	With	Apr.1943 - Mar.1945	Jan.1943 - Jul.1945
Treated cohort × age dummy										
-18	-0.00080	-0.00072	-0.00296	-0.00158	-0.00023	-0.00705	-0.01599	0.00138	-0.02114	-0.01565
	(0.00097)	(0.00098)	(0.00267)	(0.00144)	(0.00111)	(0.01049)	(0.01843)	(0.00848)	(0.01390)	(0.01324)
-17	-0.00084	-0.00066	-0.00506*	-0.00247**	-0.00129	-0.00721	-0.00876	-0.00652	-0.01162	-0.00949
	(0.00075)	(0.00078)	(0.00260)	(0.00122)	(0.00086)	(0.00970)	(0.01707)	(0.00733)	(0.01316)	(0.01225)
-16	-0.00026	-0.00017	-0.00260	-0.00160	-0.00002	-0.00894	-0.01281	-0.00550	-0.00969	-0.00512
	(0.00080)	(0.00079)	(0.00271)	(0.00111)	(0.00090)	(0.00864)	(0.01474)	(0.00817)	(0.01063)	(0.01027)
-15	-0.00054	-0.00074	0.00360	-0.00260**	-0.00085	-0.00128	0.00763	-0.01160	-0.00105	0.00205
	(0.00086)	(0.00089)	(0.00321)	(0.00113)	(0.00102)	(0.00952)	(0.01593)	(0.00839)	(0.01272)	(0.01167)
-14	0.00048	0.00053	-0.00061	-0.00005	0.00057	0.00473	0.01264	-0.00425	-0.00000	0.00236
	(0.00078)	(0.00080)	(0.00233)	(0.00109)	(0.00089)	(0.00906)	(0.01695)	(0.00881)	(0.01316)	(0.01129)
-13	-0.00028	-0.00007	-0.00480*	-0.00152	-0.00080	0.00016	0.00401	-0.00426	-0.00921	-0.00579
	(0.00080)	(0.00082)	(0.00290)	(0.00102)	(0.00098)	(0.00778)	(0.01401)	(0.00737)	(0.00911)	(0.00867)
-11	0.00087	0.00109	-0.00390	-0.00104	0.00018	-0.00406	-0.00691	-0.00115	-0.00737	-0.00502
	(0.00084)	(0.00085)	(0.00258)	(0.00120)	(0.00107)	(0.00679)	(0.01221)	(0.00570)	(0.01066)	(0.00883)
-10	0.00020	0.00031	-0.00202	-0.00176	-0.00035	-0.00038	-0.00475	0.00422	-0.00288	0.00064
	(0.00083)	(0.00084)	(0.00273)	(0.00129)	(0.00104)	(0.00921)	(0.01605)	(0.00711)	(0.01330)	(0.01109)
-9	0.00008	0.00022	-0.00292	-0.00094	0.00001	0.00241	-0.00110	0.00634	-0.00549	0.00270
	(0.00086)	(0.00086)	(0.00312)	(0.00116)	(0.00101)	(0.01094)	(0.01811)	(0.00831)	(0.01386)	(0.01225)
-8	0.00035	0.00042	-0.00142	0.00006	0.00103	-0.00144	-0.00999	0.00786	0.00258	0.00474
	(0.00079)	(0.00079)	(0.00288)	(0.00120)	(0.00093)	(0.01098)	(0.01775)	(0.00848)	(0.01674)	(0.01347)
-7	-0.00032	-0.00007	-0.00613**	-0.00190	-0.00025	0.00105	-0.00905	0.01189	0.01250	0.00833
	(0.00096)	(0.00098)	(0.00306)	(0.00151)	(0.00121)	(0.00977)	(0.01636)	(0.00819)	(0.01405)	(0.01239)
-6	0.00032	0.00032	-0.00008	-0.00080	0.00084	-0.00327	-0.01745	0.01173	0.01105	0.00717
	(0.00094)	(0.00096)	(0.00293)	(0.00157)	(0.00122)	(0.01134)	(0.01945)	(0.00848)	(0.01877)	(0.01453)
-5	0.00015	0.00042	-0.00607*	-0.00030	0.00070	-0.01348	-0.03101	0.00493	-0.00338	-0.00993
	(0.00093)	(0.00095)	(0.00317)	(0.00161)	(0.00118)	(0.01105)	(0.02069)	(0.00857)	(0.01586)	(0.01331)
-4	-0.00004	-0.00012	0.00152	-0.00091	0.00027	0.00077	0.00303	-0.00248	0.00032	-0.00432
	(0.00091)	(0.00091)	(0.00307)	(0.00151)	(0.00108)	(0.01100)	(0.01940)	(0.00978)	(0.01492)	(0.01297)
-3	-0.00089	-0.00083	-0.00273	-0.00282*	-0.00101	-0.01358	-0.02313	-0.00440	-0.01519	-0.01725
