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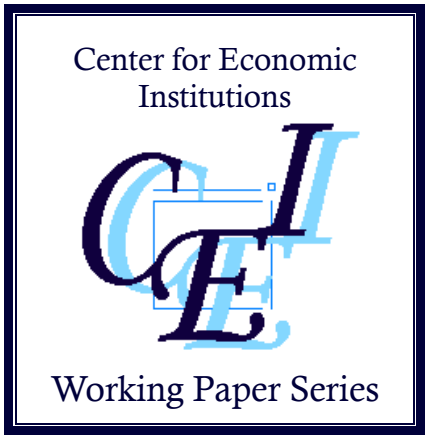

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Free for Children? Patient Cost-sharing and Healthcare Utilization[*]

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Abstract

We examine how children's healthcare utilization responds to prices by exploiting over 10,000 variations in the levels and forms of patient cost-sharing across Japanese municipalities over time. Free care significantly increases outpatient spending, with price elasticities considerably smaller for children than adults. Small copayments alongside free care reduce utilization of healthier—rather than sicker—children, suggesting that moral hazard can be reduced without increasing financial and health risks. We find that cost-sharing is a "blunt tool," affecting utilization regardless of service type. Increased outpatient spending from free care neither improves short- and medium-term health outcomes nor reduces future healthcare spending.

Keywords: Children, Patient Cost-Sharing, Healthcare Utilization, Price Elasticity, Zero-price Effects, Moral Hazard
JEL codes: I18, I13, I11, J13

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1. Introduction

Understanding how a patient responds to the price of healthcare is key to the optimal design of health insurance. However, past studies on patient cost-sharing—for which the beneficiary is responsible out of pocket—are predominantly concentrated on adults and the elderly, and surprisingly little is known about children (see Baicker and Goldman 2011 for a review). In fact, many countries subsidize child healthcare, and patient cost-sharing is often zero (free care).[1] For example, the federal government in the United States (US) regulates the share of cost paid by patients in the Children's Health Insurance Program. Similarly, child healthcare is heavily subsidized in countries with universal health insurance, including Germany, the Netherlands, Sweden, Taiwan, and Korea.[2] The lower out-of-pocket cost, however, may induce unnecessary consumption of medical services. In fact, in countries with universal coverage, one of the few demand-side approaches to contain rising medical expenditures is to fine-tune the level of patient cost-sharing.[3]

There are several good reasons to believe that evidence from adults and the elderly might not be directly applicable to children. First, parents who need to take their children to medical providers may face a higher opportunity costs than the elderly face. Second, the nature of diseases for children tends to be more acute than those for the elderly (e.g., asthma vs. diabetes). At the same time, child healthcare utilization is often preventive and self-limiting, and hence, is potentially more discretionary (Leibowitz *et al.* 1987). Finally, parents may perceive a higher return from child healthcare, and hence, they may seek healthcare regardless of the price, or at least may be less willing to reduce their children's healthcare utilization than their own.

More generally, most existing studies on the impact of cost-sharing derive estimates from only a few changes in cost-sharing, and typically just one. It is quite rare to observe multiple changes in the levels or forms of cost-sharing in a given market. Accordingly, in our view, little is known about whether healthcare utilization responds differently or similarly when different levels and forms of cost-sharing are charged. To the best of our knowledge, the only exception that utilizes many price variations is the RAND Health Insurance Experiment (RAND HIE) conducted in the US during the 1970s (Newhouse and the Insurance Experiment Group 1993), but it analyzes only variations in coinsurance and not in copayment.

[1] Childhood health has been shown to influence both short- and long-term socioeconomic outcomes and health (Case *et al*. 2005; Currie 2009; Smith 2009), providing the ground for generous subsidies for child healthcare in public health insurance programs across countries.

[2] Children and adolescents are exempt from cost-sharing up to the age of 18 years in Germany and the Netherlands. Similarly, children below the age of 3 years are exempt from payment of healthcare in Taiwan, and those below 6 years of age are subsidized in Korea. See Nilsson and Paul (2018) for Sweden.

[3] Hossein and Gerard (2013) document that, between 2000 and 2010, the cost-sharing for outpatient care increased among high-income countries, such as the United Kingdom, Germany, Japan, France, and the US.

Exploiting many price changes for child healthcare in Japan, this study examines how children's healthcare utilization responds to prices. In the last decade, municipalities have rapidly expanded subsidies for child healthcare, which cover different age groups. Moreover, there is substantial variation both in the levels and forms of cost-sharing: while most of the municipalities have reduced the coinsurance rate to zero (i.e., free care) from the national 30% set by the central government, some municipalities have reduced the coinsurance rate to 10%, 15%, or 20%, while other municipalities have used such copayments as 200, 300, or 500 JPY (2, 3, or 5 USD) per visit.[4] Since the coverage and contents of subsidy change at age in months, we have more than 10,000 price changes for child healthcare at the municipality-age-time level. It is quite rare to have so many price changes in a single market, and this unique variation enables us to estimate a number of behavioral responses to the price of healthcare among children in many ways that would be impossible with just a single policy experiment. To this end, we develop a novel dataset by hand collecting data on the exact timing as well as the contents of subsidy expansion at the exact *month* level for 10 years (April 2005–March 2015). We merge this information with individual-level monthly panel data on item-by-item healthcare utilization. We compare the individuals over time with subsidy switching on and off in a difference-in-difference framework.

The unique institutional background in Japan offers a clean setting for identifying patient price responsiveness, since the roles of insurers and medical providers—which are also major players in the healthcare market—are relatively limited. First, there is no adverse selection into insurance because of universal coverage. Second, there are no restrictions by insurers on patient choices of medical providers and, therefore, patients have direct access to specialist care without going through a gatekeeper or a referral system. Third, medical providers cannot price discriminate against patients, as the physician and hospital 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 the quantity of patient visits instead of prices, since by default, there is no room for price shopping to search for cheaper providers. Furthermore, uniform pricing allows us to quantify the monetary values of (excess) utilization easily, unlike the case of the US, which is notorious for the complexity of its pricing schedule.

We arrive at four major findings. First, demand for child healthcare is sensitive to price while free care significantly increases utilization. When cost-sharing is reduced from 30% to 0%, the probability of at least one outpatient visit per month increases by 20%. Similarly, outpatient spending per month increases by 26% under free care. The overall *semi*-arc elasticities are relatively constant for both outcomes at approximately –0.60 throughout ages 7–14 years, which is considerably smaller

[4] For simplicity, an exchange rate of 100 JPY/USD is used throughout the paper.

than the conventional estimate of –2.11 obtained by Brot-Goldberg *et al.* (2017) from the RAND HIE for the non-elderly.[5] As noted before, evidence on the price elasticity of child healthcare is scarce, and we contribute to the literature by providing clean estimates based on a large number of exogenous treatments not previously considered in the literature.

Second, we find that around the price of zero, demand is more price-sensitive and a copayment as small as 2 USD per visit elicits drastic demand responses. This result has an important implication for the optimal design of health insurance: while a small copayment substantially reduces the moral hazard associated with free care, financial risks do not increase by much when a small copayment is charged. The result is consistent with the "zero-price effect" in the literature on behavioral economics. For example, Shampanier *et al.* (2007) show that reducing price from a small positive price to zero discontinuously boosts the demand for a product in a laboratory setting. Douven *et al.* (2017) present a similar result regarding the demand for health insurance. However, to the best of our knowledge, these studies are all experiment based, while ours is the first one that uses field data.

We further investigate how small copayments substantially reduce utilization by examining the type of patient (healthy vs. sick) and the visit margin (extensive vs. intensive). We find that a small copayment especially reduces utilization by healthier children who frequently visit physicians. By contrast, a small copayment does not deter sicker children from visiting a physician at least once per month (i.e., the extensive margin), suggesting that the impact of a small copayment on health outcomes is most likely not very large.

Third, we find that reduced cost-sharing affects utilization across the board regardless of healthcare service type. For example, free care substantially increases both the appropriate and inappropriate use of antibiotics. These results echo the finding of the RAND HIE and Oregon's Medicaid expansion (e.g., Taubman *et al.* 2014) that cost-sharing is a "blunt tool" that affects utilization in a non-discriminatory fashion. This implies that policy instruments other than patient cost-sharing may play a role in further improving welfare. We consider this to be an important finding, given that there is little evidence of this type in the literature on child healthcare and healthcare systems outside the US.

Fourth, we investigate whether subsidy-induced outpatient care improves short- and medium-term health outcomes and reduces future healthcare spending. For the short term, we find that while subsidies increase the utilization of outpatient care for the Ambulatory Care Sensitive Conditions (ACSCs)—diagnoses for which proper and early outpatient treatment should reduce subsequent avoidable admissions—there is little evidence that such increases in outpatient care translate into a

[5] While most of the literature uses arc elasticity rather than semi-arc elasticity, the arc elasticity is not well defined when the starting price is zero, as in our case (e.g., Brot-Goldberg *et al.* 2017). For this reason, we report *semi*-arc elasticity throughout the paper.

reduction in hospitalization by these "avoidable" conditions. More generally, we find no evidence of "offset" effects: substantial increases in outpatient spending do not accompany a reduction in inpatient spending.

We also examine the medium-run effects of subsidies and find that the length of free care during childhood is not systematically associated with either healthcare utilization or health status in adolescents. These results suggest that subsidy-induced utilization during childhood does not improve the health of subsidy beneficiaries, even in the medium term. We interpret this fact as indicating that when universal coverage guarantees minimum access to healthcare, the additional generous subsidy that completely eliminates cost-sharing does not seem to have large health benefits, at least among the health outcomes observed in our data.

This study is most related to the RAND HIE, which still serves as the gold standard in price sensitivity among the non-elderly.[6] Leibowitz *et al.* (1985) specifically analyze children aged 13 years and under and find that, among others, the use of outpatient services decreases as cost-sharing increases. However, the small sample size of their study hinders identification of the effect for some types of services (e.g., inpatient care). Furthermore, their study is over 30 years old, so changes in the practice of medicine (e.g., reliance on managed care and development of new technologies) suggest that these results might not be directly applicable to today's context, especially for countries other than the US. A few notable exceptions from the non-experimental works are two recent studies by Han *et al.* (2016), who examine the effect of copayment at age 3 years in Taiwan, and by Nilsson and Paul (2018), who examine the effect of copayment for outpatient care among children aged 7–19 years in one region of Sweden. Our study is substantially richer in both the variety and number of changes in cost-sharing, thereby allowing us to study in more detail how healthcare utilization responds to cost-sharing, especially around the price of zero.

Finally, this study is related to the extensive literature on health insurance and child healthcare utilization, especially studies on Medicaid in the US (e.g., Currie and Gruber 1996; Dafny and Gruber 2005; Finkelstein *et al.* 2012; Goodman-Bacon 2018). However, these studies examine the effect of health insurance provision *per se* (extensive margin) and not the effect of changes in health insurance generosity (intensive margin), like this study and the RAND HIE do. This distinction is important, because the provision of health insurance entails large wealth effects, and thus, these studies capture the combined effects of price reduction and wealth effects.

The rest of the paper is organized as follows. Section 2 provides the institutional background. Section 3 describes the data, and Section 4 presents our identification strategy. Section 5 reports the

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

basic findings on children's price responsiveness to healthcare, and Section 6 documents zero-price effects. Section 7 explores treatment heterogeneity, and Section 8 examines health outcomes and future healthcare spending. Section 9 concludes.

2. Background

2.1. Healthcare system in Japan

Japan's universal health insurance system, introduced in 1961, is heavily regulated by the government. All citizens are required to enroll in either an employment-based or a residential-based insurance system (see, e.g., Ikegami and Campbell 1995; Kondo and Shigeoka 2013). The enrollees receive identical benefits—regardless of insurance type—which include outpatient services, inpatient services, dental care, and prescription drugs.

The unique institutional background in Japan offers several advantages in identifying patient price responsiveness, since the roles of insurers and medical providers are restricted. First, enrollment in health insurance is mandatory and, more crucially, enrollees cannot choose insurers. Thus, there is no adverse selection problem, which often introduces complications in other studies. Second, patients face no restrictions on their choices of medical providers. For example, patients have direct access to specialist care, including teaching hospitals, without going through a gatekeeper or a referral system, unlike the US, where insurance companies restrict the choices of medical providers through managed care. Third, patients cannot be price discriminated against by medical providers since medical providers receive the same fee for the same service regardless of the insurer based solely on the national fee schedule (i.e., fee-for-service). As a result, any changes in utilization documented in this study come from quantity (of services provided) instead of prices.

2.2. Patient cost-sharing

Patient cost-sharing has been set nationally at 30% except for the following two population segments: young children and the elderly. In particular, the cost-sharing is set at 20% for children aged below 6 years. The insurer pays the remaining fraction of expenses until the beneficiary meets the stop-loss, and then the patient pays a 1% coinsurance above the stop-loss. Unlike a typical health insurance plan in the US, there are no deductibles in Japan. In addition, supplementary private health insurance covering out-of-pocket costs virtually does not exist in Japan, probably because the stop-loss prevents catastrophic income losses resulting from illness. As a result, municipal subsidies directly affect patient cost-sharing.

The non-linearity imposed by the stop-loss is a classic but important challenge in estimating price elasticities (Keeler *et al.* 1977; Ellis 1986). A forward-looking patient who anticipates spending

beyond the stop-loss may respond to the true "shadow" price rather than the "spot" price (e.g., Aron-Dine *et al.* 2015). However, this is unlikely in this setting for the following two reasons. First, only 0.067% person-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 0.28% of person-months).[7] Second, the stop-loss resets each month in Japan, whereas it is set annually in the RAND HIE and most types of health insurances in the US. To the extent that illnesses are unpredictable, this shorter interval may make 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 provide subsidies for child healthcare to those who live in the municipality regardless of insurance type; the aim is to ensure access to essential medical care for children and lessen the financial burden on parents. The municipal subsidies are seamlessly integrated into the national health insurance. In Japan, medical providers submit claims to an intermediary, that is, a claims review and reimbursement organization (CRRO), instead of billing insurers directly. CRROs review claims on behalf of insurers and then reimburse the provider based on a national fee schedule. Most municipalities use the same arrangement for child healthcare subsidies; CRROs bill the subsidized amount to the municipality and reimburse the entire payment to the provider except for the patient out-of-pocket cost, if any. Thus, the municipal subsidy adds 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. The subsidy usually takes the form of an in-kind payment but can be a refund.

Since 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 indicate their municipality of residence. Therefore, we can identify the level of subsidy (and hence, the level of cost-sharing) that each child faces. The variations in these three dimensions are the sources of our identification strategy.

For each municipality, we collect the following four information items on outpatient care subsidies from April 2005 to March 2015[8]: (1) ages that the subsidy covers; (2) the level of patient

[7] The individual stop-loss for a regular worker, for example, is set at 801 USD per month, and he/she has to pay 1% of medical expenditures beyond the amount.

[8] We also collect information on subsidy for *inpatient* care. However, most municipalities had already covered inpatient care until the age of 15 years (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

cost-sharing (equivalently, the level of municipal subsidy); (3) whether the subsidy is a refund or an in-kind payment; and (4) whether there are any household income restrictions for subsidy eligibility. These four features determine the subsidy generosity, and to the best of our knowledge there are no other dimensions of insurance generosity that change coincident to the subsidy changes. 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 that while the eligibility age is often expressed by school grade (e.g., until the end of junior high school), we loosely use ages throughout this study for convenience, as the school grades are almost completely equivalent to age in Japan owing to the strict enforcement of the school entry rule as well as very rare grade retention and advancement rates (Shigeoka 2015).

Second, the level and form of the subsidies (and thus, cost-sharing) differ by municipality: the majority of municipalities fully subsidize child healthcare (i.e., the coinsurance rate is reduced from the national level of 30% to 0%). Some municipalities reduce the coinsurance rate to 10%, 15%, or 20%, while other municipalities take a form of copayment, such as 2, 3, or 5 USD per visit.

Third, as noted earlier in this section, the subsidy can take the form of either a refund or an in-kind payment (i.e., future vs. immediate reimbursement). While the amount of cost-sharing can be identical in the two cases, as long as the parents submit the required documents for a refund, patients may prefer in-kind payments to refunds because of time cost and/or credit constraint.

Finally, some municipalities impose income restrictions on 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 in our data (1.5%). In the empirical model, we include a dummy for income restriction at municipality×year-month levels.

As mentioned in the introduction, a 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*). Since 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. Since collecting data this way takes substantial time and effort, we limit data collection to all municipalities in the six largest prefectures in Japan, resulting in a total of 323 municipalities.[9] According to

with the RAND HIE, which finds that children respond only 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.

[9] This includes municipalities that merged during this sample period. All results throughout the study are essentially the same if we exclude these municipalities, since they tend to be very small (results are available upon request). There were a total of 47 prefectures and 1,719 municipalities in Japan as of January 2015.

national statistics, these six prefectures cover as much as 44.9% of children aged 0–15 years.

Municipalities have drastically expanded child healthcare subsidies over the last decade. Figure 1 plots the share of municipalities in our insurance claims data by maximum age for the subsidy eligibility for outpatient care in the period April 2005–March 2015. The figure clearly shows that the subsidy expanded rapidly to older ages in the last decade. For example, none of the municipalities provided a subsidy until the age of 15 years (the end of junior high school) in April 2005, the beginning of the sample period. However, this number reaches nearly 80% by the end of our sample period 1 decade later (see Appendix Figure A-1 on the precise timing of all policy changes).[10]

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. This data is used in previous studies, including Iizuka (2012) and Fukushima *et al.* (2016). While our data are not nationally representative, the average healthcare spending in our sample is similar to the national average.[11] As of November 2015, the JMDC claims database contains more than 3 million enrollees.

JMDC data consist of administrative enrollment data and claims data. For each person, the enrollment data consist of patient ID, gender, age, and municipality of residence. 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. The claims data report the monthly spending, including the months of no utilization.[12] Specifically, the claims data contain the year-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. The unit of claims data is monthly 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.

There are a number of advantages to this claims dataset. The biggest advantage is that the data encompass both outpatient (including prescription drug) and inpatient care, *and* follow the same individual over time. This allows us to examine, for example, whether childhood subsidies have a

[10] The small jump in April 2008 is explained by the fact that the central government expanded 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 expansion eased the budgetary burden on municipalities, as part of the cost to provide free care for below 6 years was covered by the central government, allowing municipalities to expand coverage to older ages.
[11] For example, the average healthcare spending for children aged 10–14 years in 2014 is 71.4 USD per month (based on our calculation from the Ministry of Health, Labour and Welfare 2015), while that of our full sample is 73.2 USD.
[12] The data do not, however, contain dental claims, and inpatient food and housing costs. The latter is small, since the length of stay is short, unlike the case of the elderly.

beneficial effect over the medium run when children become adolescents. Furthermore, we can test the possibility of the "offset" effect—whether beneficial *outpatient* care prevents avoidable *inpatient* admissions in the future. By contrast, outpatient and inpatient data are often separated in other settings. For example, hospital discharge data do not usually include information on office/outpatient visits and prescription drugs. In addition, the claims data in Japan include actual transaction prices, since the national fee schedule sets uniform prices for each procedure, which is applied to all patients. Therefore, price information enables us to quantify the monetary values of (excess) utilization easily.

Our dataset is constructed as follows. We provide the subsidy information collected to JMDC Inc., which merged it with their insurance claims data in-house by municipality and year-month, and returned it to us with the municipality ID and patient ID de-identified for confidentiality reasons. Thus, we cannot examine heterogeneity by 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 or siblings).

3.2. Sample restriction

Our data cover a period of 10 years between April 2005 and March 2015 (120 months). We focus on children aged 7–14 years (96 months) since, as shown in Appendix Figure A-2, we do not have many observations without subsidy below age 7 years and with subsidy above age 15 years. This is because the majority of municipalities (75.6%) already provided the subsidy until the age of 6 years (start of primary school) at the beginning of our sample period, and most municipalities do not provide subsidy beyond age 15 years (end of junior high school) at the end of our sample period. Therefore, we limit our sample to 6–15 year-olds (1 year wider on both sides of the ages of interest) to identify the effect of patient cost-sharing at ages 7–14 years.[13] We also limit our sample to those who stay in the same municipality (98.3% of the sample). The migration rate in our sample is lower than actual migration, since *intra*-municipality migration is not counted as the subsidy level does not change. Nonetheless, we address the possibility that children move to a municipality that offers a generous child healthcare subsidy in Subsection 5.3.

We then create two samples (i.e., the *0–30%* sample and the *full* sample). We use the 0–30% sample—focusing on 165 municipalities that only have either 0% (full municipal subsidy) or 30%

[13] While we control for the subsidy status at ages 6 and 15 years in the regressions, we do not report these estimates to save space, as they are very noisy.

(no municipal subsidy) patient cost-sharing during our sample period—for simplicity and ease of computation for the analysis of age-specific price elasticities (Section 5) and heterogeneous effects by service types (Section 7). In Section 6, we use the full sample and examine whether different levels and forms of cost-sharing differentially affect the demand for healthcare. As shown in Table 1, the transitions "30% to 0%" and "0% to 30%" for cost-sharing are by far the top two most frequently observed price changes and account for 54.2% of all the transitions at the municipality-age-time level (the unit of variation), and as much as 70.0% at the person-month level (the unit of observation). See Appendix Table A-1 for the complete list of price changes. Even after restricting our attention to the 0–30% sample, as many as 5,438 changes in subsidy status at the municipality-age-time level remain. Furthermore, these two price transitions are observed for the entire age range.[14] This large number of variations allows us to conduct detailed analysis of, for example, age-specific price elasticities.

Appendix Table A-2 shows that the characteristics of children as well as their healthcare utilization are quite similar between 165 municipalities in the 0–30% sample and the remaining municipalities. This alleviates the concern that municipalities in the 0–30% sample are specific, and thus, the results are not generalizable.

