Is Zero a Special Price? Evidence from Child Healthcare*

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Abstract

Do consumers react differently to zero prices? We test the presence of a zero-price effect in child healthcare and find that a zero price is special as it boosts demand discontinuously. A zero price affects resource allocations by encouraging healthier children to use more services and exacerbates behavioral hazard by increasing inappropriate use of antibiotics. A copayment, of as small as USD 2 per visit, alleviates these problems without substantially increasing financial risk. However, a zero price may be used to boost demand for highly cost-effective treatments. Zero and non-zero prices should be strategically chosen to achieve specific goals.

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1. Introduction

Is the zero price special in how it affects consumers? Do consumers demand disproportionally more if the price is zero instead of a very low price? The "zero-price" effect may exist if consumers strongly associate greater benefits with free products, and if a zero price increases valuation from when it is a low price (Shampanier *et al.* 2007). We investigate the zero-price effect in the context of child healthcare, where the consumer's cost-sharing is often zero (free care). In fact, providing a free service is quite common in healthcare; many tax-financed healthcare systems, such as in the UK and Canada provide care without any out-of-pocket costs. Countries with a social health insurance system, including Germany, the Netherlands, and Sweden, also provide child healthcare free of charge. State Medicaid-managed care programs in the United States also do not charge co-pays for children. More broadly, there are many instances where governments provide free public services.

Studying the zero-price effect is important because, if it exists, charging zero or non-zero prices could affect welfare. For example, in the context of healthcare services, the zero-price effect would imply that charging a very low price (instead of free care) would substantially reduce moral hazard. As a small price does not impose substantial financial risks to consumers, there is a good chance that it might improve welfare. By contrast, if certain diagnoses and procedures are highly cost effective, completely eliminating cost sharing for such services would help boost the demand for high-value care.

Although free services are quite common in healthcare, empirical evidence on the zero-price effect is rare. A fundamental challenge in identifying a zero-price effect is that one must have multiple price points near the zero price, but it is rare to observe such price changes in a single market. Thus, most existing studies estimate a demand elasticity by relying on a single price change. For example, Chandra *et al*. (2010) examine the copayment change from zero to approximately USD 10 but they only have one positive price near zero (USD 10) so they cannot tell whether the demand responses away from zero are different. The "gold standard" RAND Health Insurance Experiment (RAND HIE) has the zero-price (free) arm, but because the second lowest coinsurance rate (i.e., 25%) is far from zero, it is impossible to examine the zero-price effect.

In this paper, we take advantage of the rapid expansion in subsidies for child healthcare in the last decade in Japan to identify the zero-price effect. There is substantial variation both in the levels and forms of cost-sharing in Japan: most municipalities have reduced the coinsurance rate to zero (i.e., free care) from the national 30% set by the central government, some have reduced the coinsurance rate to 10%, 15%, or 20%, whereas others have imposed small copayments such as JPY 200, 300, or 500 (USD 2, 3, or 5) per visit.[1] In particular, having multiple positive prices (e.g., USD 2, 3, and 5 per visit) near the zero price allows us to credibly test the zero-price effect. To this end, we develop a novel dataset by hand-collecting data on the exact timing as well as the contents of the subsidy expansions at the exact *month* level for 10 years (April 2005–March 2015). We combine this information with individual-level monthly panel data on item-by-item healthcare utilization.

We determine the zero-price effect as follows. First, we estimate the effects of cost sharing on demand in a difference-in-differences (DID) framework, exploiting all variations in cost sharing created by municipal subsidies. Using the estimates, we then test whether demand elasticities vary by the level of cost sharing. Whereas this analysis reveals that demand becomes more elastic as the price approaches zero, we cannot yet say that zero is a special price; that is, demand discontinuously increases at a zero price. To obtain more concrete evidence, we next test whether the observed demand at the zero price deviates from the expected one implied by the demand responses near zero prices. To the best of our knowledge, this is the first study to credibly test the presence of the zero-price effect in a field setting.[2]

We arrive at three major findings. First, we find that the zero-price effect exists. A zero price is special, and it discontinuously boosts demand. Our estimates suggest that an infinitesimal price increase from a zero price reduces the probability of visiting a physician at least once per month by 4.8%. This is not a small effect; for example, the impact is roughly as much as half of the corresponding effect of a 10% coinsurance (9.1%). This result implies that a small copayment generally has the attractive feature of reducing the moral hazard associated with a zero price, while not increasing the financial burden to the patient by much. The benefit may be reduced, however, if paying a small fee is psychologically burdensome to consumers.

Second, we investigate how small copayments reduce utilization by examining the type of patient (healthy vs. sick) and the visit margin (extensive vs. intensive). We find that a small copayment reduces utilization by healthier children who frequently visit a physician. By contrast, a small copayment does not deter sicker children from visiting a physician at least once per month (i.e., the extensive margin). This result implies that a small copayment works as a screening device and helps allocate resources to sicker

[1] For simplicity, we use an exchange rate of 100 JPY/USD throughout the study.

[2] We are aware that two recent working papers also examine the zero-price effect. Drake *et al.* (2021) studies the effect of a zero-insurance premium on insurance take-up rates and plan choices, finding no evidence that supports the zero-price effect. However, the authors argue that this result may be driven by the specific enrollment rules adopted in Colorado. Ching *et al.* (2021) examines the choice of therapeutically equivalent medications and finds a zero-price effect. Our paper differs from that one in that we study all outpatient care rather than specific medications, whether small copayments differentially affect healthy versus sick (i.e., screening effect), and how small copayments affect behavioral hazard.

patients.

This result relates to recent work in public economics that examines the role of "ordeals" in program design. Recent studies have investigated whether ordeals—non-price transaction costs such as extensive paperwork and application complexity—increase or decrease targeting efficiency in the take-up of social insurance programs, and have shown mixed results (Deshpande and Li 2019; Finkelstein and Notowidigdo 2019; Homonoff and Somerville 2020; Domurat *et al.* 2021; Goldin *et al.* 2021). In particular, Finkelstein and Notowidigdo (2019) show that public assistance to reduce information and transaction costs associated with applying for a food assistance program results in weaker targeting by encouraging relatively less needy people to apply for the program. Echoing this finding, our results indicate that subsidies that reduce the cost of receiving care to zero tend to invite relatively healthy children to use more healthcare services.

Third, we examine whether a zero price exacerbates "behavioral hazard" (Baicker *et al*. 2015). Baicker *et al*. (2015) argue that people often misperceive the benefits and costs of treatments and overuse low-value care or underuse high-value care. They suggest that cost sharing should then reflect the degree of behavioral hazard in addition to that of moral hazard. Consistent with this argument, we find that a zero price substantially increases inappropriate use of antibiotics (low-value care), and that cost sharing as low as USD 2 per visit will help mitigate the problem. However, we also find that a zero price also increases preventive services (high-value care) such as screening of major depression and ADHD that are considered cost effective for children and youth (Maciosek *et al*. 2017; USPSTF 2020; Subcommittee on Attention-Deficit/Hyperactivity Disorder 2011). For such services, policy makers may exploit the zero-price effect to boost demand by eliminating any positive cost sharing. The key is to recognize that a zero price is not just another price and to use zero and non-zero prices strategically to achieve specific objectives. These findings support the proposition of value-based insurance design (Fendrick *et al.* 2001) that cost-sharing should be adjusted based on the value of the care.

In the domain of health economics, our paper is related to several studies that investigate whether the demand for health insurance is sensitive to the raising of premiums from zero (e.g., Buchmueller and Feldstein 1997; Dague 2014; Finkelstein *et al.* 2019). For example, Dague (2014) finds that an increase in the premium from zero to USD 10 per month reduces the enrollment in Medicaid but other discrete changes in premium amounts do not affect enrollment. However, none of these studies credibly test the presence of the zero-price effect because they do not have multiple positive prices near zero.

Outside of health economics, studies have examined whether zero is a special price in experimental settings. As discussed before, Shampanier *et al.* (2007) is a seminal study in behavioral economics that shows that reducing the price from a low positive price to zero discontinuously boosts the demand for a product in a laboratory.[3] Moreover, in a controlled laboratory experiment, Gneezy and Rustichini (2000) find that there is a discontinuity at a zero price in the effect of monetary compensation on performance. Interestingly, they find that a very small monetary compensation reduces performance relative to no compensation. In development economics, Cohen and Dupas (2010) and Ashraf *et al.* (2010) examine whether paying an initial price of zero affects subsequent use of anti-malarial bed nets and water purifying chemical, respectively, and find no such effects. Although the motivation of these papers is different from ours, they are broadly related to an economic analysis of zero prices.

This study also contributes to the growing literature on behavioral health economics (see Chandra *et al.* 2019 and Rice 2013 for reviews). Recent studies show that demand for healthcare and health insurance does not necessarily follow classical demand theory, and exhibits myopia (e.g., Einav *et al.* 2015), behavioral hazard (Baicker *et al.* 2015), inertia (e.g., Abaluck and Gruber 2011), and demand asymmetry (Iizuka and Shigeoka 2021). We contribute to the literature by further showing that zero is a special price to consumers. Finally, this study relates to a small but growing body of literature on the price elasticity of child healthcare (e.g., Nilsson and Paul 2018; Han *et al*. 2020). We contribute to the literature by examining demand elasticities at different levels and forms of cost sharing.

The remainder of the paper is organized as follows. Section 2 provides the institutional background of the study. Section 3 describes the data, and Section 4 presents our identification strategy. Section 5 reports zero-price effects, and Section 6 explores behavioral hazard. Section 7 concludes.

2. Background

2.1. Healthcare system in Japan

Japan's universal health insurance system is heavily regulated by the government. All citizens are required to enroll in either an employment- or residential-based insurance system (see, e.g., Ikegami and Campbell 1995; Kondo and Shigeoka 2013). They receive identical benefits—regardless of insurance type—which include outpatient services, inpatient services, dental care, and prescription drugs.

The unique institutional background of Japan offers several advantages in identifying patient price responsiveness because the roles of insurers and medical providers are relatively limited. First, enrollment in health insurance is mandatory, which eliminates the adverse selection problem that complicates other studies. Second, there are no restrictions from insurers on patients' choice of medical providers; patients

[3] Douven *et al.* (2020) also show that in a hypothetical experiment, demand for health insurance exhibits the zero-price effect.

have direct access to specialist care without going through a gatekeeper or a referral system, unlike the United States, where managed care restricts patients' choices. Third, physicians and hospitals are paid solely based on the same national fee schedule regardless of provider and patient type. Consequently, any changes in utilization result only from changes in quantity instead of prices. Price discrimination by medical providers and patient price shopping are nonexistent in this market. Furthermore, uniform pricing allows us to quantify the monetary values of utilization easily, unlike in the United States, which is notorious for the complexity of its pricing schedule. Finally, because medical providers are paid by fee-for-service instead of capitation, they have a strong incentive to meet demand. Thus, capacity constraints are rarely binding even under free care and patients can usually see a medical doctor on the day they become sick.