	(0.00101)	(0.00101)	(0.00340)	(0.00160)	(0.00127)	(0.01269)	(0.02130)	(0.01047)	(0.01675)	(0.01491)
-2	-0.00081	-0.00082	-0.00096	-0.00180	-0.00045	-0.00885	-0.02423	0.00685	-0.01222	-0.01430
	(0.00098)	(0.00097)	(0.00344)	(0.00158)	(0.00129)	(0.01278)	(0.02204)	(0.00996)	(0.01690)	(0.01426)
-1	-0.00338***	-0.00323***	-0.00719**	-0.00476**	-0.00434***	-0.00894	-0.02320	0.00505	-0.00556	-0.00617
	(0.00112)	(0.00111)	(0.00356)	(0.00201)	(0.00157)	(0.01271)	(0.02125)	(0.01044)	(0.01726)	(0.01433)
0	-0.01037***	-0.01057***	-0.00680*	-0.01259***	-0.01262***	-0.02361*	-0.04799**	0.00064	-0.01982	-0.02324
	(0.00209)	(0.00217)	(0.00388)	(0.00332)	(0.00250)	(0.01358)	(0.02314)	(0.01067)	(0.01957)	(0.01625)
1	0.02223***	0.02261***	0.01400***	0.02251***	0.02336***	0.03209**	0.03765	0.02474**	0.02742	0.02805
	(0.00129)	(0.00129)	(0.00336)	(0.00198)	(0.00165)	(0.01418)	(0.02459)	(0.01158)	(0.02211)	(0.01727)
2	0.01811***	0.01875***	0.00388	0.01866***	0.01899***	0.05233***	0.08526***	0.01503	0.06281***	0.05312***
	(0.00138)	(0.00140)	(0.00359)	(0.00245)	(0.00196)	(0.01642)	(0.02891)	(0.01105)	(0.02435)	(0.02003)
3	0.01793***	0.01824***	0.01072***	0.01775***	0.01753***	0.02582	0.03573	0.01337	0.03245	0.02263
	(0.00112)	(0.00117)	(0.00345)	(0.00180)	(0.00152)	(0.01785)	(0.03011)	(0.01236)	(0.02676)	(0.02202)
4	0.01866***	0.01890***	0.01286***	0.01854***	0.01857***	0.03225**	0.04418*	0.01807	0.03340	0.03009*
	(0.00096)	(0.00098)	(0.00294)	(0.00176)	(0.00123)	(0.01487)	(0.02470)	(0.01182)	(0.02229)	(0.01825)
5	0.01882***	0.01929***	0.00775**	0.01749***	0.01830***	0.02405	0.03262	0.01334	0.02419	0.01680
	(0.00109)	(0.00110)	(0.00390)	(0.00190)	(0.00156)	(0.01485)	(0.02579)	(0.01045)	(0.02326)	(0.01862)
6	0.01796***	0.01823***	0.01135***	0.01708***	0.01820***	0.02684*	0.04013	0.01156	0.02102	0.02562
	(0.00124)	(0.00127)	(0.00354)	(0.00204)	(0.00161)	(0.01533)	(0.02650)	(0.01117)	(0.02262)	(0.01867)
7	0.01836***	0.01877***	0.00845**	0.01643***	0.01718***	0.01885	0.02112	0.01587	0.02059	0.02518
	(0.00126)	(0.00130)	(0.00337)	(0.00216)	(0.00173)	(0.01543)	(0.02669)	(0.01067)	(0.02162)	(0.01905)
8	0.01790***	0.01824***	0.00952***	0.01572***	0.01684***	0.02218	0.02823	0.01527	0.02681	0.02670
	(0.00114)	(0.00114)	(0.00350)	(0.00171)	(0.00135)	(0.01722)	(0.02917)	(0.01145)	(0.02396)	(0.02036)
9	0.01800***	0.01842***	0.00776**	0.01576***	0.01669***	0.02188	0.03211	0.01080	0.01448	0.02228
	(0.00108)	(0.00110)	(0.00373)	(0.00175)	(0.00145)	(0.01770)	(0.03081)	(0.01042)	(0.02364)	(0.02018)
10	0.01750***	0.01775***	0.01095***	0.01510***	0.01661***	0.02019	0.02713	0.01299	0.02189	0.02428
	(0.00114)	(0.00117)	(0.00369)	(0.00170)	(0.00144)	(0.01711)	(0.02828)	(0.01125)	(0.02402)	(0.02018)
11	0.01775***	0.01824***	0.00592	0.01464***	0.01645***	0.01674	0.02107	0.01258	0.00188	0.01211
	(0.00120)	(0.00123)	(0.00363)	(0.00177)	(0.00149)	(0.01677)	(0.02900)	(0.01007)	(0.02235)	(0.01901)