3.3. Descriptive statistics

Table 2 provides the summary statistics of selected variables in the 0–30% sample at the municipality, individual, and person-month levels in Panels A, B, and C, respectively. Panel A shows that each municipality is observed on average for 76.6 months, and 68.5% of the municipalities have at least one subsidy expansion. As discussed in Subsection 4.1, the source of variation for identification does not come simply from the *expansion* of the subsidy but also from the *expiration* of the subsidy at a certain age. At the individual level (Panel B), we have a total of 63,590 individuals, and each individual is observed for an average of 36.2 months. At least one subsidy change is experienced by 21.8% of individuals: 16.5% experience at least one subsidy expansion (from 30% to 0%) and 19.3% experience at least one expiration (from 0% to 30%). Gender is well balanced (48.8% are female).

Finally, Panel C of Table 2 reports some key variables at the unit level of our analysis (person-month). We have a total of 2,303,335 person-months over the sample period of 120 months. Almost all the subsidy is in-kind (99.9%), and very few municipalities impose income restrictions for eligibility criteria (1.5%). In terms of utilization, 40.7% of children make at least one outpatient visit

[14] Note that since we include only municipalities that have either a 0% or 30% of coinsurance rate *throughout* our sample period in the 0–30% sample, the actual number of these two price transitions is slightly smaller than those listed in Table 1.

per month on average, and spend 60.9 USD per month, including zero-spending and 149.9 USD conditional on at least one visit.[15] Out-of-pocket payment per visit *without* subsidy is 22.3 USD, which gauges the magnitude of the financial burden on individuals if the subsidy is not available. Inpatient admission for this age range is very low (only 0.28%), but inpatient care is much more costly upon admission (4065.2 USD) than outpatient care.

Simple plots of raw data already reveal interesting patterns. Panel A of Figure 2 plots the raw means of outpatient utilization at each age for children who live in municipalities with subsidy (labeled "subsidized") and those who live in municipalities without subsidy (labeled "no subsidy"). The graph on the left for an outpatient dummy shows that the line with subsidy is always higher than the line without subsidy by 8–11 percentage points at any age range, while both age profiles are declining, since the average health may improve at older ages. The graph on the right shows a similar pattern for outpatient spending: the mean outpatient spending is 20–30 USD (40–60%) higher with the subsidy than without the subsidy, which is substantial. While this figure does not account for compositional changes in the sample, the main message from the regression analysis below is similar. Panel B of Figure 2 plots the age profile of inpatient outcomes, aggregated by age in years, as hospital admissions are very rare. In contrast to outpatient outcomes, we observe no clear difference in the inpatient dummy and inpatient spending with and without subsidy.

Appendix Table A-3 lists the major diagnosis groups in our sample by ICD10. The largest share comes from diseases of the respiratory system, which account for approximately one-third of all the diagnoses. We also list the top 10 individual diagnoses at the ICD10 4-digit level. The top ranked diagnoses (e.g., acute bronchitis and acute upper respiratory infections) tend to be more acute whereas the elderly tend to have more chronic diseases.

4. Identification Strategy

4.1. Sources of variations in patient cost-sharing

Before presenting our estimation equation, it is important to clarify the two sources of variations used in our identification strategy. Importantly, the subsidy (hence, patient cost-sharing) is uniquely determined by municipality, age, and time. Put differently, each cohort (defined by birth year-month) in each municipality has its own price schedule, unless they move across municipalities. Figure 3 illustrates one example of a patient cost-sharing schedule in a particular municipality. By drawing the two separate price schedules for two cohorts that are born just 1 month apart, we demonstrate our source of variations in subsidy status at different ages.

[15] 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).

Panel A of Figure 3 shows the price schedules for each cohort *before* subsidy expansion. The solid line corresponds to the price schedule for a cohort born in July 1998 ("younger" cohort, hereafter), and the dotted line for a cohort born in June 1998 ("older" cohort, hereafter), born 1 month before the younger cohort. Suppose that the municipality provides free care (i.e., 0% coinsurance rate) until the beginning of primary school (6 years). Since the school year starts in April in Japan, the younger cohort is 6 years and 9 months old, while the older cohort is 6 years and 10 months old, when both cohorts enter primary school in April 2005. Above this age, children pay the national level of a 30% coinsurance rate.

Suppose that in October 2007, the municipality expands the subsidy up to the end of junior high school (age 15 years). Panel B of Figure 3 shows the price schedules *after* subsidy expansion. The younger cohort (solid line) pays the full 30% from the age of 6 years and 9 months to the age of 9 years and 2 months, 1 month before the subsidy expansion in October 2007. Because of subsidy expansion, the cohort enjoys free care from the age of 9 years and 3 months until the age of 15 years and 8 months when the cohort graduates from junior high school in March 2014. Then, once again, the cohort pays the full 30% after the age of 15 years and 9 months. Meanwhile, the price schedule for the older cohort (dotted line) is shifted by 1 month to the right, as the cohort is 1 month older than the younger cohort at the entry of primary school, the subsidy expansion, and graduation from junior high school.

This simple illustration demonstrates that any cohort aged between 6 and 15 years benefited from the same subsidy expansion. As a result, each cohort uniquely experienced the subsidy expansion and the expiration at different ages. This enables us to estimate the price elasticity for broad age ranges (7–14 years) even at the monthly level. In principle, we could also investigate asymmetric price responses to the direction of the price changes, as our variation includes price changes in both directions even at the same age. See Iizuka and Shigeoka (2018) for details.

4.2. Identification strategy

We attempt to identify the effects of the subsidy for child healthcare by exploiting the unique variation in subsidies across municipality, age, and time combined with the longitudinal claims data in a difference-in-difference framework.[16] Specifically, our basic estimation equation is:

$$Y_{iamt} = \alpha + \sum_{a=85}^{179} \beta_a subsidized_{iamt} + \gamma X'_{mt} + \delta_a + \varphi_m + \pi_t + \theta_i + \varepsilon_{iamt}, \qquad [1]$$

[16] We abstract from whether this effect stems from patient-induced demand, that is, children or parents ask for more care when the price is low, or physician-induced demand, that is, physicians may provide aggressive treatments stemming from their economic motives/benevolence. See, for example, Iizuka (2007, 2012) for studies that attempt to disentangle these two effects.

where Y_{iamt} is the healthcare utilization by child i whose age is a (measured in months), at time t (year-month), and living in municipality m. The variable $subsidized_{iamt}$ is a dummy that takes the value 1 if outpatient care for children is fully subsidized at age a. Since children become eligible or ineligible for the subsidy at the beginning of the specified month, we can assign the subsidy 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. The simple illustration in the previous subsection highlights the importance of controlling for these fixed effects. 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 identical to θ_i for non-movers, and thus, we omit it hereafter).[17] We also control for two time-varying municipality variables, X_{mt}, a dummy that takes the value of 1 if the subsidy is in-kind rather than a refund, and equals 1 if there is an income restriction on subsidy eligibility. Note that the in-kind dummy is essentially interacted with the subsidy dummy because when there is no subsidy (i.e., 30% coinsurance rate), there is nothing to refund. The same argument applies to the income restriction dummy.

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-part or two-part generalized linear model, are almost identical to the OLS estimates (see Appendix Figure C-2). To ease the computational burden for estimating the bootstrapped standard errors for our elasticity measures, we report the OLS estimates throughout the paper.

While we can technically estimate β_a (age a in months), as shown later, the monthly estimates β_a are relatively stable within age in years. Therefore, we instead report β_A (age A in years) to obtain more statistical power without losing much information:

$$Y_{iamt} = \alpha + \sum_{A=7}^{14} \beta_A \{subsidized_{iamt} \times 1(Age\ A)\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt}, \qquad [2]$$

where $1(Age\ A)$ is an indicator variable that takes the value of 1 if the person is older than age A but younger than age $A+1$ (or equivalently $1(Age\ A) = 1(A \leq a < A+1)$). We construct age in year dummies in this way so that age corresponds to school grade. For example, ages 6, 12, and 15 years correspond to the age of entry to primary school, the last year of primary school, and the last year of junior high school in Japan, respectively. Our coefficients of interest are a series of β_A (A= 7–14 years), which capture the average effect of a subsidy within the age ranges. Note that we still include δ_a at the monthly level to account for any age in month-specific effects (e.g., graduation from primary school).

[17] For non-movers, since *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 identifying assumption in our difference-in-difference strategy[18] 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 while income effects simply increase utilization, our estimates may be upward biased.

To address this concern, we adopt four approaches. First, we conduct an event study that normalizes the data to the timing of the subsidy changes and examine if there are any systematic differences in the pre-trend between the treated and the control municipalities before the changes (Subsection 5.1). Second, we add time-by-municipality fixed effects (with time measured in months), to examine the robustness of our baseline estimates. This specification is most stringent, as these fixed effects capture the average effect of municipality-specific policy changes or events in a particular month, if any, such as income transfers, other subsidies, or business cycles. Third, we limit our sample to individuals who experienced at least one change in subsidy status. By exploiting only the *timing* of the changes in subsidy status, we can to some extent mitigate the concern that individuals in the treatment and control municipalities may be different. Finally, we examine selective migration: whether sicker children (and their parents) migrate to a municipality that provides free care. All the results except for the event-study (Subsection 5.1) are presented in Subsection 5.3.

5. Basic results

5.1. Event study

Before presenting the regression results, we provide graphical evidence on changes in outpatient outcomes in the form of an event study. Here, we normalize the data around the change in subsidy status at any age to increase statistical power. We then replace the subsidized dummy in estimation equation [2] by the interaction of the variables corresponding to being in 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). Thus, the estimates are the weighted average of treatment effects across all ages.

Figure 4 presents the results of the event study for an outpatient dummy (Panel A), and outpatient spending (Panel B), separately for subsidy expansion and subsidy expiration. The reference month is 3 months before the change in subsidy status (T= –3). The scales of the y-axis are set the same within panels so that two figures for opposite directions of price changes are visually

[18] This strategy may also be viewed as some form of a staggered roll-out design.

comparable.

There are a few important points to make about these graphs. First, they do not seem to show any pre-trend (except for anticipatory effects), as the estimates are mostly close to zero before the changes in subsidy status in both panels. This is a reassuring result, as it addresses the concern that municipalities that expand the subsidy may have a different trend in healthcare utilization than municipalities that do not.

Second, there is substantial anticipatory utilization, as indicated by drops in subsidy expansion and surges in subsidy expiration just before T=0. This pattern reveals that some children (and hence, parents) are aware of the upcoming price changes and behave strategically by delaying or rushing visits. On the one hand, the existence of anticipatory utilization is rather surprising, as the nature of children's diseases tends to be acute.[19] On the other hand, the fact that the magnitude is larger for subsidy expiration than for subsidy expansion indicates that at least a part of these visits is indeed acute, because one cannot delay treatments too much until the subsidy expands while one can more easily stockpile before the subsidy expires.[20] As we include age and time fixed effects (both in months), this difference is not driven by a particular age or year-month effect, such as the expiration of subsidy after graduation from junior high school. In any case, since such anticipated effects—which may overstate our estimates—seem to be concentrated within 2 months from T=0, we exclude these 4 months of the data throughout the study. For instance, a similar approach is taken by Chandra *et al.* (2010). In fact, the estimates and hence, elasticities are barely affected after removing more than 2 months from T=0 (see Appendix Figure C-1).[21]

Finally, the effect on utilization seems to be permanent rather than transitory, since the level of utilization after T=0 does not revert to the level before T=0. This result justifies the use of the difference-in-difference strategy, as we do not need to rely on observations only around T=0 to estimate the effect of cost-sharing on utilization.

5.2. Main results

Figure 5 is a graphical representation of equation [2], which plots β_A for each age (A= 7–14 years) in the upper half and the corresponding semi-arc elasticity in the lower half. Panels A and B present the results of an outpatient dummy and outpatient spending, respectively. Note that Appendix

[19] In Japan, schools provide annual health checkups for children free of charge and, consequently, well-care visits are not likely to explain the anticipatory utilization.
[20] As we report in our working paper (Iizuka and Shigeoka 2018), while we indeed observe anticipatory utilization for all service categories examined (medication, consultation fees, laboratory tests, and non-surgical procedures), the magnitude of anticipatory spending seems to be larger for medication than for non-surgical procedures.
[21] When the *net* change in utilization around the changes in subsidy status is positive, the excess mass of anticipatory utilization (e.g., delayed treatment) can be viewed as moral hazard (Cabral 2017). If so, the estimates and corresponding elasticities that remove 4 months of data may underestimate the moral hazard.

Figure B-1 plots the monthly estimates (β_a) instead of yearly estimates (β_A). Since monthly estimates are relatively stable for the corresponding age in years (and statistically significant at the 1% level for any age in months), we do not lose much information by only reporting β_A.

Panel A of Figure 5 reveals that the estimates (β_A) on an outpatient dummy are relatively stable across ages 7–14 years and statistically significant at the conventional levels for any age. With subsidy, the probability of seeing a physician at least once a month increases by 6–8 percentage points. This translates into a 19–25% increase from 0.32, the mean without subsidy for ages 7–14 years.[22]

The corresponding semi-arc elasticities presented at the bottom half range from –0.52 to –0.63.[23] Here, considerable caution is required when comparing the elasticities estimated across countries and time periods. Moreover, as we discuss later in detail, the elasticities can substantially differ depending on the level and form of cost-sharing. In this regard, the closest setting to ours (0% vs. 30% coinsurance rate) is the RAND HIE, which compares 0% versus 25% coinsurance rates. Our elasticities for children are considerably smaller than the –2.11 that Brot-Goldberg *et al*. (2017) calculate from the RAND HIE for the non-elderly.[24,25] As discussed in the introduction, the smaller elasticity for children than adults may reflect both the higher opportunity cost of parents, as they need to take their children to medical institutions, and the nature of diseases for children, as they tend to be more acute.[26] Furthermore, it is not surprising that parents may perceive a higher return from child healthcare, and hence, they may seek healthcare regardless of the price, or at least may be less willing to reduce their children's healthcare utilization than their own.

Panel B of Figure 5 plots the estimates of outpatient spending. While outpatient spending is arguably of greatest interest—as it eventually captures the size of total utilization—the estimates are slightly less precise than the extensive margin documented above. The estimates decline slightly as

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[22] To the extent the congestion at medical institutions deters some healthcare demand in order to avoid waiting costs, our estimates may be viewed as a lower bound. Unfortunately, we do not have data on waiting time.

[23] While most of the literature uses arc elasticity rather than semi-arc elasticity, arc elasticities, defined as $\varepsilon_A = \left(\frac{Q_{1A}-Q_{0A}}{(Q_{0A}+Q_{1A})/2}\right)/\left(\frac{P_{1A}-P_{0A}}{(P_{1A}+P_{0A})/2}\right) = \left(\frac{\beta_A}{Q_{0A}+Q_{1A}}\right)$, are 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. For this reason, we report the *semi*-arc elasticity defined by $\varepsilon_A = \left(\frac{Q_{1A}-Q_{0A}}{(Q_{0A}+Q_{1A})/2}\right)/(P_{1A}-P_{0A}) = \left(\frac{2\beta_A}{Q_{0A}+Q_{1A}}\right)/(0-0.3) = -\left(\frac{\beta_A}{Q_{0A}+Q_{1A}}\right)/0.15$.

[24] For a direct comparison with the RAND HIE, the arc elasticities (which are simply 0.15 times the semi-arc elasticities) range from –0.07 to –0.10. Again, these numbers are considerably smaller than –0.18 (0% vs. 25% coinsurance rate) in the RAND HIE (Keeler and Rolph 1988; Aron-Dine *et al.* 2013).

[25] A few studies examine the *extensive* margin of health insurance, that is, whether children have access to health insurance, on healthcare utilization. For example, Anderson *et al.* (2012) exploit a sharp change in insurance coverage rates that results from young adults "aging out" of their parents' insurance plans at 19 years, and find that the elasticity of emergency department visits is around –0.60.

[26] Based on Leibowitz *et al.* (1985), who specifically analyze children aged 13 years and under, we obtain the semi arc elasticity of –0.90 for outpatient spending. However, the small sample sizes make it less reliable.

individuals get older: with the subsidy, outpatient spending increases by 13.8 USD per month at age 7 years, and by 9.98 USD per month at age 14 years compared to those without the subsidy. These estimates correspond to an 18–31% increase from 44.9 USD, which is the mean value for ages 7–14 years without subsidy. The corresponding semi-arc elasticities range between –0.74 and –0.63. For brevity, the tables for these figures are reported in Appendix Table B-1.

Appendix Table B-1 also shows that in-kind payment increases outpatient visits by 4.7 percentage points, which is more than half the size of the estimates in patient cost-sharing from 30% to 0%. In addition, the outpatient visits decrease by 2.0 percentage points when there is income restriction on eligibility for the subsidy. For outpatient spending, the signs of these coefficients are also as expected, but not statistically significant at the conventional level. The former result is consistent with Zhong (2011), who shows that immediate reimbursement increases healthcare utilization compared to future reimbursement in China.

Since the total number of children aged 7–14 years in Japan was approximately 8.8 million in 2015 (Statistics Bureau 2015), a back-of-the-envelope calculation suggests that annual outpatient spending increases by 1.17 billion USD if free care is expanded to all municipalities in Japan.[27] This would create a substantial negative fiscal externality for many stakeholders: while 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.

In Appendix D, we report the results on frequency of outpatient visits as outcomes. The semi-arc elasticities are similar in magnitude to those of outpatient spending.[28] We also examine the intensive margin of outpatient use, that is, the outpatient spending and frequency of visits *conditional* on positive spending. Both spending and frequency of visits increase, suggesting that increases in outpatient use are driven by both extensive and intensive margins.[29]

5.3. Addressing Endogeneity Concerns

This study exploits the fact that municipalities introduce different subsidies at different times. On the one hand, it is a great advantage that there are local policies that result in a lot of different prices we can exploit. On the other hand, a potential concern about utilizing decentralized municipal policies for identification is that these policies could be endogenous to other conditions/policies in

[27] We multiply each β_A by the number of children aged A in 2015 and aggregate them to calculate monthly spending. Then, we multiply the outcome by 12 to convert to annual spending.
[28] In addition to OLS, we estimate count data models (Poisson and negative binominal models) to account for the discrete nature of outpatient visits (see e.g., Pohlmeier and Ulrich 1995) and find that the results are very similar (see Appendix Figure C-2).
[29] Since both outpatient spending and frequency of outpatient visits increase by similar magnitudes, subsidies do not affect the spending *per* visit (results are available upon request).

these municipalities, as municipal policies might not be enacted randomly. For example, if municipalities in better fiscal situations are more likely to implement the subsidy expansion, while income effects simply increase utilization, then our estimates may be upward biased.[30]

We address these endogeneity issues using four approaches.[31] First, we employ event study analysis to examine the potential concern that our control group—namely, children in municipalities without changes in subsidy—exhibits a different time trend than do children in municipalities with subsidy changes. As reported in Subsection 5.1, this does not seem to be a serious concern, since the estimates in the event study before T=0 are not significantly different from zero (except for anticipatory effects).

Second, we nonetheless estimate a model that includes the time-by-municipality fixed effects (where time is measured in months) to account for the time-varying municipality characteristics that are potentially correlated with both the expansion of the subsidy and utilization. This specification is especially stringent (even more stringent than municipality-specific linear trend), as these fixed effects capture any average effects of municipality-specific policy change (e.g., income transfer or other subsidies) or event (outbreak of influenza) in a particular month. We are reassured that the estimates are barely changed, as we report in Row (i) in Figure 6.

Third, another way to account for the potential endogeneity of subsidy expansion is to restrict the sample to only children who experienced at least one change in subsidy status, thereby dropping from the sample children who remain either subsidized or unsubsidized throughout the sample period. Importantly, this identification strategy exploits only the *timing* of the changes in subsidy status. In this way, we can to some extent mitigate the concern that individuals in the treatment and control groups are different. Row (ii) in Figure 6 shows that the estimates are somewhat noisier owing to the smaller sample but are qualitatively similar.[32]

Finally, although we have very few movers in our data (1.7%), we are still concerned that the estimated effects of the subsidy may be biased if sicker children move to a municipality that offers a

[30] For example, while 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 revenues, to boost low fertility rates, and to combat recent increases in child poverty (Bessho 2012), possibly amplified by spatial competition across municipalities (Nikkei 2017a). 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).

[31] We subject these results to a series of other robustness checks. For brevity, we leave a detailed description of these to Appendix C. The results in Figure 5 on the causal effects of patient cost-sharing are robust across all specifications considered.

[32] As a separate note, we collapse the data at municipality-age-time cells, which is the level of variation, to partially account for zero spending at the person-month level. Then, we run cell regression analogous to equation [2] in which the number of observations in each cell is used as a weight. Row (iii) in Figure 6 shows that the estimates from the cell-level analysis yields almost identical results to those from underlying individual micro data.

more generous subsidy. To alleviate this concern, in Appendix I, we estimate a conditional logit model that examines whether children (and their parents) migrate to a municipality that provides free care, finding little evidence that supports such a migration pattern. In addition, we later report that including movers in the sample hardly changes the results owing to the small amount of inter-municipality migration.[33]

6. Effect of a small positive price

6.1. "Zero-price" effect

Up to now, we limit the sample to 165 municipalities that only have either 0% or 30% of coinsurance rates during our sample period (*0–30%* sample), mainly for simplicity and ease of computation. While the majority of price changes are between 0% and 30%, as listed in Table 1, there are also cases in which children pay a small copayment, such as 2 USD or 5 USD per visit. This subsection exploits these variations to observe how a small positive price affects demand relative to free care. Here, we utilize all the observations (*full* sample) and all the price variations. Since the price responsiveness does not seem to vary by age, we estimate a single subsidy dummy for all ages in the rest of the paper, which increases statistical power for some price changes with relatively few treatments (see Appendix Table A-1).

Specifically, we modify the basic equation [2] to include all types of cost-sharing:

$$Y_{iamt} = \alpha + \sum_C \beta^C \{1(price = C) \times subsidized_{iamt}\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt}, \qquad [3]$$

where C takes all levels of coinsurance rates (C= 10, 15, 20, 30%) as well as copayments (C= 2, 3, 5 USD/visit). Note here that we choose free care (C= 0%) as the control group instead of full cost (C=30%) to examine the effect of introducing a small copayment to free care. While we exploit all the price variations in the estimation, we mainly focus on how the introduction of a small copayment (C= 2 USD/visit) affects utilization.