2.2. Patient cost-sharing

Patient cost-sharing has been set at 30% nationally except for the following two population segments: young children and the elderly.[4] In particular, the cost-sharing is set at 20% for children aged under 6 years. The insurer pays the remaining expenses until the beneficiary meets the stop-loss, above which the patient pays a 1% coinsurance.[5] Unlike a typical health insurance plan in the United States, there are no deductibles in Japan. In addition, supplementary private health insurance covering out-of-pocket costs are virtually non-existent in Japan, probably because the stop-loss prevents catastrophic income losses resulting from illness. As a result, municipal subsidies affect the price of health care for children.

The non-linearity imposed by the stop-loss is an important challenge in estimating price elasticities (Keeler *et al.* 1977; Ellis 1986; Aron-Dine *et al.* 2015). However, it is less so in this setting for two reasons. First, only 0.067% individual-months exceed the stop-loss, as hospitalization—which is costly and the main reason for reaching the stop-loss—is very rare among children of this age group (only 0.26% of individual-months). Second, the stop-loss resets each month in Japan, whereas it is set annually in most types of health insurance in the United States. This shorter interval makes it more difficult for patients to take advantage of the stop-loss. Thus, dynamic price issues are likely to be negligible in our case.

2.3. Municipal subsidy

Importantly, many municipalities in Japan provide subsidies for child healthcare for their residents,

[4] For studies on patient cost-sharing for the elderly, see Chandra *et al.* (2010, 2014, 2021) in the United States, and Shigeoka (2014) and Fukushima *et al.* (2016) in Japan.

[5] The individual stop-loss for a regular worker, for example, is set at USD 801 per month, and he/she has to pay 1% of any medical expenditure over this amount.

to ensure access to essential medical care for children and lighten the financial burden on parents. The municipal subsidies are seamlessly integrated into the national health insurance and add little burden to the provider. Children who are eligible for the subsidy receive an additional insurance card, and by simply showing it, they can receive discounts at medical institutions. Municipalities mail a subsidy card to eligible children approximately 1 month before the start of eligibility. In addition, parents are notified by mail approximately 1 month before the subsidy expires. The subsidy card also reports the age ranges covered and the level of cost-sharing. Thus, parents are usually aware of price changes and whether their children are eligible.

As mentioned in the introduction, a major contribution of this study is the construction of a new dataset with detailed subsidy information at the municipality-age-time level (where both age and time are measured in *months*). As information at the monthly level is not formally collected by either the prefectural or the central government, we hand-collect it through a variety of sources, including municipality web pages, local newspapers, and municipal ordinances. After collecting the data, we directly contact each municipality and verify the accuracy of our information. As collecting data in this manner takes substantial time and effort, we limit data collection to all municipalities in the six largest prefectures in Japan, resulting in a total of 294 municipalities.[6] According to national statistics, these six prefectures cover as much as 44.9% of children aged 0–15 years in Japan.

As each municipality determines whether to provide a subsidy, the level of patient cost-sharing depends on: (1) where the child lives (municipality); (2) when the child visits the medical providers (time); and (3) how old the child is at the time of the visit (age). Crucially, our claims data include information in the municipality of residence. Therefore, we can identify the level of cost-sharing that each child receives. The variations in these three dimensions are the sources of our identification strategy.

For each municipality, we collect the following information on outpatient care subsidies from April 2005 to March 2015[7]: (1) ages that the subsidy covers; (2) the level and form of municipal subsidy; (3) whether the subsidy applies at the point of service or a refund; and (4) whether there are any household income restrictions for subsidy eligibility. To the best of our knowledge, there are no other dimensions of

[6] This includes municipalities that merged during this sample period. There were a total of 47 prefectures and 1,719 municipalities in Japan as of January 2015.

[7] We also collect information on subsidies for *inpatient* care. However, most municipalities had already covered inpatient care until the age of 15 (the end of junior high school), and thus, there is not much variation in eligibility of subsidy for inpatient care. In fact, when we examine the effect of subsidy for inpatient care on inpatient spending, we detect no meaningful results (results are available upon request). These results are consistent with the RAND HIE, which finds that children respond to the price of outpatient care but not inpatient care (Newhouse and the Insurance Experiment Group 1993). Therefore, we focus on subsidies for outpatient care throughout this study for brevity.

insurance generosity that could affect healthcare demand. We explain each component in detail below.

First, the generosity of the subsidy is largely reflected by the maximum age until which the subsidy is provided. Note here that subsidy eligibility is usually determined by school grade. For example, children are often subsidized until the end of junior high school, i.e., until the end of the fiscal year (which ends in March in Japan) after reaching 15 years old. For brevity, we call this cohort "age 15" throughout the paper. The same applies to other age groups.[8] In Japan, the school grades can be almost completely assigned by age owing to the strict enforcement of the school entry rule, as well as to very rare grade retention and consistent advancement rates (Shigeoka 2015).

Second, the level and form of the subsidy differs by municipality: the majority of municipalities subsidize child healthcare fully (i.e., provide free care). Some municipalities reduce the coinsurance rate to 10%, 15%, or 20%, whereas others take a form of copayment, such as USD 2, 3, or 5 per visit. Importantly, municipalities use only one type of cost-sharing at a time, which applies to all outpatient services. Henceforth, we refer to these patient cost-sharing as prices.

Third, the subsidy is either applied at the point of service (e.g., the patient pays nothing at the medical provider) or is refunded later. Less than 2% of children have to apply for a refund, and we control for this in the regression analyses. Finally, some municipalities impose income restrictions on the eligibility for the subsidy. While we cannot identify the ineligible individuals owing to the lack of an income variable in our claims data, the fraction of municipalities with an income restriction is very small therein (1.5%). In the empirical model, we include a dummy for income restriction at municipality×year-month levels.

2.4. Price changes created by subsidy expansion and expiration

Municipalities have drastically expanded child healthcare subsidies over the last decade. Figure 1 plots the share of municipalities in our insurance claims data by child age for the subsidy eligibility. Our data cover the period from April 2005 to March 2015. The figure indicates that the subsidy expanded rapidly to older ages in the last decade (see Appendix Figure A-1 on the precise timing of all policy changes). For example, in April 2005, while the majority of municipalities (75.6%) were already providing the subsidy until the age of 6 years (start of primary school), none of the municipalities provided a subsidy until the age of 15 years (the end of junior high school). However, this number reaches nearly 80% by the end of our sample period 1 decade later.[9] This figure also shows that the

[8] The ages of 6, 12, and 15 correspond to the end of preschool, primary school, and junior high school, respectively.
[9] The sizable jump in April 2008 is explained by the central government's expansion of the eligibility age for the national-level subsidy (i.e., 20% coinsurance rate) from 3 to 6 years (the start of primary school). This national-level

subsidy status often varies by age group even within the same municipality in the same period. For example, the distance between age ≤9 and age ≤12 in Figure 1 indicates that municipalities often subsidize children up to age of 9 years (the end of 3rd grade) but not up to age 12 years (the end of primary school). We later exploit this within municipality variation across cohorts to examine the robustness of our baseline estimates.

Next, Figure 2 illustrates how the subsidy expansion creates many price changes, which we exploit in our empirical model. The figure shows the cost-sharing schedule of one cohort (born in July 1998) in a particular municipality that expanded the subsidy in October 2007. We use the price change between 0% and 30% as an example. Panel A shows the price schedules *before* the subsidy expansion. Before the subsidy expansion, the municipality provided free care (i.e., 0% coinsurance rate) until the beginning of primary school (6 years). The cohort is 6 years and 9 months old when they enter the primary school because the school year in Japan starts in April. Above this age, the cohort pays the national level of a 30% coinsurance rate.

In October 2007 (when the cohort became 9 years and 3 months old), the municipality expanded the free care to the end of primary school (age 12). Panel B shows the price schedules *after* the subsidy expansion. The price reduced to 0% again after this month until the subsidy expired in April 2011, when the cohort graduated from primary school at the age of 12 years and 9 months. Then, once again, the cohort paid the 30% coinsurance. This simple illustration demonstrates that the subsidy expansion and expiration create many price changes ("from 30% to 0%" and "from 0% to 30%") that we can use to identify the price coefficient.[10] We clarify that because municipal subsidies expire at a certain age (age 12 in this example), we observe many price increases, which we also exploit in our empirical strategy.

Table 1 summarizes all the price changes observed in the data (suppressing the age of price change). There are different levels of coinsurance (10%, 15%, 20%, and 30%), small copayments per visit (USD 2, 3, or 5), and free care. The transitions of cost-sharing "from 30% to 0%" and "from 0% to 30%" are by far the top two most frequently observed price changes; the first two rows of column (1) indicate approximately 6,000 such transitions at the municipality–age–time cell level (both time and age are measured in months), which helps identify the price coefficient. We obtain these cell-level transition

subsidy expansion eased the budgetary burden on municipalities, as part of the cost to provide free care for children under 6 years was covered by the central government, allowing municipalities to expand coverage to older ages.

[10] We could also investigate asymmetric demand responses when prices increase and decrease. However, because price changes in both directions can be seen largely in one price change (i.e., from 0% to 30% and from 30% to 0%), we discuss this issue in a separate paper (Iizuka and Shigeoka 2021). We find that the demand response for child healthcare is twice as large when price increases (from 0% to 30%) than when price decreases (from 30% to 0%).

numbers by separately counting affected age groups given a municipality at a time.[11] These two price changes account for 53.3% of all the transitions at the municipality–age–time cell level, and as much as 69.6% if weighted by the number of children in each cell (column 2).

However, there are also many price transitions to (and from) small copayments, such as USD 2 and USD 5 per visit. To obtain a clearer idea of the magnitude of patient cost-sharing for the small copayments, we compute the average (instead of individual-specific) share of out-of-pocket payments for these copayments.[12] The approximate coinsurance rate implied by the small copayments are 2.4% (USD 2/visit), 3.9% (USD 3/visit), and 6.1% (USD 5/visit), which are substantially smaller than the original coinsurance rate of 30%.

3. Data

3.1. Data description

Our outcome data come from JMDC Inc., which collects and analyzes administrative insurance claims data on behalf of large insurers. These data are used in previous studies, including Iizuka (2012), Fukushima *et al.* (2016), and Iizuka *et al.* (2021). Whereas our data are not nationally representative, the average healthcare spending in our sample is similar to the national average.[13] As of November 2015, the JMDC claims database contains more than 3 million members.

The JMDC data comprise administrative enrollment data and claims data. For each person, the enrollment data report the patient ID, gender, age, and municipality of residence at monthly frequency. The age and municipality of residence in each month are crucial in this study, as the level of cost-sharing is uniquely determined by municipality, age, and time. Specifically, the claims data contain the year and month of the visit, and line-by-line medical services received, including diagnoses (ICD10), types of services, quantity of each service, and fees charged for each service based on the national fee schedule.[14] The claims data are of monthly frequency in Japan as the reimbursement to medical institutions takes place on a monthly basis. The enrollment and claims data are linked by a unique patient ID.

