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

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Abstract

This study exploits over 5,000 variations in subsidy generosity across ages and municipalities in Japan to examine how children respond to healthcare prices. We find that free care significantly increases outpatient spending, with price elasticities considerably smaller than for adults. Price responses are substantially larger when small copayments are introduced, indicating more elastic demand around a zero price. We also find that increased utilization primarily reflects low-value and costly care: increased outpatient spending neither reduces subsequent hospitalization by "avoidable" conditions nor improves short- or medium-term health outcomes. By contrast, inappropriate use of antibiotics and costly after-hours visits increase.

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

Online Appendix HERE

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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 are predominantly concentrated on adults and especially the elderly, and surprisingly little is known regarding 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 (CHIP). 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 on containing rising medical expenditure 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 may not be simply applicable to children. First, mothers, who may need to take their children to medical providers, may face a higher opportunity cost than the elderly do. Second, the nature of diseases 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, mothers may perceive a higher return from child healthcare, as children are expected to live longer, and hence, mothers may seek healthcare regardless of prices or are at least less willing to reduce their children's healthcare than to reduce their own.

Exploiting more than 5,000 changes in subsidy status at the municipality-age-time level in Japan, this study examines how children respond to the price of healthcare. We observe a large number of price changes, because municipalities expanded subsidies for child healthcare in different timings and covered different age groups. To this end, we newly hand-collect data on the drastic expansion of subsidies in Japan in the last decade. We merge this information with individual-level monthly panel data on item-by-item healthcare utilization. This unique variation in subsidy generosity combined with individual panel data enables us to estimate a number of behavioral responses to the price of healthcare among children in a difference-in-difference framework.

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[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 the cost-sharing for outpatient care increased between 2000 and 2010 among the high-income countries of the United Kingdom, Germany, Japan, France, and the US.

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. In addition, there are no restrictions by insurers on patients' choices of medical providers and thus, patients have direct access to specialist care without going through a gatekeeper or a referral system. Meanwhile, medical providers cannot price discriminate against patients, as the physician and hospital are paid solely based on the same national fee schedule regardless of providers' and patients' types. On a related point, our insurance claims data *inherently* include the actual transaction price, which allows us to quantify the monetary values of (excess) utilization easily, unlike the case in the US, which is notorious for having a complex price schedule.

Our study contributes to the literature on demand for healthcare among children in several ways. First, the extensive variation in subsidy status, which is always tied to the age of children, enables us to estimate age-specific price elasticity of children aged 7–14 years, even at each age at month level. The price elasticity at various ages can be informative for policymakers to design a more precise cost-sharing schedule.

Second, we examine whether children *asymmetrically* respond to the price of healthcare, since prices change in opposite directions even at the same age in our setting. On the one hand, such asymmetry—if it exists—provides a cautionary note for applying the price sensitivity estimated from just one direction of price change (e.g., price increase) when the policymaker considers implementing a policy with the opposite direction of price change (e.g., price decrease). On the other hand, if such asymmetry does not exist, it is useful for welfare analysis, as the standard welfare calculation does not differentiate the direction of the price changes. Despite the importance of this asymmetric feature, past studies were unable to test it, as there is usually only a single direction of price change.

Third, we examine the effect of a small copayment on utilization. From the policy perspective, it is informative to know how a patient responds to the introduction of small fees to free care. This is related to the "zero-price" effect, according to which people substantially increase demand if the price is zero. For example, Shampanier *et al.* (2007) show that reducing price from a small positive price to a zero price discontinuously boosts the demand for the product in a lab, and Douven *et al.* (2017) present the same result for the demand for the health insurance.

Finally, we examine whether the increases in utilization primarily reflect beneficial or low-value care (Baicker *et al.* 2015). While this is always a challenging task, we adopt the following two approaches to answer this question to the extent possible. For "beneficial" care, we examine whether subsidy-induced outpatient care prevents avoidable inpatient admissions, reduces short-term mortality, and affects medium-term utilization and health outcomes. For low-value care, we investigate whether subsidy increases costly after-hours visits and inappropriate use of antibiotics. Although child healthcare is often heavily subsidized, to our knowledge, relatively little is known about whether subsidies lead to beneficial or low-value care.

The first part of the findings documents the basic behavioral price responses to healthcare among children. We find that reduced patient cost-sharing significantly increases utilization of outpatient care. When the municipal subsidy lowers the cost-sharing from a nationally set 30% to 0% (i.e., free care), the probability of at least one outpatient visit per month increases by 6–8 percentage points (or a 19–25% increase) from the mean of 32% without the subsidy. Similarly, outpatient spending per month increases by 22–31% 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 than the conventional estimate of –2.11 and –2.26 that Brot-Goldberg *et al.* (2017) calculate from the RAND Health Insurance Experiment (RAND HIE) for nonelderly (Keeler and Rolph 1988).[4]

A "back-of-the-envelope" calculation suggests that if the full subsidy is expanded to all the municipalities among children aged 7–14 years in Japan, the annual outpatient spending increases by 117 billion JPY (1.17 billion USD).[5] Importantly, this creates a substantial negative fiscal externality to many stakeholders: while the municipality is responsible for covering only 30% of total cost (i.e., the amount of the subsidy), the remaining 70% of the subsidy-induced excess spending must be financed by taxes and premiums.

Interestingly, we find little evidence of asymmetric responses, meaning that children respond to different directions of price changes in a similar magnitude. This finding implies that policymakers can reasonably employ existing elasticity estimates, including ours, regardless of the direction of the price changes that they consider. We also find that around the price of zero, a small copayment invokes much larger price responses. This result indicates that a small copayment can be an effective tool to reduce moral hazard associated with free care. This result is consistent with the "zero-price" effect.

The second part of the findings answers whether subsidy-induced outpatient spending largely reflects the increases in beneficial or low-value care. For "beneficial" care, 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 to a reduction in hospitalization by these "avoidable" conditions. More generally, we find no evidence of "offset" effects: substantial increases in outpatient spending do not accompany the reduction in inpatient spending. In

[4] 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. Thus, instead, we report the *semi*-arc elasticity throughout the study.
[5] For simplicity, the exchange rate of 100 JPY/USD is used throughout the paper.

addition, we find little impact on short-run child mortality; however, this result should be interpreted with considerable caution owing to the very low mortality rate among children of this age range in Japan. In addition to the short-run effect, we document that the length of free care during childhood is neither systematically associated with healthcare utilization nor health status in adolescents, suggesting that subsidy-induced utilization during childhood does not improve the health of subsidy beneficiaries, even in the medium term. In summary, we find little evidence of increases in "beneficial" care.

Regarding the evidence of low-value or costly care, we first find that reduced cost-sharing substantially increases after-hours visits, validating the concern that children (and hence, mothers) exploit the opportunity of free care by increasing physician visits outside of regular hours. Second, we document that reduced cost-sharing increases the inappropriate use of antibiotics on diagnoses for which antibiotics are not recommended. This is potentially problematic, as such inappropriate use of antibiotics leads to both antibiotic resistance and adverse events (Fleming-Dutra *et al.* 2016). To our knowledge, no prior studies have investigated whether financial incentives, such as subsidy for child healthcare, increase inappropriate use of antibiotics for children.[6]

Taken together, as we find little evidence of increases in "beneficial" care and ample evidence of increases in low-value care, the weight of the evidence supports the notion that generous subsidies for child healthcare leads to the increases in unnecessary and costly visits, implying that short- or medium-term benefits of such generous subsidies are at least limited among the children we examine.

This study is most related to RAND HIE, which still serves as a gold standard in price sensitivity among the nonelderly.[7] Leibowitz *et al.* (1985) specifically analyze children aged under 13 years 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 more than 30 years old, and thus, changes in the practice of medicine (e.g., reliance on managed care and development of new technologies) imply that these results might not be directly applicable to the situation today, especially for countries other than the US. A few notable exceptions from the nonexperimental works are recent papers by Han *et al.* (2016), who examine the effect of cost-sharing at the age of 3 years in Taiwan, and by Nilsson and Paul (2018), who examine a similar question for children in a region of Sweden.[8] Our study arguably has broader policy

[6] For example, Foxman *et al.* (1987) examine the impact of patient cost-sharing on inappropriate antibiotic use in RAND HIE but do not separately examine it 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.

[7] 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.

[8] Han *et al.* (2016) find that lower copayment in Taiwan increases the utilization of outpatient care, especially low-value care at high-cost hospitals, but has little effect on children's health. Nilsson and Paul (2018) exploit the abolition of copayment for outpatient care among children aged between 7 and 19 years. They find that children increased the

implications than these studies have, because of the richness in both the variety and number of changes in cost-sharing, wider coverage of age ranges, and comprehensive analysis on healthcare utilization.

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) rather than the effect of changes in health insurance generosity (intensive margin), such as our study and 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 documents the basic findings on children's price responsiveness to healthcare, and Section 6 investigates whether the changes in utilization reflect beneficial or low-value care. Section 7 concludes.

2. Background

2.1. Healthcare system in Japan

Since 1961, Japan has had a universal health insurance system, which is heavily regulated by the government. All citizens are obliged to enroll either in an employment-based insurance system or a residential-based insurance system (see, e.g., Ikegami and Campbell 1995; Kondo and Shigeoka 2013).

The unique institutional background in Japan offers several advantages in identifying patient price responsiveness, since the roles of insurers and medical providers are relatively restricted. First, enrollment in health insurance is mandatory, and more crucially, enrollees cannot choose insurers. Thus, we do not face the adverse selection problem, which often introduces complication in other studies. The enrollees receive identical benefits—regardless of insurance types—which include outpatient services, inpatient services, dental care, and prescription drugs. Second, patients face no restrictions on 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 in 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 all fees paid to the providers are based solely on the national fee schedule (i.e., fee-for-service). Consequently, medical providers receive the same fee for the same service regardless of the insurer. This prevents so-called "cost shifting" in the US (Cutler 1998), whereby medical providers charge private insurers higher prices

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number of doctor visits, and children from low-income families are three times more responsive than more advantaged peers.

to offset losses from the beneficiaries of government-funded health insurance, such as Medicaid. Another important implication is that any changes in utilization come from quantities instead of prices, since by default there is no room for price shopping to search for cheaper providers.