To obtain a better idea about 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.[34] The share of out-of-pocket costs for three small copayments are 2.4% (2 USD/visit), 3.9% (3 USD/visit), and 6.1% (5 USD/visit), which are substantially smaller than those corresponding to 30% coinsurance. This result suggests that these small copayments impose much lower cost-sharing. Our intention is not to directly compare the two forms of cost-sharing (i.e.,

[33] Almost identical results are obtained when we keep movers and assign the first municipality as an instrument (results are available upon request).
[34] 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.

copayment vs. coinsurance) but to compare different levels of cost-sharing *within* each regime.[35]

The upper graph in Figure 7 plots β^C from equation [3], where the outcome is a visit 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. These results indicate that while having positive cost-sharing or none at all has a clear impact on utilization, the level of cost-sharing has a relatively smaller effect.

We then convert these estimates into semi-arc elasticities as reported in the lower half of Figure 7. Interestingly, the semi-arc elasticity of the smaller copayment (ε^2) is substantially larger than for the larger copayment (ε^5). This result is easy to interpret, because while the changes in quantity are similar across different cost-sharing, as the upper graph shows, the changes in price (P^C) that drive the quantity changes are much smaller for 2 USD/visit (2.4%) than for 5 USD/visit (6.1%).[36] This leads to a larger elasticity for the 2 USD/visit. These results indicate that demand for healthcare is particularly price-sensitive around the price of zero. We obtain even more pronounced zero-price effects when we use frequency of outpatient visits as the dependent variable (see Appendix Figure E-1).

Our result is consistent with the "zero-price" effect discussed in the literature on behavioral economics, according to which people may be particularly sensitive to the price of zero (e.g., Shampanier *et al.* 2007; Douven *et al.* 2017). The underlying idea is that people strongly perceive the benefits associated with free products because of the utility loss related to giving up the free product (Hossain and Saini 2015). To the best of our knowledge, these and related studies are based on experiments while ours is the first that uses field data. On the one hand, our results imply that, relative to free care, a small positive price would substantially reduce healthcare utilization, and therefore, may be an effective tool to reduce the moral hazard problem. On the other hand, financial risks do not increase much when a small copayment is charged, which is an advantageous feature from the perspective of optimal health insurance.

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[35] Coinsurance and copayment may affect utilization differently. For example, from the patient perspective, the marginal out-of-pocket cost for copayments is essentially zero after paying the copayment while 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).

[36] Since 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 elasticity, that is, $\varepsilon^C = \left(\frac{2\beta^C}{Q^0+Q^C}\right)/P^C$.

6.2. What drives the "zero-price" effect?

What drives the larger price elasticity around the zero price? This subsection further explores this question by examining the type of patient (healthy vs. sick) and the margin (extensive vs. intensive) that may be affected by a small copayment. Among the small copayments we observe, we focus on the 2 USD/visit, which is the one most frequently observed in our data. Moreover, as the previous subsection shows, the 2 USD/visit copayment leads to the largest price elasticity.

We first divide children into three health statuses (sick, middle, and healthy) based on outpatient spending in the first 6 months.[37,38] Previous studies have also used prior spending as an indicator of health status (e.g., Dranove *et al.* 2003). In the analysis in this subsection, 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. We expect small copayments to reduce overall outpatient visits, because the visits with values lower than the small copayment are discouraged. Moreover, as Manning *et al.* (1987) conjecture, medical treatments are less discretionary for sicker patients, and thus, sicker patients may be less price responsive than healthier patients are.[39] If true, a small copayment would have relatively little effect on sicker patients. However, it would be a concern if a small copayment affects the extensive margin of sicker patients and sicker children stop visiting physicians.

To this end, we create dummy variables that correspond to at least one, two, and three outpatient visits per month and estimate how a small copayment differentially affects these margins. 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:

$$\Pr(Y_{iamt} \geq k) = \alpha + \sum_C \beta^C \{1(price = C) \times subsidized_{iamt}\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt}, \quad [4]$$

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

[37] 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).

[38] 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. For 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.

[39] 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).

extensive margin of utilization.

Table 3 reports β^C (C= 2 USD/visit) from equation [4]. 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 if we calculate the impact of copayment relative to mean (as reported under "% change from mean"), the copayment has a relatively larger impact 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 impact we find is on healthier children who visit physicians more than three times per month, which represents a 29.7% reduction from the mean. The result that sicker children do not completely cut back on medical care suggests that the impact of a small copayment on health outcomes might not be very large. We examine the impact on health outcomes more directly in Section 8.

7. Treatment heterogeneity by service types

In this section, we examine whether the impact of free care is heterogeneous by service types. As discussed before, we again focus on the 0–30% sample, which provides us with enough statistical power to explore heterogeneous effects.

Previous studies, including the RAND HIE, find that reduced cost-sharing affects utilization across the board, including both essential and non-essential care (Newhouse and the Insurance Experiment Group 1993). This is an important finding, because it suggests that policy instruments other than cost-sharing can play a role in further improving welfare. The aim of this section is to investigate whether the same account holds for child healthcare. To preview our results, we reach the same conclusion as previous studies, that cost-sharing is a "blunt tool" affecting utilization across the board without discriminating by type of service. We believe this is an important finding given that little such evidence has been reported for child healthcare and for healthcare systems outside of the US.

7.1. Appropriate versus inappropriate use of antibiotics

We first consider the effect of reduced cost-sharing on the use of antibiotics. It is known that antibiotics are often used on diagnoses for which antibiotics are not recommended, and such inappropriate use leads to both antibiotic resistance and adverse effects (Fleming-Dutra *et al.* 2016). 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). Of course,

however, lower cost-sharing may also increase appropriate use of antibiotics, which may lead to better health. To the best of our knowledge, little is known about whether financial incentives, such as subsidies for child healthcare, increase the appropriate or the inappropriate use of antibiotics for children.[40]

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 at all, respectively. For example, antibiotic use for children with bronchitis and asthma is considered inappropriate and is included in Tier 3. See Appendix F for details, including the list of diagnoses in each tier along with the corresponding ICD10 as well as summary statistics of antibiotic usage.[41] Appendix Table F-2 shows that, even without subsidy, roughly 20% of children diagnosed for Tier 3 are prescribed antibiotics (column 5), pointing out the potential misuse of antibiotics for children in Japan. Similarly, the average spending on antibiotics conditional on being diagnosed for Tier 3 is 2.44 USD (column 6), and the frequency of antibiotic prescriptions is 0.94 per person-month (column 7) without subsidy. Both numbers are far from zero.

For each tier, we estimate a simplified version of equation [2] with a single subsidy dummy for all ages (hereafter, referred to as equation [2']) in which the outcome is the term representing the interaction between being diagnosed with any of these diseases and total spending on antibiotics.[42] Table 4 reports the results and shows that subsidies increase spending on antibiotics in all three tiers by 21–35% from the mean. This result indicates that a generous subsidy increases both the appropriate and inappropriate use of antibiotics, which is consistent with the finding of the RAND HIE, that reduced cost-sharing increases both high- and low-value care. The fact that reduced cost-sharing substantially increases inappropriate use of antibiotics is worrisome, as it may lead to more antibiotic-resistant infections and adverse effects.

7.2. Regular versus After-Hours Visits

We next examine how a generous subsidy affects outpatient visits during regular hours and

[40] For example, Foxman *et al.* (1987) examine the impact of patient cost-sharing on inappropriate antibiotic use in the RAND HIE but do not conduct a separate analysis for children. See also Currie *et al.* (2011, 2014), who examine the relationship between the inappropriate use of antibiotics and supply-side financial incentives in China.
[41] When a patient has multiple diagnoses in a month, priority is given to Tier 1 diagnoses, followed by Tier 2 diagnoses, and then 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 the patient 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, since none of the diagnoses include those from Tiers 1 and 2.
[42] The results for frequency of antibiotic prescriptions are similar and available upon request.

after hours. One of the surprising findings from the Medicaid expansion in Oregon was that health insurance coverage increased the use of emergency department even for conditions that are treatable in primary care settings (Taubman *et al.* 2014). Whether the patient visited the emergency department or otherwise cannot be identified in the Japanese data, unlike the US data. However, we can distinguish whether the outpatient visit was made during regular hours or after hours, and we study whether a subsidy affects the visit type. If a generous subsidy increases more expensive after-hours visits, it not only results in higher medical spending but also may place a substantial burden on the workload of physicians.[43]

We divide the visits into three categories: 1) visits during regular hours, 2) after-hours visits, and 3) midnight/holiday visits. Under the national fee schedule, additional fees for after-hours visits and midnight/holiday visits are charged on top of the consultation fees for regular visits, and thus, from the billing information, we know the timing of the outpatient visit within a day.[44] As a benchmark, fees for regular visits during the sample period are approximately 28 and 7 USD for the first visit and follow-up visits, respectively. The additional fees charged for after-hours visits—which are typically 8.5 and 6.5 USD for the first visit and follow-up visits, respectively—make these visits costly. The additional fees for midnight/holiday visits are even higher than those for after-hours visits (see the last rows of Table 5). Again, after-hours visits and midnight/holiday visits do not always indicate emergency visits. Rather, their fees are set high to reflect the physician workload.

Table 5 reports the estimates from equation [2'] for regular visits (for reference), after-hours visits, and midnight/holiday visits when we use spending as the dependent variable.[45,46] Since the majority of the visits are regular visits (89.1% of total visits), column (1) shows a similar pattern to our baseline estimates reported in Table 5. Interestingly, column (2) shows that costly after-hours visits—which account for 8.4% of total visits—also increase owing to the subsidy. In fact, in relative terms, the impact of the subsidies is larger for after-hours visits than for regular visits (as reported under the heading "% change from mean"). These results indicate that reduced cost-sharing increases outpatient visits across the board regardless of the timing of the visit. These results are consistent with the Oregon experience discussed earlier in this subsection. Interestingly, we find no evidence that subsidies increase midnight/holiday visits when healthcare resources (e.g., physicians and nurses) are most scarce in column (3).

[43] Municipalities have indeed been concerned that subsidies for child healthcare may unnecessarily increase after-hours visits. See, for example, an article from the leading newspaper in Japan (Nikkei 2017b).
[44] For example, suppose the regular hours of a clinic are registered from 9 am to 5 pm. As midnight visits are normally defined by visits between 10 pm and 6 am, visits outside of regular hours but not during the midnight period are considered as after-hours visits. Holiday visits are visits on a holiday.
[45] The results for the frequency of outpatient visits are similar and are available upon request.
[46] Note that spending here includes only consultation fees and does not include any fees related to treatments during the visits.

In summary, the subsidy for child healthcare seems to increase not only regular visits but also costly after-hours visits, which may increase both the cost and workload of physicians.[47] From a policy standpoint, a *municipal* government subsidy partially undoes the effort of the *national* government to discourage costly after-hours visits for non-serious reasons by setting higher fees for these visits.

7.3. By service category

We next group medical services into six broad categories (medication, consultation fees, laboratory tests, non-surgical procedure, surgical procedure, and others) and examine whether the impact of reduced cost-sharing is heterogeneous by service type.[48] Table 6 reports the results on the four largest categories in terms of spending, that is, medication (54.1%), consultation fees (18.4%), laboratory tests (17.2%), and non-surgical procedures (5.3%), which sum up to 95.0% of the total spending. Medication includes fees for prescribing and dispensing medications, including fees at the pharmacy, and accounts for more than half of the share of total spending.

Table 6 reports the estimates from equation [2'], using spending as the dependent variable. We find that the point estimates are statistically significant in all four categories, and the subsidy increases utilization by 24–43% relative to the mean (as reported under "% change from mean"). These results indicate that reduced cost-sharing increases utilization across the board regardless of the type of service. Among the four categories, the medical services related to treatment intensity, such as laboratory tests and non-surgical procedures, appear to be relatively more price sensitive. Laboratory tests include imaging, which is often identified as having unproven medical value (e.g., Lee and Levy 2012). When imaging is separately examined, we also find a statistically significant increase (results are available upon request). Interestingly, medication is not as price responsive as other service categories are. This may be because fees for prescribing and dispensing medications are included in this category, which are fixed regardless of the amount of medication used.

8. Effect on health outcomes and future healthcare spending

The remaining important question is whether the increased outpatient utilization owing to lower

[47] It is possible that the additional cost of after-hours visits may be partially offset by the opportunity cost of working parents who may need to leave work to take children to outpatient care during regular hours. Ultimately, the availability of free after-hours visits may affect the labor supply of parents. Unfortunately, since our claims data do not include any parental information, we cannot investigate such possibilities.

[48] We also estimate equation [2'] by the broad diagnosis categories as indicated in Panel A of Appendix Table A-3. We also find an increase in outpatient visits for all categories (results are available upon request).

price (moral hazard) results in better health and reduces future healthcare spending.[49] If true, higher utilization due to subsidies may indeed be welfare improving. To investigate this issue, we first examine whether increases in outpatient care prevent avoidable inpatient admissions. We then study whether childhood subsidies reduce healthcare utilization and improve health outcomes when children become adolescents. We use the 0–30% sample to obtain the most statistical power. We expect the benefits of free care to be the largest when compared with a 30% cost-sharing. If we do not find any benefits here by this most drastic price reduction, it is less likely that lowering from the national level (30%) to other coinsurance rates or copayments will find any benefits.

8.1. Avoidable inpatient admissions

We start by examining whether increased outpatient care prevents avoidable inpatient admissions. If free care increases preventive outpatient care, and, consequently, hospitalization is avoided later, then the higher cost in outpatient care may well be justified. However, it is also possible that free care increases inpatient care use if, for example, an outpatient visit leads to a referral to a specialist for additional examination and invasive treatment for a condition that would otherwise have resolved itself in time (self-limiting) or increase the chance of detecting other health problems.[50]

Instead of looking at broad disease categories or choosing them arbitrarily, a useful set of preventive care is the utilization for the so-called ACSCs or "avoidable conditions"—diagnoses for which timely and effective outpatient care can help reduce the risks of hospitalization by preventing the onset of an illness or condition (e.g., asthma). We employ the ACSC list from Gadomski *et al.* (1998), which specifically focuses on children.[51] Appendix Table G-1 provides the lists of the ACSCs with corresponding ICD10 codes, and the fraction of each ACSC in our sample. Column (2) indicates that, conditional on visit, as much as 41% of the outpatient visits belong to the ACSC list, which verifies the acute nature of children's diseases. Among the list of 17 ACSCs, severe ear, nose, and throat (ENT) infections (56.9%), and asthma (31.5%) account for nearly 90% of total ACSCs.

Panel A in Table 7 reports the estimates from equation [2'] when the outcome is an outpatient

[49] In fact, the recent work by Baicker *et al.* (2015) suggests that welfare implications of quantity changes depend on how they occur.

[50] Whether outpatient care is a substitute for or complement to inpatient care is an important but unsettled question in health economics. Overall, the RAND HIE finds no evidence of "offset" effects (Newhouse and the Insurance Experiment Group 1993). Some studies report that outpatient and inpatient care are complements (e.g., Kaestner and Lo Sasso 2015) while a few studies that document evidence of offset effects are concentrated among the elderly (e.g., Chandra *et al.* 2010; Trivedi *et al.* 2010). To the best of our knowledge, no study examines the cross-price effects on child healthcare except for the RAND HIE, which lacks the statistical power for making decisive conclusions because of its very small sample size in terms of children (1,136 children whose families participated in a randomized trial).

[51] For example, Kaestner *et al.* (2001) and Dafny and Gruber (2005) examine the ACSCs for children.

dummy for (i) any ACSC, (ii) severe ENT infections, and (iii) asthma.[52] Panel A shows that outpatient visits for these diagnoses increase with free care, and all the estimates are statistically significant at the conventional levels. For example, the outpatient visit for any ACSC increases by 2.7 percentage points, where the mean of the variable is 0.11 without subsidy.

At first glance, these results seem to indicate that free care increases outpatient visits that can potentially reduce hospitalization. However, most past studies could not examine whether such seemingly beneficial care indeed prevents avoidable hospital admissions. Here, one big advantage of our insurance claims data is that they include information for both outpatient and inpatient care from the same individual over time, unlike most existing datasets that capture either outpatient or inpatient care alone. Thus, we can directly examine whether such increases in preventive care in the outpatient setting indeed results in lower hospitalization rates.

Panel B in Table 7 reports the estimates from equation [2'], where the outcome is an *inpatient* dummy while the explanatory variables are the subsidy status for *outpatient* care as before.[53] We do not observe any declines in the hospitalization rates associated with any ACSC or other individual ACSCs. Since hospitalization among children is very infrequent (0.28% of all person-months), the estimate on any ACSCs is imprecise. However, the estimates on severe ENT infections and asthma are *positive* and statistically significant at the conventional levels, implying that outpatient care may be a complement to rather than substitute for inpatient care, at least in this setting. Since the benefits of preventive care may emerge with a time lag, we also estimate a variant of equation [2], in which the explanatory variables are lagged outpatient subsidy dummies in a simple dynamic model. We find little evidence of any lagged effects (results are available upon request). In summary, we do not find evidence that increase in outpatient care for ACSCs translates into the lower hospital admissions with these conditions.[54]

8.2. Medium-term utilization and health outcomes

Next, we explore the medium-run effects of free care on healthcare utilization and health outcomes. It is possible that free care during childhood increases beneficial care and makes children healthy. This may reduce healthcare utilization and improve health outcomes when children become

[52] Unfortunately, insurance claims data in Japan list all the ICD10 diagnosed in a month instead of diagnosis for each visit, making it difficult to examine spending and frequency of visits by ICD10.
[53] Whenever we examine inpatient outcomes, we also control for the subsidy for inpatient care. Adding these variables does not affect our results, as there is little variation in inpatient subsidies.
[54] We also conduct the "offset" analysis for all inpatient and outpatient spending without focusing on conditions specific to ACSCs, finding no such effects. Furthermore, we find no reduction in short-term mortality (both results are available upon request).

adolescents.[55] Here, we exploit the fact that (1) in almost all the municipalities, subsidy expires at the age of 15 years (end of junior high school); (2) by contrast, the duration of free care that children receive before the age of 15 years substantially differs by municipality. Thus, we can relate the length of free care until the age of 15 years with healthcare utilization and health outcomes *after* the subsidy expiration at age 15 years. In this way, we can cleanly identify the medium-run effects of free care. This approach is in the same spirit as recent studies that relate Medicaid eligibility during childhood to later-life utilization and health outcomes (e.g., Brown *et al.* 2016; Wherry *et al.* 2018). Our study has an advantage in that we can measure the length of free care that each individual was exposed to more accurately thanks to our individual panel data.

As for the outcomes, we examine healthcare utilization and health outcomes in the first year (age 16 years) and the third year (age 18 years) after subsidy expiration at age 15 years. After turning 18 years (i.e., graduation from high school), a substantial fraction of children (28.5%) is lost in the sample, because they move away from the municipalities to enter college/university or enter the labor force.

We calculate the length of free care until the age of 15 years in the following way. As our data span precisely 10 school years, we choose the cohorts for which we observe the longest history of subsidy information (9 years) as well as 1 full year of utilization at the age of either 16 or 18 years. This minimizes the measurement errors in the length of free care. For example, the sample for age 16 years is constructed as follows. For the cohorts who were aged 6 years in April 2005—the start of our sample period—we observe 1 year of utilization at age 16 years as well as the subsidy information at ages 6–15 years. Combining the information on the maximum age of subsidy eligibility in April 2005, we construct the history of subsidy exposure from ages 0–15 years for each individual, assuming that there is no inter-municipality migration before age 6 years. We have a total of 3,643 individuals. The average length of free care between ages 0–15 years is 10.91 years ranging from 4 years to the full 15 years.[56,57]

With this setup, Panel A of Figure 8 plots the relationship between the length of free care and average monthly spending—which is the sum of outpatient and inpatient spending—during 1 year at the ages of 16 and 18 years. Each dot presents the means of the outcome by each birth year cohort×municipality. The dotted line is the predicted values of weighted least square regressions,

[55] We note that longer subsidy exposure may also increase utilization later in life if children form a habit of seeing a doctor during childhood and thus, this can have a positive net effect on utilization.
[56] The sample for age 18 years is constructed in the similar way. We have a total of 3,426 individuals. The average length of free care between the ages of 0–15 years is 8.64 years ranging from 4 to 15 years.
[57] We exclude 35(37) individuals who live in municipalities that provide subsidies after age 15 years for the samples of age 16(18) years. The average length of free care is longer for the age 16-year sample than for the age 18-year sample because the former is younger and free care has expanded over time, as shown in Figure 1.

where weight is the number of observations in each dot.

We find little evidence that more years of subsidy exposure during childhood decrease utilization after subsidy expiration. The slope for the sample aged 16 years is –0.66 (p-value= 0.689), which is far from statistically significant, and economically very small, indicating that a 1-year increase in subsidy exposure during childhood decreases the total monthly spending at age 16 years by as small as 0.66 USD.[58,59] We also examine outpatient and inpatient spending separately, but the results are qualitatively very similar (not shown).[60] The slope for the sample aged 18 years is even slightly positive, 4.06 (p-value= 0.450), and again far from statistically significant.

We next investigate whether longer exposure to free care during childhood improves health outcomes when individuals become adolescents. For this, we examine the occurrence of serious chronic health problems at the ages of 16 and 18 years. Feudtner *et al.* (2000, 2014) develop pediatric complex chronic conditions (CCCs), which are defined as "any medical condition that can be reasonably expected to last at least 12 months (unless death intervenes) and to involve either several different organ systems or 1 organ system severely enough to require specialty pediatric care and probably some period of hospitalization in a tertiary care center" (page 206, Feudtner *et al.* 2000)[61]. Appendix Table H-1 provides the list of pediatric CCCs and the descriptive statistics; 8.7% of individuals (N= 7,069) are diagnosed with one of the CCCs in 12 months for those aged either 16 or 18 years.

