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“The Longer-term Impact of Coinsurance for the Elderly - Evidence from High-access Case”

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The Longer-term Impact of Coinsurance for the Elderly - Evidence from High-access Case -*

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Abstract

We estimate the longer-term impact of coinsurance for the elderly by RDD using administrative data, focusing on the increase in coinsurance in Japan, from 10% to 20%, for those aged 70-74, born after April 1944. The reduction of utilization in the longer term is similar to, or slightly larger than, in the short term. Patients reduce potentially wasteful care more; we do not find discernible impacts on health outcome and health-related behaviors. For the moderate change of prices for the elderly, distinctive characteristics associated with medical services, like behavioral hazard and ex-ante moral hazard, seem not largely affect consumer responsiveness.

JEL Codes: I11, I12, I13, I18, J14

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1 Introduction

Researchers have long tried to estimate consumer responsiveness to prices of medical services provided under health insurance; it guides an optimal design of health insurance (Baicker and Goldman (2011); McGuire (2011); Brot-Goldberg et al. (2017)).[1] The responsiveness could be different from other goods and services in several aspects. One important aspect is that the utilization of medical services is interacted with health status and health-related behaviors so that immediate responses to price changes, short-term responses, could differ from subsequent response dynamics to the changes, longer-term responses.

Consider an increase in patient cost sharing, the patient's portion of costs for medical services. If cost sharing is blunt due to behavioral hazard (Baicker et al., 2015), as often mentioned in previous studies[2], in the sense that higher cost sharing reduces the utilization of both effective and less-effective medical services, higher cost sharing could deteriorate health in the longer term. The deteriorating health might result in requiring resource-intensive care over time: "feedback effects" from deteriorating health to utilization. If the feedback effects exist, the reduction of utilization in the longer term should be *smaller* than in the short term so that designing health insurance based on the short-term impact could be inappropriate. For example, in this case, the short-term impact overstates fiscal externality.[3]

In contrast, the reduction of utilization by higher cost sharing in the longer term could be *larger* than in the short term due to (ex-ante) moral hazard. Higher cost sharing encourages individuals to alter health-related behaviors so that it improves health in the longer term, resulting in smaller utilization over time. This also makes the short-term impact inappropriate information for designing health insurance.

[1]The impact on financial strain is also a key element for an optimal design of health insurance. For example, see Finkelstein and McKnight (2008); Engelhardt and Gruber (2011); Gross and Notowidigdo (2011); Finkelstein et al. (2012); Baicker et al. (2013); Shigeoka (2014); Barcellos and Jacobson (2015); Mazumder and Miller (2016); Hu et al. (2018).

[2]For example, see Newhouse et al. (1993), Remler and Greene (2009),Baicker and Goldman (2011), Baicker et al. (2015), Brot-Goldberg et al. (2017), and Iizuka and Shigeoka (2022). Some papers indicate that patient cost sharing is relatively sharp (Shigeoka (2014), and Fukushima et al. (2016)).

[3]Fiscal externality means the effect of behavioral response to policy of interest on the government budget (Chetty and Finkelstein (2013);Hendren and Sprung-Keyser (2020);Finkelstein and Hendren (2020)).

Although there is a strong consensus that higher cost sharing reduces utilization in the short term[4], the longer-term impact remains less known; this is especially the case for the elderly, while it is critical information for designing health insurance, given the medical cost share of the elderly is large and cost sharing for the elderly is lower in some countries, including UK and Japan.[5] Despite the criticality of estimating the longer-term impact for the elderly, researchers find it challenging to credibly estimate it. The reason is a lack of plausibly exogenous variation; only randomized experiments, like the RAND Health Insurance Experiment, are considered to be the best hope (Finkelstein et al., 2018).

This paper estimates both the short- and longer-term impact of cost sharing for the elderly by using a quasi-experimental variation. We focus on the increase in coinsurance rates, from 10 percent to 20 percent, for those who are between the ages of 70 to 74, born after April 1944. By this policy change, coinsurance rates for those born after April 1944 were 20 percent, while coinsurance rates for those born before April 1944 remained 10 percent, for the 5 years when they were ages 70-74. This difference for 5 years in coinsurance rates between those born before and after April 1944 enables us to estimate causal impacts on utilization as well as health outcome and health-related behaviors over time, not only just after, but also some years after the increase in coinsurance rates, by regression discontinuity design (RDD) with April 1944 as the cutoff.[6] This "Birth RDD" requires a relatively weak assumption, continuity at the threshold over time, to identify the longer-term impact.

Regarding data on utilization, we are allowed to use semi-aggregated information by month and year of birth (MYBirth) from the National Database of Health Insurance Claims (NDB) covering almost all insurance claims in Japan. The information includes total utilization, including the total medical expenditure and the total number of claims, and utilization

[4]For a recent review, see Baicker and Goldman (2011), McGuire (2011), and Finkelstein et al. (2018).

[5]A few studies find that the longer-term impact differs from the short-term impact for children, clarifying fiscal externalises of health insurance are smaller in the longer term (Wherry et al. (2018); Goodman-Bacon (2021)). These studies focus on a large change of cost sharing, the provision of health insurance per se.

[6]The Ministry of Health, Labour, and Welfare estimates the average life expectancy for each age in each year. In 2014, men who were 70 years old were expected to live 15.5 more years, while women were expected to live 19.8 more years on average. Thus, our study covers a reasonably long period of time for the elderly, one-third, or one-fourth of their remaining lives on average.

by type of services, including potentially effective services (e.g., preventive services) and wasteful services (e.g., imaging services) (Brot-Goldberg et al. (2017)). For health outcome, we use comprehensive measures. We first examine mortality by the universal death records. We also examine clinical measures of health, self-reported health outcome, and some health-related behaviors.

We find that the increase in coinsurance rates had constantly reduced total utilization for 5 years; the reduction in the longer-term is similar to, or slightly *larger* than, the one in the short term. Specifically, the higher coinsurance rate reduces the total medical expenditure by around 2.4 percent in 1 year and 2.6 percent in 5 years. The implied elasticity is around 0.04. For the total number of claims, the higher coinsurance rate reduces it by around 3.8 in 1 year and 4.8 percent in 5 years. The implied elasticity is 0.05 and 0.07, respectively.

We then investigate the impact on utilization by type of services. Our results suggest that the reduction of less resource-intensive and wasteful services is larger. The total number of claims is more elastic than the total medical expenditure. Outpatient visits for examination and imaging are responded more than preventive care and inpatient with surgery on both of which we do not find a statistically significant impact. Consistently, we do not find discernible impacts on any measures of health outcome and health-related behaviors.

These findings suggest that for the moderate change of prices for the elderly, consumer responses are not largely affected by distinctive characteristics of medical services under health insurance that could differentiate the short- and longer-term impact, like behavioral hazard and ex-ante moral hazard. Specifically, the increase in coinsurance rates works relatively sharply. The increase does not largely reduce resource-intensive and effective services. There is no feedback effect from deteriorating health to utilization. This is why we do not find a *smaller* impact on total utilization in the longer term. While we find that the reduction in the longer term is *larger* than in the short term, the difference is not large so that ex-ante moral hazard does not play a major role for determining consumer responsiveness.

The above conclusion on the sharpness of coinsurance may come with a surprise, given that previous studies often mention the bluntness of cost sharing. Our results are, however, broadly consistent with Shigeoka (2014) and Fukushima et al. (2016) that estimate the short-term impact of the drop of coinsurance at the age of 70 in Japan using RDD. The sharpness of cost sharing could be driven by two factors. First, our study focuses on a moderate change in coinsurance. A moderate change of prices allows patients to keep access to medical services when conditions are severe, in contrast to a large change, such as the provision of health insurance per se. In fact, previous studies finding that the impact on health outcome typically focus on the provision of health insurance per se (Card et al. (2009); Baicker et al. (2013); Wherry et al. (2018); Goodman-Bacon (2021)). Here, it should be noted that while the impact of the provision of health insurance per se is critical, the impact of moderate changes of cost sharing is also policy-relevant given most developed countries offer universal coverage of health insurance. Second and relatedly, our paper focuses on a country with relatively good access to medical services. Good access could make cost sharing sharper because better access leads to higher utilization, keeping the marginal value of care low, mitigating behavioral hazard proposed by Baicker et al. (2015). Consistently, Finkelstein and McKnight (2008) find that the longer-term impact of the Medicare on mortality rates depends on access to care. Notice that if access contributes to the sharpness of coinsurance, our findings and implications should apply for countries with high access to care, while our study also suggests that considering cost sharing and access to medical services simultaneously will produce better policy discussions for designing health insurance in any country.

The remainder of this paper is organized as follows. Section 2 describes the Japanese healthcare system and the policy change of coinsurance rates that we exploit to identify the longer-term impact. Section 3 explains our data and identification strategy. Section 4 shows our results. Section 5 offers discussion and conclusion.