2.2. Patient cost-sharing

Patient cost-sharing—for which the beneficiary is responsible out of pocket—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, in Japan, there are no deductibles. In addition, supplementary private health insurance covering out-of-pocket costs virtually does not exist in Japan, probably because the stop-loss prevents the catastrophic income loss upon illness.

The nonlinearity imposed by the stop-loss is a classic but important challenge in estimating price elasticities (Keeler *et al.* 1977; Ellis 1986). The issue is that 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). In this sense, the spot and shadow prices are arguably very similar. Second, the stop-loss is set monthly in Japan, whereas it is set annually in the RAND HIE and most 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, bias stemming from the nonlinearity associated with the stop-loss is likely to be negligible in our case.

Importantly, many municipalities provide subsidies for child healthcare for those who live in the municipality regardless of their insurance types. This Medical Subsidy for Children and Infants (MSCI) has drastically expanded in last decade. Children who are eligible for the subsidy receive an additional insurance card, which entitle them to discounts at medical institutions. Crucially, our claims data indicate their municipality of residence; thus, we can identify the level of subsidy (and hence, the level of cost-sharing) that each child faces.

3. Data

3.1. Explanatory variables

Since each municipality determines whether to provide the 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). The variations in these three dimensions are the sources of our identification strategy.

For each municipality, we collected the following four information items on the subsidy for outpatient care from April 2005 to March 2015: 1) ages that the subsidy covers; 2) the level of patient cost-sharing (equivalently, the level of municipal subsidy); 3) whether the subsidy is a refund or in-kind; and 4) whether there are any household income restrictions for subsidy eligibility. We explain each component in detail below.

First, the generosity of the subsidy is reflected largely by the maximum age until which the subsidy is provided. Note that while the eligibility age is often expressed by the 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 (Shigeoka 2015). Second, the level and form of subsidy (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 200, 300, or 500 JPY (2, 3, or 5 USD) per visit.

Third, the subsidy payment to patients can take the forms of either refund or in-kind (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 payment to refund 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 lack of an income variable in our claims data, the fraction of municipalities with an income restriction is very small in our data. In the empirical model, we include a dummy for income restriction at municipality×year-month levels.

One contribution of this study is that we construct a new dataset on detailed subsidy information at each municipality-age-time level (where both age and time are measured in *months*). Since information disaggregated at the monthly level is not formally collected even by either the prefectural or central government, we hand-collect it through a variety of sources, including the municipality web page, local newspaper, and municipal ordinances. After collecting the data, we directly contact each municipality and verify the accuracy of our information. Since such information for this long period (10 years) is not well kept in records in small municipalities, we limit the data collection to municipalities in the six largest prefectures in Japan, which results in 323 municipalities.[9] According to national statistics, these six prefectures cover as much as 44.9% of children aged 0–15 years. While our data are not nationally representative, one benefit of restricting the sample to these large prefectures is that municipalities are likely more comparable, which is useful for our difference-in-difference identification strategy.

Figure 1 plots the share of municipalities in our insurance claims data by the maximum age for the subsidy eligibility for outpatient care during April 2005–March 2015 among the 165 municipalities mainly used in this study, as explained in Subsection 3.3.[10] Note that Figure 1 reflects the compositional changes of municipalities, as the number of municipalities increases in the later period in our claims data. Within the municipalities, the subsidy expansion is always monotonic—that is, *no* single municipality *lowers* the maximum age during this period (April 2005–March 2015).

Figure 1 clearly shows that the subsidy expanded rapidly to older ages in the last decade. For example, none of the municipalities provides the 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% within 10 years by March 2015, the end of our sample period. The spike 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). While Figure 1 clearly shows that all municipalities in our sample already provided the subsidy until the age of 6 years by April 2008, this national-level subsidy expansion eases the budgetary burden on municipalities, as part of the cost is now covered by the central government. For this reason, we observe the highest number of municipality-level subsidy expansions in April 2008 for ages above 6 years (see Appendix Figure A-1 on the precise timing of all policy changes).

While the main reason for the subsidy provision is to ensure access to essential medical care for children and lessen financial burden on parents, the exact reasons for such rapid expansion in the last decade are not fully understood. A few other justifications for the subsidy provision mentioned in the literature are to attract young couples with children for tax revenues, boost low fertility rates, and combat recent increases in child poverty (Bessho 2012). We discuss potential endogeneity of subsidy expansions in Subsection 4.2.

[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.

[10] We also collected 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 with 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 to save space.

3.2. Outcome variables

Our outcome data come from the Japan Medical Data Center (JMDC), which collects and analyzes administrative insurance claims data on behalf of insurers of large corporations. Since parents of the children in the JMDC data work for large firms, our sample does not include children with extremely low household income, such as those who receive public assistance. Therefore, the liquidity constraint is unlikely to explain the results described below. 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 the municipality-age-time. The claims data report the monthly spending, including the months of no utilization.[11] 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 occurs monthly. The enrollment and claims data are linked by a unique patient ID.

There are a few advantages of this claims data. The biggest advantage is that the data observe both outpatient (including prescription drug) and inpatient care, *and* follow the same individual over time. This allows us to examine, for example, whether childhood subsidy has a 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, the outpatient and inpatient data are often separated in the other settings. For example, hospital discharge data do not usually include information on office 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. This price information enables us to quantify the monetary values of (excess) utilization easily.

Our dataset is constructed as follows. We provide the subsidy information we collected to the JMDC, 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 could not examine the 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

[11] 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.

gender and age of children), such as maternal education, household income, and family structure (e.g., number of children or siblings).

3.3. 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 (81.3%) 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 (one year wider on both sides of the ages of interest) to identify the effect of patient cost-sharing at ages 7–14 years.[12]

Then, we create the two samples (*main* sample and *full* sample). We create the main sample by limiting it to 165 municipalities, which only have either 0% (full subsidy) or 30% (no subsidy) patient cost-sharing during our sample period, for the following reasons. First, the transitions of "30% to 0%" and "0% to 30%" in cost-sharing are by far the top two variations, as shown in Table 1, which lists the top 10 combinations of cost-sharing transitions (see Appendix Table A-1 for the complete list). These two variations 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). In fact, even after imposing such restrictions, we maintain as many as 5,438 changes in subsidy status at the municipality-age-time cell, which is the level of variation for identification in our empirical analysis.[13] Second, these two price transitions are observed for entire age ranges, which allows us to estimate age-specific price elasticities across wide age ranges. Third, it is easy to compare the asymmetric price sensitivity to the opposite directions of price changes, as detailed later.

Appendix Table A-2 shows that the characteristics of children as well as their healthcare utilization are quite similar between 165 municipalities in the main sample and the remaining municipalities. This alleviates the concern that municipalities in the main sample are specific, and thus, that the results are not generalizable. Because the nonlinear price effect (especially how a small copayment affects demand) is an important empirical and policy question, we later use the full sample and exploit all price variations to estimate such effects, recognizing the lack of statistical power on some

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[12] 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.

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

occasions.

3.4. Descriptive statistics

Table 2 provides the summary statistics of selected variables in the main 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 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 on average 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 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 restriction for eligibility criteria (1.5%). In terms of utilization, 40.7% of children make at least one outpatient visit per month on average, and spend 6,090 JPY (60.9 USD) per month, including zero-spending, and 14,950 JPY (149.5 USD) conditional on at least one visit.[14] Out-of-pocket payment per visit *without* subsidy is 2,230 JPY (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 (406,520 JPY or 4065.2 USD) than outpatient care is.

The 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 demonstrates a similar pattern for outpatient spending: the mean outpatient spending is 2–3,000 JPY (20–30 USD) higher with the subsidy than without the subsidy or 40–60% higher, 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, which are aggregated in age in years, as hospital admission is very rare. In contrast to outpatient outcomes, we observe no clear difference in the

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

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 infection) tend to be more acute whereas the elderly tend to have more chronic diseases.

4. Identification Strategy

4.1. Source 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 experiences its own unique 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 as well as the concept of asymmetry in price changes.

Panel A of Figure 3 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 of Figure 3 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.

This simple illustration holds two important points. First, 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), technically, even at the monthly level.

Second, we can investigate *asymmetric* price responses to the direction of the price changes, as our variation includes the price changes in both directions even *at the same age*. Conventional price theory suggests that the directions do not matter, as the individuals should respond only to the absolute price differences (Δ=30% for both cases), irrespective of the starting price level (putting aside income effects). However, evidence from behavioral economics raises the possibility that this might not be the case. On the one hand, the elasticities can be *smaller* in a price increase (labeled "worse" or from 0% to 30%) than in a price decrease (labeled "better" or from 30% to 0%) if the exposure to free care leads to the habit formation of visiting doctors, and hence, the utilization does not decrease much even after the price increases. On the other hand, the elasticities can be *larger* in a price increase ("worse") than in a price decrease ("better") if individuals are sensitive to differences relative to a reference price ("relative thinking") in addition to absolute price differences (e.g., Tversky and Kahneman 1981; Azar 2007, 2011; Saini and Thota 2010). If 0% is an individual's reference price for healthcare, 30% seems very expensive, as the ratio of any positive price to zero is infinity, whereas if 30% is an individual's reference price, 0% does not seem much more inexpensive (of course, zero can be a special price, which we discuss in Subsection 5.6).[15] Thus, it is ultimately an empirical question.

The research design of past studies (e.g., a randomized control trial in RAND HIE or a regression discontinuity design) does not allow testing this question, because there is only a single direction of price change. In our setting, out of 5,438 changes in subsidy status at the municipality-age-time cell, the directions of price changes are split nearly half: 2,505 changes are the expansion of the subsidy ("better") while 2,933 changes are the expiration of the subsidy ("worse"). We exploit this unique price variation to identify asymmetric price responses.