Panel B of Figure 8 plots the relationship between the length of free care and an outcome that takes the value one if any visits/admissions are diagnosed with any CCCs at the ages of 16 or 18 years. Again, we observe no discernable pattern. The slopes for the samples aged 16 and 18 years are economically very small (0.0001 and 0.0010, respectively) and far from statistically significant (p-value= 0.953 and 0.651).[62]

Our results are contrary to recent studies on Medicaid that find a positive effect of Medicaid

[58] We formally run the specification in which we also include birth year-month fixed effects (12 cohort fixed effects) and a dummy for gender. The results are essentially the same (and available upon request).
[59] The 95% confidence intervals of our estimates rule out that an additional year of free care decreases future spending at age 16 years by more than 3.93USD per month.
[60] We also investigate whether free care at different ages has differential impacts on later-life utilization. Specifically, we break the total length of free care by age, but none of the estimates is statistically and economically significant (results are available upon request).
[61] These measures are widely used and "aimed to identify infants, children, and adolescents diagnosed with complex chronic conditions, with an emphasis on examining patterns of mortality and of end-of-life healthcare utilization associated with CCCs" (Feudtner *et al.* 2014). In fact, these pediatric CCCs are derived using the sample of children aged 0 to 18 years old.
[62] For example, we can rule out that an additional year of free care decreases CCC at age 16 years more than 0.45 percentage points (95% confidence intervals).

eligibility on long-term health outcomes.[63] However, these studies are likely to find larger impacts, as the policy change is more drastic: these studies focus on the provision of health insurance (extensive margin) and the targeted population is more disadvantaged. In our setting, in which universal coverage guarantees the minimum access to healthcare, the additional subsidy that reduces the coinsurance rate from 30% to 0% (intensive margin) does not seem to have any short-term and medium-term health benefits, at least among the health outcomes observed in our data. Nonetheless, we need to view these results with caution, as adolescents are generally a very healthy population, and we do not observe any long-term outcomes.

9. Conclusions

Understanding the price responsiveness to healthcare is a central question in health economics and also a fundamental issue for the optimal design of health insurance. However, past studies on price elasticity are predominantly concentrated on adults and the elderly, and surprisingly little is known about children. In this study, we examine the effect of patient cost-sharing on healthcare utilization among children by exploiting many variations on the levels and forms of patient cost-sharing.

We find that free care, which reduces coinsurance from 30% (national level) to 0%, increases outpatient spending by 26% with semi-arc elasticities at approximately –0.6 throughout ages 7–14 years. The municipal subsidy substantially exacerbates moral hazard by removing cost sharing implemented by central government. The elasticity estimates are considerably smaller than those of the RAND HIE for non-elderly in the US with a similar price change, while considerable caution is needed when comparing the elasticities estimated across countries or time periods. The subsidy also creates a negative externality, because 70% of increased spending is born by health insurers, like the negative effect of supplemental private health insurance on Medicare (OECD 2004; Chandra *et al.* 2010).

Further exploiting our data on copayment changes, we find substantially large demand responses when a small positive price, such as 2 USD per visit, is introduced to free care ("zero-price" effects). Interestingly, the small price has the largest impact on healthy children who frequently visits physicians, while it has little impact on sicker children's extensive margin. These results suggest that a small copayment can reduce moral hazard with little adverse effect on both financial and health risks, and policymakers may consider implementing such a small payment instead of providing care for free.

We further examine the utilization patterns from various dimensions and find that reduced

[63] Examples include Medicaid introduction (e.g., Boudreaux *et al.* 2016; Goodman-Bacon 2016) and Medicaid expansion (e.g., Currie *et al.* 2008; Sommers *et al.* 2012; Brown *et al.* 2016; Thompson 2017; Miller and Wherry 2018; Wherry *et al.* 2018).

cost-sharing affects utilization across the board regardless of the type of healthcare service. This result is consistent with the previous literature, such as the RAND HIE and the Oregon experiment, that cost-sharing is a "blunt tool" that affects utilization in a non-discriminatory fashion. This implies that policy instruments other than patient cost-sharing may play a role in further improving welfare.

Finally, we show that while free care increases outpatient utilization, it does not translate into clear benefits in the form of reduction in hospitalization by "avoidable" conditions, improvement in short-run and medium-run health outcomes, and reduction in future healthcare spending. Our results contrast with studies on Medicaid in the US, which document a positive effect of Medicaid eligibility on both short- and long-term utilization and health outcomes. In our setting, in which universal coverage guarantees minimum access to healthcare, the additional subsidy that completely eliminates cost-sharing does not seem to have a large positive impact, and policymakers. therefore, should be cautious about implementing such a policy.

This study is subject to a few limitations. One is that we can investigate medium-term health outcomes but not long-term ones, because many children move away from municipalities when they enter college/university or start working. Another limitation is that our insurance claims data do not include basic parental characteristics, such as income and education, and consequently we cannot examine heterogeneous effects by these dimensions. To the best of our knowledge, such monthly data with age, municipality of residence, healthcare utilization, and household characteristics, do not currently exist in Japan, which leaves the construction and inference with such an expanded dataset as an avenue for future research.

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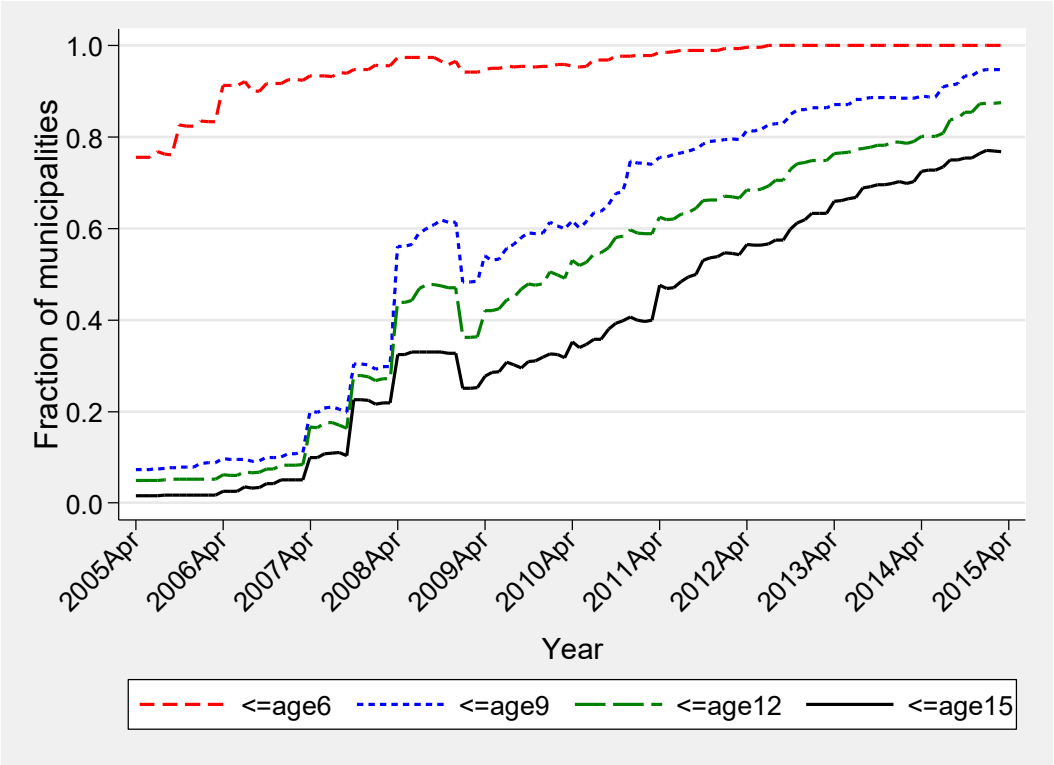
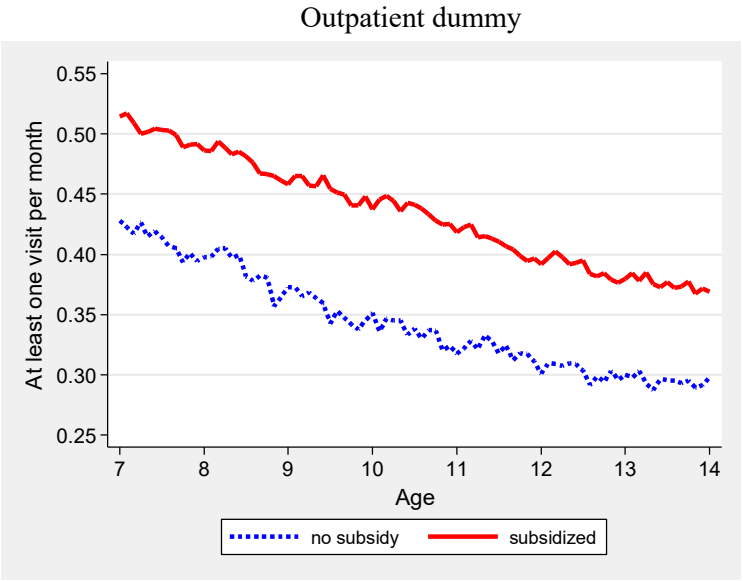
Figure 1: Time series of maximum age covered by healthcare subsidy

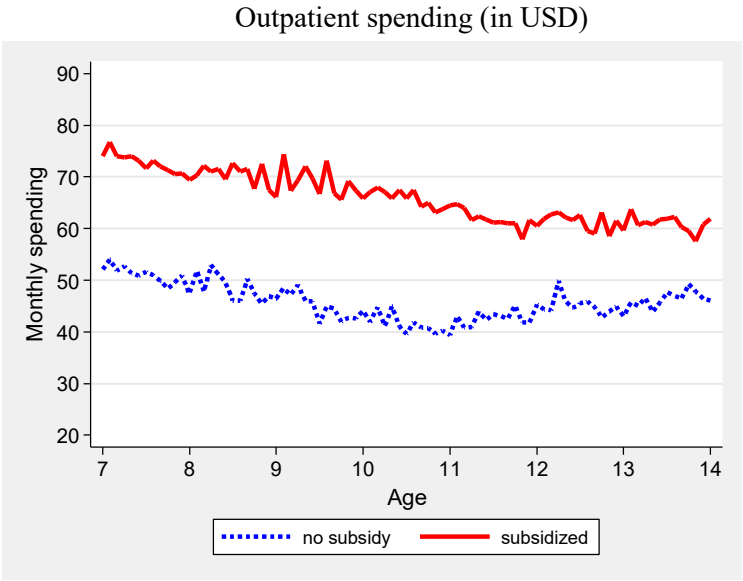


Notes: The figure plots the share of municipalities in our insurance claims data by the maximum age for the subsidy eligibility for outpatient care at the monthly level during April 2005–March 2015 (see Appendix Figure A-1 on the precise timing of all policy changes). There are total of 323 municipalities.

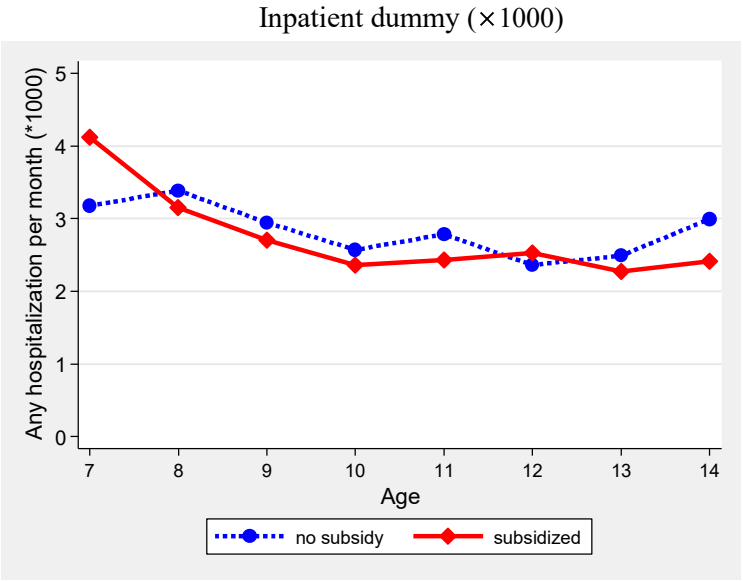
Figure 2: Utilization with or without subsidy by age

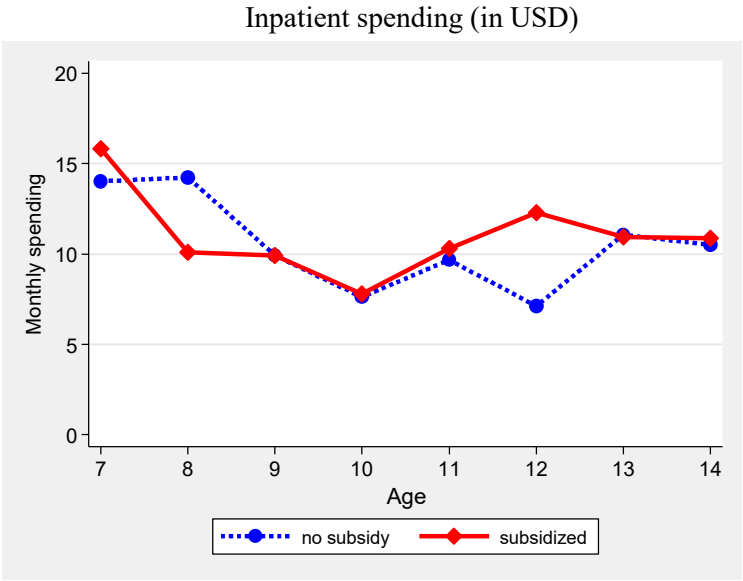
A. Outpatient care (Age in months)





B. Inpatient care (Age in years)

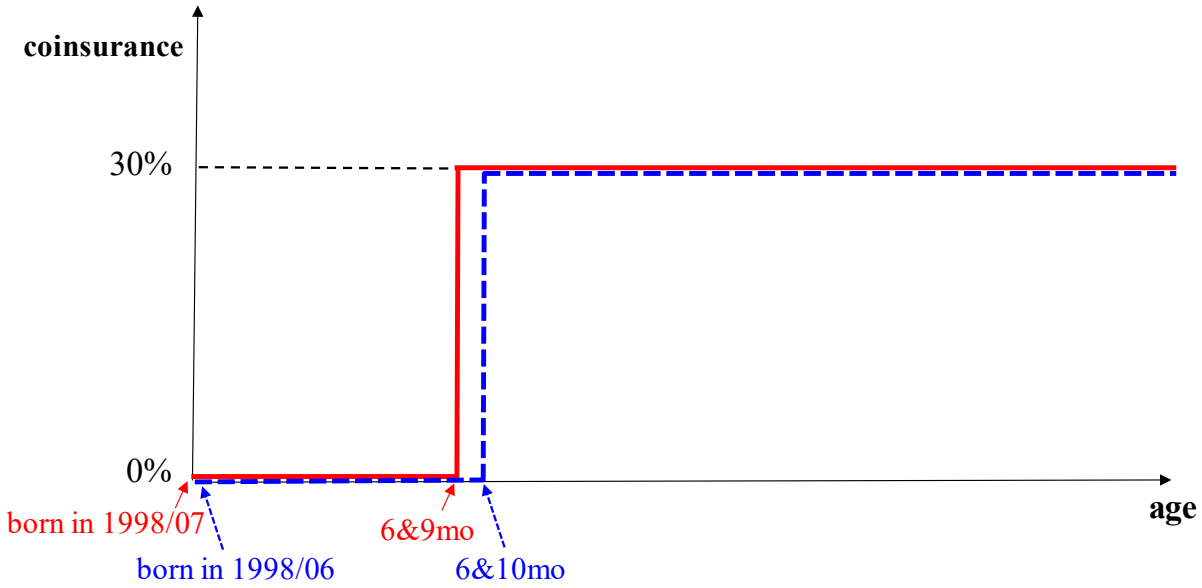




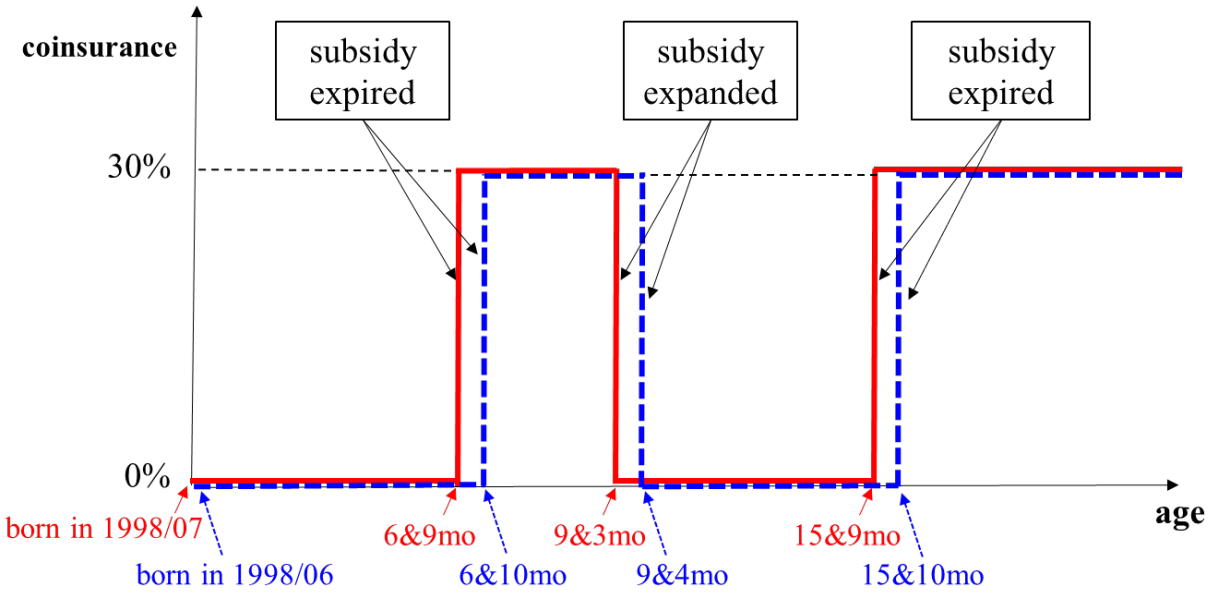
Notes: The 0-30% sample is used. See the main text for the sample construction. Panel A plots the monthly mean of outpatient outcomes, and Panel B plots the monthly mean of inpatient outcomes aggregated at age in years as inpatient admission is a rare event. An outpatient dummy takes one if there is at least one outpatient visit per month, and an inpatient dummy takes one if there is at least one hospitalization per month (×1000). Outpatient spending is the monthly spending on outpatient care and inpatient spending is monthly spending on inpatient care, both of which are measured in USD (100JPY/USD). The dotted lines are age profiles of utilization without subsidy (30% coinsurance rate, labeled "no subsidy"), and the solid lines are age profiles of utilization with subsidy (0% coinsurance rate, labeled "subsidized").

Figure 3: An example of *asymmetry* in price change

A. *Before* the subsidy expansion in 2007/10



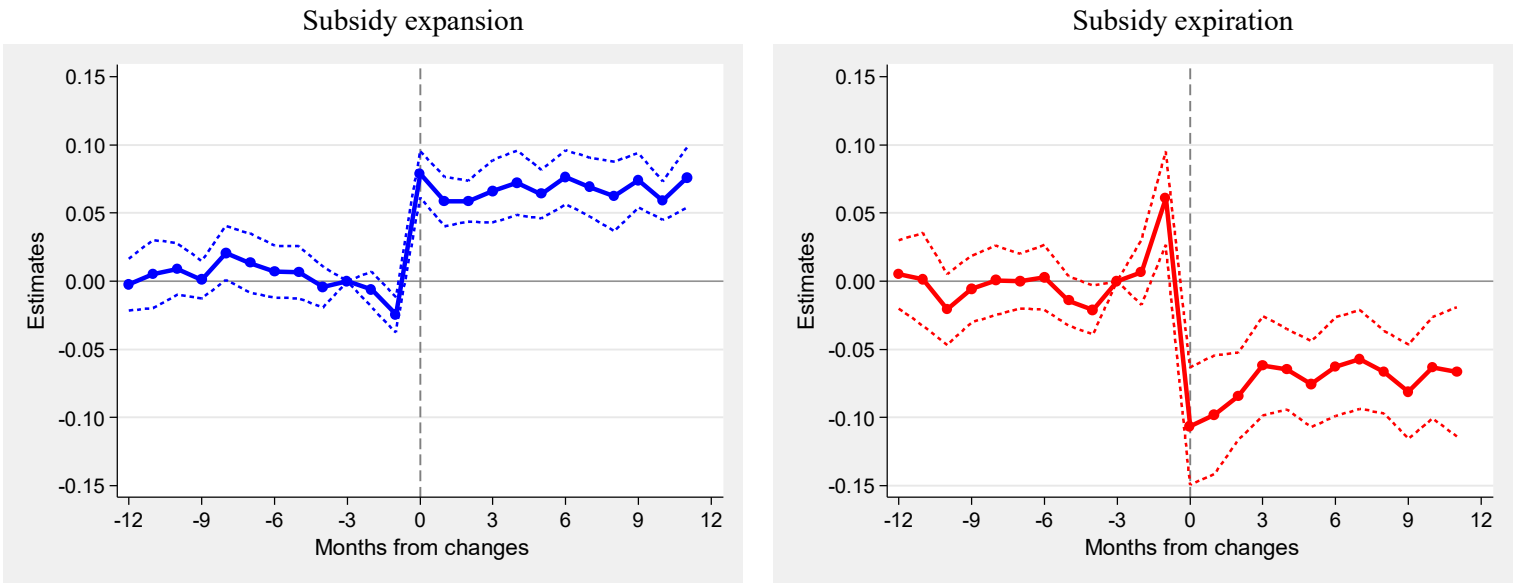
B. *After* the subsidy expansion in 2007/10