2 The Institutional Background

In this section, we briefly provide an overview of the Japanese healthcare system, focusing on characteristics relevant to our study.[7]

2.1 Japanese Healthcare System

Japan has a public universal health insurance system. Patients have access to any medical provider without going through a gatekeeper or having a referral letter. For example, patients can visit large hospitals, rather than clinics, for outpatient care even with relatively less serious conditions.[8]

Under the public health insurance, patients have the same benefit package of medical services. The package is comprehensive; for example, it includes both outpatient and inpatient services, and prescription drugs.

All fees for medical services are determined by the unique national fee schedule set by the Japanese government. In other words, as long as the same services are used, the same fees are applied to all patients and all medical providers. When patients use medical services, patients pay cost sharing of the fees at medical institutions, and medical providers are reimbursed from insurers.

2.2 Patient Cost Sharing and Policy Change

Patient cost sharing is characterized by coinsurance rates with cap on a monthly basis; there is no deductible. The history of policy changes is summarized in Table 1.[9] Until the policy change in April 2014, for those aged between 6-70, their coinsurance rate is 30 percent. From

[7]For details about Japanese healthcare system, see Ikegami et al. (2011), Kondo and Shigeoka (2013), Shigeoka (2014), and Fukushima et al. (2016).

[8]Outpatient visits to very large hospitals, like university hospitals, without a referral letter may require additional co-payment.

[9]The coinsurance rate for high-income earners is 30 percent, irrespective of their age. As noted in Shigeoka (2014), only a limited number of patients is classified into this category, 7 percent (Ikegami et al., 2011), because the criteria for the high-income earners is high.

the next month of reaching the age of 70, individuals become eligible to lower coinsurance rates, 10 percent.

In December 2013, the Japanese government announced a policy change to raise coinsurance rates from 10 percent to 20 percent for those between the ages of 70-74. This new coinsurance rates, 20 percent, were applied to only those who were born after April 2nd, 1944 (after April 1944), while the lower coinsurance rates, 10 percent, remained assigned to those born before April 1st, 1944 (before April 1944). Put differently, the coinsurance rates for those born after April 1944 were 20 percent for 5 years, between the ages of 70-74; in contrast, the coinsurance rates for those born before April 1944 were 10 percent after they reached at the age of 70.

Figure 1 graphically describes coinsurance rates for those born before and after April 1944 by their age. For those born before April 1944 (dashed navy line), their coinsurance rates are 30 percent until reaching at the age of 70 and drop to 10 percent afterwards. For those born after April 1944 (solid maroon line), their coinsurance rates are 30 percent until the age of 70 but become 20 percent for ages 70-74. The different coinsurance rates (20 percent versus 10 percent) are assigned based on the timing of birth (after April 1944 versus before April 1944) for 5 years between ages 70-74.

This variation of coinsurance rates by the policy change enables us to identify the longer-term impact of coinsurance rates for the elderly. Those born just before April 1944 and just after April 1944 should be comparable but have different coinsurance rates for 5 years between aged 70-74. Furthermore, since enrolling in public health insurance is mandatory and all individuals face the same fee schedule, there is no endogeneity stemming from the individual's choice of insurance plan, which is a notable challenge for studies focusing on the United States (Baicker and Goldman (2011); Ellis et al. (2017)). In addition, since medical providers get the same fees for the same medical services, regardless of cost sharing, medical providers should have few incentives to influence patient's demand, depending on cost sharing (Shigeoka, 2014).

It should be noted that because of the unique national fee schedule, the impact of higher coinsurance we estimate should reflect outright quantity reduction and/or quantity substitutions to low-cost services, not price shopping for cheaper providers. Thus, if we find the reduction of utilization by higher coinsurance, it might be welfare-improving or decreasing depending on what occurs for utilization by type of services and health outcome (Chandra et al. (2010); Baicker et al. (2015); Brot-Goldberg et al. (2017)).

One difficulty for summarizing the impact on utilization as elasticity is nonlinearlity imposed by cap where prices fall to zero (Keeler et al. (1977); Ellis (1986); Aron-Dine et al. (2013)). As noted in Shigeoka (2014), we argue that this difficulty is mitigated by two factors in Japan. First, the cap is set monthly. This means that the time for taking advantage of this zero price is limited. Even if patients immediately exceed the cap at the beginning of month, time with zero price is just one month, not one year. Second, the monthly stop-loss is set at the relatively high level. Shigeoka (2014) shows that the probability of reaching the stop-loss for those who are above the age of 70 is 0.6 percent for outpatient visits and 0.0 percent for inpatient admissions, when coinsurance rates are 10 percent.

3 Data and Identification Strategy

3.1 Data

For utilization, our data source is the National Database of Health Insurance Claims (NDB). The NDB covers almost all health insurance claims in Japan. From the NDB, we are allowed to use semi-aggregated data on utilization summed up three months from September to November in each year, from 2012 to 2019, by month and year of birth (MYBirth).[10] Thus, we cannot distinguish those who were born on April 1st and April 2nd who belong to a different side of the cutoff in our RDD; we decide not to use information of those who born in April 1944 in our analysis. Our data include the total medical expenditure, the number

[10]We do not use micro data of the NDB.

of health insurance claims, medical expenditure for outpatients, medical expenditure for inpatients, and medical expenditure for some specific medical services. The specific services are examination for outpatients, imaging for outpatients, medicine for outpatients, outpatient visits with diagnosis classified as Ambulatory Care Sensitive Conditions (ACSCs) that the Agency for Healthcare Research and Quality develops for studying preventive care (Shigeoka, 2014), and inpatients with surgery.

For health outcome, we use several measures. The first is mortality rates. We use vital statistics to compute mortality rates from 2009 to 2019 for each MYBirth. The vital statistics report the date of birth and death, and cause of death by the International Classification of Diseases (ICD) 10.

The second is clinical measures. We use the National Health and Nutrition Survey (NHNS) conducted every year in November by the Ministry of Health, Labour, and Welfare Japan (MHLW).[11] The NHNS contains three type of data: physical conditions, nutrition, and health-related behaviors. Physical conditions includes the level of blood pressure, cholesterol, and blood sugar, measured by physicians. For nutrition, respondents record their food intake, with support from nutritionists. Health-related behaviors, including exercises, is examined by a paper-based questionnaire. A drawback of the survey is small sample size.[12]

Our third measure is self-reported assessments of health from the Comprehensive Survey of Living Conditions (CSLC) by the MHLW. The health-related survey of the CSLC is conducted in June every three years, and we use the 2016 wave. Thus, estimates using this data capture the impact on outcomes around 2 years after the policy change. The CSLC includes information on self-assessed physical and mental health, and health-related behaviors. The date of birth is reported at the MYBirth level. Thus, we do not to use information of those who born in April 1944.

We also investigate some health-related behaviors: exercises, nutrition, drinking alcohol,

[11]We thank Hitoshi Shigeoka for his suggestion to use this data.

[12]For example, the number of respondents over age 70 for physical conditions is 1,677 in 2015, 6,365 in 2016, 1,577 in 2017, and 1,527 in 2018.

and smoking. These data are obtained by the NHNS (exercises and nutrition) and the CSLC (drinking alcohol and smoking) as described above.

3.2 Identification Strategy

To identify the impact of coinsurance rates over time, we exploit the difference in coinsurance rates between for those born after April 1944, 20 percent, and for those born before April 1944, 10 percent, for 5 years when they were 70-74 years old, as described in Section 2.[13] We use RDD with April 1944 as the cutoff. While all of the equations below are based on this strategy, we show three regression equations for utilization, for mortality, and for other health outcome and health-related behaviors, depending on characteristics of our data.

We first explain the equation for utilization. In the NDB, only individuals who used medical services are observed. By following Card et al. (2004) and Shigeoka (2014), we assume that the probability of utilization for underlying population smoothly changes with MYBirth. Our regression equation is

$$\ln\left(\frac{y}{pop}\right)_b = \beta \cdot post_b + f(b) + f(b) \cdot post_b + \varepsilon_b \tag{1}$$

where $(y/pop)_b$ is utilization per capita for those whose MYBirth is b, $f(b)$ is a smooth function of MYBirth b, and ε_b is an unobserved error component.[14] The $post_b$ denotes a dummy variable that takes the value of one if MYBirth b is after April 1944. The coefficient of our main interest is β. It represents the percent change of utilization by the increase in coinsurance rates. In particular, by using data in time t before or after April 2014, β captures the impact t years before or after the policy change. For example, if data in October 2018 is used, β represents the impact 4.5 years after the policy change.

[13]The recent paper by Iizuka and Shigeoka (2021) shows an asymmetric demand response when prices increase and decrease. In our study, since both those born before and after April 1944 experience a drop of coinsurance at the age of 70 from 30 percent, asymmetric responses are not issue in our study.

[14]To estimate population by MYBirth, we use the 2015 Census and the vital statistics. Specifically, Census is conducted every 5 years, including 2015, and targets and asks MYBirth to all individuals living in Japan in October 1st in survey year. By adding and subtracting the number of deaths calculated from the vital statistics, we estimate population by MYBirth at the beginning of each month in each year.