Our data cover a period of 10 years between April 2005 and March 2015 (120 months). We focus

[11] For example, if a price change "from 30% to 0%" occurs in municipality m at time t (year-month) and affects two age groups (a), say ages 6 years and 2 months and 6 years and 3 months, this will increase the N in column (1) by 2.
[12] Specifically, we divide the average out-of-pocket payment (average number of visits per month times the copayment) by the total average monthly outpatient spending.
[13] For example, the average healthcare spending for children aged 10–14 years in 2014 was USD 71.4 per month (based on our calculation from the Ministry of Health, Labour and Welfare 2015), whereas that of our sample is USD 73.2.
[14] The data do not, however, contain dental claims, and inpatient food and housing costs. The latter is small because the children's length of stay in the hospital is short, unlike the case of the elderly.

on children aged 7–14 years (96 months) because, as shown in Appendix Figure A-2, we do not have many observations without subsidy for those under 7 years and with subsidy for those over 15 years. Therefore, we limit our sample to 6–15 year-olds (1 year wider on either ends of the age range of interest) to identify the effect of patient cost-sharing at ages 7–14 years. We also limit our sample to those who stay in the same municipality (98.3% of the sample). We address the possibility of children moving to a municipality that offers a generous child healthcare subsidy in Section 5.5.

Our dataset is constructed as follows. We provided the subsidy information to JMDC Inc., who merged it in-house with their insurance claims data by municipality and year-month, and returned it to us with the municipality ID and patient ID anonymized for confidentiality reasons. Thus, we cannot examine heterogeneity from the characteristics of the municipality (e.g., the average household income or maternal education), as the municipality ID is scrambled. Another drawback—albeit usual for insurance claims data—is that the data do not include individual characteristics (except gender and age of children), such as parental education, household income, and family structure (e.g., number of children).

3.2. Descriptive statistics

Table 2 provides the summary statistics of selected variables at the municipality, individual, and individual-month levels in Panels A, B, and C, respectively. Panel A shows that each municipality is observed on average for 76.5 months[15], and 77.9% of the municipalities have at least one policy change (N = 294). As discussed in Section 2.4, the source of variation for identification does not come simply from the *expansion* of the subsidy (i.e., price decrease) but also from the *expiration* of the subsidy (i.e., price increase) at a certain age. At the individual level (Panel B), we have a total of 90,184 individuals, and each is observed for an average of 34.8 months. At least one subsidy change is experienced by 24.4% of individuals. Gender is well balanced (48.8% are female).

Panel C of Table 2 reports certain key variables at the individual-month level, for a total of 2,992,982 individual-months. Almost all the subsidy is applied at the point of service (98.3%), and few municipalities impose income restrictions (6.0%). Notably, 39.7% of children make at least one outpatient visit per month on average.[16] Outpatient spending per child is USD 48.8 per month (USD 122.9 conditional on at least one visit). The out-of-pocket payment per visit *without* subsidy is USD 19.2, which gauges the magnitude of the financial burden on individuals if the subsidy is not available.

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[15] During our sample period, 37 out of 294 municipalities (12.6%) have merged, which makes the data unbalanced panel data. However, many of those municipalities are small, and the share of children between ages 5–14 years in these municipalities only account for 7.41% of the total according to population census.

[16] Among the OECD countries, Japan has the second highest number of doctor consultations (12.8 per year in 2015), including the elderly, resulting in one visit per month on average (OECD 2015).

Inpatient admission for this age range is rare (only 0.26%), but inpatient care is much more expensive upon admission (USD 4,313) than outpatient care.

Simple plots of the raw data reveal interesting patterns. Panel A of Figure 3 plots the raw means of outpatient utilization at each age for children who live in municipalities with a zero price (labeled "0%"), USD 2/visit (labeled "USD 2"), and 30% coinsurance (labeled "30%"). The graph on the left for an outpatient dummy shows that the line with a zero price ("0%") is always higher than the line with 30% coinsurance ("30%") by 8–11 percentage points at any age range, whereas both age profiles are declining because the average health may improve at older ages. Moreover, the line with USD 2 per visit is always below the 0% line, indicating that a copayment as small as USD 2 per visit significantly reduces healthcare demand relative to a zero price.[17] The graph on the right shows a similar pattern for outpatient spending: the mean outpatient spending is USD 20–30 (40–60%) higher with a zero price ("0%") than 30% coinsurance ("30%"), which is substantial. Again, outpatient spending with USD 2 per visit lies between the two lines. Whereas this figure does not account for compositional changes in the sample, the main message from the regression analysis below is similar.

4. Empirical model

4.1. Estimation and identification

We attempt to identify the effects of the cost-sharing for child healthcare by exploiting the unique variation in subsidies across municipality, age, and time, using a DID framework. In our preliminary analysis, we find that demand responses are quite similar across the ages of 7–14 years (Iizuka and Shigeoka 2018).[18] Thus, we estimate a single price coefficient for all ages in this study, which increases statistical power for some price changes with relatively few observations. Specifically, our basic estimation equation is:

$$Y_{it} = \alpha + \textstyle\sum_C \beta_C \mathbf{1}(Price = C)_{amt} + \gamma X'_{mt} + \delta_a + \pi_t + \varphi_m + \theta_i + \varepsilon_{it}, \quad [1]$$

where Y_{it} is the healthcare utilization by child i at time t (year-month). The price dummy variable $\mathbf{1}(Price = C)_{amt}$ takes the value 1 if cost sharing of outpatient care is C for children at age a (measured in months) at time t and living in municipality m. C takes all levels of coinsurance rates (C=

[17] Again, the approximate coinsurance rate implied by the copayment of USD 2 per visit is 2.4%.

[18] As illustrated in Figure 2, we can estimate the price elasticity for broad age ranges (7–14 years) even at the *monthly* level (e.g., 12 years and 9 months). See our early working paper, Iizuka and Shigeoka (2018) for the monthly-level analysis.

10, 15, 20, 30%) as well as copayments (C= USD 2, 3, 5/visit).[19] Note here that we choose free care (C= 0) as the control group to examine the effect of introducing a small copayment to free care. As children become eligible or ineligible for the subsidy at the beginning of the specified month, we can assign the price dummies using the age in months without measurement errors.

δ_a, φ_m, and π_t are fixed effects for age, municipality, and time, respectively. Note that we include δ_a at the monthly level to account for any age in month-specific effects (e.g., graduation from primary school). In addition, θ_i is the individual fixed effect, which captures the unobserved time-invariant characteristics of patients and addresses the compositional changes in the unbalanced panel data (note that φ_m is subsumed by θ_i for children who do not move, and thus, we omit it hereafter).[20] We also control for two time-varying municipality variables, X_{mt}, a dummy that equals 1 if the subsidy is applied at the point of service rather than refunded later and 0 otherwise, and a second dummy variable that equals 1 if there is an income restriction on subsidy eligibility and 0 otherwise.

We estimate this equation using ordinary least squares (OLS). Standard errors are clustered at the municipality level to account for serial correlation in the error terms within the municipalities. The estimates from alternative models, such as the one- or two-part generalized linear model, are almost identical to the OLS estimates (see Appendix Figure C-3). To ease the computational burden for estimating the bootstrapped standard errors for our elasticity measures, we report the OLS estimates throughout the study.

The identifying assumption in our DID model is that there are no unobserved municipality-specific changes that: (1) are correlated with changes in subsidy in the municipality; and (2) are correlated with municipality-specific changes in healthcare utilization. For example, if municipalities in a better financial situation are more likely to implement the subsidy expansion whereas income effects simply increase utilization, our estimates may be biased. We address these concerns in Section 5.5.

4.2. Identifying the zero-price effect

The previous section describes how to identify demand parameters. Now, we discuss how we identify the zero-price effect. We define the zero-price effect as a discontinuity in the demand function at

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[19] We note that coinsurance and copayment may affect utilization differently. For example, from the patient perspective, the marginal out-of-pocket cost for copayments is zero after paying the copayment, whereas the marginal out-of-pocket cost for coinsurance is always at the coinsurance rate. In addition, the exact amount of cost-sharing is unknown ex-ante for coinsurance, thereby introducing some uncertainty in the actual payment (Dor and Encinosa 2010). Unfortunately, however, we do not have price variations in copayment and coinsurance that lead to the same level of cost-sharing so that we can identify how the two forms of cost sharing differentially affect demand.

[20] For non-movers, because *time* = (*birth* + *age*), controlling for age and time fixed effects essentially determines the cohort (i.e., birth year-month), which experiences the same patient cost-sharing schedule.

the price of zero. Suppose that demand for child healthcare, $D(P, X)$, is determined by the price, P, and covariates X. In our context, we observe the demand when the price is 0, that is, $D(0, X)$. Then, the zero-price effect, $z(x)$, is defined as follows:

$$z(x) = E[D|P = 0, X = x] - \lim_{p \downarrow 0} E[D|P = p, X = x]$$

If $z(x)$ is zero, we conclude that the zero-price effect does not exist. Ideally, if we have continuous variation in price near zero, we can non-parametrically identify the zero-price effect, using, for example, a local linear regression (see, Caetano (2020), for such an approach.) In practice, however, when the observed prices are not continuous enough, some parametric assumptions are necessary to pin down the zero-price effect. This indeed is the case in our case: whereas we observe multiple small prices near zero such as USD 2, 3, and 5 per visit, we do not observe infinitesimally small price points, such as one penny or two pennies away from zero. Therefore, we parametrically identify the zero-price effect by estimating the following equation:[21]

$$Y_{it} = \alpha + \beta C_{amt} + \gamma \mathbf{1}(C = 0)_{amt} + \delta_a + \pi_t + \theta_i + \varepsilon_{it}, \quad [2]$$

where C is the level of cost-sharing, and $\mathbf{1}(C = 0)$ is a dummy variable which takes a value of 1 when cost-sharing is zero.[22] γ of equation [2] tests for a zero-price effect: if $\gamma > 0$ (and $\beta < 0$), we reject the null hypothesis of no zero-price effect. Graphically, we fit a linear line for the effects of different levels of cost sharing on demand and test whether the intercept at the price of zero is different from zero (we will illustrate this later, in Figure 5). We also estimate equation [2] by allowing a more flexible relationship between utilization and cost-sharing by introducing quadratic and cubic functions in C, and by replacing C in equation [2] by a natural logarithmic function in C.[23]

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[21] Douven *et al.* (2020) employs a similar approach.

[22] For small copayments, we use the approximate coinsurance rate implied by the copayment, which we reported in Section 3.2. One may be concerned that the cost share is endogenous in the sense that it is a function of quantity demanded. To see this, cost share can be given by $\frac{out-of-pocket\ payment}{total\ outspending} = \frac{\#\ of\ visits * copayment}{\#\ of\ visits * spending\ per\ visit} = \frac{copayment}{spending\ per\ visit}$. As the zero-price effect, γ, is likely to be smaller when the calculated cost sharing is greater, we check the robustness of the results by using the smallest number of "spendings per visit" among different cost-sharing levels (USD 70.5/visit at C=0). In fact, we find that "spendings per visit" does not substantially change across the levels of copayments and thus the zero-price effect was hardly affected (results not shown).