4.2. Identification strategy

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

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[15] We consider that our situation is not directly related to "loss aversion" (Kahneman and Tversky 1979), as the starting price is different for two directions of price changes, while loss aversion refers to the asymmetric responses to gain and loss from the same starting (reference) price.

[16] We abstract from whether this effect stems from the patient-induced demand, that is, children or mothers ask for more care when the price is low, or physician-induced demand, that is, doctors 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.

changes for now) is:

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

where Y_{iamt} is the healthcare utilization by a child i whose age is a (measured in months), in time t (year-month), and living in municipality m. $subsidized_{iamt}$ is a dummy, which takes the value 1 if the 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 heightens 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 of individuals in the unbalanced panel data. We also control for two time-varying municipality variables, X_{mt}, a dummy that takes the value of one if the subsidy is in-kind rather than a refund, and equals one if there is income restriction on subsidy eligibility, while recognizing the lack of power to identify these effects (which, thus, is not the focus of this study). We estimate this equation using ordinary least squares (OLS). Standard errors are clustered at the municipality to account for serial correlation in the error terms within the municipalities. As discussed in Subsection 5.3, the estimates from alternative models, such as the one-part or two-part generalized linear model (GLM), are almost identical to the OLS estimates. 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:

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

where $1(Age\ A)$ is an indicator variable that takes the value of one if the person is more than age A but less 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), which capture the average effect of subsidy within the age ranges. Importantly, we still include δ_a at the monthly level to account for any age in month-specific effects (e.g., graduation from primary school).

For our main analysis, we focus on the sample of nonmovers (98.3% of the sample) who stay in the same municipality. 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. 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 generous subsidy. To alleviate this concern, in Appendix P, we estimate a discrete choice 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 report that including movers in the sample hardly changes the results owing to the small amount of inter-municipality migration. For nonmovers, since φ_m and θ_i are identical, our final estimation equation is written as[17]

$$Y_{iamt} = \alpha + \sum_{A=7}^{14} \beta_A \{subsidized_{iamt} \times 1(Age\ A)\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt} \quad \text{–[3]}$$

The identifying assumption in our difference-in-difference strategy 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. With more than 5,000 changes in subsidy statuses at the municipality-age-time cell, where both age and time are measured in months, it is difficult to imagine that such omitted variables are likely to influence our estimates. Nonetheless, it is still possible that the municipality with a different pre-trend in utilization may implement the subsidy expansion at a different timing, which may bias our estimates. 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 account for such concern, we adopt three approaches. First, we conduct an event study that normalizes the data to the timing of the subsidy changes, and examine whether there are any systematic differences in the pre-trend between the treated and control municipalities before the changes. Second, we add a municipality-specific time trend and even time-by-municipality fixed effects (where time is measured in months), to examine the robustness of our baseline estimates. The latter specification is most stringent, as these fixed effects capture any municipality-specific policy change or event in a particular month, if any, such as income transfers, other subsidies, or business cycles. Finally, 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 might be different.

[17] For nonmovers, 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.

5. Basic results

5.1. Event study

Before presenting the regression results, we provide the 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. Then, we replace the subsidized dummy in the estimation equation [3] 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). 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 ("better") and subsidy expiration ("worse"). 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 the panels so that two figures for opposite directions of price changes are visually comparable.

There are a few important points to make about these graphs. First, they do not seem to show any pre-trend, as the estimates are mostly close to zero before the changes in subsidy status in both panels. We are reassured, as this result addresses the concern that the 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 ("better") and surges in subsidy expiration ("worse") just before T=0. This pattern reveals that some children (and hence, mothers) are aware of the price changes and behave strategically by delaying or rushing visits. On one hand, the existence of anticipatory utilization is rather surprising, as the nature of diseases for children tends to be acute.[18] 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.[19] These differential responses can be behavioral in that mothers of children may react more to a price increase than a price decrease owing to utility loss from giving up the inexpensive service.[20] Importantly, as we include age and time fixed effects (both in months), this

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[18] In Japan, schools provide annual health checkups for children free of charge and thus, well-care visits are not likely to explain the anticipatory utilization.

[19] In fact, Appendix E shows that while we see anticipatory utilization for all service categories examined (medication, consultation fees, laboratory tests, and nonsurgical procedures), the magnitude of anticipatory spending seems to be larger in medication than nonsurgical procedures.

[20] Another potential explanation is that people may be more informed of subsidy expiration ("worse") than subsidy expansion ("better"). Suppose the free care is expanded from age 6 to 15 years. Then, a 6-year-old child had to be aware of the end date of the subsidy at the maximum 9 years. On the other hand, while the subsidy expansion should be

difference is not driven by a particular age or year-month effect, such as the expiration of subsidy after graduation from primary 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, as shown later, the estimates and hence, elasticities are barely affected after removing more than 2 months from T=0.[21]

Finally, the effect on utilization seems to be permanent rather than transitory, since the level of the 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 demonstrates the graphical presentation of estimating equation [3], 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 Figure C-1 plots the monthly estimates (β_a) instead of yearly estimates (β_A). Since monthly estimates are relatively stable within age in years (and statistically significant at the 1% level for any age in months), we do not lose much information by 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 are statistically significant at the conventional level for any age. With subsidy, the probability of seeing a doctor at least once a month increases by 6–8 percentage points. This translates into 19–25% increases from 0.32, the mean without subsidy among 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 necessary when comparing the elasticities estimated across countries and time periods, and as noted by Aron-Dine *et al*. (2013), the price elasticity might not be simply captured

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announced at least a few months (or even longer) in advance, children have arguably less time to know the start date of the subsidy.

[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 potentially viewed as a particular form of moral hazard (Cabral 2017). If so, the estimates and corresponding elasticities without removing the data may provide the upper bound.

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

[23] While most of the literature uses arc elasticity rather than semi-arc elasticity, the arc elasticities, which are defined by $\varepsilon_A = \left(\frac{Q_{1A}-Q_{0A}}{(Q_{0A}+Q_{1A})/2}\right)/\left(\frac{P_{1A}-P_{0A}}{(P_{1A}+P_{0A})/2}\right) = \left(\frac{\beta_A}{Q_{0A}+Q_{1A}}\right)$, are not well defined when the starting price is zero, as in our case. This is because they reflect only the changes in quantity but not the changes in price. Thus, instead, we report the *semi*-arc elasticity, which is defined by $\varepsilon_A = \left(\frac{Q_{1A}-Q_{0A}}{(Q_{0A}+Q_{1A})/2}\right)/(P_{1A} - P_{0A}) = \left(\frac{\beta_A}{Q_{0A}+Q_{1A}}\right)/\left(\frac{0-0.3}{2}\right) = \left(\frac{\beta_A}{Q_{0A}+Q_{1A}}\right)/0.15$. See Online Appendix B for details.

by a single price, as the price is often nonlinear. Nonetheless, these numbers are considerably smaller than the range of –2.11 and –2.26 that Brot-Goldberg *et al*. (2017) calculate from the RAND HIE for the nonelderly and similar to –0.55 at age 7 years and –0.88 at age 20 years by Nilsson and Paul (2018) in one region of Sweden.[24] See Appendix B for more details on the derivation of these elasticities.

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 one gets older: with the subsidy, the outpatient spending increases by 1,380 JPY (13.8 USD) per month at age 7 years, and by 998 JPY (9.98 USD) per month at age 14 years compared to those without the subsidy. These estimates correspond to 18–31% increases from 4,490 JPY (44.9 USD), which is the mean value for ages 7–14 years without subsidy. Semi-arc elasticities also decline slightly with age, decreasing from –0.74 at age 7 years to –0.63 at age 14 years. To save space, all the corresponding tables of these figures are reported in Appendix Table C-1.

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 117 billion JPY (1.17 billion USD) if free care is expanded to all municipalities in Japan.[25] This creates a substantial negative fiscal externality to many stakeholders: while the municipality is responsible for covering only 30% of total cost (i.e., the amount of the subsidy), the remaining 70% of the subsidy-induced excess spending must be financed by taxes and premiums.

5.3. Robustness checks

We subject these results to a series of robustness checks. For brevity, we leave the detailed descriptions of the exercises to Appendix D. Critically, the results in Figure 5 on the causal effects of patient cost-sharing are robust across all specifications considered.

First, we address the potential concern that our control group—namely, children in municipalities without changes in subsidy—exhibits a different time trend to children in municipalities with subsidy changes. 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. Nonetheless, we add the time-by-municipality fixed effects (where time is measured in months) to account for the time-varying municipality characteristics that can

[24] For a direct comparison with 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 –0.17 to –0.31 from the RAND HIE (Keeler and Rolph 1988), and similar to Han *et al.* (2016), which document arc elasticities of –0.12 and –0.08 for regular and emergency outpatient care among 3-year-old children in Taiwan, respectively. See Appendix Table A-4 for comparisons with the related literature.

[25] We multiply each β_A by the number of children aged A in 2015 and sum them to calculate monthly spending. Then, we multiply the outcome by 12 to convert to annual spending.

be potentially correlated with both the expansion of the subsidy and utilization. We are reassured that the estimates are barely changed in Figure D-1. We also limit our sample to only those individuals who experienced at least one change in subsidy status, and exploit only the timing of the changes. While the estimates are noisier owing to the smaller sample, the estimates are qualitatively similar. We also collapse the data at the municipality-age-time cells, which is the level of variation, to partially account for zero spending, but the estimates are almost identical.

Second, Figure D-2 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. Finally, Figure D-3 presents the estimates from two nonlinear models (one-part and two-part GLM), which may better account for the highly skewed distribution of outpatient spending with the large mass at zero (e.g., Mullahy 1998; Blough *et al.* 1999; Deb and Norton 2018). As shown in the figure, the estimates from these alternative models are qualitatively very similar to the OLS estimates. Again, for ease of estimating the bootstrapped standard errors for our elasticity measures, we report the OLS estimates throughout the paper.