There is one challenge in estimating equation (1). Since our running variable is MYBirth, if MYBirth specific effects exist, including birth-month specific effects, they pose a challenge to estimate β. In particular, we observe that utilization by those born in summer is systematically higher. Thus, we include birth-month fixed effects in estimating (1).

Next, we explain the equation for mortality rates. The vital statistics also include only those who died. Thus, we put the same assumption as utilization. Furthermore, we use the change of mortality rates from a reference age by MYBirth as a dependent variable. The equation is

$$\Delta \ln \left(\frac{y}{pop} \right)_b = \beta \cdot post_b + f(b) + f(b) \cdot post_b + \varepsilon_b. \tag{2}$$

where $\Delta \ln(y/pop)_b$ represents the change of mortality rates from a reference age by MYBirth. By using this as the dependent variable, we control MYBirth specific effects. If MYBirth specific effects that do not vary with age exist, the effects should be cancelled out by taking changes from a reference age.[15][16][17] We choose age 69 as the reference age because the age is closest to ages for which the policy change of coinsurance rates occurs, 70-74.

Lastly, to measure the impact on other health outcome and health-related behaviors, we use individual-level data from the NHNS and the CSLC. Thus, a equation is similar to the equation (1) but a unit of observations is an individual level. The equation is

$$y_{ib} = \beta \cdot post_{ib} + f(b) + f(b) \cdot post_{ib} + \varepsilon_{ib} \tag{3}$$

where y_{ib} is health outcome or health-related behaviors for individual i whose MYBirth is b,

[15] In Appendix **??**, we formally show that estimating equation (2) gives an unbiased estimate for β, even if MYBirth specific effects exist.

[16] One may argue that we should apply the same method to utilization. Unfortunately, since we have data for the sum of three months in each year and the data does not give us information at the same age, here 69 years old, we cannot exploit this method. This is why we simply use birth-month fixed effects for utilization for utilization.

[17] In principle, year specific effects could also exist. In fact, we observe a surge of deaths in 2011 because of the Great East Japan Earthquake. We therefore exclude deaths with T75.1 (drowning) and T14.9 (injury, unspecified) in ICD-10 from our sample.

$f(b)$ is a smooth function of MYBirth b, and ε_{ib} is an unobserved error component.

Our identification strategy based on RDD with birthday as the cutoff, "Birth RDD," is related to "Age RDD" frequently used in literature (e.g., Card et al. (2008); Card et al. (2009); Anderson et al. (2012); Shigeoka (2014); Fukushima et al. (2016); Nilsson and Paul (2018); Han et al. (2020)). The "Age RDD" enables researchers to cleanly estimate the short-term impact of cost sharing; the main focus of those studies are the short-term impact based on cross-section data. The "Age RDD" does not allow researchers to precisely estimate the longer-term impact, although having some implication. In contrast, the "Birth RDD" enables us to identify the causal impact of changes in coinsurance both in the short and longer term, which is considered to be difficult to credibly estimate (Finkelstein et al., 2018), with relatively weak assumption, continuity at a threshold over time. The similar strategy is used to estimate the longer-term impact for children by Wherry et al. (2018).

4 Results

4.1 Total Utilization

Total Medical Expenditure

Before showing the result of regressing equation (1), in Figure 2 we show the pattern of total utilization by age both for those born from April 1942 to March 1944 (non-treated group, 10 percent, represented by dash navy line) and for those born from April 1944 to March 1946 (treated group, 20 percent, represented by solid maroon line) by using multiple years of data. We plot the log of total medical expenditure per capita controlling year specific effects and birth-month specific effects.[18] The figure suggests that utilization is affected by

[18] These effects are identified by regressing the log of the total medical expenditure per capita on quadratic in age, year fixed effects, and birth-month fixed effects for the non-treated group. Then, we subtract these effects from the raw number of the log of the total medical expenditure per capita for both groups. We choose this procedure to absorb these effects because a common procedure that regresses outcome on fixed effects and gets its residuals does not work for our data. Since we have data in September-November in each year by MYBirth, month of birth are systematically correlated with age. For example, suppose that we use sample who were born between April 1943 and March 1945. Then, those born in April are older than

coinsurance rates. Before the age of 70, the coinsurance rate is 30 percent for both groups, and their pattern of utilization is similar. After reaching the age of 70, the coinsurance rate is 10 percent for the non-treated group while 20 percent for the treated group. Although both groups increase utilization reflecting the drop of coinsurance rates at the age of 70, the level of utilization diverges between those groups: lower utilization for the treated group. Furthermore, and more importantly, the difference of the level of utilization between those groups remains unchanged, not narrowing or widening, until the age of 75 when the same coinsurance rate, 10 percent, is applied for both groups. This indicates that the longer-term impact on the total medical expenditure is surprisingly similar to the short-term impact.

Next, we move to the formal estimation by RDD. We start it by pooling the data in 2015-2018 and collapse them by MYBirth.[19] Figure 3 plots the log of total medical expenditure per capita controlling birth-month specific effects in the vertical axis against MYBirth in the horizontal axis. In the horizontal axis, we normalize the cutoff, April 1944, to zero; negative (positive) values represent those who were born after (before) the cutoff whose coinsurance rates are 20 percent (10 percent). We use 15 months both sides of the cutoff. Figure 3 shows that while utilization increases smoothly as cohorts get older, there is a clear jump at the cutoff, indicating lower utilization for those born after April 1944 whose coinsurance rates are 20 percent. The jump corresponds to a 2.8 percent decrease with standard error 0.5. The implied elasticity is 0.04 ($= 0.028/(\ln(0.2) - \ln(0.1))$).[20]

The above result based on data pooling multiple years could mask heterogeneous impacts over time. To examine whether the longer-term impact differs from the short-term impact,

those born in March. Thus, if we take the common procedure by regressing outcome on fixed effects without controlling age, birth-month fixed effects captures not only birth-month specific effects but also the effects of age. As a result, the common procedure produces a figure that looks like a step function.

[19] We do not use the data in 2014 here because the number of cohorts having 20 percent coinsurance rates is limited. However, even if we use the data in 2014-2018 and include a dummy that takes the value of one for those aged above 70, the result is almost the same.

[20] Aron-Dine et al. (2013) argues that summarizing the price responsiveness of non-linear insurance contract by single elasticity needs considerable caution. With this caution in mind, a comparison of elasticity with recent studies targeting adults or the elderly tells us that our estimated elasticity is close to theirs that range from around one-quarter to one-half of the well-mentioned RAND estimates by Keeler and Rolph (1988) (Chandra et al. (2010); Shigeoka (2014); Fukushima et al. (2016); Brot-Goldberg et al. (2017)) once we use the same measure of elasticity, including arc elasticity. See Appendix **??** for details.

we estimate equation (1) by year. We use 30 months both sides of the cutoff to obtain precise estimates. We add a dummy for 30 percent of coinsurance rates, if necessary. Figure 4 shows RD estimates with 95 percent confidence intervals from 2012 to 2019, namely from 1.5 years before to 5.5 years after the policy change that different coinsurance rates were assigned. All coefficients and standard errors are multiplied by 100 so that they are interpreted as percent changes. Before the policy change, RD estimates are small and statistically insignificant at the conventional level, while slightly positive.[21] After the policy change, RD estimates turn to negative statistically significantly. RD estimates are almost unchanged at around -2.5 percent, or become slightly larger in absolute value, over time from 0.5 to 4.5 years until going back to around 0 percent in 5.5 years when individuals around the cutoff reaches the age of 75. This result suggests that the longer-term impact of coinsurance rates on total medical expenditure is similar to, or slightly *larger* than, the short-term impact.

Total Number of Claims

We repeat the same analysis for the total number of claims per capita as an alternative measure of total utilization. Compared to the total medical expenditure that depends on the amount of medical resources used, the total number of claims reflects whether individuals use medical resources within a specific length of time thathealth insurance claims are issued.[22]

The results are showed in Figure 5, Figure 6, and Figure 7. A comparison between the results for the total number of claims and the total medical expenditure in previous section provides three points worth mentioning. First, we confirm that utilization is affected by coinsurance; this is the same as the total medical expenditure.

Second, in line with the total medical expenditure, we find that the longer-term impact on the total number of claims is modestly larger than the short-term impact as showed in Figure 5 and Figure 7. The higher coinsurance rate reduces it by around 3.8 in around 1 year

[21] The possible reason that RD estimates before the policy change are slightly positive is that birth-month fixed effects cannot perfectly control MYBirth specific effects. Unfortunately, we cannot take changes from a reference age as a dependent variable, like mortality rates, because of characteristics of data.