[23] One remaining issue is the choice of bandwidth when estimating equation [2]. A wider bandwidth will lead to more precise estimates with more observations, but it may bias the estimates by including observations distant from the price of zero. We address this "bias-variance" tradeoff by implementing a cross-validation approach similar to Lee and Lemieux (2010). We find that as we increase the bandwidth, the cross-validation criterion monotonically decreases, which indicates that a wider bandwidth has merit. However, we can conduct this exercise only up to the bandwidth of 15%; for this test, we need at least two price points to the right of the index price but no such data points exist for 20% or 30%. As an alternative, we test whether the estimated coefficients from the full sample (bandwidth=30%) differ from those that use only up to 15% by stacking the two datasets, finding the estimates are not statistically different (results

5. Results

5.1. Event study

We begin by providing graphical evidence on changes in outpatient outcomes in the form of an event study. Here, we focus on the price changes between 0% and 30%—those most frequently observed in the data—to gain statistical power. The results for the price change with a small copayment are similar, although it is somewhat noisier because of fewer observations (Appendix Figures B-1 and B-2). We replace the price dummy in estimation equation [1] through the interaction of the variables corresponding to being in the treatment group (i.e., experiencing the change in price changes from 30% to 0% or from 0% to 30%) and a series of dummies for each month, ranging from 12 months prior to the price change to 12 months after the change (T= –12 to +11, where T=0 is the time of the price change).[24]

Figure 4 presents the results of the event study for an outpatient dummy (Panel A), and outpatient spending (Panel B), separately for price decreases (left panel) and price increases (right panel). The reference month is 3 months before the change in subsidy status (T= –3). There are three noteworthy points. First, the figures do not show any pre-trends (except for anticipatory effects), as the pre-treatment estimates are mostly close to zero. This is reassuring, as it supports the parallel trend assumption crucial for the DID model. Second, there is substantial anticipatory utilization, as indicated by drops in utilization before price decreases (left panel) and surges in utilization before price increase (right panel). This pattern reveals that some children (and hence, parents) are aware of the upcoming price changes and behave strategically by delaying or rushing visits. As these anticipatory effects—which may overstate our estimates—seem to be concentrated within 2 months from T=0, we exclude these 4 months of data throughout the study. Chandra *et al.* (2010) take a similar approach. The estimates, and hence, the elasticities are barely affected after removing more than 2 months from T=0 (results are available upon request). Finally, the effect on utilization seems to be permanent rather than transitory because the level of utilization after T=0 does not revert to the level before T=0. This result justifies the use of the DID strategy, as we do not need to rely on observations only around T=0 to estimate the effect of cost-sharing

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available upon request). This result indicates that widening the bandwidth from 15% to 30% does not significantly increases "bias." On the other hand, widening the bandwidth substantially helps reduce "variance," because doing so increases observations approximately by 50%, which reflects that many individuals face the 30% coinsurance. For these reasons, we use the 30% bandwidth in the analysis (i.e., full data).

[24] For price increases and price decreases, we separately normalize the spending data around the change in subsidy status. For price increases, we construct data by using individuals who experience price increases and those who experience no price change. Here, we exclude observations if there is another price change within 2 months of the observation. For example, if there is another price change at T=10 for an individual, we include data only up to T=7 for that individual. We did the same for price decreases.

on utilization. Note that the event-study does not speak directly to identification of zero-price effects, which we discuss in Section 5.3.

5.2. Demand parameter and price elasticity

The upper graph in Figure 5 plots β_C from equation [1], where the outcome is an outpatient dummy, for three levels of copayment together with the four levels of coinsurance rate. First, we find that all point estimates are negative and statistically significant at the 5% level, indicating that cost-sharing, even if it is very small, significantly reduces utilization. Second, regardless of the level of cost-sharing, the point estimates are similar especially among copayments and are in the range of 2–4 percentage points (or 6–7% from the mean of 43.9% at *C*=0). These results indicate that whereas having positive cost-sharing or none at all has a clear effect on utilization, the level of cost-sharing has a relatively smaller effect.

We convert these estimates into semi-arc elasticities as reported in the lower half of Figure 5.[25] Interestingly, the semi-arc elasticity of the smallest copayment (ε_2= –2.65) is substantially greater in magnitude than those for slightly larger copayments (ε_3= –1.49, ε_5= –0.83). This result is easy to interpret because, as the upper graph shows, whereas the changes in quantity are similar across different cost sharing, the changes in cost sharing (*C*) that drive the quantity changes are much smaller for USD 2 per visit (2.4%) than those for USD 3 per visit (3.9%) and USD 5 per visit (6.1%).[26] This leads to a larger elasticity for USD 2 per visit.

To examine whether the demand is more price sensitive near the zero price, we compare the elasticity of the price nearest to zero (ε_2), and the elasticities of slightly larger copayments (ε_3, and ε_5). We find that the elasticity difference between ε_2 and ε_3 (= 1.16) is statistically significant at the 5 percent level (SE= 0.538), and similarly, the elasticity difference between ε_2 and ε_5 (= 1.82) is statistically significant at the 1 percent level (SE= 0.433).[27] These results indicate that demand for healthcare is particularly price-sensitive around the price of zero.

We obtain even more pronounced differences in elasticities between ε_2 and those of ε_3, and ε_5 when we use outpatient spending and frequency of outpatient visits as the dependent variable (see

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[25] Whereas most of the literature uses arc elasticity, defined as $\varepsilon_C = \left(\frac{Q_C - Q_0}{(Q_0+Q_C)/2}\right) / \left(\frac{P_C - P_0}{(P_C+P_0)/2}\right)$, it is not suited to capture price responsiveness when the starting price is zero, as in our case (e.g., Brot-Goldberg *et al*. 2017). This is because they reflect only the changes in quantity but not the changes in price as the denominator cancels each other $\left(\varepsilon_C = \left(\frac{\beta_C}{Q_0+Q_C}\right)\right)$. For this reason, we report the *semi*-arc elasticity defined by $\varepsilon_C = \left(\frac{Q_C - Q_0}{(Q_0+Q_C)/2}\right) / (P_C - P_0) = \left(\frac{2\beta_C}{Q_0+Q_C}\right) / P_C$.

[26] As β_C is similar across different copayment levels, the Q_C (which can be expressed by Q_C=Q_0+ β_C) are by construction similar to each other, as Q_0 is common to all of them. Thus, the remaining P_C is the key determinant of the semi-arc elasticity, that is, $\varepsilon_C = \left(\frac{2\beta_C}{Q_0+Q_C}\right) / P_C = \left(\frac{2\beta_C}{2Q_0+\beta_C}\right) / P_C$.

[27] The standard errors are bootstrapped with 200 repetitions.

Appendix Figure A-4) although they are slightly less precise than the extensive margin documented above.

5.3. The zero-price effect

Next, we discuss the results of the zero-price effect. Table 3 reports results from the estimating equation [2]. We find that the zero-price effect, γ, is positive and statistically significant in all regressions when the dependent variable is an outpatient dummy (Panel A) regardless of the functional form. The estimated γ in column (1) indicates that if the price is zero instead of an infinitesimally small price, the probability of visiting a physician at least once a month increases by at least 2.1 percentage points (or 4.8% from the mean at C=0). This is not a small effect because the zero-price effect accounts for roughly as much as half of the corresponding effect of a 10% coinsurance (4.0 percentage points or 9.1% from the mean at C=0) as shown in the upper half of Figure 5. The results are similar for outpatient spending but somewhat weaker (Panel B). We also estimate a model that replaces C in equation [2] by a natural logarithmic function in C and finds that the zero-price effect is almost identical to that of column (1).[28]

These results indicate that zero is not just another price and there is a discontinuity between zero and non-zero prices. On the one hand, this result implies that, relative to a zero price, a low positive price would substantially reduce healthcare utilization, and therefore, may be an effective tool to reduce the moral hazard problem. The primary benefit of health insurance is to reduce the risk of catastrophic medical expenses and allow consumption smoothing. As a small copayment does not increase such financial risks by much, it has an advantageous feature from the perspective of optimal health insurance. We note, however, that the benefit would be reduced if paying a small fee is psychologically costly to the consumer. On the other hand, our results also imply that one may be able to exploit the zero-price effect and boost demand for certain products and services by intentionally setting the price to zero. For example, setting the price to zero and increase the use of high-value care may be beneficial, where moral hazard is not an issue. We examine such a possibility later in Section 6.1.

Our results have two additional implications for optimal health insurance design. First, there is also a welfare implication to the finding that the level of cost sharing (such as USD 2 per visit, USD 5 per visit, and 10% coinsurance) has a relatively small, secondary effect on demand. It suggests that a very small copayment, such as USD 2 per visit, may be more advantageous in improving welfare than

[28] For a more concrete explanation, we replace C in equation [2] by ln $(C + \tau)$ and search for τ that best fits the data. We find that a larger value of τ, such as τ =1,000, best fits the data, and the corresponding parameter value for γ is almost identical to that of column (1). The β is estimated to be negative. This result indicates that a flatter linear-like function that crosses the y-axis at a point far from the origin fits the data better than a smooth non-linear function that changes rapidly near zero (results are available upon request).

somewhat larger cost sharing (such as the 10% coinsurance). This is because whereas they nearly equally reduce moral hazard, the former substantially limits the financials risk to consumers than the latter.

Second, our results highlight that price elasticities vary substantially along the demand curve and cannot be represented by a single number. Although Aron-Dine *et al.* (2013) emphasize this point in their study, it is still common in the literature to report a single elasticity estimate without referring to its context (such as the level of price change observed in the data).[29] This may be because researchers rarely observe multiple price changes in a single market but doing this can be misleading and can result in erroneous policies.

As there were approximately 8.8 million children aged 7–14 years in Japan in 2015 (Statistics Bureau 2015), a back-of-the-envelope calculation suggests that if child healthcare is free in all Japanese municipalities instead of charging as small as USD 2/visit copayment, then annual outpatient spending increases by 564 million USD (95% CI : [287, 841] million USD).[30] This would create a substantial negative fiscal externality for many stakeholders: whereas the municipality is responsible for covering only 30% of total cost (i.e., the subsidy amount), the remaining 70% of the subsidy-induced excess spending must be financed by taxes and premiums, similar to the negative fiscal externality that supplemental private health insurance imposes on public Medicare spending in the United States (Chandra *et al*. 2010; Cabral and Mahoney 2019).

5.4. Screening effect of a small copayment

An important finding from the previous section is that a copayment, of as small as USD 2 per visit, significantly reduces demand for healthcare. This section further examines the type of patient (healthy vs. sick) and margin (extensive vs. intensive) most affected by a small copayment. That is, we study the screening properties of a small copayment. We focus on the USD 2/visit copayment, which leads to the largest price elasticity. Moreover, it is the copayment most frequently observed in our data.

As discussed in the Introduction, our analysis relates to recent work in public economics that studies whether "ordeals" screen out certain types of consumers who apply for public programs. Studying a screening effect is important in our case as well, because if a small copayment only affects certain types

[29] In other words, we can compare elasticities only if they are derived from similar price changes. In this regard, the closest setting to ours (0% vs. 30% coinsurance rate) is the RAND HIE, which compares 0% versus 25% coinsurance rates. ε_{30} (= –0.562) in Figure 5 is considerably smaller than the –2.11 that Brot-Goldberg *et al.* (2017) calculate from the RAND HIE for the non-elderly. While considerable caution is still required when comparing the elasticities estimated across countries and time periods, our results may indicate that the elasticity for children is smaller than that of the non-elderly adults.