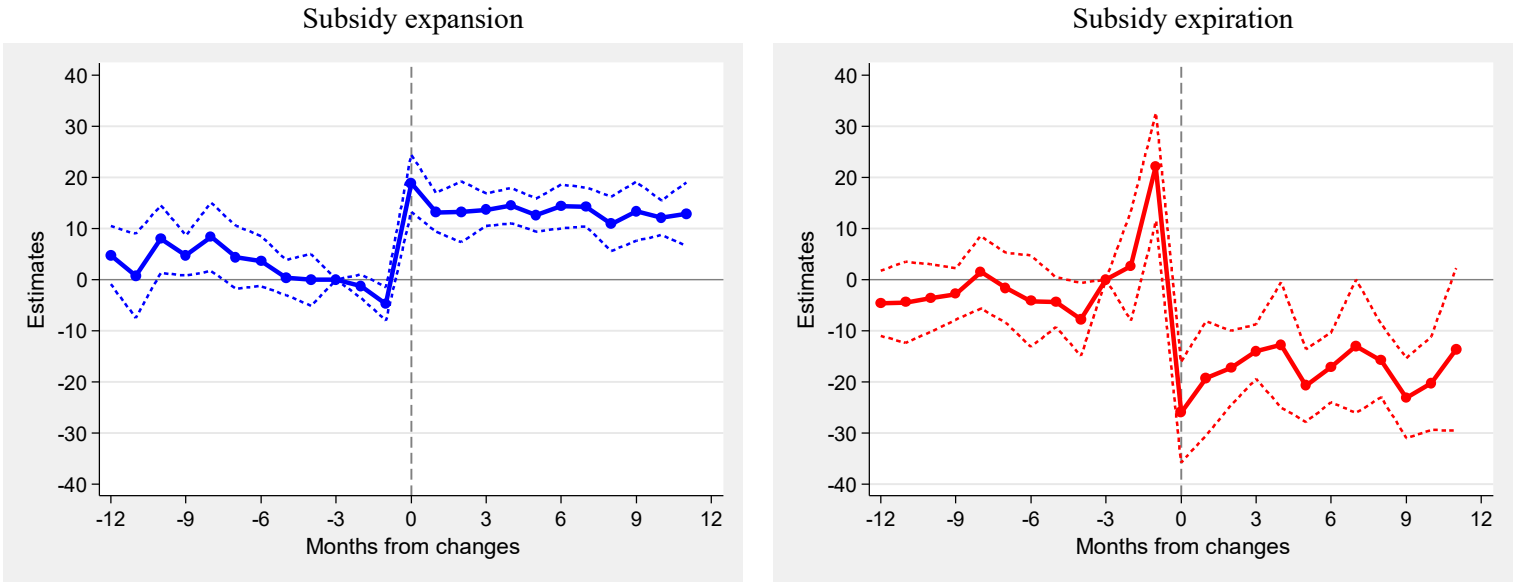
Notes: Panel A draws the price schedules for each cohort *before* subsidy expansion. The solid line draws the price schedule for a cohort born in July 1998 ("younger" cohort, hereafter), and the dotted line for a cohort born in June 1998 ("older" cohort, hereafter), born a month before the younger cohort. Suppose that the municipality provides a full subsidy (i.e., 0% coinsurance rate) until the beginning of primary school (6 years). Since the school year starts in April in Japan, the younger cohort is 6 years and 9 months old, while the older cohort is 6 years and 10 months old, when both cohorts enter primary school in April 2005. Above this age, children pay the national level of a 30% coinsurance rate. Suppose that in October 2007 the municipality expands the subsidy up to the end of junior high school (age 15). Panel B draws the price schedules *after* subsidy expansion. The younger cohort (solid line) pays the full 30% from the age of 6 years and 9 months to the age of 9 years and 2 months, a month before the subsidy expansion in October 2007. Because of subsidy expansion, the cohort enjoys free care from the age of 9 years and 3 months until the age of 15 years and 8 months when the cohort graduates from junior high school in March 2014. Then, once again, the cohort pays the full 30% after the age of 15 years and 9 months. On the other hand, the price schedule for the older cohort (dotted line) is shifted by 1 month to the right, as the cohort is 1 month older than the younger cohort at the entry of primary school, the subsidy expansion, and graduation from junior high school.

Figure 4: Event study

A. Outpatient dummy

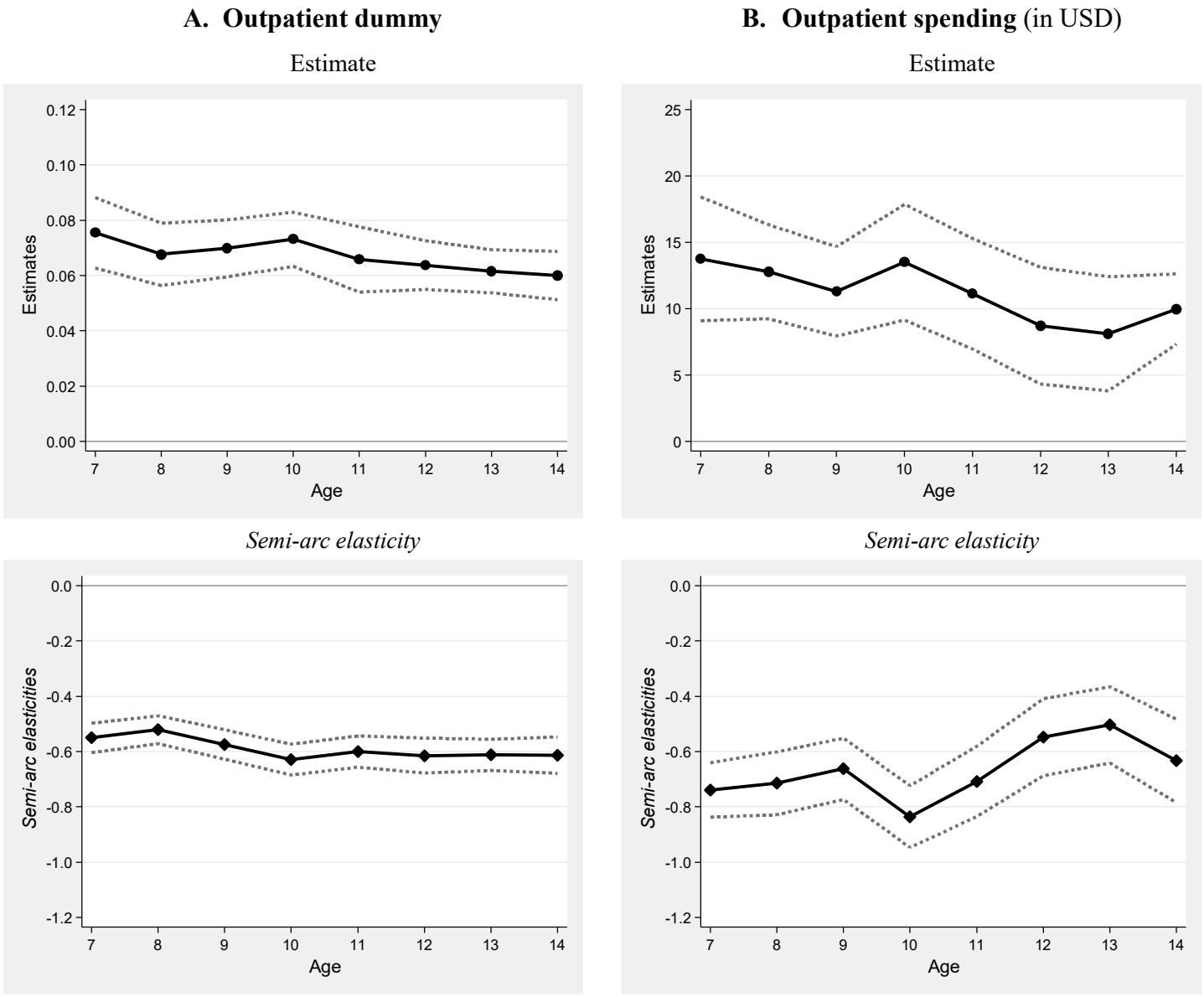


B. Outpatient spending (in USD)



Notes: The 0-30% sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in USD (100JPY/USD). "Better" indicates the subsidy expansion which lowers the price of healthcare from 30% to 0%, and "worse" indicates subsidy expiration that raises the price from 0% to 30%. The solid lines plot the estimates from a variant of estimation equation [2] 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 municipality level are used to construct them. The reference month is 3 months before the change (T= –3). The scales of y-axis are set the same within the panels so that two figures for opposite directions of price changes are visually comparable.

Figure 5: Basic results

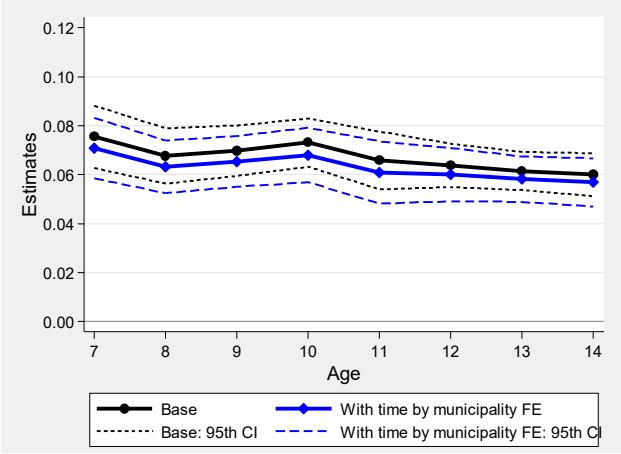


Notes: The 0-30% sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in USD (100JPY/USD). The upper half plots β_A for each age (A=7–14) from estimating equation [2], and the bottom half plots the corresponding semi-arc elasticity. The dotted lines are the 95th confidence intervals. The standard errors clustered at 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 corresponding table is found in Online Appendix Table B-1.

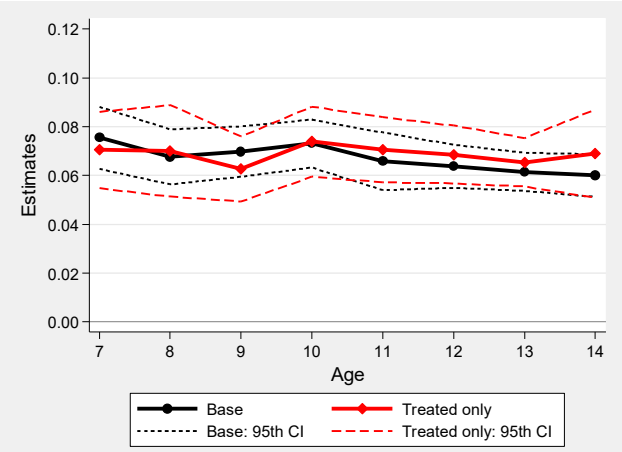
Figure 6: Robustness checks (Estimates only)

A. Outpatient dummy

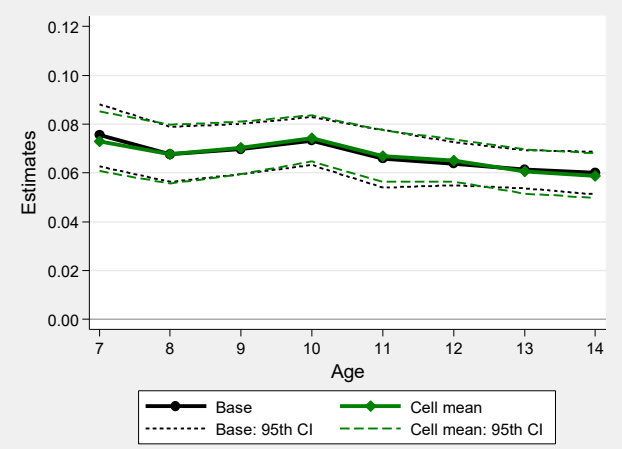
(i) Base vs. With time by municipality FE



(ii) Base vs. Treated only

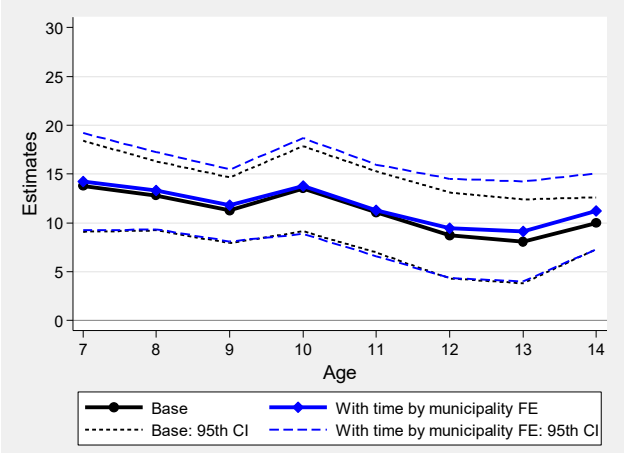


(iii) Base vs. cell means

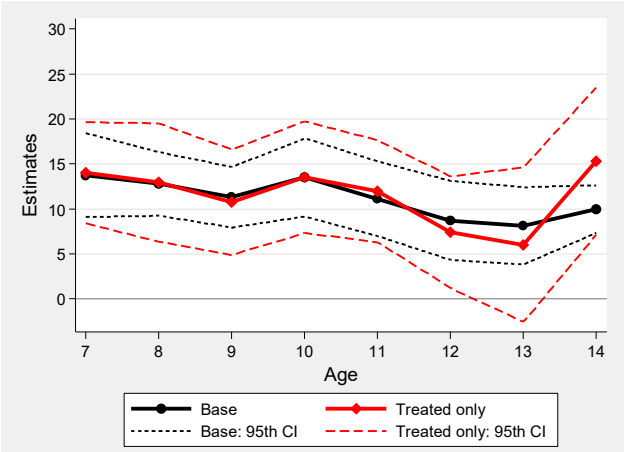


B. Outpatient spending (in USD)

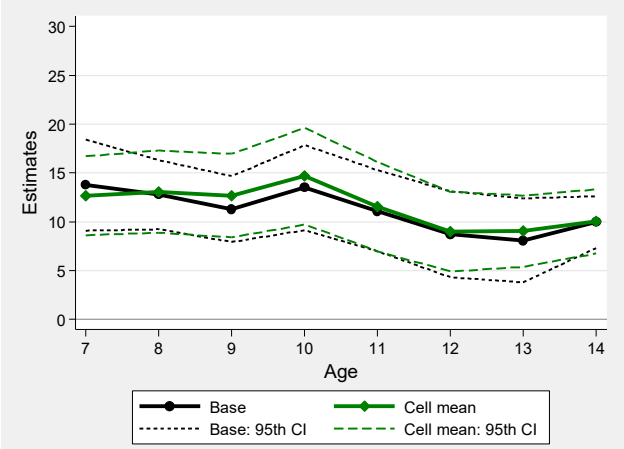
(i) Base vs. With time by municipality FE



(ii) Base vs. Treated only



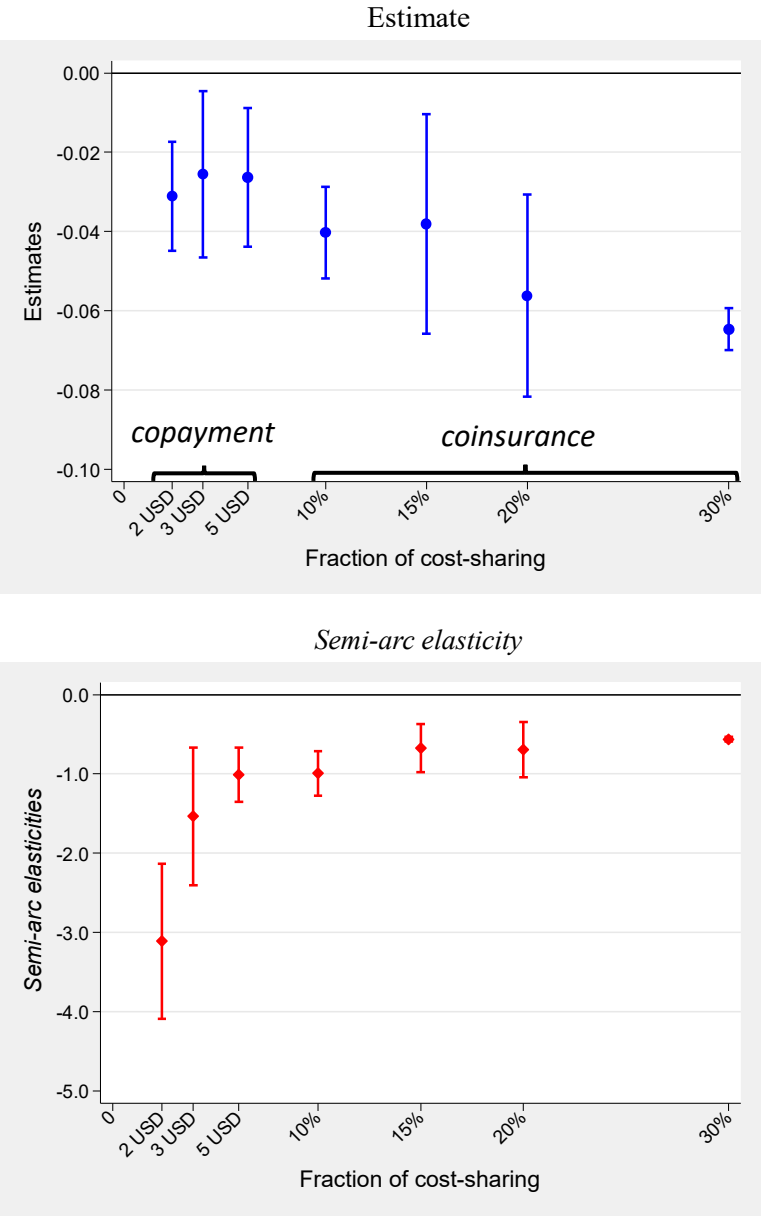
(iii) Base vs. cell means



Notes: The 0-30% sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in USD (100JPY/USD). The estimates β_A for each age (A=7–14) from estimating equation [2] are reported. The dotted lines are the 95th confidence intervals derived from standard errors clustered at municipality level except for 2) where standard errors clustered at individual level. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. Along with our baseline estimates, (i) reports the estimates which include time-by-municipality FE, (ii) reports the estimates from the sample limited to those individuals which experienced at least one change in subsidy status, and (iii) reports the estimates from cell means where the cell is defined by municipality-age-time and the number of observations in each cell is used as a weight.

Figure 7: Effect of different cost-sharing

Outcome: Outpatient dummy

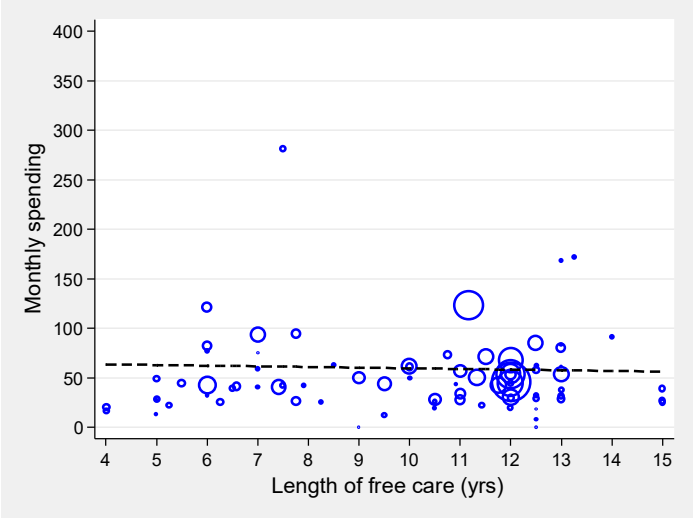


Notes: The full sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month. The upper half plots β^C from estimating equation [3], and the lower half plots the corresponding semi-arc elasticity. The control group is children with free care (C= 0%). The mean for the control group is 0.46. The upper and lower bars indicate the 95th confidence intervals where the standard errors clustered at 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 fraction of cost-sharing for three small copayments are 2.4% (2 USD/visit), 3.9% (3 USD/visit), and 6.1% (5 USD/visit).

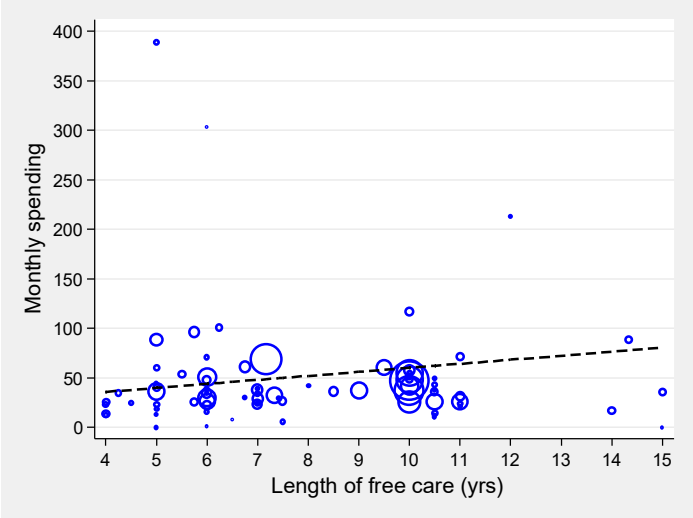
Figure 8: Medium-term utilization and health outcomes

A. Average monthly spending (in USD)

(i) Age 16

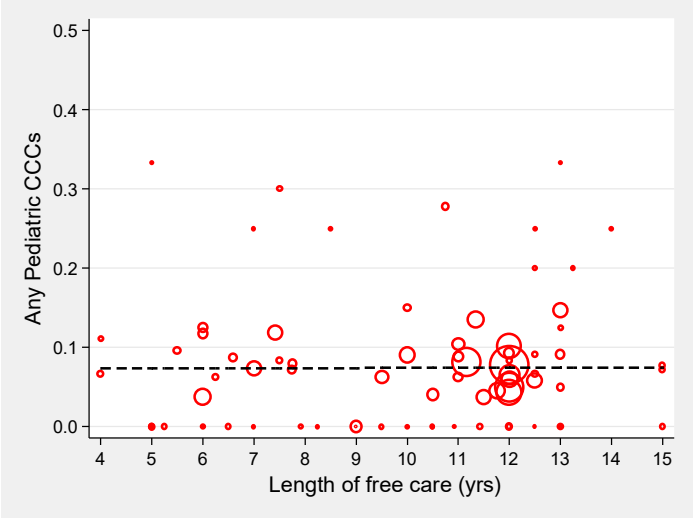


(ii) Age 18

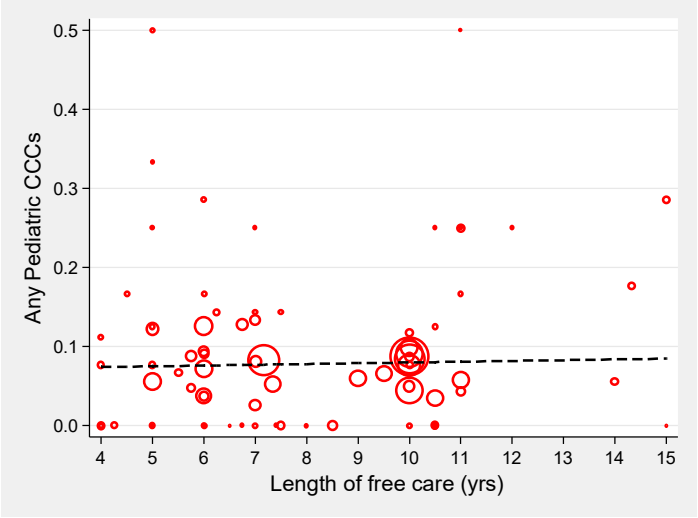


B. Any pediatric CCCs

(i) Age 16



(ii) Age 18



Notes: For the samples aged 16 (18) years, we have a total of 3,643 (3,426) individuals. See the main text for the sample construction. The *x*-axis is the length of free care (in years) between ages 0–15. The average length of free care between the ages of 0–15 years for the sample aged 16 (18) years is 10.91 (8.64) years ranging from 4 years to the full 15 years. The *y*-axis in Panel A is average monthly spending measured in USD (100JPY/USD), which is the sum of outpatient and inpatient spending. The *y*-axis in Panel B is a dummy variable that takes the value of one if any visits/admissions at the ages of 16 and 18 years are diagnosed with any pediatric complex chronic conditions (CCCs). See Online Appendix H for the list of pediatric CCCs. For consistency with the analysis so far, when we examine the utilization during age 16 years, we exclude two months of the utilization right after the subsidy expiration at age 15 years to account for the intertemporal substitution. Thus, we observe 10 months of utilization (including these 2 months does not change the results). The dotted line is the predicted values of weighted least square regressions where weight is the number of observations in each dot as reflected by the size of each dot. For Panel A, the slope for the samples aged 16 (18) years are –0.660 (4.057) with p-values of 0.689 (0.450), both of which are far from statistically significant and economically small. Similarly, for Panel B, the slopes for both samples aged 16 and 18 are economically very small (0.0001 and 0.0010) and far from statistically significant (p-value= 0.953 and 0.651).