[22] In Japan, if a patient goes to a medical provider, one health insurance claim is issued per month.

and 4.8 percent in around 4-5 years. Furthermore, and interestingly, even after reaching the age of 75, utilization by those whose coinsurance rates are 20 percent is lower than those 10 percent. This evidence is against the existence of strong feedback effects from deteriorating health to utilization.

Third, the impact on the total number of claims is larger than the total medical expenditure. The jump in Figure 6 corresponds to a 4.9 percent decrease with standard error 0.4. The implied elasticity is 0.07 ($= 0.049/(\ln(0.2) - \ln(0.1))$). This suggests that individuals reduce utilization involving less medical resources per visit or admission more.[23] This in turn indicates that patients with relatively less serious conditions reduce their utilization more, because in general, more medical resources are needed for patients with serious conditions.

Importantly, the third point helps partially explain our finding on total utilization, both the total medical expenditure and the total number of claims, that the longer-term impact is similar, or modestly larger than, the short-term impact. Specifically, the third point implies that the increase in coinsurance rates works relatively sharply. It reduces less effective services more and does not adversely affect health. As a result, there is no feedback effects from deteriorating health to utilization over time. This is why we find that the reduction of utilization in the longer term is not smaller than in the short term. In the following sections, we further examine the validity of this explanation by investigating the impact on utilization by type of medical services, on health outcome, and on health-related behaviors.

4.2 Utilization by Type of Medical Services

To further investigate why the longer-term impact is similar to, or modestly larger than, the short-term impact, we estimate equation (1) by type of medical services. We use the data

[23] Subtly, while our finding that the total number of claims is more elastic than the total medical expenditure indicates that *in net* individuals reduce utilization involving less medical resources more, it could be compatible with the increase in utilization involving more medical resources *in gross*. Although we cannot exclude this possibility, we find some evidences against it. For example, inpatient with surgery does not increase over time. Furthermore, as showed in section 4.3 and 4.3, we do not find impacts on health outcome. See Appendix **??** for detailed discussion.

in 2015-2018 and 15 months both sides of the cutoff. Results are reported in Table 2.[24] All coefficients and standard errors are multiplied by 100 so that they are interpreted as percent changes. The results show that the reduction of outpatient services is larger than inpatient services. Outpatient services decrease by 3.4 percent while inpatient services decrease by 2.4 percent. Furthermore, the reduction for potentially effective care (ACSCs and inpatient with surgery) is smaller than for potentially wasteful care (examination and imaging); the estimates for ACSCs and inpatient with surgery are not statistically significant. These results supports our explanation mentioned in Section 4.1. In particular, while caution is needed due to heterogeneity of effectiveness within each service, the increase in coinsurance rates works relatively sharply.

4.3 Health Outcome

Mortality

We next examine the impact on health outcome directly, starting from mortality. As we explained in Section 3.2, we use the log of mortality rates minus the log of mortality rates at the age of 69 by MYBirth as an outcome. Figure 8 plots the outcome in the vertical axis against MYBirth in the horizontal axis. We pool the data from April 2014 to December 2018 and collapse them by MYBirth. We use 40 months both sides of the cutoff because the mortality data is noisier than the utilization data. Figure 8 shows that in contrast to utilization, there is not a clear jump at the cutoff for mortality. The jump corresponds to a 0.2 percent decrease, with standard error 1.1.

We then examine the pattern of the impact on mortality rates over time from -3.5 to 5.5 years from the policy change that different coinsurance rates are assigned by regressing equation (2).[25] For each regression, we pool 2 years; for example, to estimate the impact in October 2017, 3.5 years later, we pool the data in April 2016 to March 2018. The results are

[24] Relevant figures are in Appendix **??**.

[25] We thank Toshiaki Iizuka for his suggestion to examine the impact over time.

reported in Figure 9. The figure shows that the RD estimates do not exhibit a clear pattern over time. The estimates are not very large and statistically insignificant. In particular, the estimates ranges from -1.2 percent in 4.5 years later to 0.8 percent in 2.5 years later. The monthly mortality rate for those born in March 1944 between ages 70-74, for example, is around 120 deaths per 100,000. Thus, the estimates roughly indicates that the increase in coinsurance rates might cause ±1 deaths per 100,000 and are not statistically significant.[26]

Other Health Outcome

As a supplement analysis, we examine other measures of health outcome, both objective and subjective measures. The results are reported in Table 3. For objective measures, we use popular measures in literature, the levels of blood pressure, cholesterol, and blood sugar (e.g., Baicker et al. (2013)). We pool the data in 2015-2018. We use 365 days both sides of the cutoff. Panel A in Table 3 shows that none of the outcome are statistically different from zero, while the sample size is relatively small as noted in Section 3.1.

For subjective measures, we use three binary measures. First, by using the question about self-reported health (poor, fair, good, very good, or excellent) in the CSLC, we construct a variable on whether individuals report that their health is poor or fair. Second, the CSLC asks whether poor health impairs usual activities within a month, and we use a binary response to this question. Third, we also use the response to the question about whether individuals have any subjective symptoms. Related to this question, the CSLC asks whether they get any treatment for the symptoms among which they consider most critical.

Panel B in Table 3 reports the results. All coefficients and standard errors are multiplied by 100. The estimates are small and statistically insignificant. Interestingly, while we find a reduction of overall utilization in Section 4.1, we here find that patients do not reduce treat-

[26] In Appendix **??**, we show RD estimates over time with mortality rates, instead of the change of mortality rates from age 69, as a dependent variable. While the level of mortality rates is slightly higher due to MYBirth specific effects, higher mortality rates for those born in summer, the RD estimates with mortality rates exhibit almost the same pattern over time as the change of mortality rates from age 69. The associated figure is like just shifting up Figure 9. This result also suggests that higher coinsurance does not affect mortality rates.

ment for the most crucial symptoms much, 0.6 percent, and the estimate is not statistically significant. This is a contrasting to the results for total utilization in Section 4.1, suggesting that patients reduce utilization with less important symptoms.

Health Related Behaviors

We investigate variables capturing health-related behaviors: exercises, nutrition, drinking alcohol, and smoking. Panel C in Table 3 reports the results. We do not find a large change for those variables and none of them are statistically significant.

In sum, we do not find discernible impacts on mortality rates both in short term and in the longer term, on other measures of health outcome, and on health-related behaviors. These findings further support the explanation mentioned in Section 4.1 that the increase in coinsurance works relatively sharply. At the same time, these findings also suggest that (ex-ante) moral hazard does not play a critical role for determining utilization in the longer term, consistent with our findings for total utilization that the impact in the longer term is similar to in the short term.[27]

To conclude this Result Section, our findings suggest that for the moderate change of medical prices for the elderly, consumer responses are not largely affected by distinctive characteristics of medical services under health insurance that could differentiate the short- and longer-term impact, like behavioral hazard and ex-ante moral hazard. The longer-term impact is similar to the short-term impact. This conclusion supports the validity of relying on the short-term impact, which is typically estimated in literature, for understanding fiscal externality of health insurance.

[27]Notice that it is difficult to fully capture health outcome and health-related behaviors so that unobserved behaviors might improve unobserved health outcome slightly. For example, higher coinsurance might induce individuals to wash hands more and sleep longer, making them less likely to catch a cold over time. This is a possible reason why we find that the reduction in the longer term is slightly larger than in the short term.

5 Conclusion and Discussion

This paper estimates the longer-term impact, as well as the short-term impact, of coinsurance on utilization and health outcome. The estimation is considered to be challenging because of a lack of plausible variation (Finkelstein et al., 2018). This study overcomes this challenge by exploiting a quasi-experimental variation that brings us to the "Birth RDD." Specifically, we use the increase in coinsurance rates, from 10 percent to 20 percent, for those between ages 70-74, born after April 1944. We conduct RDD with April 1944 as the cutoff. This "Birth RDD" enables us to estimate the longer-term impact with a relatively weak assumption.

We find that the impact of coinsurance on utilization in the longer run is similar to, or modestly *larger* than, the short run. We also find that patients reduce less resource-intensive services and potentially less effective services more; we do not find a clear impact on a wide range of measures of health outcome and health-related behaviors.

These findings suggest that consumer responses are not largely affected by special characteristics of medical services under health insurance that could differentiate the short- and longer-term impact, including behavioral hazard and ex-ante moral hazard. While this conclusion is surprising given that patient cost sharing is often regarded as a blunt tool in literature, the similar conclusion is reached by the previous studies focusing on the elderly in Japan, including Shigeoka (2014) and Fukushima et al. (2016) that mainly focus on the immediate impact of the drop of coinsurance rates at the age of 70. Our study consolidates the foundation that cost sharing for the elderly could work relatively sharply by providing the evidence in the longer term.