In Appendix E, we report the results on frequency of outpatient visits as outcomes. The semi-arc elasticities are similar in magnitude to those of outpatient spending.[26] 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 the increases in outpatient use are driven by both extensive and intensive margins.[27]

In Appendix F, we examine each type of medical service: the medication accounts for more than half the share of total spending (54.1%), followed by consultation fees (18.4%), laboratory tests (17.2%), and nonsurgical procedures (5.3%).[28] The consultation fees—which are charged at each visit and thus, are closely related to the frequency—are least price sensitive. On the other hand, the medical services related to the treatment intensity, specifically laboratory tests (including imaging) and nonsurgical procedures, are more price sensitive. This result is consistent with our finding that the spending conditional on positive spending also increases. Interestingly, the medication is not as price sensitive as other service categories are.

[26] 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). Appendix Figure E-2 shows that the estimates are very similar.

[27] Since both outpatient spending and frequency of outpatient visits increase by similar magnitudes, subsidy does not affect the spending *per* visit (results are available upon request).

[28] Note here that medication includes fees not only for medicine itself but also those related to prescribing and dispensing medications, including fees at the pharmacy.

5.4. Price responsiveness by health status

Here, we examine the heterogeneity by patient health status.[29] One might expect that, 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. If true, a generous subsidy or lack of it would have relatively little effect on sicker patients. If, on the other hand, sicker patients are more price responsive, lack of subsidy may substantially affect the chance of the sick to receive care.

Our longitudinal data allow us to examine history-dependent demand responses. We determine each child's health status by the outpatient spending in the first 6 months, since each child is observed in the claims data at different times. Then, we divide children into two types (i.e., sicker or healthier) by the median spending in each cell: (age in years)×(with or without subsidy) at the first 6 months of observations.[30] Previous studies have used prior spending as an indicator of health status (e.g., Dranove *et al.* 2003).

Appendix Figure G-1 shows that healthier children are much more price sensitive than sicker children are. For an outpatient dummy, while the semi-arc elasticities for the sick range from –0.36 to –0.50, those for the healthy range from –0.80 to –1.07, which is considerably larger in magnitude than that for the sick at any age. While it is slightly noisier, the same observation holds for outpatient spending. These results indicate that it is not sickly children but healthy children who cut back medical care most in the absence of a generous subsidy. These results imply that subsidy-induced medical spending for healthier children is more discretionary.

5.5. Asymmetric price responses

In this subsection, we investigate whether children asymmetrically respond to the price of healthcare by exploiting the unique variation of price changes in opposite directions. To do so, a subsidized dummy in equation [3] is decomposed into two sets of dummies as

$$Y_{iamt} = \alpha + \sum_{A=7}^{14} \beta_A^{better}\{subsidized_{iamt} \times better_{iamt} \times 1(Age\ A)\} + \sum_{A=7}^{14} \beta_A^{worse}\{subsidized_{iamt} \times worse_{iamt} \times 1(Age\ A)\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt} \quad \text{–[4]}$$

where $better_{iamt}$ is an indicator equal to zero before the subsidy is not available, and equal to one in all periods after the subsidy is introduced, even if the subsidy expires. We define $worse_{iamt}$ similarly.[31]

[29] A few papers examine the heterogeneity in price responsiveness by the 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).

[30] Appendix Figure G-2 experiments with different windows (9 and 12 months) to calculate the patient health status and finds qualitatively similar results across the windows.

[31] Currie *et al.* (2015) employ a similar strategy to examine the asymmetric effects of the opening and closing of toxic plants on housing values. Note that this way of constructing variables in our data only makes sense when the changes of the subsidy status within the individual are less than or equal to two. Thus, we remove 921 individuals (1.45%) who

Because of the way in which the indicators are defined, β_A^{better} tests the effect of the *decrease* in cost-sharing on utilization, relative to individuals in other municipalities without the subsidy, after the subsidy is expanded, relative to the period when the subsidy was not available. β_A^{worse} can be interpreted in an opposite manner.

Figure 6 graphically presents estimating equation [4], which plots β_A^{better} and β_A^{worse} for each age (A=7–14 years) in the upper half and the corresponding semi-point elasticity in the lower half.[32] Here, we report semi-*point* elasticity instead of semi-*arc* elasticity, as we exactly know the starting price as well as the direction of the price change. Panels A and B report the results of an outpatient dummy and outpatient spending, respectively.

Two things are noteworthy. First, the estimates take completely opposite signs for opposite directions of changes in subsidy status, indicating that our estimates are not driven by just one direction of price change. For both outcomes, the estimates are statistically significant at the 1% level at any age range. Second, while the semi-point elasticity for an outpatient dummy is slightly larger in magnitude for subsidy expansion ("better") than subsidy expiration ("worse") at some ages, the semi-point elasticities for outpatient spending are nearly identical for both directions of price changes.[33] Since we are eventually interested in the overall spending, we conclude that there is little evidence of asymmetric price responses.

The nonexistence of asymmetry at least in this setting has an important implication, as the price sensitivity estimated from one direction of price change may be applicable to the opposite direction of price change. In addition, it is very useful for welfare analysis, as the standard welfare calculation does not differentiate the direction of price changes. Since we observe little asymmetry in price sensitivity for our baseline results, we focus on the estimates from equation [3] without asymmetry hereafter.

5.6. Effect of small copayment

To date, we limit the sample to 165 municipalities that only have either 0% or 30% of coinsurance rates during our sample period, mainly because of statistical power and simplicity of interpretation. 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 200 JPY (or 2 USD) or 500 JPY (or 5 USD) per visit.

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experience more than two changes in subsidy status. We confirm that our baseline estimates are essentially unchanged once we remove these individuals from the sample (results are available upon request).

[32] Specifically, the semi-*point* elasticity for each direction of price changes is defined as: $\varepsilon_A^{better} = \left(\frac{Q_{1A}-Q_{0A}}{Q_{0A}}\right)/(P_{1A}-P_{0A}) = -\left(\frac{\beta_A^{better}}{Q_{0A}}\right)/0.3$, $\varepsilon_A^{worse} = \left(\frac{Q_{0A}-Q_{1A}}{Q_{1A}}\right)/(P_{0A}-P_{1A}) = \left(\frac{\beta_A^{worse}}{Q_{1A}}\right)/0.3$, where β_A^{better} and β_A^{worse} are estimates from equation [4]. See Online Appendix B for details.

[33] This is because, while the numerator in semi point-elasticity is larger for "worse" (β_A^{worse}) than "better" (β_A^{better}), the denominator is also larger for "worse" (Q_{1A}) than "better" (Q_{0A}) for an obvious reason.

This subsection exploits these variations to observe how a small copayment affects demand relative to free care. Here, we utilize all the observations (*full* sample) and all the price variations to examine the effect of a small copayment, while we recognize the lack of statistical power to gain meaningful estimates for some outcomes.

Specifically, we expand the basic equation [3] to include all types of subsidies:

$$Y_{iamt} = \alpha + \sum_C \sum_{A=7}^{14} \beta_A^C \{1(price = C) \times subsidized_{iamt} \times 1(Age\ A)\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt} \quad \text{–[5]}$$

where C takes all levels of coinsurances rates (C=10, 15, 20, 30%) as well as copayments (C=200, 300, 500 JPY/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 report only the estimates on two small copayments (C=200 and 500 JPY/visit or 2 or 5 USD), for both of which we have relatively modest sample sizes (the full estimates are available upon request).

To obtain a better idea about the magnitude of patient cost-sharing for these two small copayments, we compute the share of out-of-pocket payment for these copayments. Concretely, we divide the out-of-pocket payment (number of visits per month times the copayment) by the total monthly outpatient spending. The share of out-of-pocket cost for 200 JPY and 500 JPY per visit are 2–3%, and 5–7%, respectively, which is substantially smaller than that of 30% coinsurance, suggesting that these two copayments impose much lower cost-sharing.

The upper graph in Figure 7 plots the series of β^C (age A is suppressed), where the outcome is a visit dummy, for two levels of copayment (β^{200} and β^{500}) together with the full cost ($\beta^{30\%}$), just as a reference. Note that the estimates on 30% coinsurance ($\beta^{30\%}$) merely flip the sign of the main estimates reported in Figure 5, as the treatment and control groups are just reserved here. Figure 7 shows that even though the estimates for two levels of copayment are smaller than that of full cost (6–8 percentage points), both small copayments reduce the probability of an outpatient visit by 2 to 4 percentage points.

We then convert these estimates into semi-arc elasticities, which are the metrics comparable across different price levels.[34] The bottom half of Figure 7 plots the semi-arc elasticities for three price

[34] The semi-arc elasticities for each price C (age A is suppressed) are written as: $\varepsilon^C = \left(\frac{2(Q^C - Q^0)}{Q^0 + Q^C}\right) / (P^C - P^0) = \left(\frac{2\beta^C}{Q^0 + Q^C}\right) / P^C$ where β^C are estimates from equation [6]. Q^0 is the average outcome at free care (C=0%), which is common across all the price levels, and Q^C is the average outcome at each price C. Similarly, P^C is the fraction of out-of-pocket at each price C (see Online Appendix B for details). Since β^C is similar across two copayment levels, Q^C (which can be expressed by Q^C=Q^0+ β^C) is by construction similar to each other, as Q^0 is common. Thus, the remaining P^C is the important determinant of the differences in ε^C between the two copayment levels.

changes. Interestingly, while the elasticities for the two copayment levels are noisy and have wide confidence intervals, the semi-arc elasticities of the smaller copayment (ε^{200}) are substantially larger than those of the larger copayment (ε^{500}). This occurs because the changes in price (P^{C}) that drive the similar magnitude of changes in outcomes is much smaller for 200 JPY (≈ 2–3%) than 500 JPY/visit (≈ 5–7%), leading to larger elasticity for the 200 JPY/visit. Furthermore, the semi-arc elasticities for the larger copayment (ε^{500}) are larger than that of 30% coinsurance ($\varepsilon^{30\%}$).[35] This result indicates that a small copayment can be an effective tool to reduce moral hazard associated with free care.