12	0.01864***	0.01895***	0.01064***	0.01428***	0.01701***	0.01621	0.01005	0.02352**	0.01209	0.01595
	(0.00119)	(0.00120)	(0.00384)	(0.00179)	(0.00147)	(0.01809)	(0.03077)	(0.01003)	(0.02494)	(0.02043)
# observations	179103740	172321622	6782118	95285630	121922690	179103740	172321622	6782118	95285630	121922690
# patients	5777540	5558762	218778	3073730	3932990	5777540	5558762	218778	3073730	3932990

The unit of observation is the individual-month. The dependent variables are the log of monthly healthcare expenditures, separately for outpatient visits (Columns (1)-(5)) and inpatient care (Columns (6)-(10)). "Prior Admission" refers to hospital admission history during the one-year period from age 67 years 0 months to 67 years 11 months. "Without Prior Admission" indicates the relatively healthy group, and "With Prior Admission" indicates the prior-admission group. The reference period is Month -12 (age 69 years and 0 months). All models include individual fixed effects, age-in-months fixed effects, birth cohort fixed effects, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. Significance levels: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table B.4: Results of Event Study Models (Robustness Check against Reference Month)

	(1) Outpatient (log)	(2) Inpatient (log)	(3) Office visit	(4) Hospital admission
Treated cohort × age dummy				
-18	0.00164	0.01331	-0.00068	-0.00004
	(0.00220)	(0.01296)	(0.00070)	(0.00011)
-17	0.00306	0.00680	-0.00070	-0.00004
	(0.00240)	(0.01450)	(0.00069)	(0.00010)
-16	-0.00132	0.01186	-0.00035	-0.00006
	(0.00222)	(0.01454)	(0.00063)	(0.00010)
-15	0.00126	0.00064	-0.00053	0.00002
	(0.00246)	(0.01239)	(0.00072)	(0.00010)
-14	0.00278	-0.00134	0.00010	0.00008
	(0.00238)	(0.01122)	(0.00062)	(0.00010)
-13	0.00111	0.00532	-0.00036	0.00003
	(0.00226)	(0.01111)	(0.00073)	(0.00011)
-12	0.00184	0.01215	-0.00019	0.00003
	(0.00224)	(0.01145)	(0.00057)	(0.00011)
-11	0.00135	0.00583	0.00034	-0.00001
	(0.00229)	(0.01318)	(0.00073)	(0.00008)
-10	0.00247	0.00970	-0.00007	0.00003
	(0.00199)	(0.01147)	(0.00059)	(0.00010)
-9	0.00106	0.01563	-0.00014	0.00006
	(0.00218)	(0.01239)	(0.00060)	(0.00009)
-8	-0.00148	0.00821	0.00002	0.00002
	(0.00228)	(0.01212)	(0.00062)	(0.00007)
-7	0.00091	0.01325	-0.00039	0.00004
	(0.00248)	(0.01128)	(0.00073)	(0.00006)
-5	-0.00230	0.00980	-0.00010	-0.00010*
	(0.00189)	(0.00953)	(0.00072)	(0.00006)
-4	-0.00225	0.01122	-0.00021	0.00004
	(0.00228)	(0.01276)	(0.00049)	(0.00008)
-3	-0.00446**	0.02578*	-0.00074	-0.00010
	(0.00204)	(0.01322)	(0.00063)	(0.00009)
-2	-0.00355*	0.01492	-0.00069	-0.00006
	(0.00190)	(0.01232)	(0.00048)	(0.00009)
-1	-0.00852***	0.01698	-0.00225***	-0.00006
	(0.00301)	(0.01350)	(0.00082)	(0.00010)
0	-0.01589***	0.01781	-0.00652***	-0.00020*
	(0.00335)	(0.01218)	(0.00117)	(0.00011)
1	0.06845***	0.03273**	0.01336***	0.00035***