Is there any other factors contributing to the sharpness than age? While identifying its exact answer(s) only from this paper is difficult, we highlight potential factors associated with the focus in this study. First, our study focuses on a moderate change of coinsurance. Because of the moderate change of prices, patients do not lose their access to medical services. In fact, although the impact on health outcome for adults tends to be not discernible compared to children, several studies finding that health insurance improves health outcome for (low-income) adults, including mortality and self-reported physical and mental health, focus on the provision of health insurance per se (Card et al. (2009); Finkelstein et al. (2012); Miller et al. (2021)). In addition, Brot-Goldberg et al. (2017) find that the introduction of deductibles works bluntly. As long as patients respond to spot prices, deductibles can be a large change of prices. Also, changes from or to zero price could make cost sharing blunt (Iizuka and Shigeoka, 2022).

Second and relatedly, we focus on Japan where access to medical services is relatively good. Better access to medical services could make coinsurance sharper. Access to medical services not only depend on cost sharing but also other factors, including full price of medical services, a gatekeeping, and location of medical providers. Some statistics indicate that in Japan, access to medical services is relatively good. For example, the number of beds per 1,000 is 13.0 while 2.9 for the United States (OECD Health Statistics 2020).

Good access could make cost sharing sharper because better access leads to higher utilization, making the marginal value of care lower. For example, the annual average number of outpatient visits is around 13.0 in Japan while 4.0 for the United States (OECD Health Statistics 2020). It mitigates behavioral hazard articulated by Baicker et al. (2015). Consistently, Finkelstein and McKnight (2008) find that the longer-term impact of the Medicare on mortality rates depends on access to care. If access is a reason for coinsurance to work sharply, our findings and implications should be interpreted as the ones for the increase in coinsurance rates for the elderly in setting where access is relatively good; at the same time, our results suggest that considering the level of cost sharing and access to medical services simultaneously will produce better policy discussions for designing health insurance.

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Tables

Table 1: Patient Cost Sharing in Japan

	Below 70		From 70 to 74				Above 75	
			Born Before April 1944		Born After April 1944			
	Coinsurance (%)	Stop-loss (thousand yen)	Coinsurance (%)	Stop-loss (thousand yen)	Coinsurance (%)	Stop-loss (thousand yen)	Coinsurance (%)	Stop-loss (thousand yen)
April 2008	30	80.1+(TC-267) ×0.01	10	44.4	-	-	10	44.4
April 2014	30	80.1+(TC-267) ×0.01	10	44.4	20	44.4	10	44.4
August 2017	30	80.1+(TC-267) ×0.01	10	57.6	20	57.6	10	57.6
August 2018	30	80.1+(TC-267) ×0.01	10	57.6	20	57.6	10	57.6

Notes: Stop-loss showed in the table is imposed monthly at the household level. From August 2017, if households exceed stop-loss three times within a single year, stop-loss for the remaining months drops to 44.4 thousand yen. In addition to the stop-loss showed in the table, there is stop-loss for outpatient visits for individuals above 70 years old: 12.0 thousand yen until July 2017, 14.0 thousand yen from August 2017 to July 2018, and 18.0 thousand yen from August 2018. TC means household's total medical expenditure per month.

Table 2: RD Estimates for Utilization by Type of Services

	Coefficient	Elasticity		Coefficient	Elasticity
Total			*Outpatient*		
Expenditure	-2.80	0.040	All	-3.38	0.049
	(0.54)			(0.49)	
No. of Claims	-4.87	0.070	Examination	-3.55	0.051
	(0.43)			(0.42)	
			Image	-3.76	0.054
Inpatient				(0.50)	
All	-2.44	0.035	Medicine	-4.17	0.060
	(0.69)			(0.29)	
With Surgery	-1.24	0.018	ACSCs	-1.34	0.019
	(1.29)			(1.10)	

Notes: Each cell in the column "Coefficient" reports the estimate of coefficient for post dummy that takes the vale of one for those born after April 1944 in equation (1). Robust standard errors are in parentheses. All estimates of coefficients and standard errors are multiplied by 100 so that they can be interpreted as percent changes. In the column "Elasticity," we report the implied elasticity that is obtained by dividing the estimate of coefficient by $(ln(0.2) - ln(0.1))$. We use data in 2015-2018, and collapse them by month and year of birth. We use 15 months in both sides from the cutoff, April 1944; sample size is 30. The specification is linear in month and year of birth, interacted with post dummy, and fixed effects for month of birth. See RD figures in appendix **??**

Table 3: RD Estimates for Health Outcome

Panel A. Objective Measures	
Blood Pressure	
Systolic (mm Hg)	
Coefficient	0.81
	(2.89)
Mean	138.3
Observations	1,099
Diastolic (mm Hg)	
Coefficient	-0.38
	(1.79)
Mean	79.8
Observations	1,098
Cholesterol	
Total (mg/dl)	
Coefficient	-1.00
	(5.34)
Mean	203.4
Observations	1,018
HDL (mg/dl)	
Coefficient	-2.23
	(2.82)
Mean	61.2
Observations	1,018
LDL (mg/dl)	
Coefficient	-1.03
	(5.00)
Mean	116.2
Observations	1,018
Blood Suger	
HbA1c (%)	
Coefficient	-0.03
	(0.10)
Mean	5.9
Observations	1,014

Panel B. Subjective Measures	
Self-reported Health Not-fair or Poor (1)	
Coefficient (2)	-0.73
	(0.84)
Observations	78,816
Poor Health Imparing Usual Activities (1)	
Coefficient (2)	-0.26
	(0.61)
Observations	76,722
Subjective Symptoms Last a few Days (1)	
Coefficient (2)	-1.27
	(1.09)
Observations	78,021
Treatment for the Most Important One (1)	
Coefficient (2)	-0.59
	(1.39)
Observations	33,984
Panel C. Health Related Behaviors	
No. of Days per Week, Doing Exercise	
Coefficient	-0.33
	(0.46)
Observations	1,168
Nutrition (Calories)	
Coefficient	10.54
	(27.01)
Observations	1,084
No. of Days per Week, Drinking (1)	
Coefficient	0.01
	(0.06)
Observations	77,101
Smoking (1)	
Coefficient (2)	-0.58
	(0.71)
Observations	76,890

Notes: Each cell reports the estimate of coefficient for the dummy that takes the vale of one for those born after April 1944. Robust standard errors are in parentheses. Note (1) indicates that the 2016 CSLC is used. We use 60 months in both sides from the cutoff. Others use the NHNS. We use 365 days in both sides from the cutoff. The specification is quadratic in month and year of birth, fully interacted with post dummy. Note (2) indicates that all estimates of coefficients and standard errors are multiplied by 100 so that they can be interpreted as percent changes. See RD figures in appendix **??** and **??**.