Table 1: List of changes in patient cost-sharing (Top 10)

		Mun-time-age cell		Year-month	
Before change	*After* change	N	*Share*	N	*Share*
30%	0%	3,623	*30.6%*	15,472	*39.7%*
0%	30%	2,790	*23.6%*	11,814	*30.3%*
5 USD/visit	30%	1,029	*8.7%*	2,516	*6.5%*
30%	2 USD/visit	855	*7.2%*	1,502	*3.9%*
30%	5 USD/visit	706	*6.0%*	1,556	*4.0%*
2 USD/visit	20%	535	*4.5%*	1,050	*2.7%*
2 USD/visit	0%	475	*4.0%*	981	*2.5%*
2 USD/visit	30%	331	*2.8%*	460	*1.2%*
3 USD/visit	30%	260	*2.2%*	482	*1.2%*
10%	30%	249	*2.1%*	712	*1.8%*
Total		11,205	*100%*	36,923	*100%*

Notes: The full sample is used. This table lists top 10 combinations of transitions in patient cost-sharing. See Appendix Table A-1 for the complete list. The shares are calculated based on all price changes (not just by top 10 combinations). In this study, we mainly focus on the first two price transitions (the 0-30% sample). For simplicity, the exchange rate of 100 JPY/USD is used throughout the paper.

Table 2: Summary statistics (0-30% sample)

Variable	Mean	SD	Min	Max
A. Municipality (N = 165)				
Average length observed (months)	76.59	32.77	5	120
Subsidy info				
Number of policy changes	1.20	1.12	0	5
At least one policy change	68.5%	0.47	0	1
B. Individual (N = 63,590)				
Average length observed (months)	36.22	31.14	2	119
Subsidy info				
Number of subsidy status changes	0.39	0.80	0	5
At least one subsidy status change	21.8%	0.41	0	1
At least one subsidy expansion ("better")	16.5%	0.37	0	1
At least one subsidy expiration ("worse")	19.3%	0.39	0	1
Characteristics				
Female	48.8%	0.50	0	1
Age (in years)	10.86	2.85	6.08	15.92
C. Person-month (N = 2,303,335)				
Subsidy info				
Subsidized	71.0%	0.45	0	1
In-kind (when subsidized)	99.9%	0.03	0	1
Income restriction (when subsidized)	1.5%	0.12	0	1
Utilization				
Outpatient dummy	40.7%	0.49	0	1
Outpatient spending	60.9	253.3	0	93355.2
Outpatient spending (outpatient spending >0)	149.9	380.4	2.6	93355.2
N of outpatient visits	0.83	1.46	0	34
N of outpatient visits (outpatient spending >0)	2.05	1.65	1	34
OOP payment per visit *without* subsidy	22.3	46.3	0.7	2293.5
Inpatient dummy	0.28%	0.05	0	1
Inpatient spending	11.5	352.4	0.0	60835.9
Inpatient spending (inpatient spending >0)	4065.2	5235.7	52.3	60835.9

Notes: The 0-30% sample is used. Outpatient spending, inpatient spending, and out-of-pocket (OOP) payment are all measured in USD (100JPY/USD).

Table 3: 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			
2 USD/visit	-0.040***	-0.028***	-0.015***
	(0.011)	(0.008)	(0.005)
Mean	0.286	0.116	0.051
% change from mean	*-14.0%*	*-24.2%*	*-29.7%*
Sick			
2 USD /visit	-0.013	-0.037**	-0.026**
	(0.015)	(0.014)	(0.012)
Mean	0.617	0.364	0.207
% change from mean	*-2.1%*	*-10.2%*	*-12.6%*

Notes: The full sample is used. The estimates β^C (C= 2 USD/visit) from equation [4] are reported. We construct the health status in the following way: 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 an in-kind dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and an income restriction dummy that takes one if the municipality imposes income restriction for subsidy eligibility. 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%). Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 4: Appropriate versus Inappropriate Use of Antibiotics

	(1) Tier1	(2) Tier2	(3) Tier3
Subsidized	0.086***	0.310***	0.112***
	(0.029)	(0.052)	(0.019)
R-squared	0.08	0.13	0.08
N	2,205,647	2,205,647	2,205,647
N of Individual	63,530	63,530	63,530
Mean wo subsidy	0.258	0.900	0.520
% change from mean	*34.9%*	*34.4%*	*21.2%*

Notes: The 0-30% sample is used. The estimates β from estimating equation [2'] are reported. The outcome is monthly outpatient spending on antibiotics for patients in each tier measured in USD (100JPY/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 an in-kind dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and an income restriction dummy that takes one if the municipality imposes restriction for subsidy eligibility. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 5: Regular- versus After-Hours Visits

	(1) Regular-hour visits	(2) After-hours visits	(3) Midnight/Holiday visits
Subsidized	1.358***	0.183**	-0.006
	(0.297)	(0.075)	(0.031)
R-squared	0.21	0.10	0.04
N	2,205,647	2,205,647	2,205,647
N of Individual	63,530	63,530	63,530
Mean wo subsidy	5.089	0.567	0.323
% change from mean	*26.7%*	*32.3%*	*-1.9%*
Share	85.1%	9.5%	5.4%
Typical fees per visit (USD)	28 (first visit) 7 (revisit)	+8.5 (first visit) +6.5 (revisit)	+48/+25(first visit) +42/+19 (revisit)

Notes: The 0-30% sample is used. The estimates β from estimating equation [2'] are reported. The outcome is monthly outpatient spending for each type of visit measured in USD (100JPY/USD). The last rows list the typical consultation fees per visit for regular-hour visits (column 1), and additional fees for after-hours visits (column 2) and midnight/holiday visits (column 3) charged on top of the consultation fees for regular-hour visits measured in USD. All the regressions include age (in months) FE, time (in month) FE, and individual FE. We also control for an in-kind dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and an income restriction dummy that takes one if the municipality imposes income restriction for subsidy eligibility. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 6: By Service Categories

	(1) Medication	(2) Consultation Fees	(3) Laboratory tests	(4) Non-surgical procedure
Subsidized	5.037***	1.923***	2.321***	0.889***
	(0.910)	(0.360)	(0.277)	(0.123)
R-squared	0.53	0.24	0.12	0.17
N	2,205,647	2,205,647	2,205,647	2,205,647
N of Individual	63,530	63,530	63,530	63,530
Mean wo subsidy	21.30	7.23	6.79	2.09
% change from mean	*23.6%*	*26.6%*	*34.2%*	*42.5%*
Share	54.1%	18.4%	17.2%	5.3%

Notes: The 0-30% sample is used. The estimates β from estimating equation [2'] are reported. The outcome is monthly outpatient spending for each service category measured in USD (100JPY/USD). All the regressions include age (in months) FE, time (in month) FE, and individual FE. We also control for an in-kind dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and an income restriction dummy that takes one if the municipality imposes income restriction for subsidy eligibility. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 7: Ambulatory Care Sensitive Conditions (ACSC)

	A. An *outpatient* dummy			**B. An *inpatient* dummy(×1000)**		
	Any ACSC	ENT	Asthma	Any ACSC	ENT	Asthma
	(1)	(2)	(3)	(4)	(5)	(6)
Subsidized	0.027***	0.019***	0.013***	0.526	0.392**	0.468***
	(0.002)	(0.002)	(0.001)	(0.445)	(0.198)	(0.105)
R-squared	0.24	0.16	0.35	0.14	0.06	0.15
N	2,205,647	2,205,647	2,205,647	2,205,647	2,205,647	2,205,647
N of Individual	63,530	63,530	63,530	63,530	63,530	63,530
Mean wo subsidy	0.114	0.078	0.036	1.040	0.318	0.245

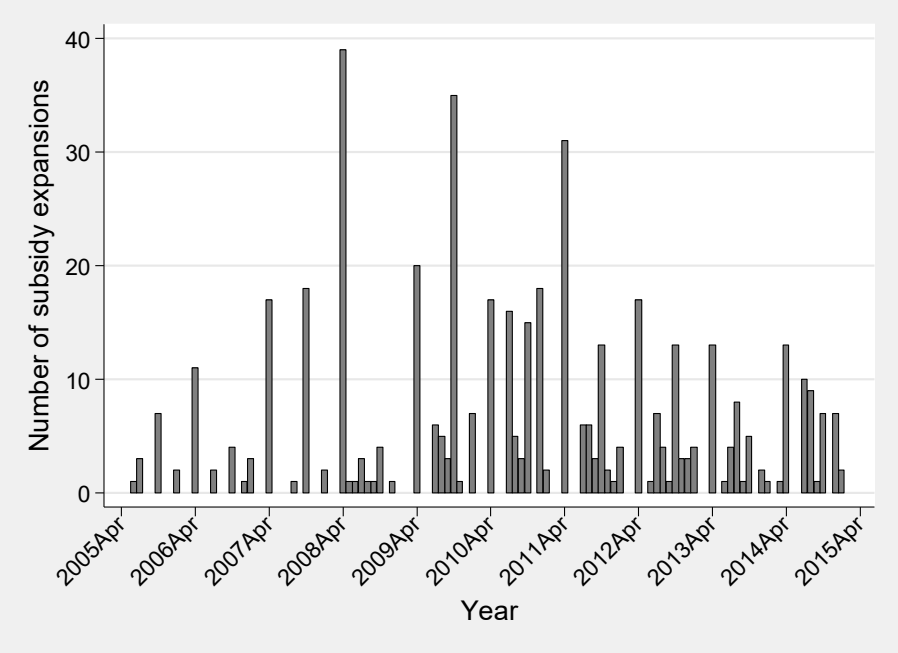
Notes: The 0-30% sample is used. The estimate β from estimating equation [2'] is reported. An outpatient dummy takes one if there is at least one outpatient visit per month, and an inpatient dummy takes one if there is at least one hospitalization per month (×1000). See Online Appendix Table G-1 for the list of ACSC and summary statistics. All the regressions include age (in months) FE, time (in month) FE, and individual FE. We also control for an in-kind dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and an income restriction dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For Panel B, we also control for a dummy that takes one if the municipality also offers subsidy for inpatient care. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. ENT stands for Ear, Nose, and Throat. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Online Appendix

(Not for Publication)

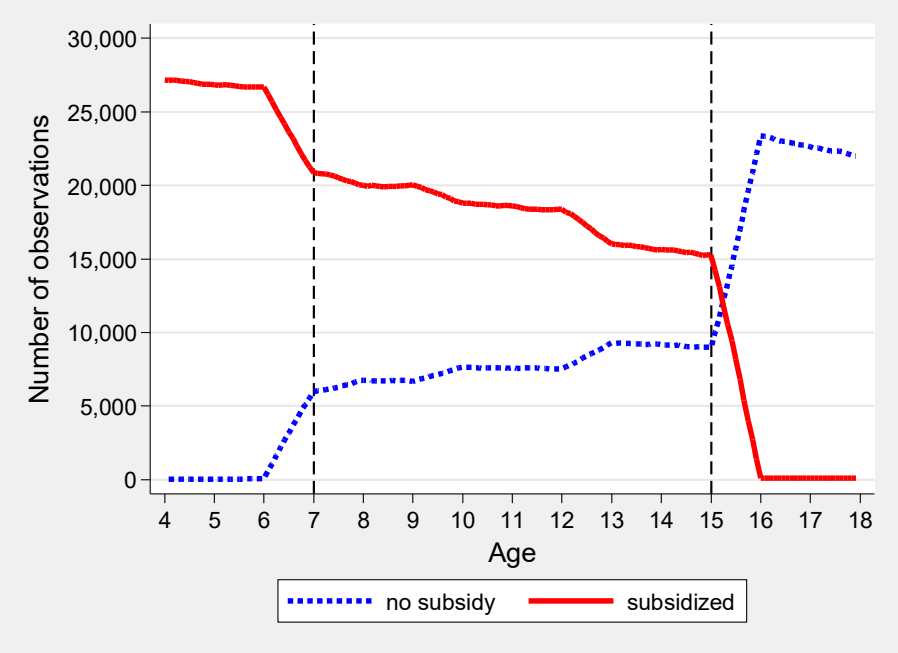
Appendix A: Additional figures and tables

Figure A-1: Timing of the subsidy expansions



Notes: The full sample is used. The total number of municipalities is 323. The data spans from April 2005 to March 2015 (10 years). There are total of 480 expansions of a child healthcare subsidy.

Figure A-2: The number of observations by subsidy status



Notes: The full sample is used. The two vertical dotted lines indicate the ages of children focused in this study since we do not have many observations without subsidy below age 7 years and with subsidy above age 15 years. This is because the majority of municipalities (81.3%) already provided the subsidy until the age of 6 years (start of primary school) at the beginning of our sample period (April 2005), and most municipalities do not provide subsidy beyond age 15 years (end of junior high school) at the end of our sample period (March 2015).

Table A-1: Complete list of changes in cost-sharing

		Mun-time-age cell		Year-month	
Before change	*After* change	N	*Share*	N	*Share*
30%	0%	3,623	*30.6%*	15,472	*39.7%*
0%	30%	2,790	*23.6%*	11,814	*30.3%*
5 USD/visit	30%	1,029	*8.7%*	2,516	*6.5%*
30%	2 USD/visit	855	*7.2%*	1,502	*3.9%*
30%	5 USD/visit	706	*6.0%*	1,556	*4.0%*
2 USD/visit	20%	535	*4.5%*	1,050	*2.7%*
2 USD/visit	0%	475	*4.0%*	981	*2.5%*
2 USD/visit	30%	331	*2.8%*	460	*1.2%*
3 USD/visit	30%	260	*2.2%*	482	*1.2%*
10%	30%	249	*2.1%*	712	*1.8%*
30%	3 USD/visit	166	*1.4%*	319	*0.8%*
10%	0%	162	*1.4%*	445	*1.1%*
0%	10%	126	*1.1%*	281	*0.7%*
3 USD/visit	2 USD/visit	125	*1.1%*	425	*1.1%*
30%	10%	124	*1.0%*	264	*0.7%*
0%	20%	51	*0.4%*	218	*0.6%*
15%	0%	51	*0.4%*	66	*0.2%*
15%	30%	39	*0.3%*	49	*0.1%*
30%	20%	37	*0.3%*	106	*0.3%*
30%	15%	35	*0.3%*	154	*0.4%*
0%	2 USD/visit	28	*0.2%*	31	*0.1%*
0%	15%	17	*0.1%*	33	*0.1%*
2 USD/visit	3 USD/visit	14	*0.1%*	14	*0.0%*
5 USD/visit	20%	12	*0.1%*	13	*0.0%*
3 USD/visit	0%	1	*0.0%*	1	*0.0%*
20%	0%	1	*0.0%*	1	*0.0%*
Total		11,205	*100%*	36,923	*100%*

Notes: The full sample is used. This table lists all combinations of transitions in price cost-sharing. In this study, we mainly focus on the first two transitions (the 0-30% sample). For simplicity, the exchange rate of 100 JPY/USD is used throughout the paper.

Table A-2: Sample selection

Variable	*0-30% sample* (1)	Not in *0-30% sample* (2)	*Dif* (3)=(1)-(2)
Characteristics			
Female	0.49	0.49	0.00
	[0.50]	[0.50]	(0.01)
Age (in years)	11.95	12.15	-0.20
	[2.89]	[2.75]	(0.15)
Utilization			
Outpatient dummy	0.32	0.31	0.01
	[0.47]	[0.46]	(0.01)
Outpatient spending	44.49	41.78	2.71
	[220.23]	[163.95]	(1.96)
N of outpatient visits	0.62	0.57	0.05***
	[1.25]	[1.17]	(0.01)
Inpatient dummy (×1000)	2.37	2.66	-0.29
	[48.64]	[51.55]	(0.25)
Inpatient spending	9.68	12.79	-3.12
	[328.17]	[400.24]	(2.10)
N	689,441	311,464	
N of individuals	25,419	12,328	

Notes: The full sample is used and the sample is further limited to person-month observations *without* subsidy. The 0-30% sample in column (1) limits to 165 municipalities which only have either 0% (full subsidy) or 30% (no subsidy) patient cost-sharing during our sample period. Columns (1) and (2) report the means of variables in the far-left column in 0-30% sample and not in 0-30% sample (but in the full sample), respectively. The standard deviations are in brackets. Column (3) reports the difference in means between columns (1) and (2) with standard errors clustered at the municipality in parentheses. Outpatient and inpatient spending are measured in USD (100JPY/USD). Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table A-3: List of diagnosis groups and ICD10

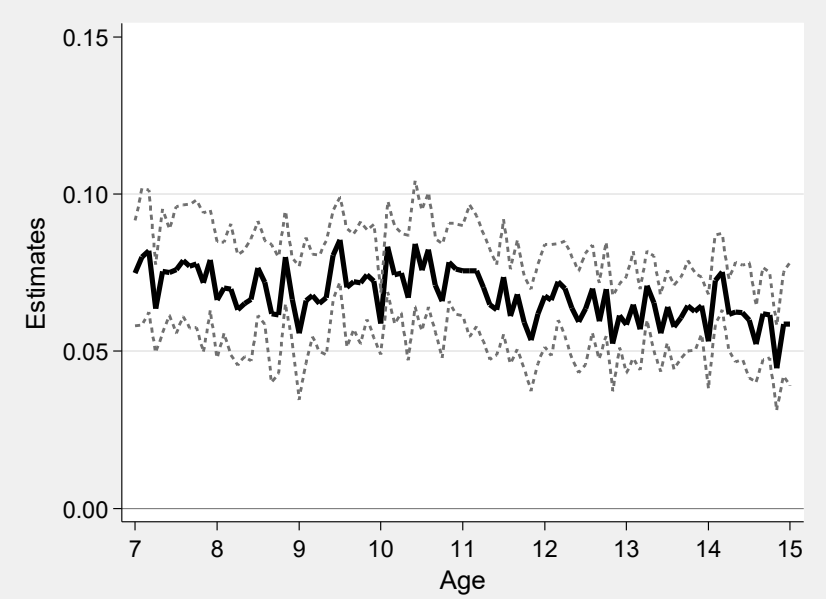
	ICD10	Share
A. Broad Category (Top 6)		
Diseases of the respiratory system	J00 – J99	31.4%
Diseases of the skin and subcutaneous tissue	L00 – L99	13.2%
Diseases of the eye and adnexa	H00 – H59	13.0%
Certain infectious and parasitic diseases	A00 – B99	10.0%
Diseases of the ear and mastoid process	H60 – H95	6.5%
Injury, poisoning and external causes	V01 – Y98	6.4%
B. ICD10 4digit (Top 10)		
Allergic rhinitis, unspecified	J304	9.5%
Acute bronchitis, unspecified	J209	4.9%
Asthma, unspecified	J459	4.8%
Acute atopic conjunctivitis	H101	4.1%
Acute sinusitis, unspecified	J019	3.6%
Acute laryngopharyngitis	J060	3.5%
Astigmatism	H522	3.1%
Acute pharyngitis, unspecified	J029	2.5%
Dermatitis, unspecified	L309	2.5%
Diarrhea and gastroenteritis of infectious origin	A09-	2.5%

Notes: The 0-30% sample is used.