[30] We multiply β_2 (= 5.34) from Appendix Table A-3 by the number of children aged 7–14 years in 2015 to calculate monthly spending. Then, we multiply the outcome by 12 to convert to annual spending.

of consumers (such as the healthy), it may potentially serve as a targeting device. However, it would be a concern if a small copayment deters sicker patients from visiting physicians.[31]

To this end, we first divide children into three health statuses (sick, middle, and healthy) based on outpatient spending in the first 6 months of observations for each individual.[32,33] Previous studies have also used prior spending as an indicator of health status (e.g., Dranove *et al.* 2003). We compare the sick and healthy types, omitting the middle type. We then examine how a small positive price affects the extensive and intensive margin of outpatient visits by creating dummy variables that correspond to at least one, two, and three outpatient visits per month. These regressions are often called distribution regressions (Foresi and Peracchi 1995; Chernozhukov *et al.* 2013). Specifically, we estimate the following linear probability model for each health type:

$$\Pr(Y_{it} \geq k) = \alpha + \sum_C \beta_C \mathbf{1}(Price = C)_{amt} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{it}, \qquad [3]$$

where Y_{it} is the frequency of outpatient visits for $k = 1, 2, 3$. Note that k=1 corresponds to the extensive margin of utilization.

Table 4 reports β_C (C= USD 2 per visit) from equation [3]. It shows that the small copayment generally reduces outpatient visits at both margins regardless of health status. There is an exception, however, in that the coefficient of the sicker children's extensive margin is not significantly different from zero and is economically small. Thus, there is no evidence that sicker children stop visiting physicians when a small copayment is charged. The table also shows that when calculated relative to the mean (as reported under "% change from mean"), the copayment has a relatively larger effect on the intensive margin (i.e., $\Pr(Y \geq 2)$ and $\Pr(Y \geq 3)$) than on the extensive margin (i.e., $\Pr(Y \geq 1)$). In fact, the biggest effect that we find is on healthier children who visit physicians more than three times per month, which represents a 29.7% reduction from the mean. Thus, a small copayment primarily affects the utilization of healthier children. This result may be interpreted as—similar to the finding of Finkelstein

[31] A few studies examine the heterogeneity in price responsiveness by patient health status but the evidence is mixed (e.g., Manning *et al.* 1987; Chandra *et al.* 2014; Fukushima *et al.* 2016; Brot-Goldberg *et al.* 2017).

[32] Specifically, we first calculate the total spending for the first 6 months of observations for each individual whose subsidy status does not change during this period, and then divide each individual into three groups (lowest spending corresponds to healthy, and highest spending corresponds to sick) within each cell: (age in years)×(with or without subsidy). We experiment with different windows (9 and 12 months) to calculate the patient health status and find qualitatively similar results across the windows (results are available upon request).

[33] One concern about using past spending as a proxy for severity is that it is arguably subject to mean reversion: high spenders in one period are likely to spend less in the next period, and vice versa, which may bias our results. However, we find little evidence of mean reversion. Those who belong to the bottom third of the spending distribution in the first 6 months (i.e., healthy) have a 59.3% chance of staying in the bottom third in the next 12 months and only a 9.57% chance of moving up to the top third. Similarly, those who belong to the top third in the first 6 months (i.e., sick) have a 64.2% chance of staying in the top third in the next 12 months and only a 9.48% of chance of moving down to the bottom third.

and Notowidigdo (2019)—a small copayment working as a screening device rather than a hurdle, and allocating more resources to sicker patients.[34,35]

5.5. Addressing endogeneity concerns

Whereas municipal policies provide us with unique price variations to identify the zero-price effect, a potential concern for exploiting local policies is that they could be endogenous.[36] This section summarizes our attempts to address endogeneity concerns related to equation [1], introduced in Section 4.1. Appendix Section C provides more detailed discussions and estimation results. First, as reported in Section 5.1, we conduct an event study analysis to show that the parallel trend assumption crucial for the DID model holds; our treatment municipalities do not follow different pre trends than the controls. Second, we extend equation [1] by including the time-by-municipality fixed effects that account for any municipality-specific policy changes or events in a particular month (e.g., income transfers, other subsidies, or business cycles.) We can identify such a model because the subsidy status often varies by age group even within the same municipality in the same period as discussed in Section 2.4. Reassuringly, the estimates barely change. Third, we show that the results do not change even when we drop individuals who do not experience any price changes (i.e., never-treated children), exploiting only the *timing* of the changes in subsidy status.[37] Fourth, we estimate a conditional logit model to study whether cost sharing affects where children live, finding little evidence that supports such a migration pattern.

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[34] Targeting sicker patients to provide resources may be more justifiable if cost sharing does not worsen health outcomes or increase healthcare spending in the future. We address these questions in Iizuka and Shigeoka (2018), finding no support that higher cost sharing in childhood (even as high as 30% coinsurance rate) leads to worse health outcomes or increase healthcare spending in the adulthood.

[35] The results of this section also speak to the concern that the zero-price effect may be driven by supply-side behavior. For example, if physicians feel much more comfortable about inducing demand when patients pay nothing, the physician behavior may cause the zero-price effect. In this case, we would expect the demand effect to be bigger for sicker children because they see the physician more often. However, contrary to this argument, we find bigger demand effects for healthier children. Thus, supplier-induced demand may not be the main cause of the zero-price effect. See, Iizuka (2007, 2012), for studies that explicitly consider physician agency in healthcare demand.

[36] For example, whereas the main reason for the subsidy provision is to ensure access to essential medical care for children and to lessen the financial burden on parents, a few other explanations for the subsidy provision mentioned in the literature are as follows: to attract young couples with children as a means to increase local tax revenue, to boost low fertility rates, and to combat recent increases in child poverty (Bessho 2012), possibly amplified by spatial competition across municipalities (Nikkei 2017). However, we are not aware of any other policies that 1) *precisely* coincide with the subsidy expansion at the *monthly* level, *and* 2) directly affect children's healthcare utilization (e.g., expansion of daycare and preschool subsidies).

[37] We also perform a falsification test by using the data *outside* the age ranges of 7–15 (our main focus), where we continue to assign them to the cost-sharing arrangements of the closest age in 7–15 *as if* they were 7 and 15. The results also support the parallel trend assumption in the DID model. See Appendix Section C for details.

6. Small copayment and behavioral hazard

In a recent study, Baicker *et al*. (2015) emphasizes that optimal cost sharing should reflect not only the degree of moral hazard but also the extent of "behavioral hazard," that is, the consumer's misperception of the benefit and cost of a treatment. This section investigates whether a small copayment affects behavioral hazard by studying their effect on high- and low-value care, respectively.

6.1. High-value care: preventive care

Certain types of health screening, counseling, and treatments prove to be highly cost-effective and using such services may improve welfare. For example, for children and youth, childhood immunization, treatment of major depression, and ADHD are considered some of the most cost-effective preventive services (Maciosek et al. 2017; USPSTF 2020; Subcommittee on Attention-Deficit/Hyperactivity Disorder 2011). This section examines whether a small copayment affects the use of these high-value care.

It should be noted that in the Japanese context, local governments provide a series of childhood immunizations free of charge (such as DTaP and MR (measles-rubella) vaccines), and thus immunizations are not subject to the price change that we examine in this study. Schools are also mandated to conduct annual health checkups for their children, which cover items similar to a well-care visit in the United States, including hearing tests, vision screening, and blood tests. Again, these high-value care do not show up in our claims data.

Because of this institutional background, high-value care that we can analyze is relatively limited. Nonetheless, there is still other high-value care, such as screening of major depression, ADHD, and counseling to prevent tobacco use, and obesity, which the above references consider important. Appendix D lists these high-value care that we consider along with its frequency as well as corresponding ICD10 codes. Because such visits are rare, we combine all of them and create a dummy variable that equals 1 if any of the preventive care listed in Appendix D is provided and 0 otherwise. We then estimate equation [1] by using the dummy variable as the dependent variable. We note that the results should be viewed with caution, as only a small fraction of the dependent variable equals 1 (0.69%).

With this caveat in mind, we report the result in column (1) of Table 5. Column (1) shows that preventive care significantly decreases when children pay a small copayment of USD 2/visit relative to free care (*p-value*<0.05). Relative to the mean, the small copayment reduces preventive care by approximately 10%, which is comparable or even larger than our main result on the overall visit dummy of 7.6% (= 0.031/0.439) reported in Figure 5 and Appendix Table A-2. This result supports the behavioral hazard argument that even a small copayment of USD 2/visit significantly decreases care that is considered as high value.

6.2. Low-value care: inappropriate use of antibiotics

We use the inappropriate use of antibiotics as an example of low-value care. It has been argued that antibiotics are often used on diagnoses for which they are not necessarily recommended, and such low-value care may cause adverse events on the individual ranging from allergic reactions to diarrhea (Fleming-Dutra *et al.* 2016).[38] We follow Fleming-Dutra *et al.* (2016) and divide the diagnoses into three tiers by the degree of appropriateness of antibiotic use. Specifically, Tiers 1, 2, and 3 are diagnostic categories in which antibiotic use is always indicated, is occasionally indicated, and not indicated, respectively. For example, antibiotic use for children with bronchitis and asthma is considered inappropriate and is included in Tier 3. See Appendix E for details, including the list of diagnoses in each tier along with the corresponding ICD10 and summary statistics of antibiotic usage.[39]

For each tier, we estimate equation [1], where the outcome is monthly outpatient spending on antibiotics for patients in each tier, measured in USD.[40] Column (4) of Table 5 shows that a small copayment decreases inappropriate use of antibiotics (i.e., Tier 3) as much as 18.3% (*p-value*<0.05). This result indicates that a very small cost sharing can be a valuable tool to reduce behavioral hazard associated with a zero price. This result is consistent with the proposition of the value-based insurance design (Fendrick *et al*. 2001) that argues that cost sharing should reflect the value of care.

Table 5 also indicates that the effect of a small copayment is confined to Tier 3, and Tier 1 and Tier 2 visits are hardly affected in columns (2) and (3) of Table 5. This result is consistent with the findings reported in Section 5.4, where a small copayment tends to deter physician visits particularly by healthier children; a small copayment may reduce the visits of healthier children who often have relatively light health problems such as common cold and bronchitis, for which antibiotic use is inappropriate.

While our focus in this section is on how a small copayment affects behavioral hazard, it should also be noted that misuse of antibiotics affects society by increasing antibiotic resistance. Given this clear negative externality, a zero price should be further avoided in this context.

In sum, consistent with the behavioral hazard argument, we find that both high- and low-value care is

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38 In the US, antibiotic-resistant infections annually affect at least 2 million people, and 23,000 people die as a direct result of these infections (Centers for Disease Control and Prevention 2013).

39 When a patient has multiple diagnoses in a month, Tier 1 diagnoses are prioritized, followed by Tier 2, and finally Tier 3 diagnoses so that a patient in each month is assigned to mutually exclusive tiers. Specifically, we assign a patient to Tier 1 when he or she has any diagnosis in Tier 1 in the month and to Tier 2 when the patient has any diagnosis in Tier 2 but not Tier 1, and the rest to Tier 3. In this way, Tier 3 includes only patients for whom antibiotics should not be recommended at all, because none of the diagnoses include those from Tiers 1 and 2.