Figures

Figure 1: The Schedule of Coinsurance Rates around Ages 70-74

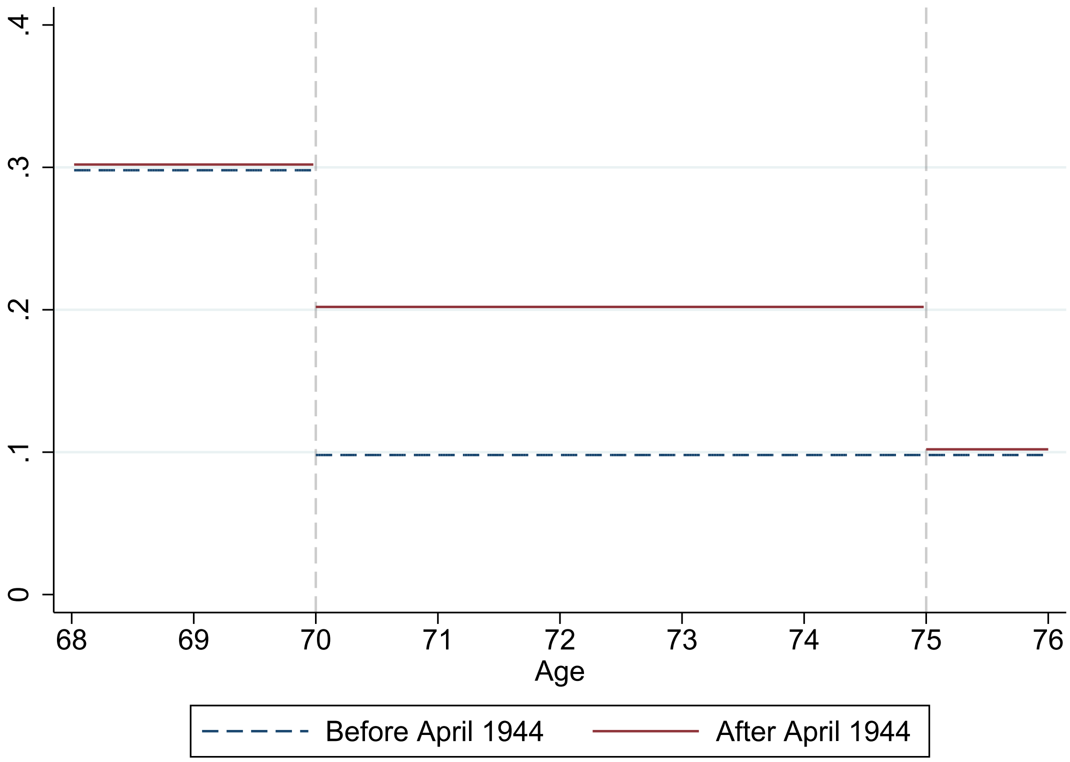
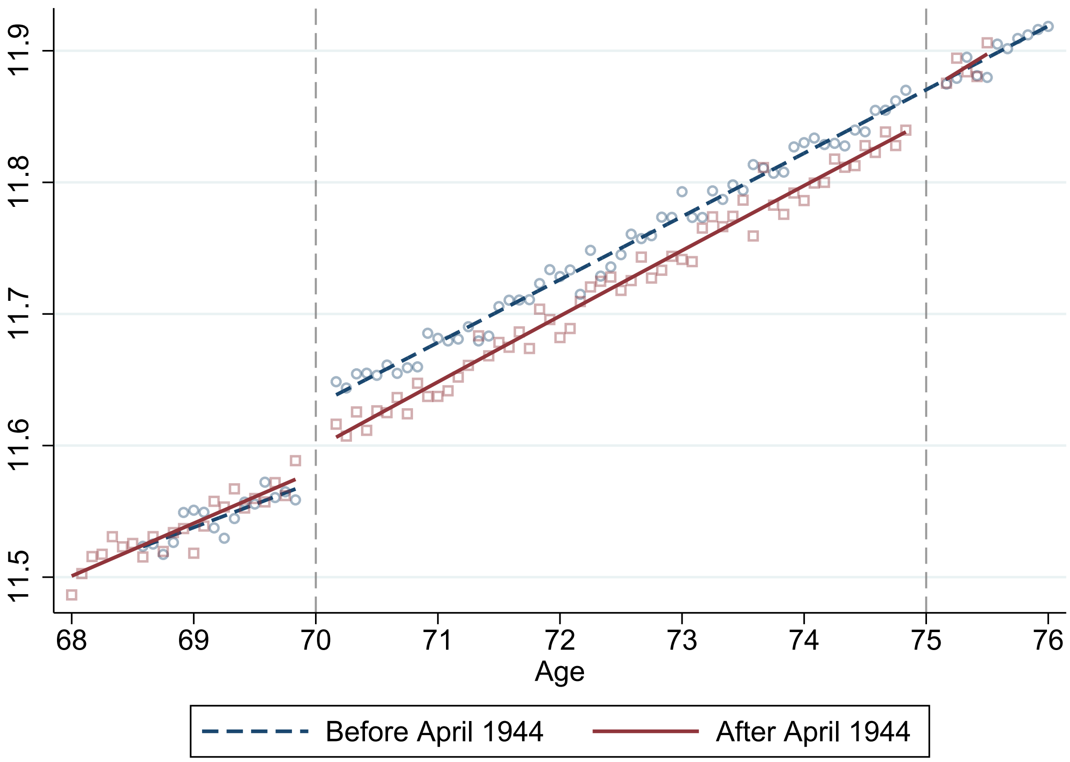
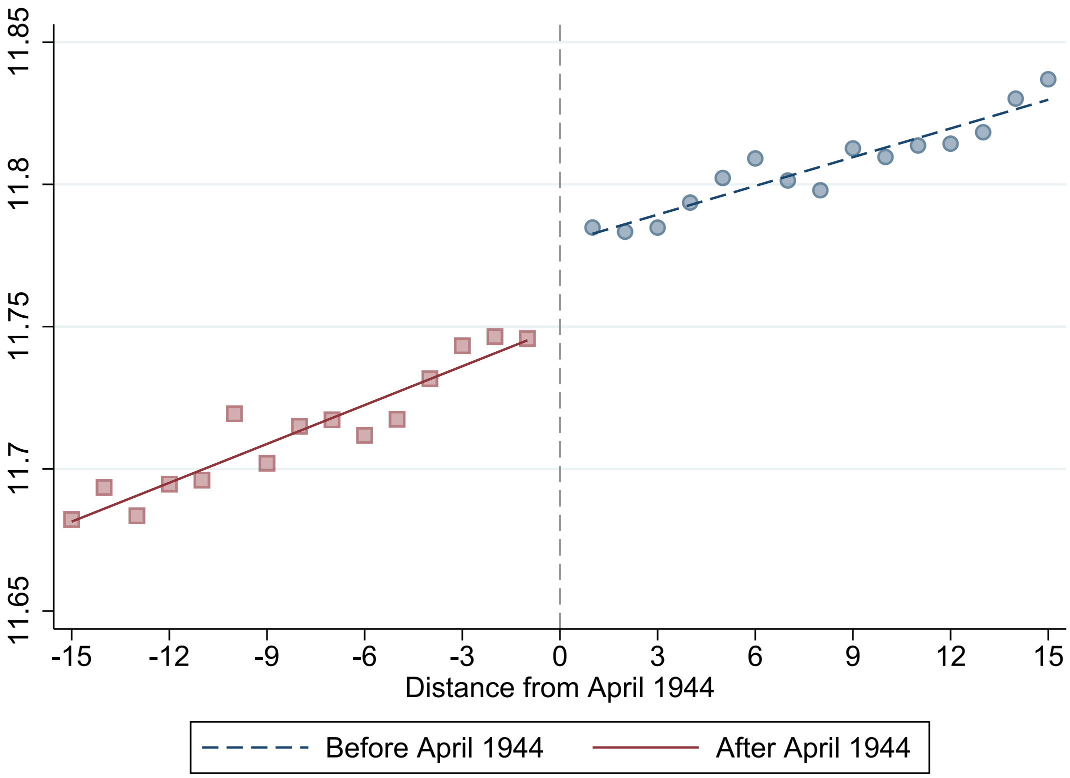


Figure 2: Total Medical Expenditure for Treated and Non-treated Group



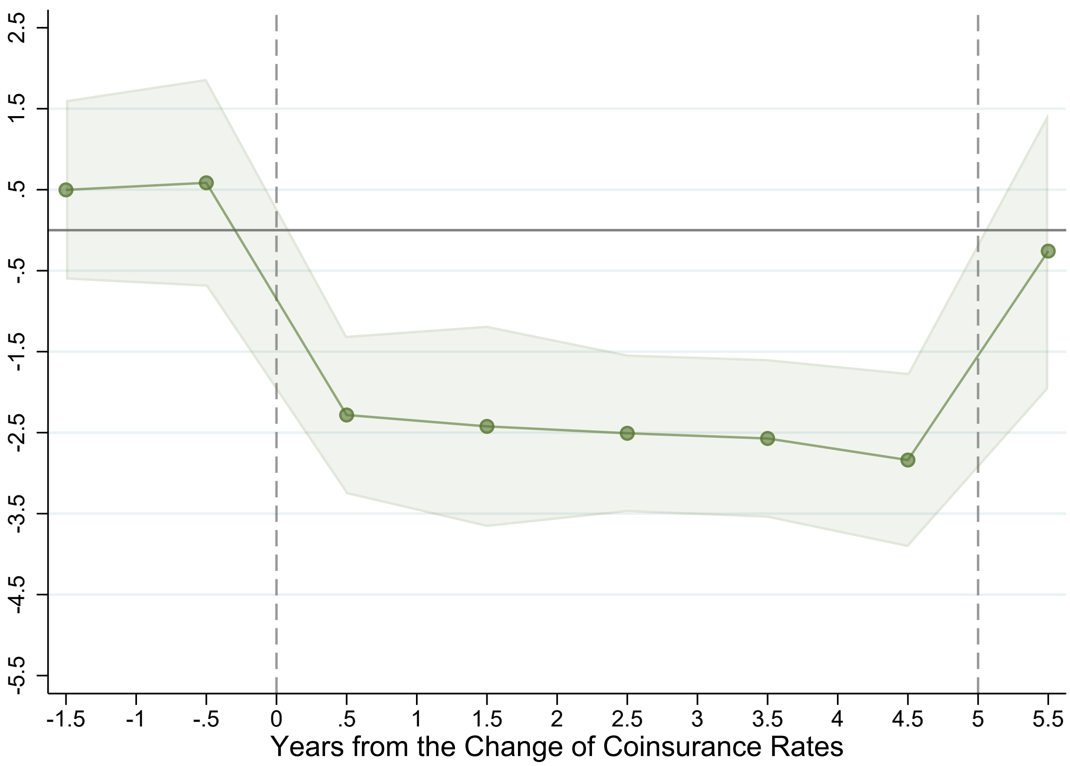
Notes: This figure plots the log of total medical expenditure per capita by age for treated group including those born after April 1944 (maroon solid curve) and non-treated group including those born before April 1944 (navy dash curve). The treated group includes those who were born between April 1942 and March 1944. Their coinsurance rate between 70-74 is 20 percent. The non-treated group includes those born between April 1944 and March 1946. Their coinsurance rate between 70-74 is 10 percent. Each marker represents the log of outcome excluding year specific effects and birth-month specific effects. These effects are identified by regressing the log of outcome on quadratic in age, year fixed effects, and birth-month fixed effects for the non-treated group. Since the data is the sum of three months (September to November) of the total medical expenditure, age in months in this figure represents age in October. Markers around age 70 and 75 are excluded.