These results are related to two existing strands of literature. First, our results are broadly consistent with the "zero-price" effect, according to which people might 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 utility loss due to having to give up the free product (Hossain and Saini 2015). Our results imply that relative to free care, a small positive price would substantially reduce healthcare utilization. Second, our results echo the recent argument that representing elasticities by a single number is potentially problematic (Aron-Dine *et al.* 2013). While the context is different, past literature provides such evidence by exploiting the variations in a nonlinear budget set induced by deductible and stop-losses.[36] Exploiting the variations in cost-sharing across municipalities and ages, we find that depending on the choice of two price points, price elasticities can be substantially different.

Again, we should view these results with caution, as they are not fully robust to other outcomes. In fact, Appendix I presents the same estimates from equation [5] when the outcomes are the frequency of outpatient visits and outpatient spending. While the estimates for the frequency of visits show qualitatively similar patterns to the visit dummy, the estimates on spending are substantially noisier and detract from making any conclusive statements.[37] For the analysis in Section 6, we return to the main sample with either 0% or 30% of coinsurance rates during our sample period.

6. Beneficial or low-value care

The remaining important question is whether the increased outpatient utilization owing to lower price (moral hazard) reflects beneficial or low-value care. In fact, the recent work by Baicker *et al.*

[35] We are aware that copayment and coinsurance are conceptually different, and thus, direct comparison warrants considerable caution. From the patient perspective, the marginal out-of-pocket cost is essentially zero after paying the copayment. On the other hand, the marginal out-of-pocket cost is always at the coinsurance rate, implying that there is more scope for the patients to demand care and/or suppliers to provide care under copayment than under coinsurance.
[36] Examples include Ellis (1986), Dalton (2014), Aron-Dine *et al.* (2015), Einav *et al.* (2015), Kowalski (2015), and Brot-Goldberg *et al.* (2017).
[37] Our results on outpatient spending are not model sensitive. In fact, Appendix Figure I-2 shows that the estimates are very similar across different models (OLS, one-part GLS, and two-part GLS).

(2015) suggest that welfare implications of quantity changes depend on how they occur. This is always a challenging task, as we recognize that subsidy-induced utilization should include some aspects of both essential and nonessential care, as documented in RAND HIE (Newhouse and the Insurance Experiment Group 1993). Thus, in order to answer this question as best as possible, we focus on the relatively extreme cases of utilization patterns. In particular, we take the following two approaches. First, we examine whether we can find any evidence of increases in "beneficial" care (Subsection 6.1). Second, we examine whether we can find any evidence of low-value or costly care (Subsection 6.2). If we find little evidence of the first, and ample evidence of the second, the weight of evidence would support the view that a generous subsidy for child healthcare leads to increases in unnecessary and costly visits.

6.1. Evidence of "beneficial" care

We start by investigating whether subsidy-induced care clearly benefits children. For this, we examine whether increases in outpatient care prevent avoidable inpatient admissions and reduce short-term mortality. Furthermore, we examine whether childhood subsidy reduces healthcare utilization and improves health outcomes when children become adolescents.

6.1.1. Ambulatory Care Sensitive Conditions (ACSC)

We start by examining whether outpatient care prevents avoidable inpatient admissions. Instead of looking at broad disease categories or choosing them arbitrarily, a useful set of preventive care is the utilization for the so-called Ambulatory Care Sensitive Conditions (ACSC) or "avoidable conditions"—diagnoses for which timely and effective outpatient care can help to reduce the risks of hospitalization by preventing the onset of an illness or condition (e.g., asthma).[38]

We employ the ACSC list from Gadomski *et al.* (1998), who specifically focus on children.[39] Appendix Table J-1 provides the lists of the ACSC 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 belongs to ACSC, which verifies the acute nature of diseases for children. 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.

Figure 8 plots the estimates from equation [3] when the outcome is an outpatient dummy for (i) any ACSC, (ii) severe ENT infections, and (iii) asthma.[40] Panel A shows that outpatient visits for these

[38] Nonetheless, we estimate equation [4] by the broad diagnosis groups as indicated in Appendix Table A-3. We do not find much heterogeneity except for the "Injury, poisoning and certain other consequences of external causes," which shows a slightly smaller elasticity, as these conditions may be more urgent and less discretionary (results are available upon request).

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

[40] Unfortunately, insurance claims data in Japan list all the ICD10 diagnosed in the month instead of diagnosis for each visit, making it difficult to examine spending and frequency of visits by ICD10.

diagnoses increase when subsidized, and all the estimates are statistically significant at the conventional level. For example, the outpatient visit by any ACSC increases by 2–4 percentage points during the ages of 7–14 years, where the mean without subsidy is 0.11.

These results at a glance seem consistent with the literature—that people are price sensitive not only to nonessential or low-value care but also to essential care. For example, RAND HIE documents that price sensitivity for preventive care is similar to that for acute or chronic care among children (Leibowitz *et al.* 1985). However, most past studies could not examine whether such seemingly beneficial care indeed leads to better health of patients or 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. Thus, we can directly examine whether such increases in preventive care in the outpatient setting indeed lead to reduction in hospitalization.

Panel B in Figure 8 plots the estimates from equation [3], where the outcome is an *inpatient* dummy while the explanatory variables are subsidy status for *outpatient* care as before.[41] We do not observe any declines in the hospitalization associated with any ACSC or other individual ACSCs. Since hospitalization among children is very infrequent (0.28% of all person-months), the estimates are overall imprecise. However, at least the point estimates are always positive instead of negative and even statistically significant at some ages in the case of asthma. Since the benefit of preventive care may emerge with a lag, we also estimate a variant of equation [3], 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).

6.1.2. Offset effects

More generally, we examine the possibility of the cross-price effect (widely known as the "offset" effect in health economics)—whether *outpatient* spending replaces *inpatient* spending. If patients cut spending for preventive outpatient care in response to a price increase in outpatient care and, consequently, need to be hospitalized later, then cost-saving through reduction in outpatient care can be eventually "offset" by the subsequent increase in costly inpatient admission. However, it is also possible that if the outpatient visits lead 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 simply increase the chance of detecting serious health problems, a price increase in outpatient care will decrease both outpatient and inpatient care use. Note that the analysis on ACSCs in the previous subsection is a special case of the "offset" effect, which focuses on conditions specific to ACSCs.

[41] Whenever we examine the inpatient outcomes, we also control for the subsidy for inpatient care, while adding these variables does not affect our results, as there is little variation in inpatient subsidy.

Whether outpatient care is a substitute for or complement to inpatient care is an important but unsettled question in health economics. Overall, 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 the evidence of offset effects are concentrated among the elderly (e.g., Chandra *et al.* 2010, Trivedi *et al.* 2010). To our knowledge, there is no study that examines the cross-price effects for child healthcare except for RAND HIE, which lacks statistical power for making decisive conclusions owing to its very small sample size of children (1,136 children whose families participated in a randomized trial).

To investigate this question, we replace the outcome in equation [3] with an inpatient dummy or inpatient spending while the explanatory variable is still subsidy for outpatient care. In this way, we can investigate whether the change in subsidy for outpatient care has any impact on inpatient care.[42]

Panel A of Figure 9 plots the estimates on the probability of hospitalization, and Panel B plots the estimates on inpatient spending. Out of 16 point estimates (8 for each age for each outcome), none of the estimates except one are statistically significant at the conventional level (see Appendix Table K-1). In addition, the estimates are mostly positive albeit statistically insignificant, implying that the generous subsidy for outpatient care does not reduce the utilization of inpatient care and, if anything, increases hospitalization, despite the large increases in outpatient visits and spending documented so far. These results echo the findings on ACSC in the previous subsection.

6.1.3. Short-term mortality

Next, we investigate whether subsidy affects the most drastic health outcome: short-term (cotemporaneous) mortality. We recognize that the mortality rate among this age range in Japan is extremely low (in fact, there are only 68 deaths or 0.107% of our sample) and hence, we may lack the statistical power to draw precise inferences.[43] Nonetheless, we still examine mortality for the sake of completeness, as we believe that it is arguably the most objective health outcome.

In this analysis, instead of simple OLS, we account for the interval-censored nature of the mortality data in a discrete-time duration model (Jenkins 1995). Specifically, we estimate the following complementary log-log regression model (which is a discrete analog of the continuous proportional hazards model) through maximum likelihood:

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[42] Following Kaestner and Lo Sasso (2015), we also estimate the effect of outpatient spending on inpatient use by instrumenting outpatient spending by outpatient subsidy. We find that the estimates β_A are always positive, as in Figure 9, but they are not precisely estimated (results are available upon request).

[43] The mortality dummy takes one if either 1) enrollment data indicate death as the reason for drop-out from the data, or 2) claims data indicate death as a result of treatment at the medical institutions. We recognize that our data might not capture all deaths if, for example, children die outside of medical institutions (e.g., home) *and* the death is not reported to the insurers, although as many as 83.1% of children aged 5–14 years die in a hospital (Ministry of Health, Labour and Welfare 2010).

$$Pr(death = 1)_{iat} = \alpha + \sum_{A=7}^{14} \beta_A \{subsidized_{iat} \times 1(Age\ A)\} + f(a) + \delta_{year} + \theta_{month} + \gamma Female_i + \varepsilon_{iat} \quad \text{–[6]}$$

where $Pr(death = 1)_{iat}$ takes one if child i dies at age a (in months) in time t (in months), and $f(a)$ captures the underlying baseline hazard. We also control for year fixed effects, month fixed effects, and a female dummy.[44]

Appendix Figure L-1 plots the series of β_A when $f(a)$ is log of age in months. While the estimates are somewhat noisy, none of the estimates at any age is statistically and economically significant. We also experiment with different functional forms of $f(a)$, including linear in age, and age (in year) dummies, which capture the underlying baseline hazard more flexibly. Appendix Table L-1 shows that the estimates are very similar. Thus, at least in the short run, we find little evidence on reduction in child mortality; however, we need to interpret this finding with considerable caution owing to the very low child mortality rate.