40 The results for antibiotic prescription frequency are similar (results not shown).

reduced by imposition of small copayment (USD 2/visit), which indicates that patient cost-sharing is a blunt tool. Zero and non-zero prices should be strategically chosen to achieve specific goals.

7. Conclusions

This study aimed to examine whether the demand for child healthcare exhibits the zero-price effect. Although a zero price is common in healthcare services as well as in many public services, few studies have examined the zero-price effect because of the difficulty in observing multiple exogenous price changes near zero. Exploiting price variations created by municipal subsidies, we find that zero is a special price and it discontinuously boosts healthcare demand. Our estimates suggest that an infinitesimal price increase from a zero price reduces the probability of visiting a physician at least once per month by 4.8%. Interestingly, a small copayment that reduces healthcare demand has the largest effect on healthy children who frequently visit physicians, whereas it has little effect on sicker children's extensive margin. This result implies that a small copayment serves as a screening device and helps allocate resources to the sick.

An important takeaway from this study is that zero is not just another price, and there is a discontinuity in how zero and non-zero prices affect demand. Thus, these two prices should be carefully chosen to achieve specific goals. For example, a zero price should be avoided if there is a severe moral hazard problem. Charging a very low positive price, instead, would substantially alleviate this problem without imposing many financial risks on the consumer, although the benefit may be reduced if paying a small fee is psychologically costly to the consumer. By contrast, a zero price may be exploited if one wishes to boost demand for certain care, such as highly cost-effective preventive care, including childhood immunization.

Although we document a discontinuity in demand at a zero price, this paper does not fully explain why zero prices are special. A leading explanation based on behavioral economics is that people strongly associate greater benefits with free products as if a zero price increases valuation, which discontinuously increases demand. Alternatively, if paying a fee, even of a very small amount, is a burden to the consumer, it can also create a zero-price effect. The hassle cost of signing a contract, the cost of writing a check, and the liquidity constraints are just some of the examples discussed in the literature (Kremer and Glennerster 2011). Perhaps the most relevant of these discussions in our context may be the mental cost of decision-making. When paying a fee, the consumer considers whether the goods or service is really necessary, but this process of thinking is unnecessary in the case of a free product or service.[41] Such cost-benefit calculations can be particularly difficult for credence goods (Darby and Karni 1973), including healthcare services, where it is difficult to determine the quality of service even after a purchase. Additional research is clearly needed to distinguish competing explanations for the zero-price effect.

[41] Shampanier *et al.* (2007) show that the zero-price effect disappears when individuals are forced to carefully consider options before making the decision, which supports the conjecture that the mental decision cost causes the zero-price effect.

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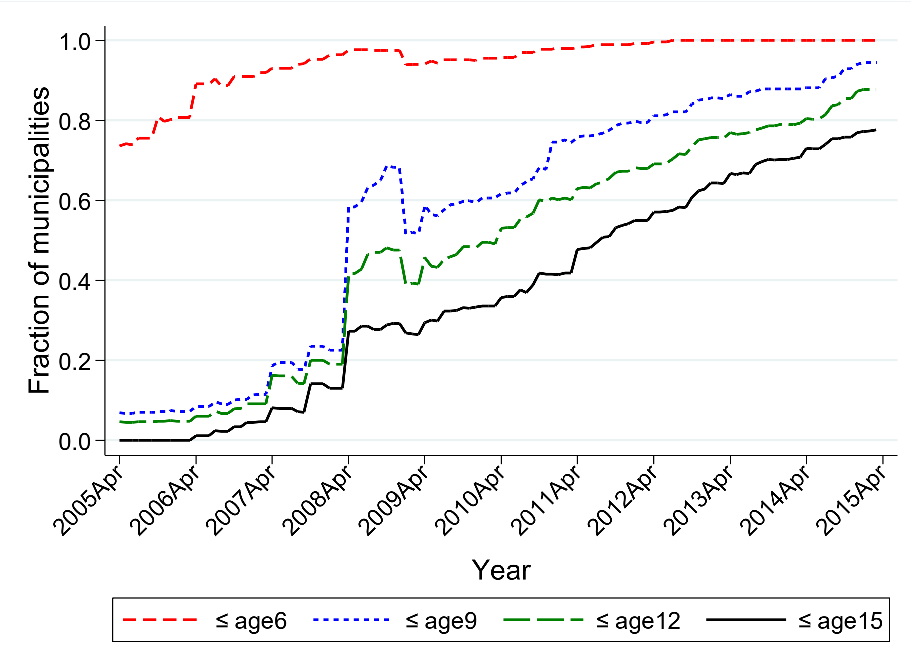
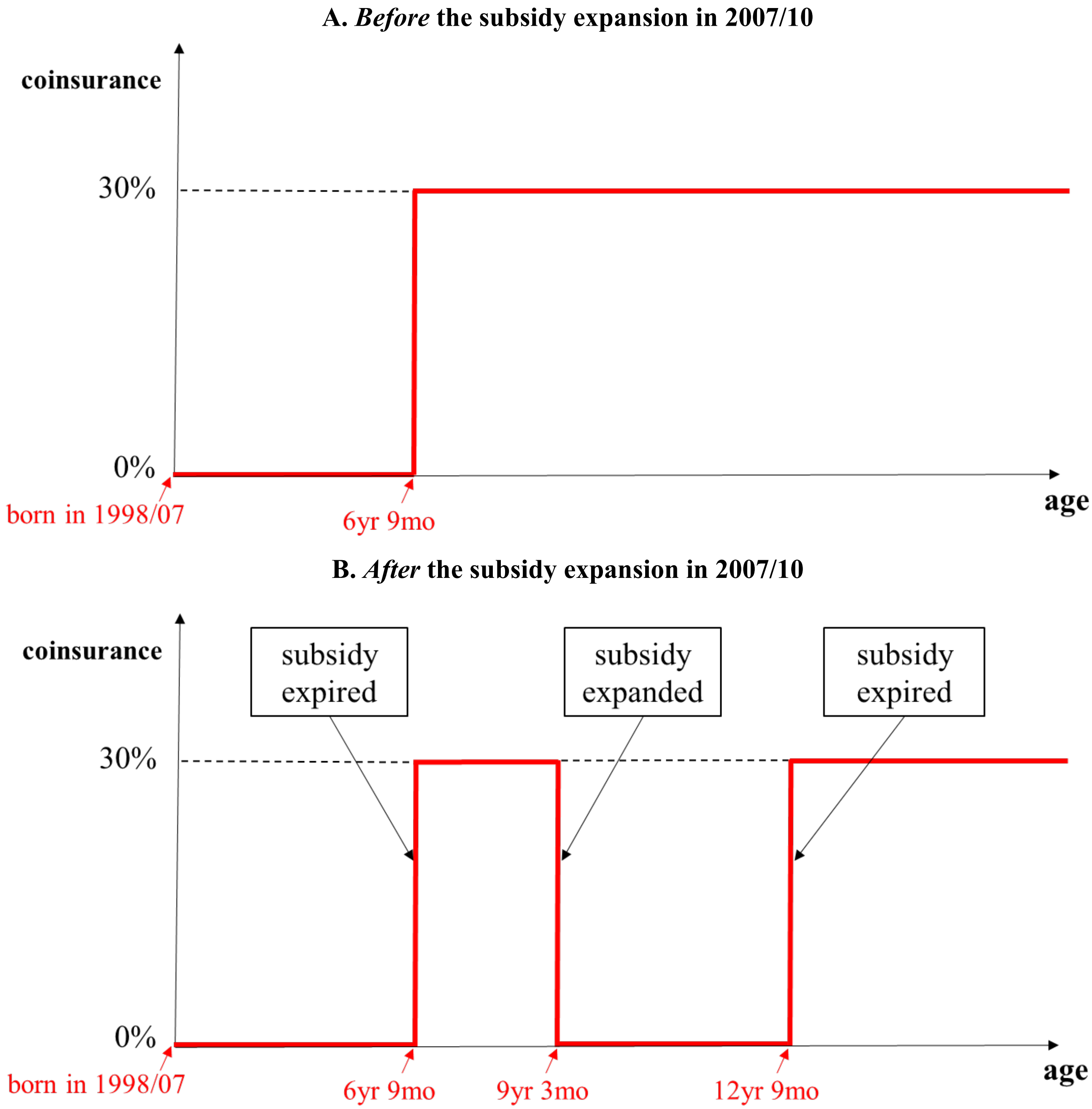
Figure 1: Time series of child age covered by healthcare subsidy



Notes: The figure plots the share of municipalities in our insurance claims data by the child age for the subsidy eligibility (any levels and forms of subsidies) for outpatient care at a monthly frequency between April 2005 and March 2015 (see Appendix Figure A-1 for the precise timing of all policy changes). There is a total of 294 municipalities. Subsidy eligibility is usually determined by school grade. For example, children are often subsidized until the end of junior high school, i.e., until the end of the fiscal year (which ends in March in Japan) after reaching 15 years old. For brevity, we call this cohort "age 15" in the figure. The same applies to other age groups. The ages of 6, 9, 12, and 15 correspond to the end of preschool, 3rd grade, primary school, and junior high school, respectively.

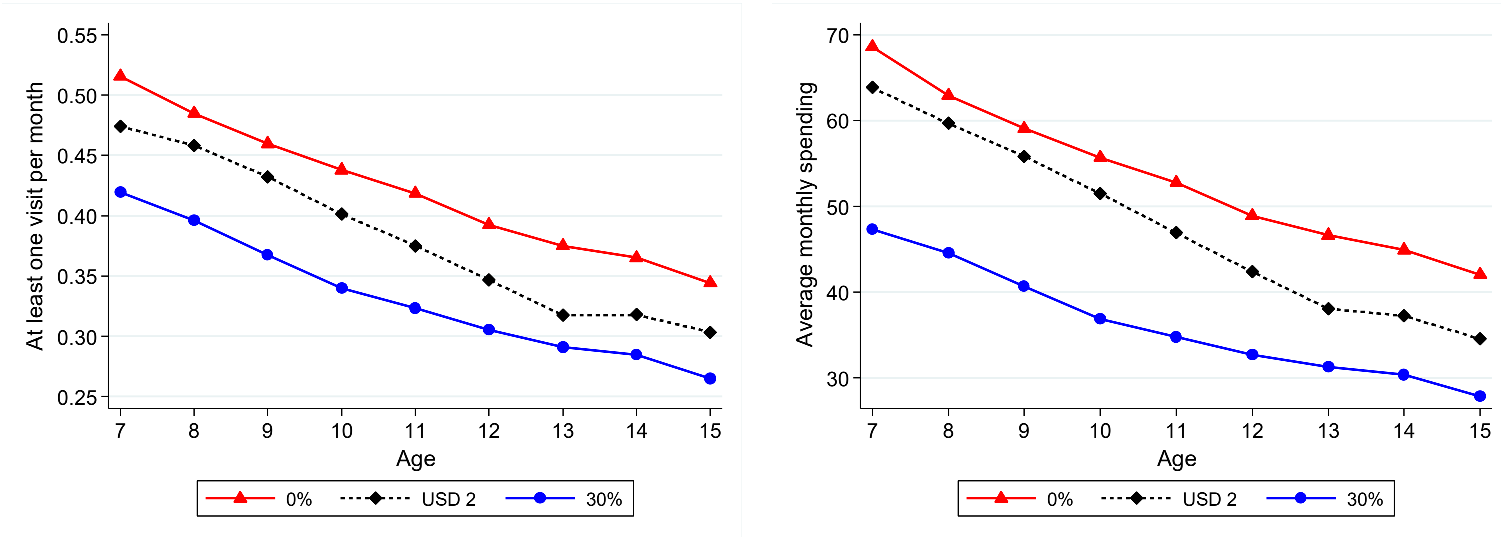
Figure 2: An example of price changes



Notes: The figure shows the cost-sharing schedule of one cohort (born in July 1998) in a particular municipality, which expands the subsidy in October 2007. We use the price change between 0% and 30% as an example. Panel A shows the price schedules before the subsidy expansion. Before the subsidy expansion, the municipality provides free care (i.e., 0% coinsurance rate) until the beginning of primary school (6 years). The cohort is 6 years and 9 months old when they enter the primary school because the school year starts in April in Japan. Above this age, the cohort pays the national level of a 30% coinsurance rate. In October 2007 (when the cohort turns 9 years and 3 months), the municipality expands the free care to the end of primary school (age 12 years). Panel B shows the price schedules after the subsidy expansion. The price reduces to 0% again after this month until the subsidy expires in April 2011, when the cohort graduates from primary school at the age of 12 years and 9 months. Then, once again, the cohort pays the 30% coinsurance.