Figure 3: Total Medical Expenditure



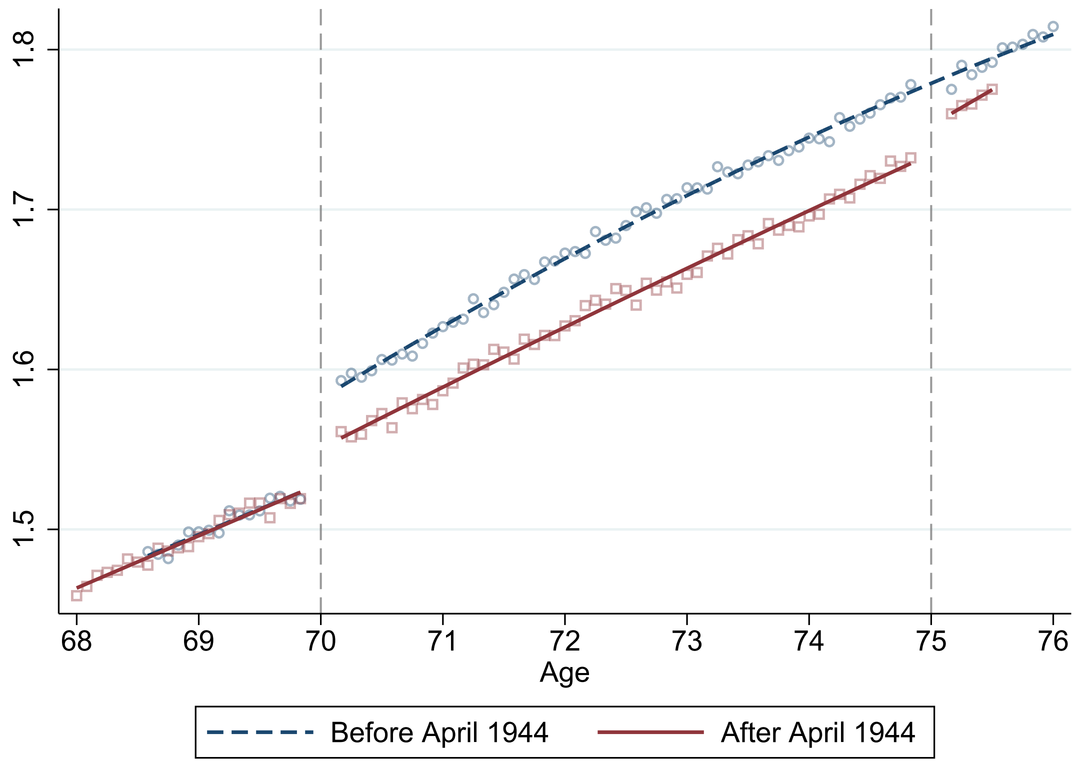
Notes: We pool the claims data from 2015-2018, and collapse them by month and year of birth. Each marker represents the log of outcome excluding birth-month specific effects. The effects are identified by regressing the log of outcome on quadratic in age, and birth-month fixed effects for those born between April 1942 and March 1944. The lines represent fitted values regressing on linear in month and year of birth, interacted with a dummy that represents those born after April 1944. Distance from April 1944 in a horizontal axis represents older cohorts as it gets larger. For example, the Distance 12 indicates those born in April 1945.

Figure 4: RD Estimates by Year: Total Medical Expenditure



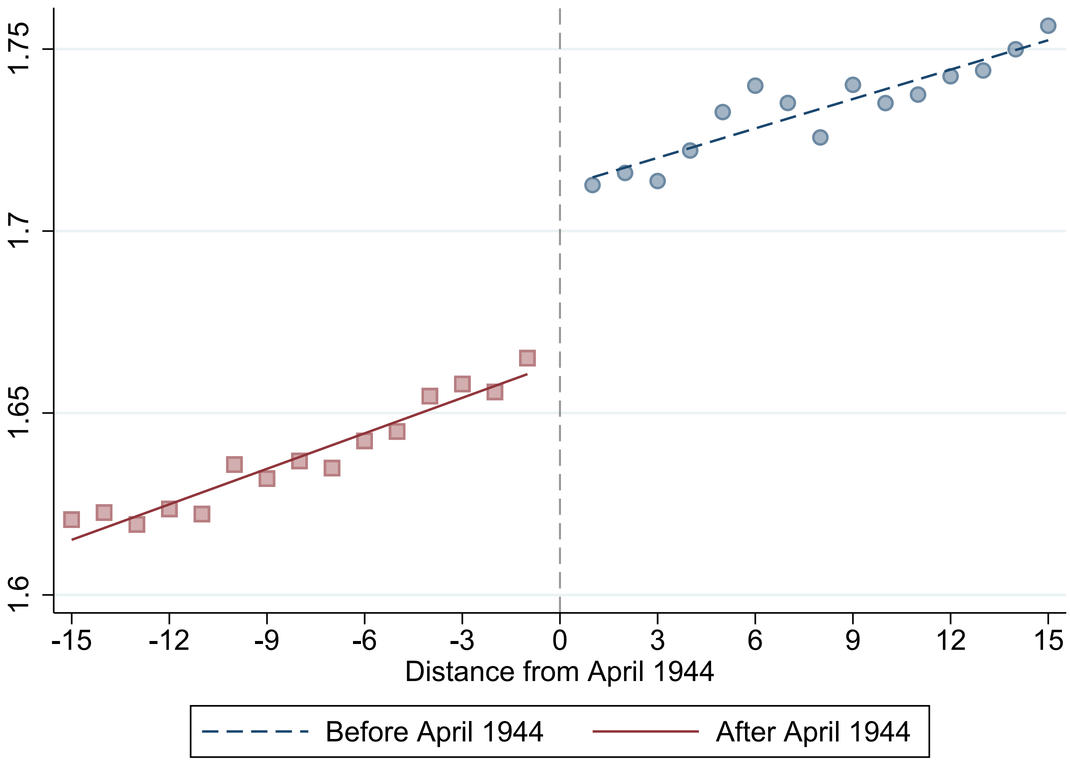
Notes: Each marker represents the RD estimate by year for post dummy in equation (1) with 95% confidence interval. For example, the marker for 3.5 years represents the RD estimate by using the data in September-November 2017. In each regression, we use 30 months both sides of the cutoff. We include a dummy that takes the value of one for those who are below the age of 70 for -1.5, -0.5, 0.5, and 1.5. The specification is linear in month and year of birth, interacted with post dummy, and fixed effects for month of birth. All RD estimates and standard errors are multiplied by 100 so that they can be interpreted as percent changes.

Figure 5: Total Number of Claims for Treated and Non-treated Group



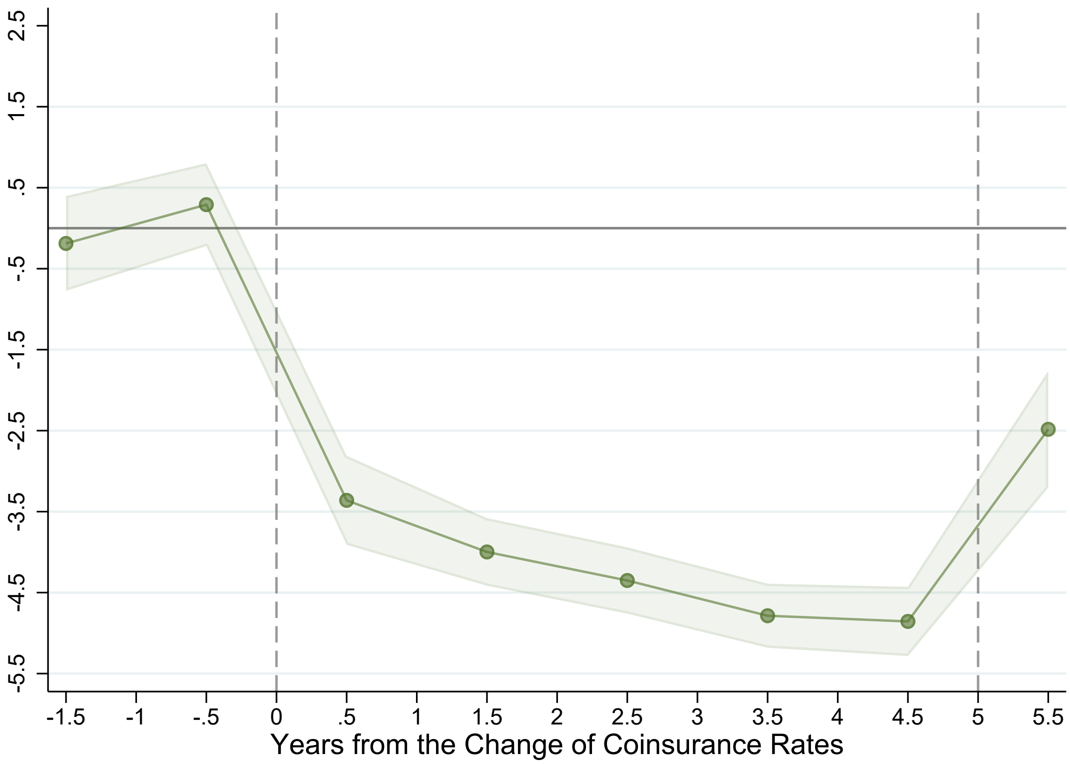
Notes: This figure plots the log of total number of health insurance claims per capita by age for treated group including those born after April 1944 (maroon solid curve) and non-treated group including those born before April 1944 (navy dash curve). The treated group includes those who were born between April 1942 and March 1944. Their coinsurance rate between 70-74 is 20 percent. The non-treated group includes those born between April 1944 and March 1946. Their coinsurance rate between 70-74 is 10 percent. Each marker represents the log of outcome excluding year specific effects and birth-month specific effects. These effects are identified by regressing the log of outcome on quadratic in age, year fixed effects, and birth-month fixed effects for the non-treated group. Since the data is the sum of three months (September to November) of the total number of claims, age in months in this figure represents age in October. Markers around age 70 and 75 are excluded.