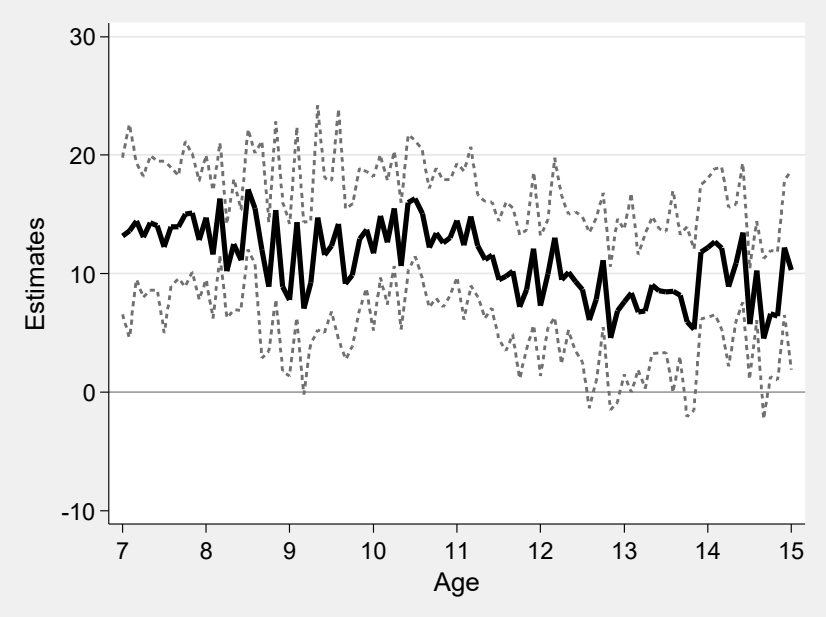
Appendix B: Basic results

Figure B-1: Basic results (monthly plots)

A. Outpatient dummy



B. Outpatient spending (in USD)



Notes: The 0-30% sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in USD (100JPY/USD). The estimates β_a (where a is age in months) are plotted. The dotted lines are the 95th confidence intervals and the standard errors clustered at municipality level are used to construct them. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization.

Table B-1: Basic results

	A. Outpatient dummy				**B. Outpatient spending** (in USD)			
	(1)		(2)		(3)		(4)	
	Estimate	(SE)	*Semi-arc elasticity*	[SE]	Estimate	(SE)	*Semi-arc elasticity*	[SE]
Subsidized ×								
Age7	0.075***	(0.006)	*-0.550****	[0.027]	13.762***	(2.363)	*-0.739****	[0.050]
Age8	0.068***	(0.006)	*-0.521****	[0.026]	12.782***	(1.793)	*-0.715****	[0.058]
Age9	0.070***	(0.005)	*-0.574****	[0.027]	11.305***	(1.707)	*-0.662****	[0.057]
Age10	0.073***	(0.005)	*-0.629****	[0.028]	13.505***	(2.207)	*-0.835****	[0.057]
Age11	0.066***	(0.006)	*-0.600****	[0.029]	11.133***	(2.106)	*-0.708****	[0.065]
Age12	0.064***	(0.004)	*-0.614****	[0.032]	8.722***	(2.227)	*-0.548****	[0.071]
Age13	0.062***	(0.004)	*-0.612****	[0.029]	8.111***	(2.178)	*-0.503****	[0.070]
Age14	0.060***	(0.004)	*-0.613****	[0.034]	9.978***	(1.339)	*-0.633****	[0.077]
In-kind	0.047***	(0.014)			4.397	(3.880)		
Income restriction	-0.020**	(0.009)			-5.614	(3.722)		
R-squared	0.23				0.51			
N	2,205,647				2,205,647			
N of individuals	63,530				63,530			
Mean wo subsidy	0.32				44.91			

Notes: This table corresponds to Figure 5 in the main text. The 0-30% sample is used. An outpatient dummy in Panel A takes one if an individual makes at least one outpatient visit per month and zero otherwise. Outpatient spending in Panel B is total monthly spending on outpatient care measured in USD (100JPY/USD). The estimates β_A from estimating equation [2], and the corresponding semi-arc elasticity for each age (A=7–14) are reported. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the semi-arc elasticity, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Appendix C: Robustness checks

Figure C-1 presents the sensitivity of our estimates to the size of the "donut-hole." The estimates and hence elasticities are barely affected after excluding 2 months from both sides of T=0.

We also run the alternative models for outpatient spending. In particular, we run two non-linear models (one-part and two-part GLM models) to account for highly skewed distribution of outpatient spending with the large mass at zero (e.g., Mullahy 1998; Blough et al. 1999).[1] For two-part models, we use the logit model for the first part, and the GLM model with a log link and a gamma distribution family for the second part.[2] For one-part GLM, we also choose the log link and gamma distribution. Figure C-2 shows that estimates from these alternative models are qualitatively very similar to the OLS estimates.[3] To ease the computational burden for estimating the bootstrapped standard errors for our elasticity measures, we report the OLS estimates throughout the study.

References:

Aron-Dine, Aviva, Liran Einav, Amy Finkelstein, and Mark Cullen. (2015) "Moral Hazard in Health Insurance: Do Dynamic Incentives Matter?" *Review of Economics and Statistics* 97(4): 725–741.

Blough David K., Carolyn W. Madden, and Mark C. Hornbrook. (1999) "Modeling risk using generalized linear models." *Journal of Health Economics* 18: 153–171.

Brot-Goldberg, Zarek C., Amitabh Chandra, Benjamin R. Handel, and Jonathan T. Kolstad. (2017) "What does a Deductible Do? The Impact of Cost-Sharing on Health Care Prices, Quantities, and Spending Dynamics." *Quarterly Journal of Economics* 132(3): 1261–1318.

Buntin, Melinda Beeuwkes, and Alan M. Zaslavsky. (2004) "Too much ado about two-part models and transformation?: Comparing methods of modeling Medicare expenditures." *Journal of Health Economics* 23(3): 525–542.

Deb, Partha, and Edward C. Norton. (2018) "Modeling Health Care Expenditures and Use." *Annual Review of Public Health* 39: 489–505.

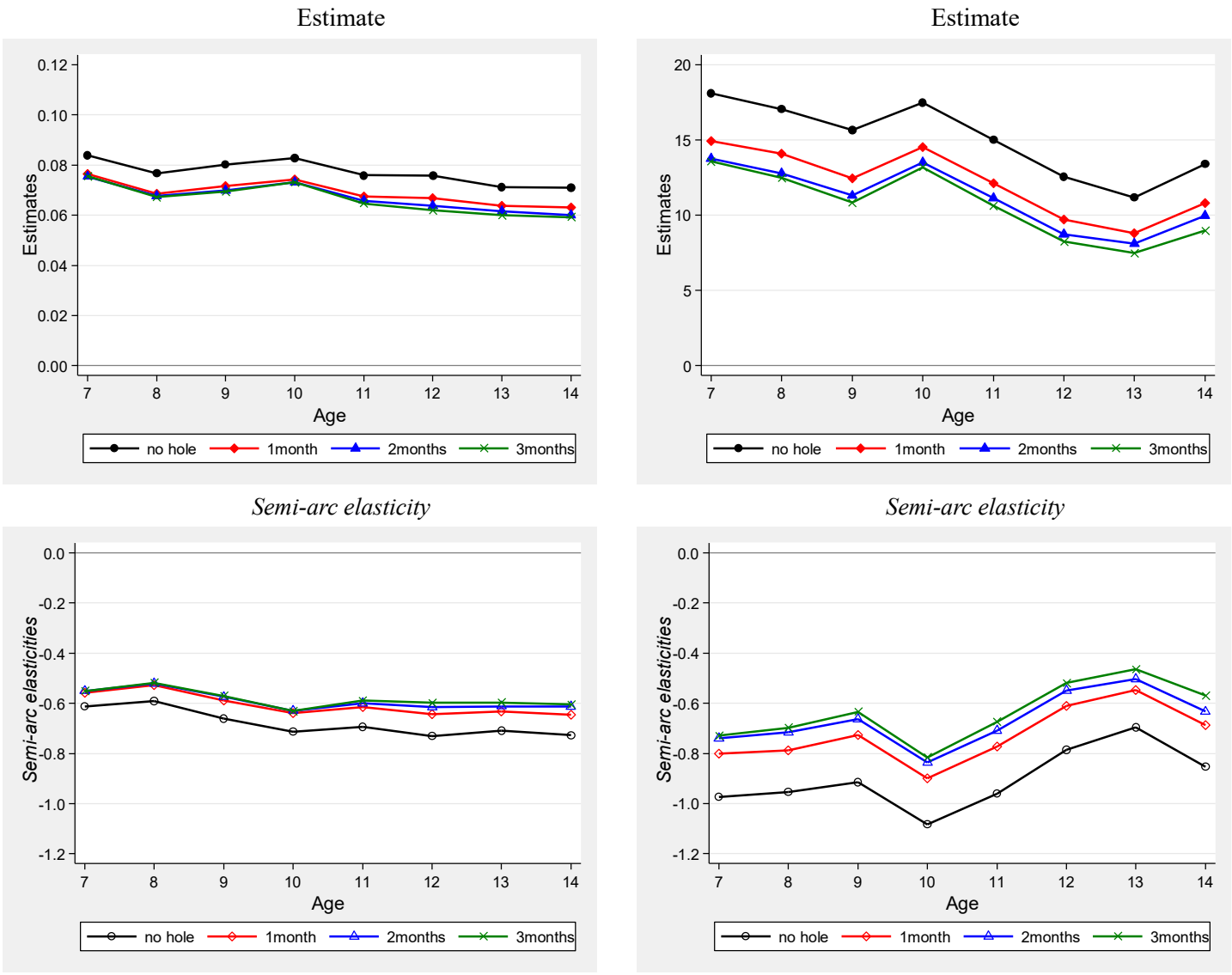
Mullahy, John. (1998) "Much ado about two: reconsidering retransformation and the two-part model in health econometrics" *Journal of Health Economics* 17(3): 247–281.

[1] Another widely used but rather ad-hoc approach is to take the logarithm of spending variable after adding an arbitrary small constant to account for zero spending (e.g., Aron-Dine *et al.* 2015; Brot-Goldberg *et al.* 2017). However, with a large number of zero observations, this model is very sensitive to the choice of small constant added to zero, and thus we do not take such an approach here (results are available upon request).

[2] The choices of a link function and a distribution family for two-part model are conducted as follows. First, Box-Cox test indicates that the estimated coefficient is close to zero (–0.033), leading to the choice of the log link. Second, a modified Park test, which empirically tests the relationship between the mean and the variance, turns out to be close to two (2.27), suggesting that a gamma family is appropriate. See for example, Buntin and Zaslavsky (2004) and Deb and Norton (2018) for details on these procedures.

[3] Here, we report the estimates from a variant of the main specification [2] where individual FE is replaced by municipality FEs to ease the computation burden of GLM models. The margin command in Stata14 is used to obtain the treatment effects.

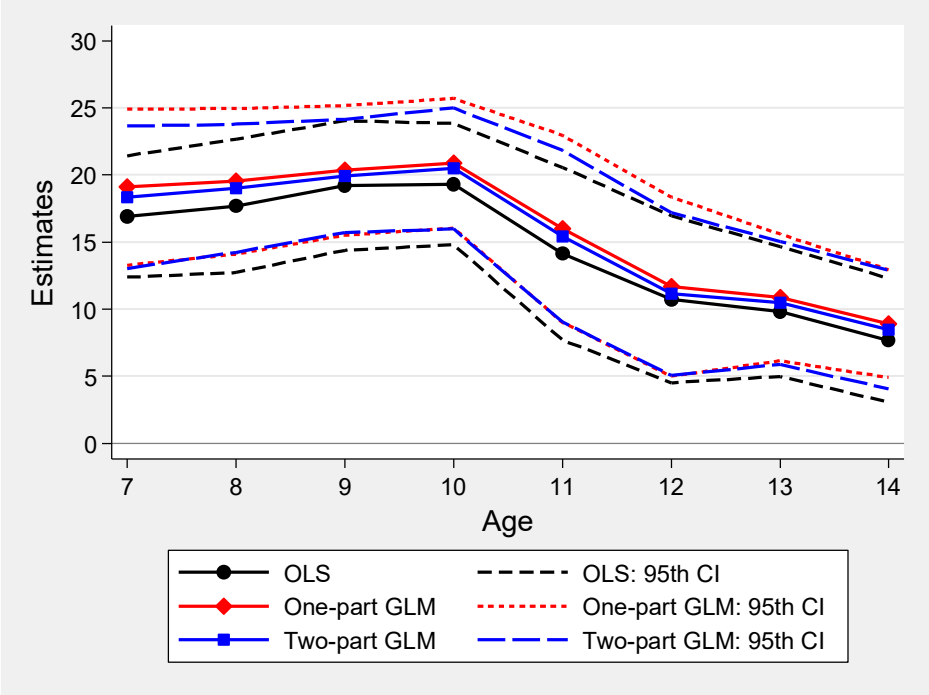
Figure C-1: Sizes of "donut" holes and the estimates/elasticities



Notes: The 0-30% sample is used. An outpatient dummy in Panel A takes one if there is at least one outpatient visit per month, and outpatient spending in Panel B is the monthly spending on outpatient care measured in USD (100JPY/USD). The upper half plots β_A for each age (A=7–14) from estimating equation [2], and the bottom half plots the corresponding semi-arc elasticity. Each line plots the estimates from the sample where the observations within 1, 2, and 3 months from price changes are excluded along with the estimates of no exclusion ("no hole").

Figure C-2: Different models (Estimates only)

Outcome: Outpatient spending (in USD)

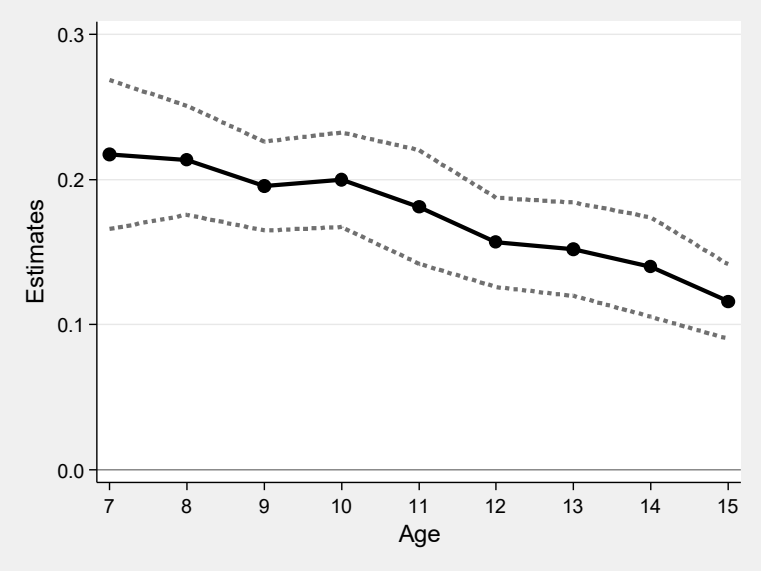


Notes: The 0-30% sample is used. Outpatient spending is the monthly spending on outpatient care measured in USD (100JPY/USD). The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. The graph plots the estimates β_A for each age (A=7–14) with three separate models (OLS, one-part GLM, and two-part GLM). For two-part GLM, we use the logit model for the first part, and the GLM model with a log link and a gamma distribution family for the second part. For one-part GLM, we also choose the log link and gamma distribution. Here, we report the estimates from a variant of the main specification [2] where individual FE is replaced by municipality FEs to ease the computational burden of GLM models. The margin command in Stata14 is used to obtain the treatment effects.

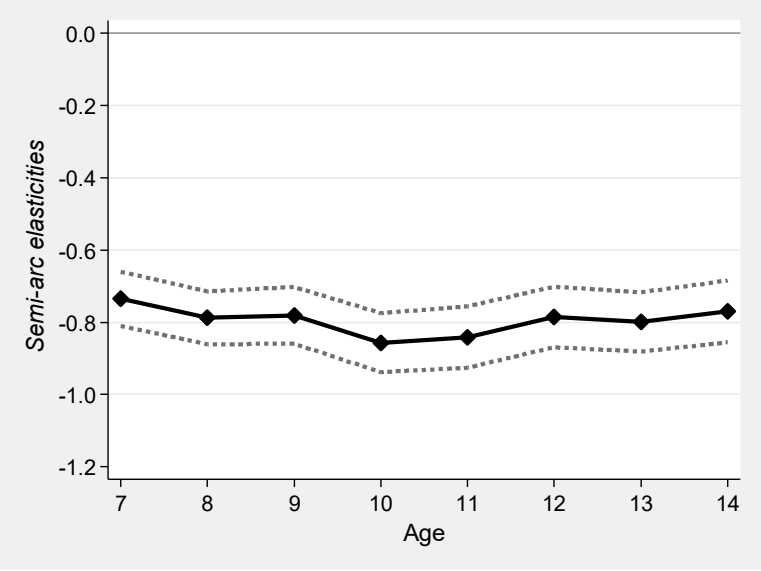
Appendix D: Other outcomes

Figure D-1: Frequency of outpatient visits

Estimate

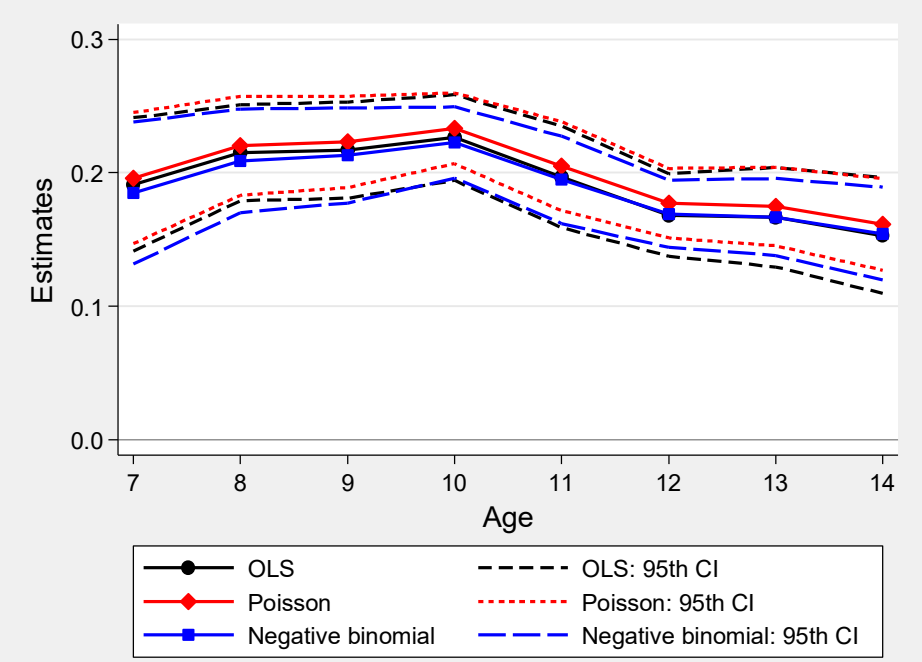


Semi-arc elasticity



Notes: The 0-30% sample is used. The frequency of outpatient visits is the number of outpatient visits per month. The estimates β_A for each age (A=7–14) from estimating equation [2] are reported. The dotted lines are the 95th confidence intervals. The standard errors clustered at 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.

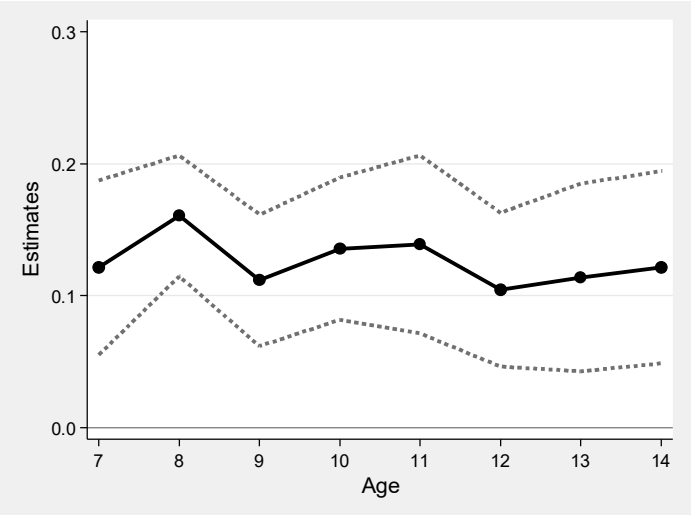
Figure D-2: Different models for frequency of outpatient visits



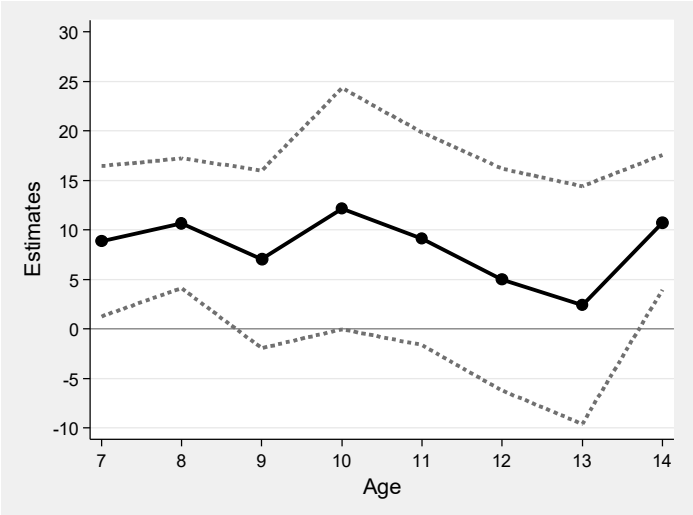
Notes: The 0-30% sample is used. The frequency of outpatient visits is the number of outpatient visits per month. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. The graph plots the estimates β_A for each age (A=7–14) with three separate models (OLS, Poisson, and negative binominal). Here, we report the estimates from a variant of the main specification [2] where individual FE is replaced by municipality FEs to ease the computational burden of count models. The margin command in Stata14 is used to obtain the treatment effects.