Figure 3: Utilization with different price levels

A. Outpatient dummy | **B. Outpatient spending** (in USD)

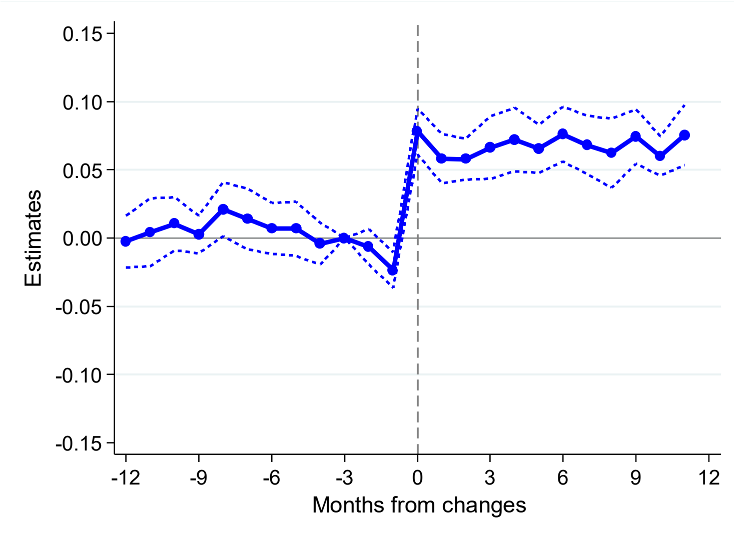


Notes: Panel A is an outpatient dummy that takes the value of 1 if there is at least one outpatient visit per month. Panel B is outpatient spending, which is the monthly spending on outpatient care measured in USD (100 JPY/USD). Both figures plot the raw means of outpatient utilization of individuals at each age of children who live in municipalities with a zero price (labeled "0%"), USD 2 per visit (labeled "USD 2"), and 30% coinsurance (labeled "30%"). The approximate coinsurance rate implied by the USD 2 per visit copayment is 2.4%, which lies between 0% and 30%. We derived this rate by dividing the average out-of-pocket payment (average number of visits per month times the copayment) by the total average monthly outpatient spending.

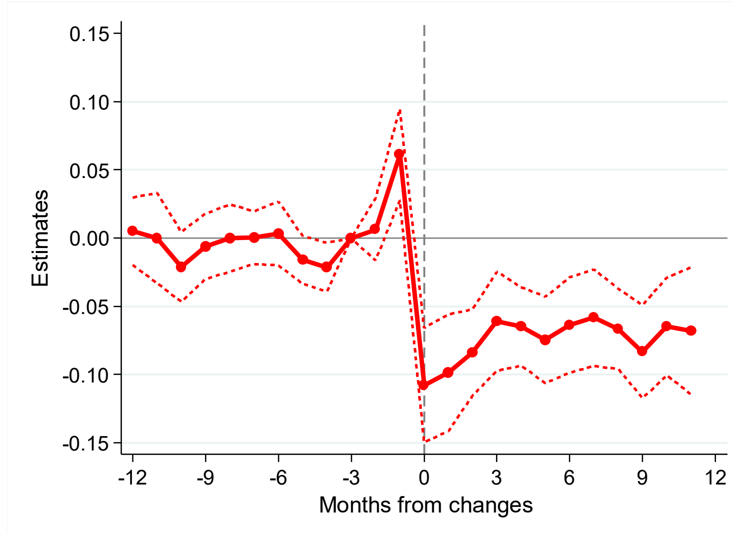
Figure 4: Event study (0% ↔ 30%)

A. Outpatient dummy

30% → 0%

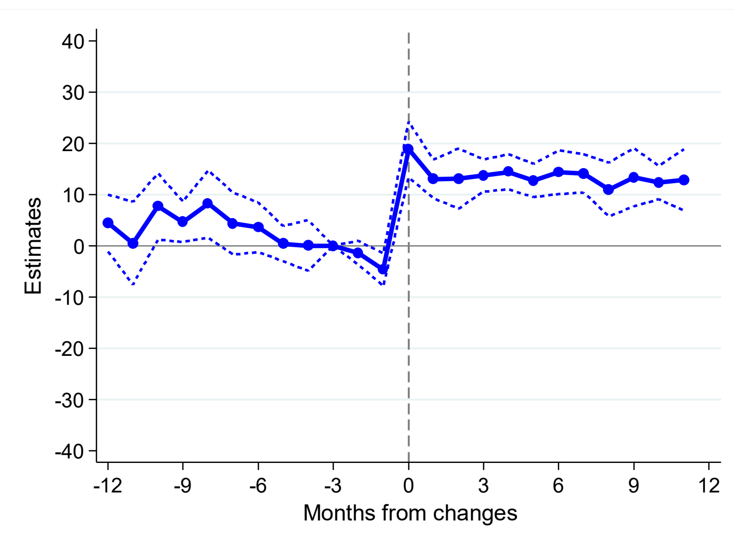


0% → 30%

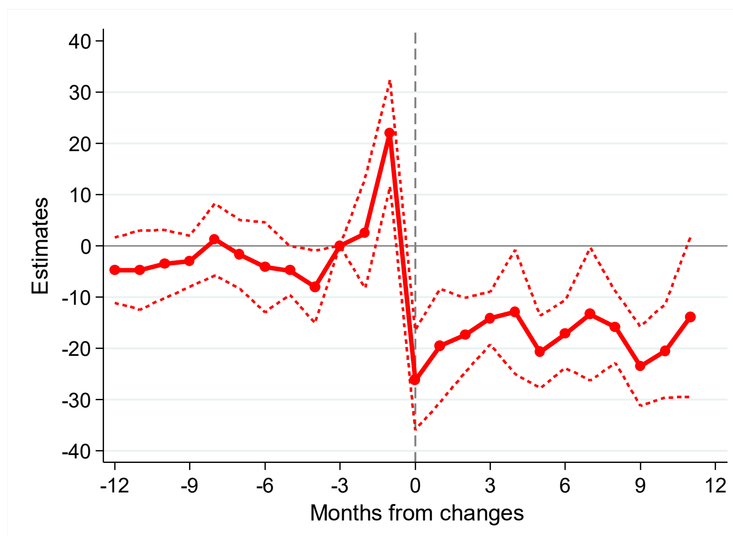


B. Outpatient spending (in USD)

30% → 0%



0% → 30%

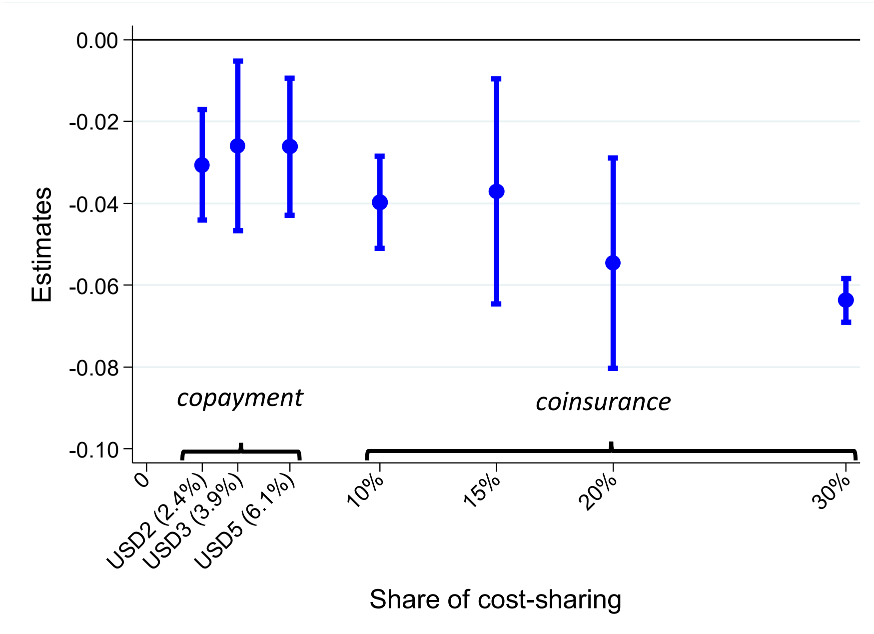


Notes: Panel A is an outpatient dummy which takes the value of 1 if there is at least one outpatient visit per month, and panel B is outpatient spending, which is the monthly spending on outpatient care measured in USD (100 JPY/USD). The solid lines indicate the estimates from a variant of equation [1] where the subsidized dummy is replaced by the interaction of belonging to the treatment group (i.e., experiencing the change in subsidy status) and a series of dummies for each month, ranging from 12 months prior to the change in subsidy status to 12 months after the change (T= –12 to +11, where T=0 is the change in subsidy status). The dotted lines are the 95th confidence intervals where standard errors clustered at the municipality level are used to construct them. The reference month is 3 months before the change (T= –3). The scales of the y-axis are set the same for the two panels so that the two figures for the opposite directions of the price changes are visually comparable.