Figure 6: Total Number of Claims



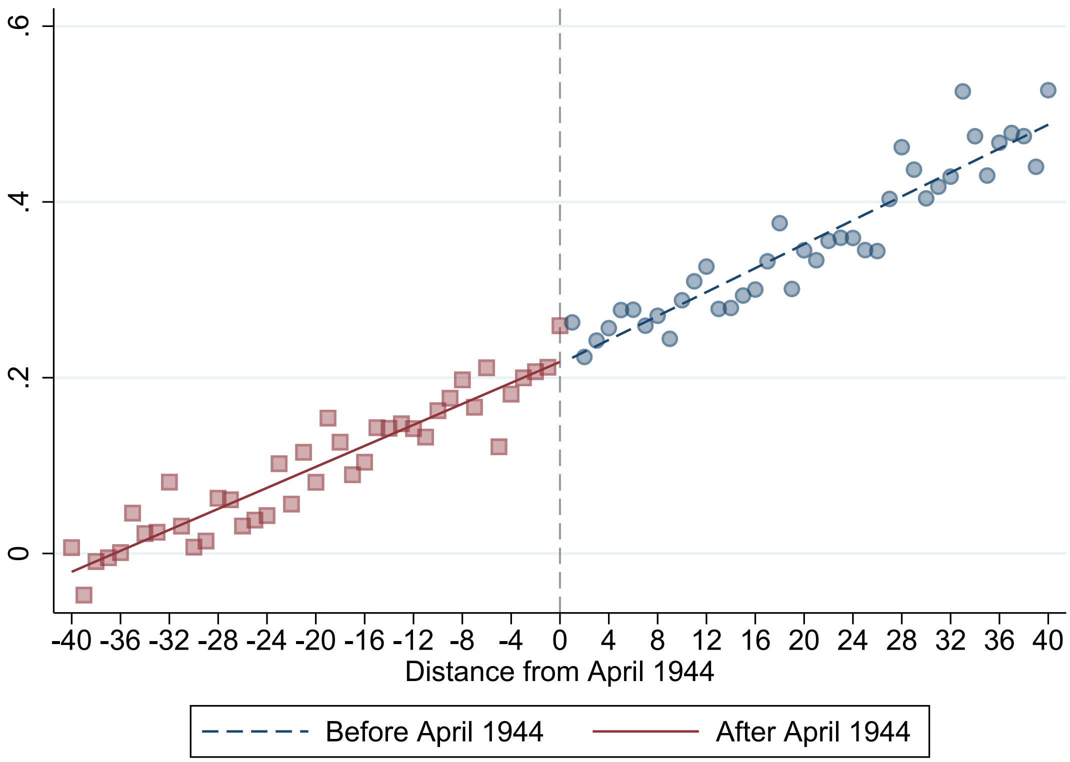
Notes: We pool the claims data from 2015-2018, and collapse them by month and year of birth. Each marker represents the log of outcome excluding birth-month specific effects. The effects are identified by regressing the log of outcome on quadratic in age, and birth-month fixed effects for those born between April 1942 and March 1944. The lines represent fitted values regressing on linear in month and year of birth, interacted with a dummy that represents those born after April 1944. Distance from April 1944 in a horizontal axis represents older cohorts as it gets larger. For example, the Distance 12 indicates those born in April 1945.

Figure 7: RD Estimates by Year: Total Number of Claims



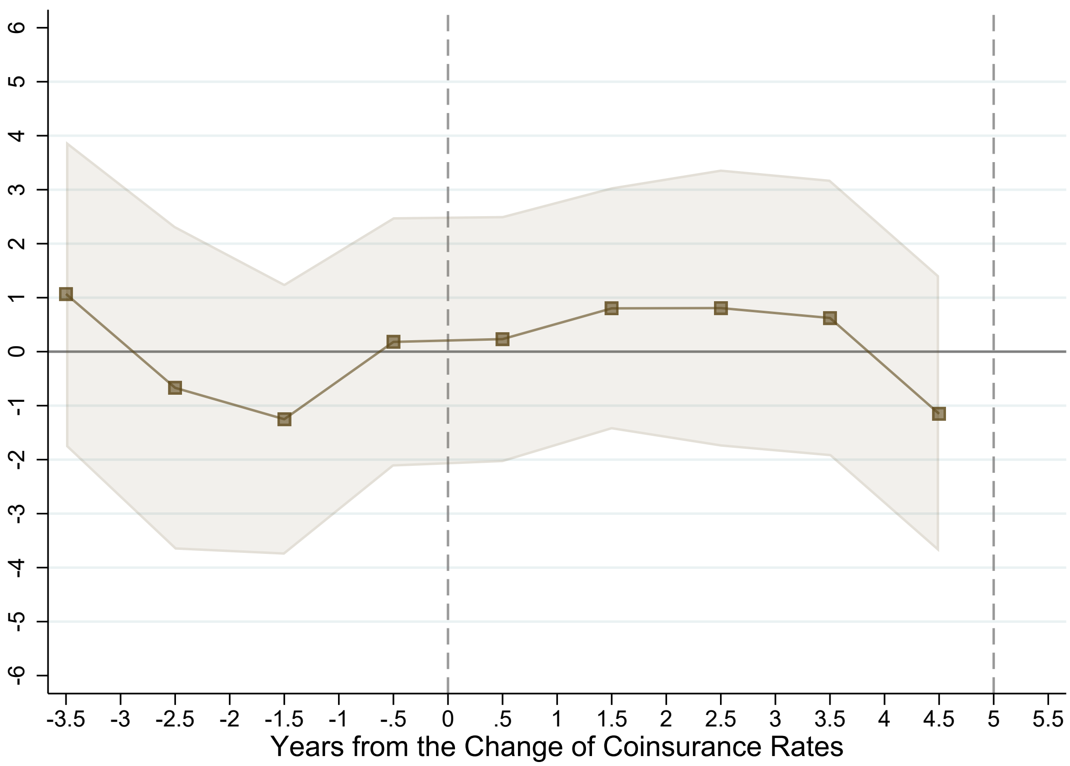
Notes: Each marker represents the RD estimate by year for post dummy in equation (1) with 95% confidence interval. For example, the marker for 3.5 years represents the RD estimate by using the data in September-November 2017. In each regression, we use 30 months both sides of the cutoff. We include a dummy that takes the value of one for those who are below the age of 70 for -1.5, -0.5, 0.5, and 1.5. The specification is linear in month and year of birth, interacted with post dummy, and fixed effects for month of birth. All RD estimates and standard errors are multiplied by 100 so that they can be interpreted as percent changes.

Figure 8: Mortality Rates



Notes: We pool the universal death records from 2014-2018, and collapse them by month and year of birth. Each marker represents the log of mortality rates minus the log of mortality rates whose age is 69. The lines represent fitted values regressing on linear in month and year of birth, interacted with a dummy that represents those born after April 1944. Distance from April 1944 in a horizontal axis represents older cohorts as it gets larger. For example, the Distance 12 indicates those born in April 1945.

Figure 9: RD Estimates by Year: Mortality Rates



Notes: Each marker represents the RD estimate for post dummy in equation (2) at each point of time, with 95% confidence interval. We estimate equation (2) at each point of time by pooling the data for 2 years. For example, the marker for 3.5 years represents the RD estimate by using the data from October 2016 to September 2018. In each regression, we use 40 months both sides of the cutoff. The specification is linear in month and year of birth, interacted with post dummy, and fixed effects for month of birth. All RD estimates and standard errors are multiplied by 100 so that they can be interpreted as percent changes.