6.1.4. Medium-term utilization and health outcomes

Finally, we explore the medium-run effects of subsidy on healthcare utilization and health outcomes. It is possible that a generous subsidy during childhood increases beneficial care and makes children healthy, which, in turn, may reduce healthcare utilization and improve health outcomes when they become adolescents.

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 subsidy that children receive before the age of 15 years substantially differs by municipalities. Thus, we can relate the length of the free care until the age of 15 years with healthcare utilization and health outcomes *after* the subsidy expiration at 15 years. In this way, we can cleanly identify the medium-run effects of subsidy. This approach is in the same spirit as the recent studies that relate Medicaid eligibility during childhood to later-life utilization and health outcomes (e.g., Wherry *et al.* 2018; Brown *et al.* 2016). Our study has an advantage in that we can more accurately measure the length of free care that each individual was exposed to.

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

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

[44] We cannot include year-month fixed effects and municipality fixed effects, since there is no death at every year-month and in every municipality. As a result, many observations are dropped from the sample if we include them in the complementary log-log regression.

subsidy information (9 years) as well as 1 full year of utilization at the age of either 16 years or 18 years. This minimizes the measurement errors in the length of free care. Specifically, for the cohorts who were aged 6 (8) years in April 2005—the start of our sample period—we observe one year of utilization at age 16 (18) years as well as the subsidy information at ages 6–15 (8–17) 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 (8) years. For the samples aged 16 (18) years, we have a total of 3,643 (3,426) individuals. 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 with SD of 2.09 (2.06).[45] The minimum and maximum are 4 years and the full 15 years, respectively, for both samples.

With this set-up, Panel A of Figure 10 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-month cohort×municipality. The dotted line is the predicted values of weighted least square regressions, 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 –0.066 (p-value= 0.760), 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 66 JPY (or 0.66USD).[46] We also examine outpatient and inpatient care separately, but the results are qualitatively very similar (not shown).[47] The slope for the sample aged 18 years is even slightly positive, 0.406 (p-value= 0.341) and again economically small.

One might argue that utilization might not fully capture the health status of children, if for example, children form a habit of seeing a doctor owing to longer subsidy exposure. To account for this concern, we next examine the occurrence of serious chronic health problems at the ages of 16 or 18 years. Feudtner *et al.* (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

[45] The age 16 (18) sample includes 12 birth-month cohorts born between April 1998 and March 1999 (April 1996 and March 1997). Among 3,678 (3,463) individuals, we exclude 35 (37) individuals who live in municipalities that provide subsidy after age 15. The average length of free care is longer for the age 16 sample than the age 18 sample is because the former is younger and free care has expanded over time, as shown in Figure 1.
[46] We formally run the specification where we also include birth year-month fixed effects (12 cohort fixed effects) and a dummy for female. The results are essentially the same (results are available upon request).
[47] We also investigate whether the 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).

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."[48] Appendix Table M-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 10 plots the relationship between the length of free care and an outcome that takes 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 both samples aged 16 and 18 years are economically very small (0.0001 and 0.0010) and far from statistically significant (p-value= 0.957 and 0.674).

Our results contrast with the recent studies on Medicaid that find some positive effect of Medicaid eligibility on long-term health outcomes.[49] 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.

6.2. Evidence of low-value or costly care

Then, we next turn to examine whether we can find clear evidence that subsidy induced care results in low-value or costly care.

6.2.1. After-hours visits

One concern with a generous subsidy is that children (and hence, mothers) exploit the opportunity by increasing after-hours visits outside of regular hours, because additional fees for after-hours visits are also subsidized. This may place a substantial burden on the workload of physicians as well as increase medical spending as the fees for after-hours visits are set higher by a national fee schedule. On the other hand, if these visits are indeed urgent and not discretionary, the generous subsidy may have little impact on this type of visit. While this issue has been raised repeatedly in the media, no formal analysis has

[48] 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 0 to 18 years old.

[49] Examples include Medicaid introduction (e.g., Goodman-Bacon 2016; Boudreaux *et al.* 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).

been undertaken.[50]

We divide the visits into three categories: 1) regular-hour visits, 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-hour visits, and thus, from the billing information, we know the timing of the outpatient visit within a day.[51] Appendix Table N-1 provides the list of billing codes for these after-hours visits and midnight/holiday visits and corresponding additional fees. As a benchmark, fees for regular-hour visits during the sample period are approximately 2,800 and 700 JPY (28 and 7 USD) for the first visit and revisits, respectively. The additional fees charged for after-hours visits—which are typically 850 and 650 JPY (8.5 and 6.5 USD) for the first visit and revisits, respectively—are relatively high, making these visits costly. Moreover, the additional fees for midnight/holiday visits are set even higher than those of after-hours visits.

Figure 11 plots the estimates (β_A) for regular-hour visits (for references), after-hours visits, and midnight/holiday visits. Since the consultation fees are charged by each time of visit, the frequency of visits is the natural candidate of outcome. Since the majority of the visits are regular-hour visits (89.1% of total visits), Panel A shows a similar pattern to our baseline estimates reported in Figure 5, and the semi-arc elasticities are stable at approximately –0.6 throughout the age ranges.

Interestingly, Panel B of Figure 11 shows that costly after-hours visits—which account for 8.4% of total visits—also increase owing to subsidy. The estimates are slightly increasing in age and statistically significant at least above the age of 9 years. These results validate the concern that a generous subsidy increases the burden on the workload of physicians by inducing children to make after-hours visits. The semi-arc elasticities are also increasing in age, ranging from as low as –0.51 at the age of 7 years to –1.23 at the age of 14 years.

Importantly, the semi-arc elasticities for after-hours visits are much larger in magnitude than those of regular-hour visits at older ages. This indicates that at least at older ages, after-hours visits seem to be more discretionary and less urgent than regular-hour visits, casting some doubt on the provision of a generous subsidy for old children. By contrast, we observe no increases in midnight/holiday visits (which account for only 2.5% of total visits) in Panel C. The semi-arc elasticities are not statistically distinguishable from zero while they are not precisely estimated. These results suggest that the midnight/holiday visits are indeed very serious cases, and thus, children and mothers are less price elastic for these unavoidable visits, which seems plausible. Appendix Figure N-1 reports the estimates

[50] 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 2017).

[51] For example, suppose the regular hours of a clinic are registered from 9 am to 5 pm. As the midnight visits are normally defined by visits between 10 pm and 6 am, the visits outside of regular hours but not during midnight are considered to be the after-hours visits. Holiday visits are visits on the holiday.

on outpatient spending (instead of frequency of visits) with similar results.[52]

In summary, the subsidy for child healthcare seems to increase not only regular-hour visits but also costly after-hours visits, which may increase both the cost and workload of physicians.[53] From a policy standpoint, a *municipal* government subsidy partially undoes the effort of the *national* government to discourage costly after-hours visits for nonserious reasons by setting higher fees for these visits. However, we find no evidence that the subsidy increases midnight/holiday visits when the healthcare resources (e.g., physicians and nurses) are most scarce.

6.2.2. Inappropriate use of antibiotics

Another concern with generous subsidy for children is that it may increase the inappropriate use of medications. Since more than half of outpatient spending consists of medication and related expenses (54.1%), this is a valid concern worth investigating. In particular, the biggest worry is the use of antibiotics for diagnoses that are not recommended, because such inappropriate use leads to both antibiotic resistance and adverse events. For example, antibiotic-resistant infections annually affect at least 2 million people, and 23,000 people die as a direct result of these infections in the US (Centers for Disease Control and Prevention 2013). The Japanese government has only recently started addressing misuse of antibiotics by issuing a prescription guideline in 2017.

We follow Fleming-Dutra *et al.* (2016) to create the list of diagnoses for which antibiotics are not recommended. See Appendix O for details. Appendix Table O-1 presents the list with the corresponding ICD10 as well as summary statistics of antibiotic usage. For example, antibiotic use for children with bronchitis and asthma is considered inappropriate. Even without subsidy, roughly 20% of children diagnosed with any of these diseases are prescribed antibiotics (Column 5), pointing out the potential misuse of antibiotics for children in Japan. Similarly, the average antibiotics spending conditional on being diagnosed for any of them is 240 JPY (Column 6), and the frequency of antibiotic prescriptions is 0.94 per person-month (Column 7) without subsidy. Both numbers are far from zero.

It could be problematic if the subsidy were to increase the number of children in these diagnoses who were prescribed antibiotics. To investigate this possibility, we estimate equation [3] in which the outcome is the interaction of being diagnosed with any of these diseases and total spending on antibiotics in Panel A of Figure 12, and frequency of antibiotic prescriptions in Panel B. Panel A shows that the subsidy increases spending on antibiotics by 9 to 20 JPY, which is 17–38% from the mean.

[52] Note that the spending here includes only consultation fees and does not include any fees related to treatments during the visits.

[53] It is possible that the additional cost of after-hours visits may be partially offset by the opportunity cost of working mothers 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.

Similarly, the frequency of antibiotic prescriptions increases by 0.039 to 0.070 (20–36% from the mean) in Panel B. Thus, our results suggest that a generous subsidy increases the inappropriate use of antibiotics, potentially leading to more antibiotic-resistant infections and adverse effects.

7. Conclusions

Understanding the price responsiveness to healthcare is a central question in health economics and the 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 more than 5,000 regional and over-time variations on subsidy availability.

We find that the reduction in cost-sharing from 30% (national level) to 0% (free) increases outpatient spending by 22‐31% with the semi-arc elasticities at approximately –0.6 throughout ages 7–14 years. While considerable caution is needed when comparing the elasticities estimated across countries or time periods, the elasticities for children estimated in this study are considerably smaller than those of RAND HIE for nonelderly in the US, and Shigeoka (2014) and Fukushima *et al.* (2016) for the elderly in Japan. We also document other behavioral price responses. We find little evidence of asymmetric responses to the price changes of the opposite directions. Furthermore, we find substantially large price responses of introducing a small copayment to free care ("zero-price" effects).