	(0.00319)	(0.01548)	(0.00085)	(0.00011)
2	0.04465***	0.02924*	0.01085***	0.00056***
	(0.00317)	(0.01507)	(0.00096)	(0.00012)
3	0.03747***	-0.00083	0.01074***	0.00029**
	(0.00323)	(0.01359)	(0.00076)	(0.00013)
4	0.03738***	0.02975*	0.01118***	0.00036***
	(0.00299)	(0.01497)	(0.00072)	(0.00012)
5	0.03583***	0.03086**	0.01128***	0.00027**
	(0.00318)	(0.01393)	(0.00069)	(0.00012)
6	0.03553***	0.02478*	0.01076***	0.00030**
	(0.00308)	(0.01429)	(0.00081)	(0.00013)
7	0.03450***	0.02946*	0.01100***	0.00022*
	(0.00301)	(0.01508)	(0.00077)	(0.00012)
8	0.03318***	0.02418	0.01072***	0.00025*
	(0.00274)	(0.01496)	(0.00067)	(0.00014)
9	0.03206***	0.03292**	0.01078***	0.00025*
	(0.00285)	(0.01506)	(0.00065)	(0.00015)
10	0.03329***	0.02259	0.01048***	0.00023*
	(0.00316)	(0.01596)	(0.00081)	(0.00014)
11	0.03085***	0.01859	0.01063***	0.00020
	(0.00289)	(0.01542)	(0.00064)	(0.00016)
12	0.03046***	0.01499	0.01117***	0.00019
	(0.00341)	(0.01726)	(0.00072)	(0.00016)
# observations	120174780	2067857	179103740	179103740
# patients	6753900	384489	5777540	5777540

Note: The unit of observation is the individual-month. The dependent variables are the log of monthly healthcare expenditures (Column (1) and (2)) and the probability of any office visit and hospital admission dummies (Column (3) and (4)). Column (1) and (2) are estimated using the intensive margin sample, while columns (3) and (4) are estimated using a balanced panel of individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The reference period is Month -6 (age 69 years and 6 months). All models include individual fixed effects, age-in-months fixed effects, birth cohort fixed effects, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. Significance levels: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table B.5: Results of Event Study Models (Robustness Check against Reference Month, Extensive margin, Proportionate Change)

	(1) Office visit	(2) Hospital admission
Treated cohort × age dummy		
-18	-0.00112	-0.00377
	(0.00114)	(0.01100)
-17	-0.00115	-0.00394
	(0.00113)	(0.01002)
-16	-0.00057	-0.00566
	(0.00104)	(0.01003)
-15	-0.00086	0.00199
	(0.00118)	(0.01001)
-14	0.00016	0.00801
	(0.00102)	(0.00979)
-13	-0.00059	0.00343
	(0.00119)	(0.01065)
-12	-0.00032	0.00327
	(0.00094)	(0.01134)
-11	0.00056	-0.00078
	(0.00120)	(0.00845)
-10	-0.00011	0.00289
	(0.00097)	(0.00958)
-9	-0.00023	0.00568
	(0.00098)	(0.00898)
-8	0.00003	0.00183
	(0.00102)	(0.00747)
-7	-0.00064	0.00433
	(0.00120)	(0.00625)
-5	-0.00016	-0.01020*
	(0.00118)	(0.00577)
-4	-0.00035	0.00404
	(0.00081)	(0.00815)
-3	-0.00121	-0.01030
	(0.00103)	(0.00906)
-2	-0.00113	-0.00558
	(0.00079)	(0.00918)
-1	-0.00370***	-0.00566
	(0.00134)	(0.00998)
0	-0.01069***	-0.02034*
	(0.00193)	(0.01070)
1	0.02192***	0.03537***
	(0.00140)	(0.01099)
2	0.01780***	0.05560***
	(0.00157)	(0.01219)
3	0.01762***	0.02909**