Figure D-3: *Conditional on* positive spending (intensive margin)

A. Frequency of outpatient visits
(outpatient spending>0)



B. Outpatient spending
(outpatient spending>0)

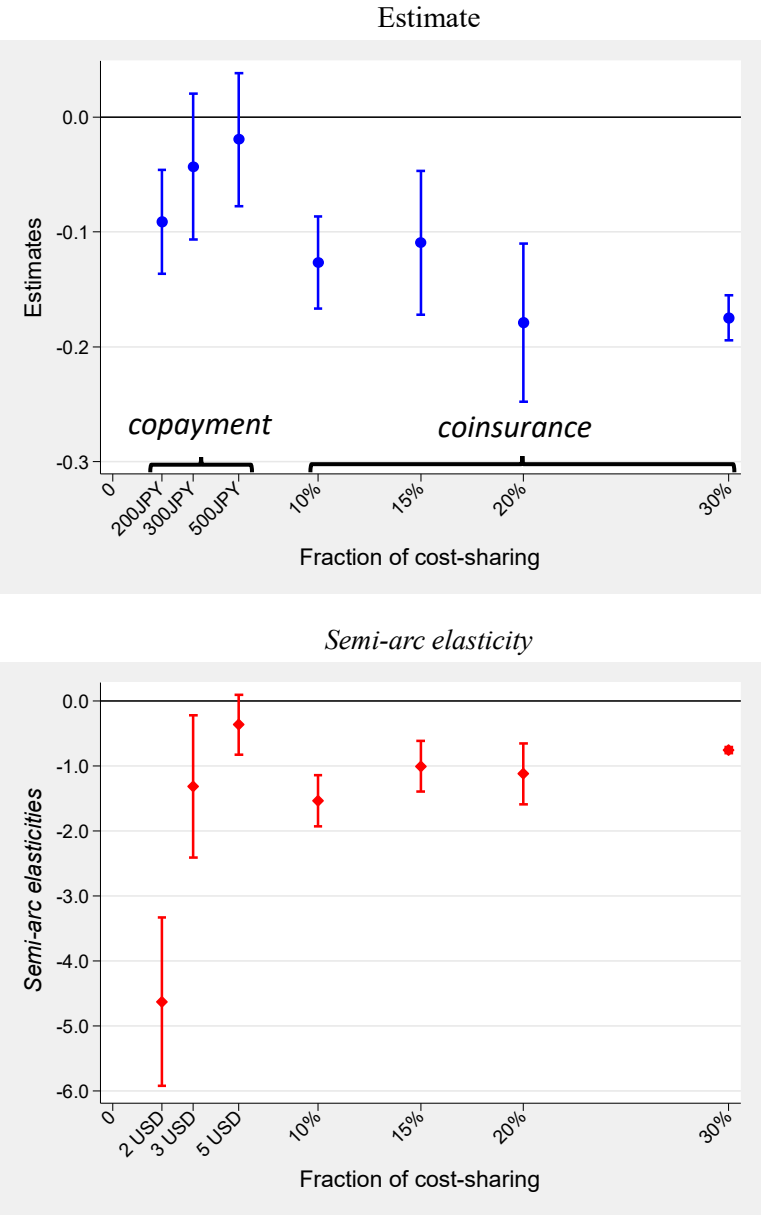


Notes: The 0-30% sample is used where the sample is further limited to observations with positive spending (N= 891,829). The frequency of outpatient visits is the number of outpatient visits per month, and outpatient spending is the monthly spending on outpatient care measured in USD (100JPY/USD). The estimates β_A for each age (A=7–14) from estimating equation [2] are reported. The dotted lines are the 95th confidence intervals derived from standard errors clustered at municipality level. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization.

Appendix E: Effect of different cost-sharing

Figure E-1: Effect of different cost-sharing

Outcome: Frequency of outpatient visits



Notes: The full sample is used. The frequency of outpatient visits is the number of outpatient visits per month. The upper half plots β^C from estimating equation [3], and the lower half plots the corresponding semi-arc elasticity. The control group is children with free care (C= 0%). The mean for the control group is 0.93. The upper and lower bars indicate the 95th confidence intervals where the standard errors clustered at 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 fraction of cost-sharing for three small copayments are 2.4% (2 USD/visit), 3.9% (3 USD/visit), and 6.1% (5 USD/visit).

Appendix F: Antibiotics Use

Table F-1: List of tiers for antibiotics use

Name of diseases	ICD-10
Tier 1	
Miscellaneous bacterial infections	A15-A28, A30-A32, A35-A37, A39-A44, A48-A59, A63-A71, A74-A75, A77-A79, A82, A96, B07, B15-B19, B25-B27, B30, B34, B50-B60, B64, B85-B91, B94-B97, B99, D86, G00-G02, G05, G14, G92, H70, J36, R11 A881, A983, A984, A985, B081, B084, B085, B088, B330, B332, B333, B334, B338, B451, B471, B479, B600, B608, B834, G030, G031, G038, G039, G040, G042, G048, G049, H950, H951,K908, L081, L946, M023, M352, M600, N341
Pneumonia	J13-J17, B440, J180, J181, J189
Urinary tract infections	N10, N12, N16, N151, N159, N300, N309, N390
Tier 2	
Acne	L70
Gastrointestinal infections	A00-A09, R10, R12-R16, R18, R190, R191, R192, R193, R194, R195, R197, R198, K522, K528
Pharyngitis	J02, J03, A38
Sinusitis	J01, J32
Skin, cutaneous and mucosal infections	A46, B35, B36, H62, H66, H67, L01-L03, L05, L88, E832, H600, H601, H602, H603, H610, H619, K122, L049, L080, L088, L089, L663, L731, L738, L980, L983, M726, P390
Suppurative otitis media	H66, H67
Tier 3	
Asthma, allergy	J30, J44, J45, T784
Bronchitis, bronchiolitis	J20, J21, J40
Influenza	J09, J10, J11
Non-suppurative otitis media	H65, H68, H69
Viral pneumonia	J12
Viral upper respiratory infection	J00, J04, J05, J06, R05
Other respiratory conditions	All remaining respiratory conditions (J00-J99) not coded above and R060-R064, R068-R069, R042, R048, R049, R093
All other codes not listed elsewhere	All other codes not listed elsewhere

Source: From Fleming-Dutra *et al.* (2016) eTable "2. Diagnostic categories by tier with corresponding ICD-9CM code".

Table F-2: Summary statistics of antibiotic use

Name of disease	Fraction of the diagnosis	Uncond.			Cond. on having the diagnosis		
		Antibiotics use (dummy)	Spending on antibiotics (in USD)	Freq. of antibiotics prescriptions	Antibiotics use (dummy)	Spending on antibiotics (in USD)	Freq. of antibiotics prescriptions
	(1)	(2)	(3)	(4)	(5) = (2)/(1)	(6) = (3)/(1)	(7) = (4)/(1)
Tier1	0.053	0.021	0.395	0.155	0.40	7.67	2.93
Tier2	0.137	0.086	1.258	0.597	0.62	9.27	4.35
Tier3	0.219	0.042	0.532	0.207	0.19	2.44	0.94
Tier1							
Miscellaneous bacterial infections	0.048	0.018	0.323	0.133	0.38	6.84	2.74
Pneumonia	0.003	0.002	0.080	0.022	0.80	26.73	7.20
Urinary tract infections	0.003	0.002	0.030	0.013	0.49	9.79	3.94
Tier2							
Acne	0.009	0.004	0.053	0.048	0.39	5.49	5.31
Gastrointestinal infections	0.027	0.012	0.181	0.075	0.46	6.87	2.79
Pharyngitis	0.036	0.026	0.377	0.160	0.73	10.60	4.42
Sinusitis	0.072	0.052	0.798	0.394	0.72	11.22	5.44
Skin, cutaneous and mucosal infections	0.012	0.007	0.106	0.047	0.53	8.67	3.77
Suppurative otitis media	0.006	0.005	0.131	0.048	0.82	21.71	7.64
Tier3							
Asthma, allergy	0.058	0.016	0.213	0.085	0.27	3.67	1.46
Bronchitis, bronchiolitis	0.036	0.023	0.320	0.120	0.66	9.12	3.36
Influenza	0.019	0.007	0.092	0.034	0.37	4.80	1.78
Non-suppurative otitis media	0.002	0.001	0.009	0.004	0.27	3.92	1.72
Viral pneumonia	0.000	0.000	0.000	0.000	0.33	5.07	2.34
Viral upper respiratory infection	0.033	0.018	0.223	0.088	0.53	6.81	2.62
Other respiratory conditions	0.000	0.000	0.000	0.000	0.25	3.05	1.65
All other codes not listed elsewhere	0.168	0.019	0.240	0.096	0.11	1.43	0.57

Notes: The spending on antibiotics is measured in USD (100JPY/USD). See Appendix Table F1 for the list of ICD10 codes for each Tier.

Appendix G: Ambulatory Care Sensitive Conditions (ACSC)

Table G-1: List of ACSC

ACSC categories	Uncond. (N=2,205,647) Mean	Cond. on visit (N=901,070) Mean	*Share*	ICD-10
Congenital syphilis	0.000	0.000	*0.0%*	A50.0-A50.9
Immunization preventable conditions	0.002	0.005	*1.0%*	A35, A36, A37, A80, G00
Grand mal status and other epileptic convulsions	0.005	0.012	*2.4%*	G40, G41
Convulsions "A" & "B"	0.001	0.004	*0.7%*	R56
Severe ENT infections	0.114	0.280	*56.9%*	H66, H67, J02, J03, J06, J31.2
Bacterial pneumonia	0.003	0.007	*1.5%*	J13, J14, J15.3, J15.4, J15.7, J15.9, J16.8, J18, J18.1
Asthma	0.063	0.155	*31.5%*	J45, J46
Tuberculosis	0.000	0.000	*0.0%*	A15, A16, A17, A18, A19
Cellulitis	0.005	0.013	*2.6%*	L03 L04 L08.0 L08.8 L08.9 L88 L98.0
Diabetes "A", "B", "C"	0.000	0.001	*0.1%*	E10.0-E10.8, E11.0-E11.8, E12.0-E12.8, E13.0-E13.8, E14.0-E14.8
Hypoglycemia	0.000	0.001	*0.2%*	E16.2
Gastroenteritis	0.001	0.001	*0.3%*	K52.2, K52.8, K52.9
Kidney/urinary infection	0.000	0.001	*0.2%*	N10, N11, N12, N13.6
Dehydration-volume depletion	0.003	0.008	*1.5%*	E86
Iron deficiency anemia	0.002	0.005	*0.9%*	D50.1, D50.8, D50.9
Nutritional deficiencies	0.000	0.001	*0.2%*	E40, E41, E42, E43, E55.0, E64.3
Failure to thrive	0.000	0.000	*0.0%*	R629
Any ACSC	0.166	0.407	*100%*	

Notes: "Unconditional" includes observations (person-month) with no outpatient visits in a month, and "Conditional" limits to observations with at least one outpatient visit per month. We convert the ICD9-CM listed in Gadomski *et al*. (1998) to ICD10.

References:

Gadomski, Anne, Paul Jenkins, and Melissa Nichols. (1998) "Impact of a Medicaid primary care provider and preventive care on pediatric hospitalization." *Pediatrics* 101(3).

Appendix H: Pediatric complex chronic conditions

Table H-1: List of pediatric complex chronic conditions (CCCs)

CCC categories	Mean	*Share*
Neurologic and neuromuscular	0.0093	*8.2%*
Cardiovascular	0.0315	*27.7%*
Respiratory	0.0003	*0.2%*

Renal and urologic	0.0037	*3.2%*
Gastrointestinal	0.0050	*4.3%*
Hematologic or immunologic	0.0059	*5.2%*
Metabolic	0.0143	*12.5%*
Other congenital or Genetic defect	0.0126	*11.1%*
Malignancy	0.0307	*27.0%*
Premature and neonatal	0.0001	*0.1%*
Technology dependence	0.0004	*0.4%*
Transplantation	0.0000	*0.0%*
Any CCC	0.086	*100%*

Notes: There are 7,069 individuals. The means of a dummy that takes one if an individual is diagnosed with ICD10 in each CCC category during the 12 months are reported. See Feudtner *et al.* (2014) for corresponding ICD10, as well as Feudtner *et al.* (2000) for original list in ICD9.

References:

Feudtner *et al.* (2014) "Pediatric complex chronic conditions classification system version 2: updated for ICD-10 and complex medical technology dependence and transplantation." *BMC Pediatrics* 14:199.

Feudtner *et al.* (2000) "Pediatric Deaths Attributable to Complex Chronic Conditions: A Population-Based Study of Washington State, 1980–1997" *Pediatrics* 106: 205–209.

Appendix I: Inter-municipality migration

In the main text, we focus on the children who do not move across municipalities, as there are only 1,079 such children which account for only 1.7% of total children (63,530 vs. 64,609). The migration rate in our sample is lower than actual migration, since intra-municipality migration is not counted as migration, as the subsidy level is the same. However, if a family with very sick children is more likely to move to more generous municipality, our estimates—which may fail to control for the time-varying unobserved health conditions—can be potentially biased. We think that this is very unlikely for a couple of reasons. First, the migration rate is declining function of age of children and is already low by age 7 as parents tend to move before their children enter primary school. Second, there may be many other municipality characteristics than subsidy generosity for child healthcare that may affect the migration decision such as quality of school, availability of daycare, and other childrearing support in the districts. Nonetheless, we include those who move across municipalities into the sample and re-estimate the equation [2]. Figure I-1 compares the estimates with and without movers. It is reassuring that estimates are very similar.

More direct way to test *selective* migration is to examine 1) whether children who move are more likely to choose more generous municipality, and 2) particularly whether sicker children are more likely to move to more generous municipality. To investigate such possibilities, we estimate a location choice model, limiting our sample to a month when children move across municipalities. For the first question, we estimate the following equation of the conditional logit model:

$$Pr(Y_{iat} = m) = F(\sum_{A=7}^{14} \beta_A\{subsidized_{amt} \times 1(Age\ A)\} + \delta_m + \varepsilon_{iat}) \quad \text{–[I1]}$$

where $Pr(Y_{iat} = m)$ is the locational choice of municipality m among M municipalities by a child i whose age is a at time t, and $subsidized_{amt}$ is a dummy which takes one if the municipality m provides subsidy for age a at time t. We also control for municipality of choice fixed effects δ_m to control for time-invariant municipality characteristics that may attract (families of) children. Our coefficients of interest are series of β_A (A= 7–14) where $\beta_A > 0$ indicates that children are more likely to choose the municipality which provides the subsidy for her/his age a in time t. The standard errors are clustered at individual level.

For the second question, we further interact the series of subsidy dummies with the proxy for health status—the average outpatient spending for the six months just before the month of move (denoted by $prior\ spending_{iat-1}$ below) [4]:

$$Pr(Y_{iat} = m) = F(\sum_{A=7}^{14} \beta_A\{subsidized_{amt} \times 1(Age\ A)\} + \sum_{A=7}^{14} \gamma_A\{subsidized_{amt} \times 1(Age\ A) \times prior\ spending_{iat-1}\} + \delta_m + \varepsilon_{iat}) \quad \text{–[I2]}$$

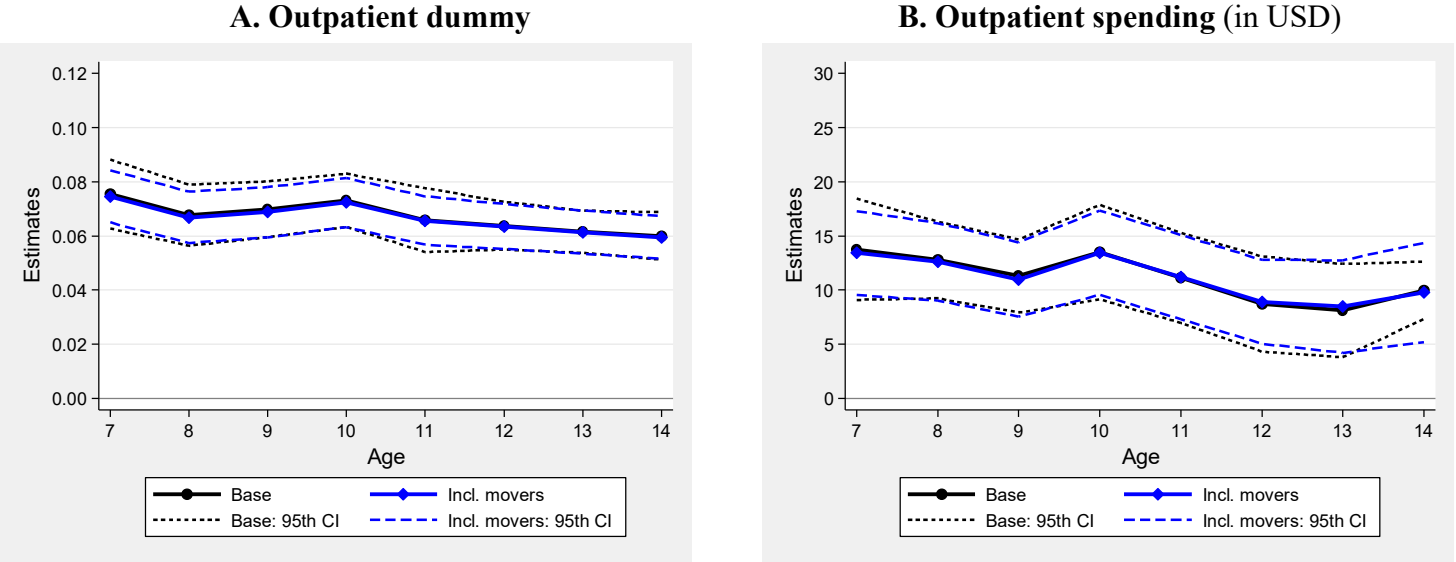
where $\gamma_A > 0$ indicates that the sickly children are more likely to choose the municipality with subsidy. Note that in both regressions, only the children who moved across the 165 municipalities are included in the sample.

The graph on the left in Figure I-2 demonstrates the graphical presentation of estimating equation [I1] which plots β_A for each age (A=7–14). Even though the estimates are quite noisy, β_A are mostly negative and are not statistically significant at the conventional level. Thus, these results at least do not support that children are more likely to choose the municipality with the subsidy for her/his age.

[4] We experiment the length of prior months to calculate the average prior spending from X months (X= 3, 6, 9 and 12) but the estimates are very similar. The benefit of taking longer span to compute the average spending is that we may be able to capture the health status with more accuracy while the cost is that we lose individual who move within the first X months from the start of the data.

The graph on the right in Figure I-2 demonstrates the graphical presentation of estimating equation [I2] which plots β_A in the upper half, and γ_A in the lower half. Again, β_A are not statistically significant and mostly negative. Furthermore, γ_A are close to zero, and far from statistically significant, suggesting that sickly children are no more likely to choose the municipality with subsidy. Taken together, we do not find any evidence of selective inter-municipality migration at least in the current setting.

Figure I-1: Baseline vs. including movers

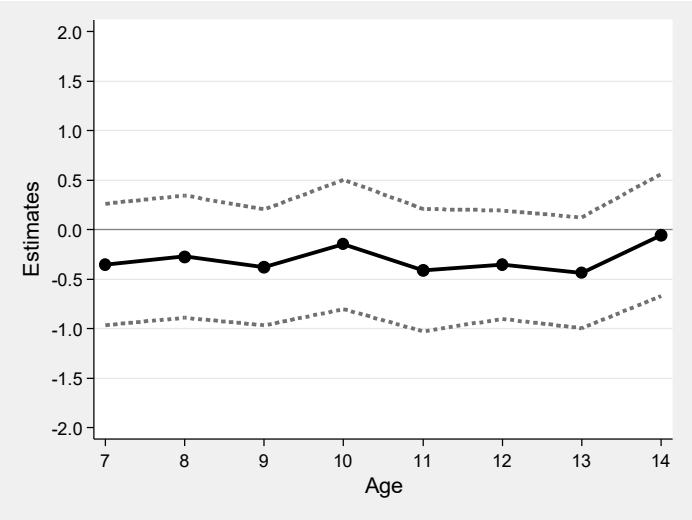


Notes: The 0-30% sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in USD (100JPY/USD). The estimates β_A for each age (A=7–14) from estimating equation [2] are plotted. The dotted lines are the 95th confidence intervals derived from standard errors clustered at individual level. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. Along with our baseline estimates from the sample without movers, we report estimates from the sample that include inter-municipality movers (1.7%).

Figure I-2: Selective inter-municipality migration

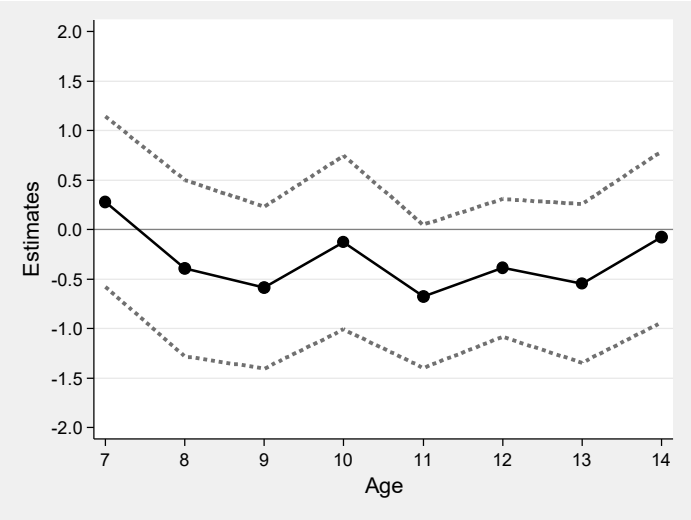
Equation [I1]

Plot of β_A

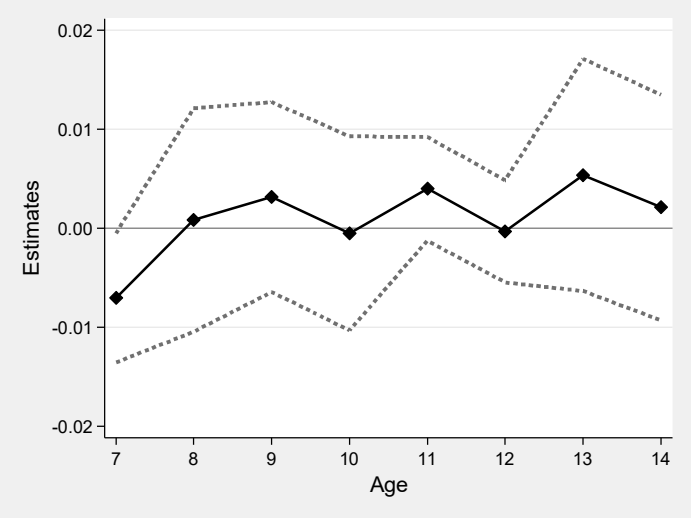


Equation [I2]

Plot of β_A



Plot of γ_A



Notes: The estimates from conditional logit model are plotted. The dotted lines are the 95th confidence intervals derived from standard errors clustered at individual level. The graph on the left plots β_A from estimating equation [I1] while the graphs on the right plot β_A in the upper half, and γ_A in the lower half from estimating equation [I2].