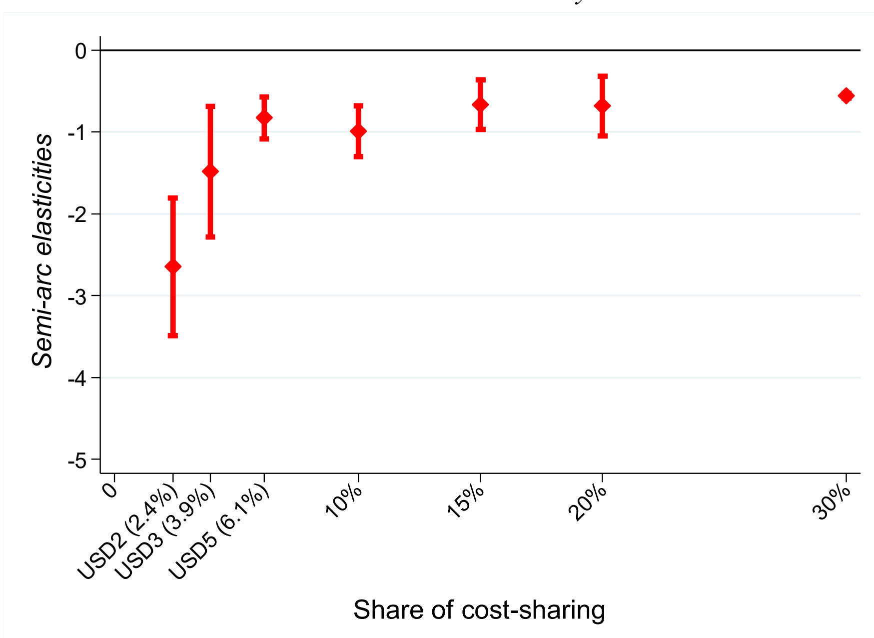
Figure 5: Effect of different cost-sharing

Outcome: Outpatient dummy

Estimate



Semi-arc elasticity



Notes: An outpatient dummy takes a value of 1 if there is at least one outpatient visit per month. The upper half plots β_C from equation [1], and the lower half plots the corresponding semi-arc elasticity. The corresponding table can be found in Appendix Table A-2. The control group is children with free care (C= 0). The mean for the control group is 0.439. The upper and lower bars indicate the 95th confidence intervals where the standard errors clustered at the municipality level are used for estimates, and the bootstrapped standard errors clustered at municipality with 200 repetitions are used for the semi-arc elasticity. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. The approximate coinsurance rates implied by the copays are 2.4% (USD 2 per visit), 3.9% (USD 3 per visit), and 6.1% (USD 5 per visit). We derived these rates by dividing the average out-of-pocket payment (average number of visits per month times the copayment) by the total average monthly outpatient spending.

Table 1: List of changes in patient cost-sharing

Cost-sharing before price change	Current cost-sharing	(1) Number of price changes at the municipality-age-time cell level		(2) Number of children affected by the price change	
		N	*Share*	N	*Share*
0%	30%	3,323	*29.5%*	14,447	*38.7%*
30%	0%	2,678	*23.8%*	11,546	*30.9%*
30%	USD 5/visit	1,014	*9.0%*	2,478	*6.6%*
USD 2/visit	30%	787	*7.0%*	1,376	*3.7%*
USD 5/visit	30%	669	*5.9%*	1,499	*4.0%*
0%	USD 2/visit	527	*4.7%*	1,035	*2.8%*
20%	USD 2/visit	473	*4.2%*	970	*2.6%*
30%	USD 2/visit	325	*2.9%*	453	*1.2%*
30%	USD 3/visit	257	*2.3%*	477	*1.3%*
30%	10%	248	*2.2%*	708	*1.9%*
USD 3/visit	30%	161	*1.4%*	311	*0.8%*
0%	10%	159	*1.4%*	440	*1.2%*
USD 2/visit	USD 3/visit	124	*1.1%*	278	*0.7%*
10%	0%	124	*1.1%*	421	*1.1%*
10%	30%	116	*1.0%*	245	*0.7%*
0%	15%	51	*0.5%*	218	*0.6%*
20%	0%	49	*0.4%*	64	*0.2%*
20%	30%	39	*0.3%*	49	*0.1%*
30%	15%	37	*0.3%*	104	*0.3%*
15%	30%	33	*0.3%*	144	*0.4%*
USD 2/visit	0%	27	*0.2%*	30	*0.1%*
15%	0%	17	*0.2%*	32	*0.1%*
USD 3/visit	USD 2/visit	14	*0.1%*	14	*0.0%*
20%	USD 5/visit	12	*0.1%*	13	*0.0%*
0%	USD 3/visit	1	*0.0%*	1	*0.0%*
0%	20%	1	*0.0%*	1	*0.0%*
Total		11,266	*100%*	37,354	*100%*

Notes: This table shows the frequency of price changes occurring in our claims data. "Number of price changes at the municipality-time-age cell level" shows the frequency of each price change at the municipality-age-time cell level where both age and time are measured in months. For example, if a price change from 30% to 0% occurs in municipality *m* at time *t* (in months) and affects two age groups (*a*), say ages 6 years and 2 months and 6 years and 3 months, this will increase the N in column (1) by 2. In other words, the "municipality-age-time cell" counts the number of treatments that help identify the price coefficients. "Number of children affected by the price change" is obtained by weighting the "municipality-time-age cell" by the number of children in each cell. The "*Share*" columns reflect the proportion of price changes out of all price changes (column 1), and children who experienced the corresponding price change out of all children who experienced any price changes (column 2). Appendix Table A-1 also reports this in the form of the transition matrix of price changes.

Table 2: Summary statistics

Variable	Mean	SD	Min	Max
A. Municipality (N = 294)				
Average length observed (months)	76.50	29.00	2	120
Subsidy info				
Number of policy changes	1.43	1.17	0	6
At least one policy change	77.9%	0.42	0	1
B. Individual (N = 90,184)				
Average length observed (months)	34.75	29.25	1	120
Subsidy info				
Number of subsidy status changes	0.41	0.81	0	5
At least one subsidy status change	24.4%	0.43	0	1
Characteristics				
Female	48.8%	0.50	0	1
Age (in years)	10.76	3.04	6.00	15.92
C. Individual-month (N = 2,992,982)				
Subsidy info				
Subsidized	69.9%	0.46	0	1
At the point of service (when subsidized)	98.3%	0.13	0	1
Income restriction (when subsidized)	6.0%	0.24	0	1
Utilization				
Outpatient dummy	39.7%	0.49	0	1
Outpatient spending	48.8	81.8	0.0	462.3
Outpatient spending (outpatient spending >0)	122.9	88.1	0.8	462.3
N of outpatient visits	0.8	1.3	0.0	34.0
N of outpatient visits (outpatient spending >0)	1.9	1.5	0.0	34.0
OOP payment per visit *without* subsidy	19.2	11.7	0.3	138.6
Inpatient dummy	0.26%	0.050	0	1
Inpatient spending	11.0	349.0	0.0	70115.4
Inpatient spending (inpatient spending >0)	4313.2	5392.8	52.3	70115.4

Notes: Outpatient spending, inpatient spending and out-of-pocket (OOP) payment are measured in USD (100 JPY/USD). All the spending amounts except for OOP payment are the sum of insurer payments and OOP payments.

Table 3: Presence of the zero-price effect

	A. Outpatient dummy			**B. Outpatient spending** (in USD)		
	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
	(1)	(2)	(3)	(4)	(5)	(6)
Zero price dummy	0.021	0.027	0.033	2.020	4.478	7.841
	(0.006)	(0.008)	(0.017)	(1.308)	(1.495)	(3.048)
C	-0.144	-0.008	0.185	-31.722	17.982	133.599
	(0.023)	(0.149)	(0.537)	(4.468)	(27.049)	(98.404)
C^2		-0.379	-2.101		-138.663	-1168.114
		(0.427)	(4.604)		(79.501)	(854.736)
C^3			3.804			2274.192
			(10.089)			(1887.792)
R-squared	0.23	0.23	0.23	0.28	0.28	0.28
N	2,992,982	2,992,982	2,992,982	2,992,982	2,992,982	2,992,982
Mean at *C*=0	0.439	0.439	0.439	56.2	56.2	56.2

Notes: The estimates from equation [2] are reported. Panel A shows the results of an outpatient dummy that takes a value of 1 if there is at least one outpatient visit per month. Panel B shows the results of outpatient spending, which is the monthly spending on outpatient care measured in USD (100 JPY/USD). *C* is the share of cost-sharing. Zero price dummy takes 1 if the price of the healthcare is zero. All the regressions include age (in months) fixed effects (FE), time (in months) FE, and individual FE. We also control for a dummy that equals 1 if the subsidy is applied at the point of service rather than refunded later and 0 otherwise, and a second dummy variable that equals 1 if there is an income restriction on subsidy eligibility and 0 otherwise. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. The mean is the average of the control group (*C*= 0).

Table 4: The effect of a small positive price

	Y = Frequency of outpatient visits		
	Extensive margin	Intensive margin	
	Pr(Y≥1)	Pr(Y≥2)	Pr(Y≥3)
By health status			
Healthy			
USD 2/visit	-0.051	-0.028	-0.014
	(0.015)	(0.010)	(0.006)
Mean	0.305	0.121	0.050
% change from mean	*-16.7%*	*-23.1%*	*-27.9%*
Sick			
USD 2 /visit	-0.014	-0.038	-0.020
	(0.014)	(0.019)	(0.011)
Mean at C=0	0.569	0.311	0.163
% change from mean	*-2.5%*	*-12.2%*	*-12.3%*

Notes: The estimates β_C (C= USD 2 per visit) from equation [3] are reported. We construct the health status as follows: we first calculate the total spending at the first 6 months of observations for each individual whose subsidy status does not change during this period, and then divide each individual into three groups (lowest spending corresponds to healthy, and highest spending corresponds to sick) within each cell: (age in years)×(with or without subsidy). We omit the middle type. All the regressions include age (in months) FE, time (in month) FE, and individual FE. We also control for a dummy that equals 1 if the subsidy is applied at the point of service rather than refunded later and 0 otherwise, and a second dummy variable that equals 1 if there is an income restriction on subsidy eligibility and 0 otherwise. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. The mean is the average of the control group (C= 0).

Table 5: High vs. low-value care

	(1) Preventive care dummy (×100)	(2) Outpatient spending on antibiotics (in USD)	(3)	(4)
		Tier1	Tier2	Tier3
USD 2/visit	-0.070	0.001	-0.066	-0.096
	(0.035)	(0.040)	(0.085)	(0.043)
R-squared	0.37	0.07	0.13	0.08
N	2,992,982	2,992,982	2,992,982	2,992,982
Mean at C=0	0.686	0.382	1.311	0.523
% change from mean	*-10.2%*	*0.3%*	*-5.0%*	*-18.3%*

Notes: The estimates β_C (C= USD 2 per visit) from equation [1] are reported. The outcome in column (1) is a dummy that takes 1 if any of outpatient visits in a month include the diagnoses related to preventive care for children. Note that the dummy is multiplied by 100 to represent the change in percentage points. See Appendix Table E-1 for the list of diagnoses related to preventive care for children along with the corresponding ICD10. The outcomes in columns (2)–(4) are monthly outpatient spending on antibiotics for patients in each tier measured in USD (100 JPY/USD). See Appendix Table F-1 for the list of diagnosis in each tier along with the corresponding ICD10. All the regressions include age (in months) FE, time (in month) FE, and individual FE. We also control for a dummy that equals 1 if the subsidy is applied at the point of service rather than refunded later and 0 otherwise, and a second dummy variable that equals 1 if there is an income restriction on subsidy eligibility and 0 otherwise. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. The mean is the average of the control group (C = 0).