	(0.00125)	(0.01346)
4	0.01834***	0.03552***
	(0.00118)	(0.01185)
5	0.01850***	0.02733**
	(0.00114)	(0.01195)
6	0.01764***	0.03011**
	(0.00133)	(0.01300)
7	0.01804***	0.02213*
	(0.00127)	(0.01161)
8	0.01758***	0.02546*
	(0.00110)	(0.01421)
9	0.01768***	0.02516*
	(0.00106)	(0.01486)
10	0.01718***	0.02347*
	(0.00134)	(0.01366)
11	0.01743***	0.02002
	(0.00105)	(0.01551)
12	0.01832***	0.01948
	(0.00119)	(0.01565)
# observations	179103740	179103740
# patients	5777540	5777540

Note: The unit of observation is the individual-month. The dependent variables are the probability of any office visit and the probability of any hospital admission, reported as proportionate changes calculated by dividing the estimated coefficients by the pre-treatment mean of the dependent variable for the treatment group. The analysis is conducted using a balanced panel of individuals to ensure consistent tracking of utilization behavior and to account for months with zero utilization. The reference period is Month -6 (age 69 years and 6 months). All models include individual fixed effects, age-in-months fixed effects, birth cohort fixed effects, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. Significance levels: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table C.1: DID Estimates of Inpatient Expenditures by Prior Hospitalization Status at Age 67

		Prior admission at age 67	
	(1) Whole	(2) No prior hospital admission	(3) Prior hospital admission
Treated cohort × Months 1-3	0.00609	0.00349	0.01327*
	(0.00817)	(0.01535)	(0.00721)
Treated cohort × Months 4-6	0.01526**	0.02775*	0.01175*
	(0.00722)	(0.01530)	(0.00697)
Treated cohort × Months 7-9	0.01478*	0.04272**	0.00390
	(0.00753)	(0.01901)	(0.00947)
Treated cohort × Months 10-12	0.00434	0.03614*	-0.00172
	(0.00966)	(0.02044)	(0.01024)
# observations	2067857	1179052	888805
# patients	384489	310186	74303

Note: The unit of observation is the individual-month. The dependent variable is the log of monthly healthcare expenditures. The sample is stratified into three groups based on hospital admission history during the one-year period from age 67 years 0 months to 67 years 11 months: the full sample ("Whole"), individuals with no prior hospital admissions ("No prior hospital admission"), and individuals with at least one prior hospital admission ("Prior hospital admission"). Each estimate represents the DID coefficient for the relevant three-month interval relative to the pre-threshold baseline, identified by comparing the treated cohort (30% to 10% copayment reduction) with the control cohort (30% to 20% copayment reduction). All models include individual fixed effects, age-in-months fixed effects, birth cohort fixed effects, and calendar month fixed effects. Standard errors are clustered at the birth-month cohort level. Significance levels: * $p < .1$, ** $p < .05$, *** $p < .01$.