We further examine the utilization patterns from various dimensions to understand whether changes in utilization largely reflect beneficial or low-value care. We show that the increases in outpatient visits do not translate to clear benefits in the form of reduction in hospitalization by "avoidable" conditions or improvement in short-run and medium-run health outcomes. We also document that the subsidy has some negative effects by increasing the inappropriate use of antibiotics and costly after-hours visits.

Taken individually, each piece of empirical evidence might not be sufficient to establish the existence of wasteful utilization. However, taken together, the weight of the evidence supports the notion that the drastic expansion of a child healthcare subsidy may lead to increases of low-value and costly outpatient visits. Importantly, 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 generous subsidy does not seem to have any meaningful positive impacts, and policy makers should be cautious about implementing such a policy.

This study is subject to a few limitations. The biggest limitation is that we cannot investigate long-term health outcomes except for short-term mortality and medium-term utilization. Since health is stock, better access to preventive care during childhood may translate into an improvement in long-run health beyond the ages of our sample, which could justify the generous subsidy for child healthcare. Another important limitation is that our insurance claims data do not include basic parental characteristics, such as income and education. This may be especially important in the case of young children, as their decision making is heavily influenced by mothers. To the best of our knowledge, such monthly data with age, municipality of residence, healthcare utilization, and household characteristics, do not exist in Japan—mainly because of the lack of an individual identifier in Japan, such as a social security number in the US—which leaves an avenue for future research.

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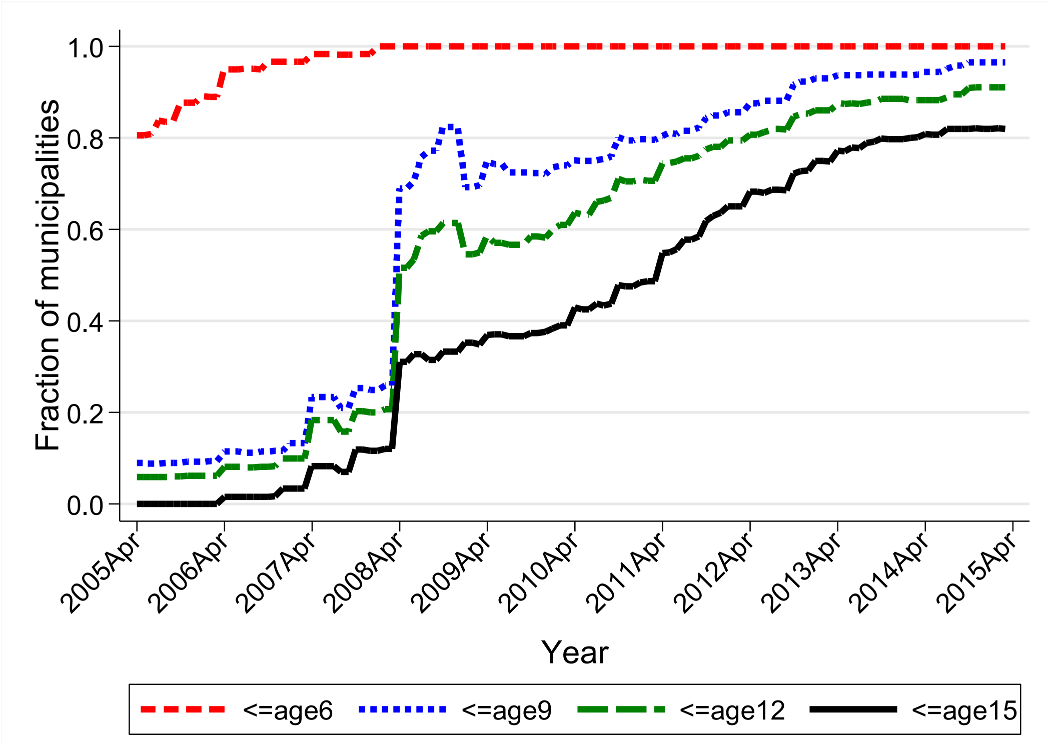
Figure 1: Time series of maximum age fully covered by child care subsidy

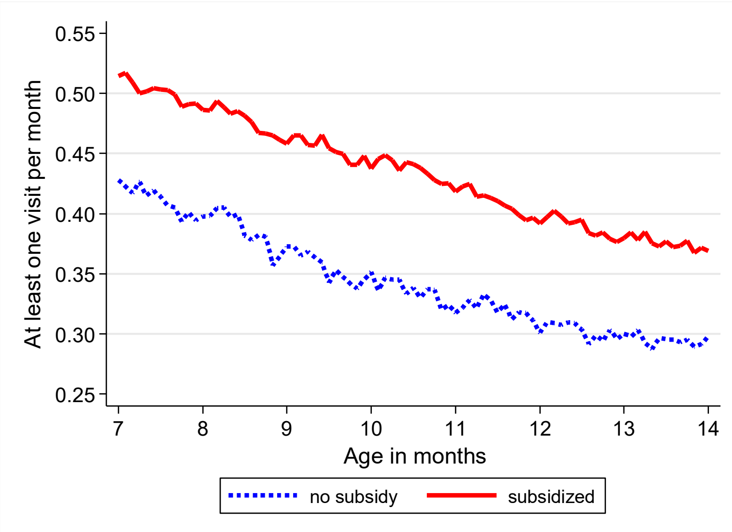


Notes: The data is unbalanced monthly panel where the unit of observation is municipality. There are total of 165 municipalities which are mainly used in this study (the main sample). Note that this figure reflects the compositional changes of municipalities, as the number of municipalities increases in the later period in our claims data. Importantly, within the municipalities, the subsidy expansion is always monotonic—that is, *no* single municipality *lowers* the maximum age during this period (April 2005–March 2015). The spike 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 age 3 to 6 (until the beginning of primary school). While Figure 1 clearly shows that all municipalities in our sample already provided the subsidy until the age of 6 years by April 2008, this national-level subsidy expansion eases the budgetary burden on municipalities, as part of the cost is now covered by the central government. For this reason, we observe the highest number of municipality-level subsidy expansions in April 2008 for ages above 6 years (see Appendix Figure A-1 on the precise timing of all policy changes).

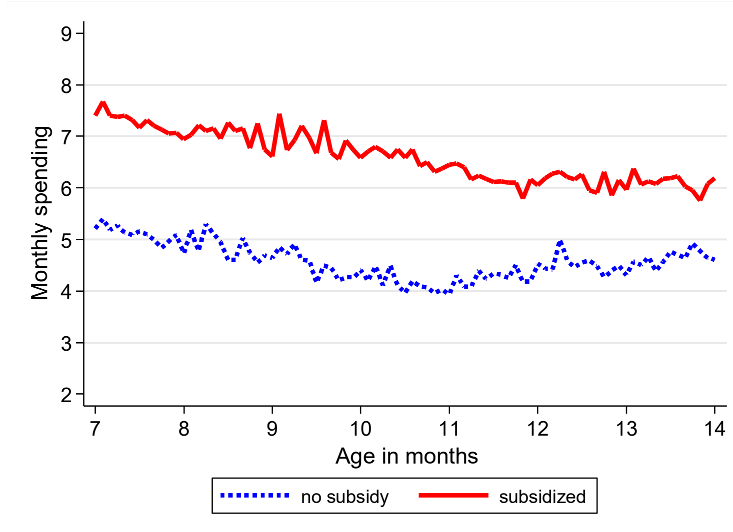
Figure 2: Utilization with or without subsidy by age

A. Outpatient care (monthly)

Outpatient dummy

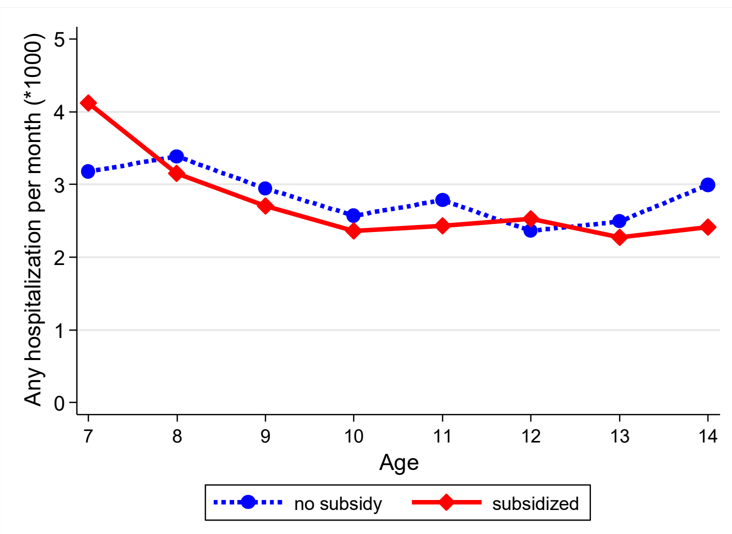


Outpatient spending (in 1000 JPY)

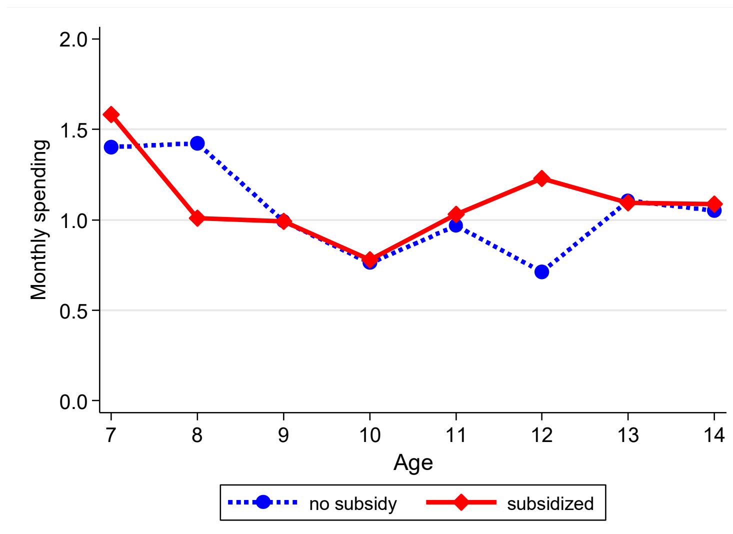


B. Inpatient care (yearly)

Inpatient dummy (×1000)

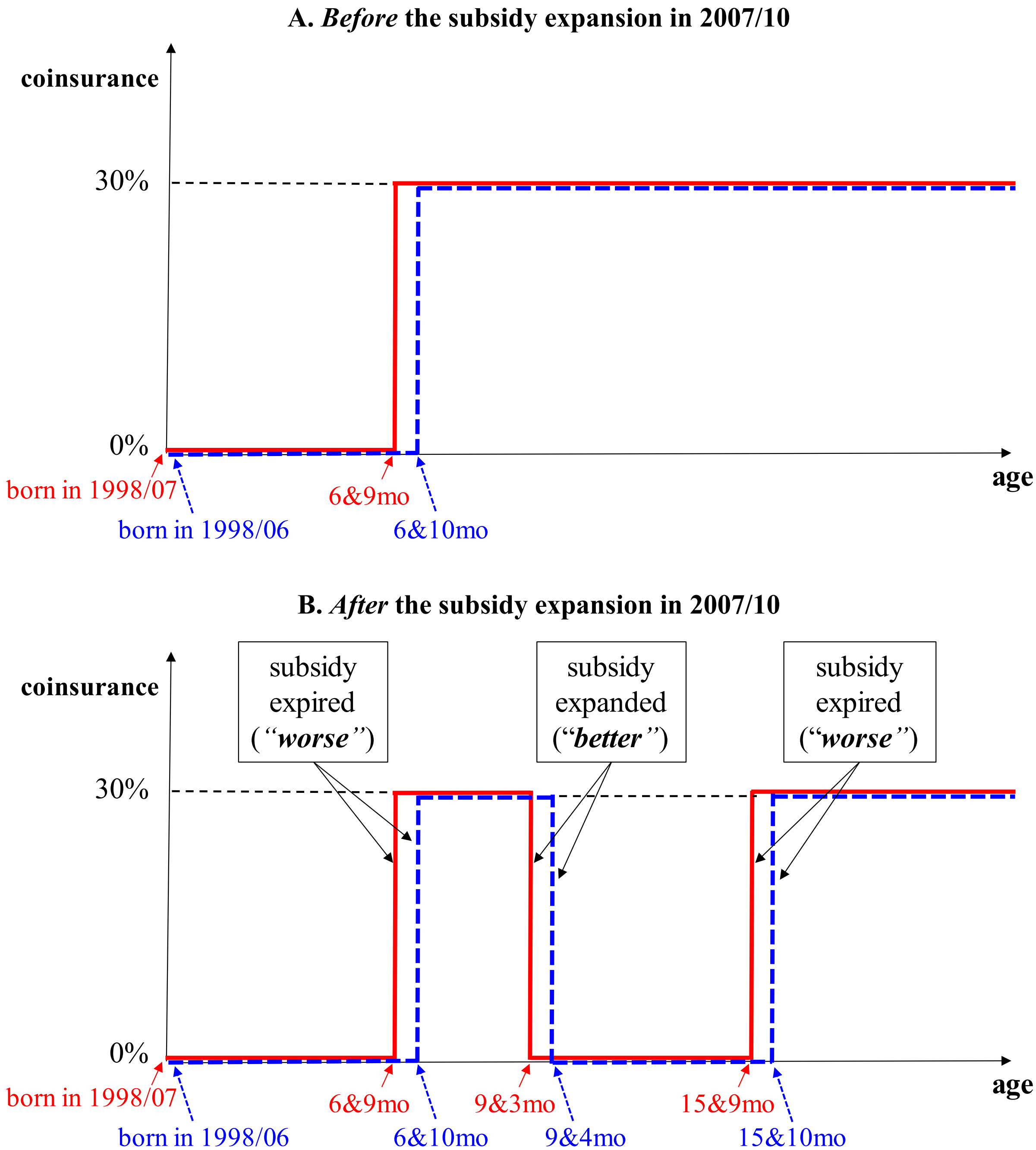


Inpatient spending (in 1000 JPY)



Notes: The main 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 yearly mean of inpatient outcomes 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 1000 JPY (approximately 10 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

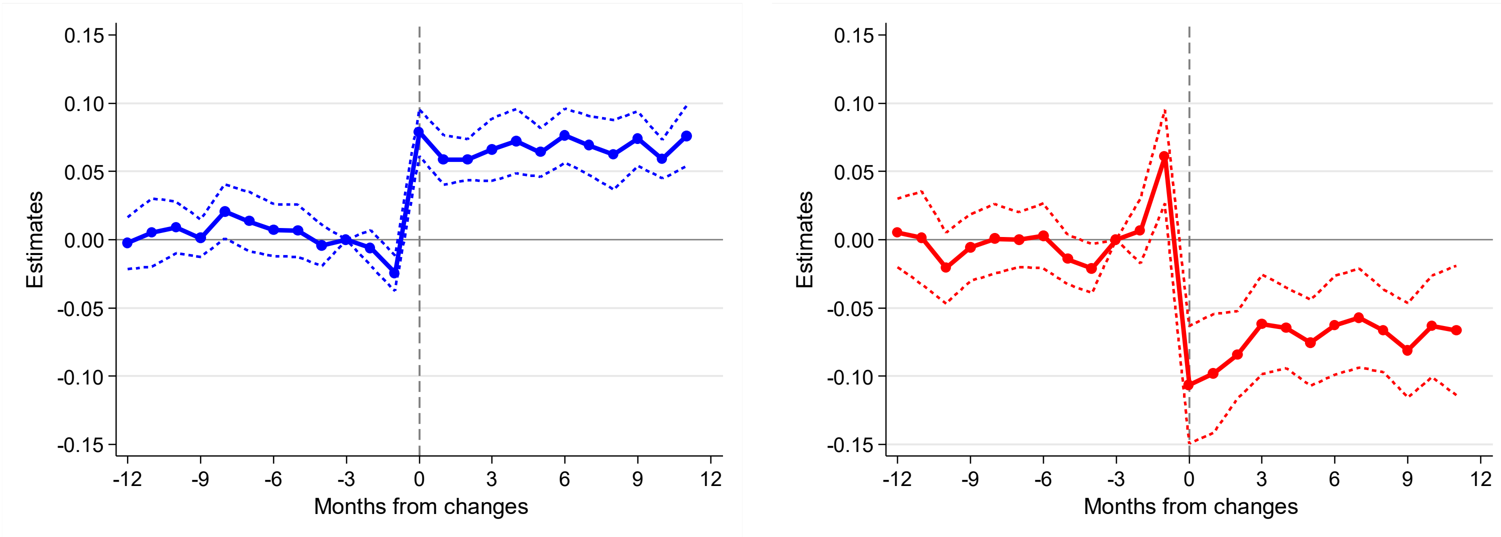


Notes: Panel A draws the price schedules for each cohort *before* subsidy expansion. The solid line draws the price schedule for a cohort born in July 1998 ("younger" cohort, hereafter), and the dotted line for a cohort born in June 1998 ("older" cohort, hereafter), born a month before the younger cohort. 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

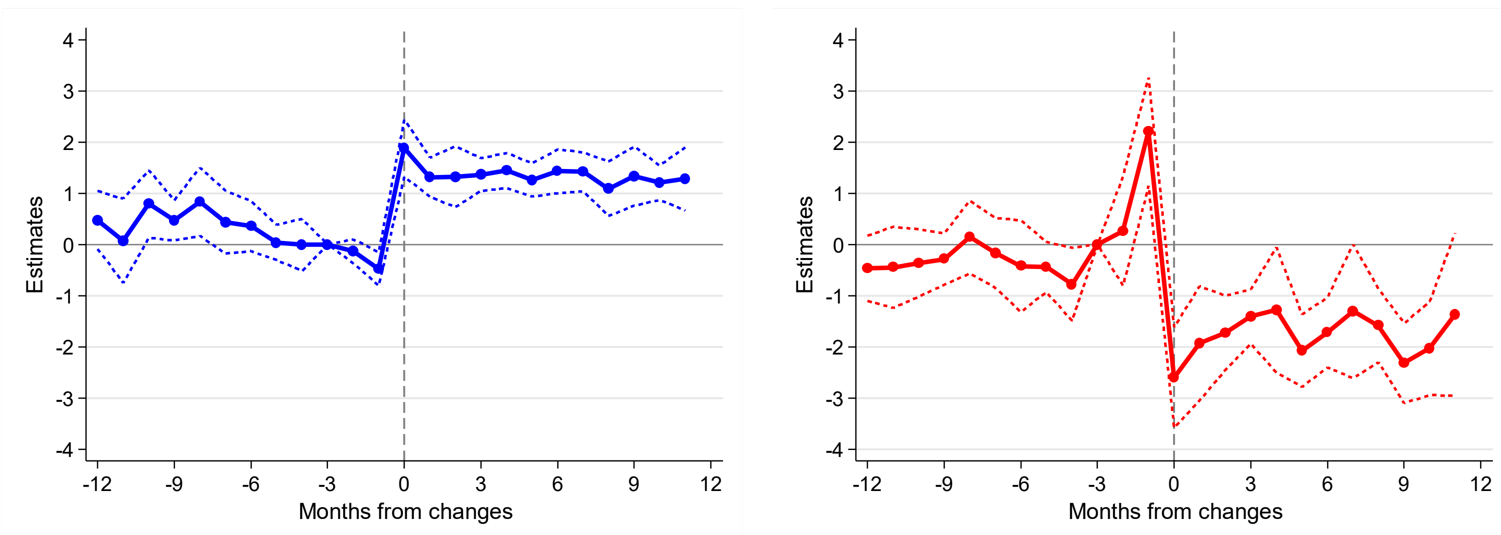
A. Outpatient dummy

Better (subsidy expansion) Worse (subsidy expiration)



B. Outpatient spending (in 1000 JPY)

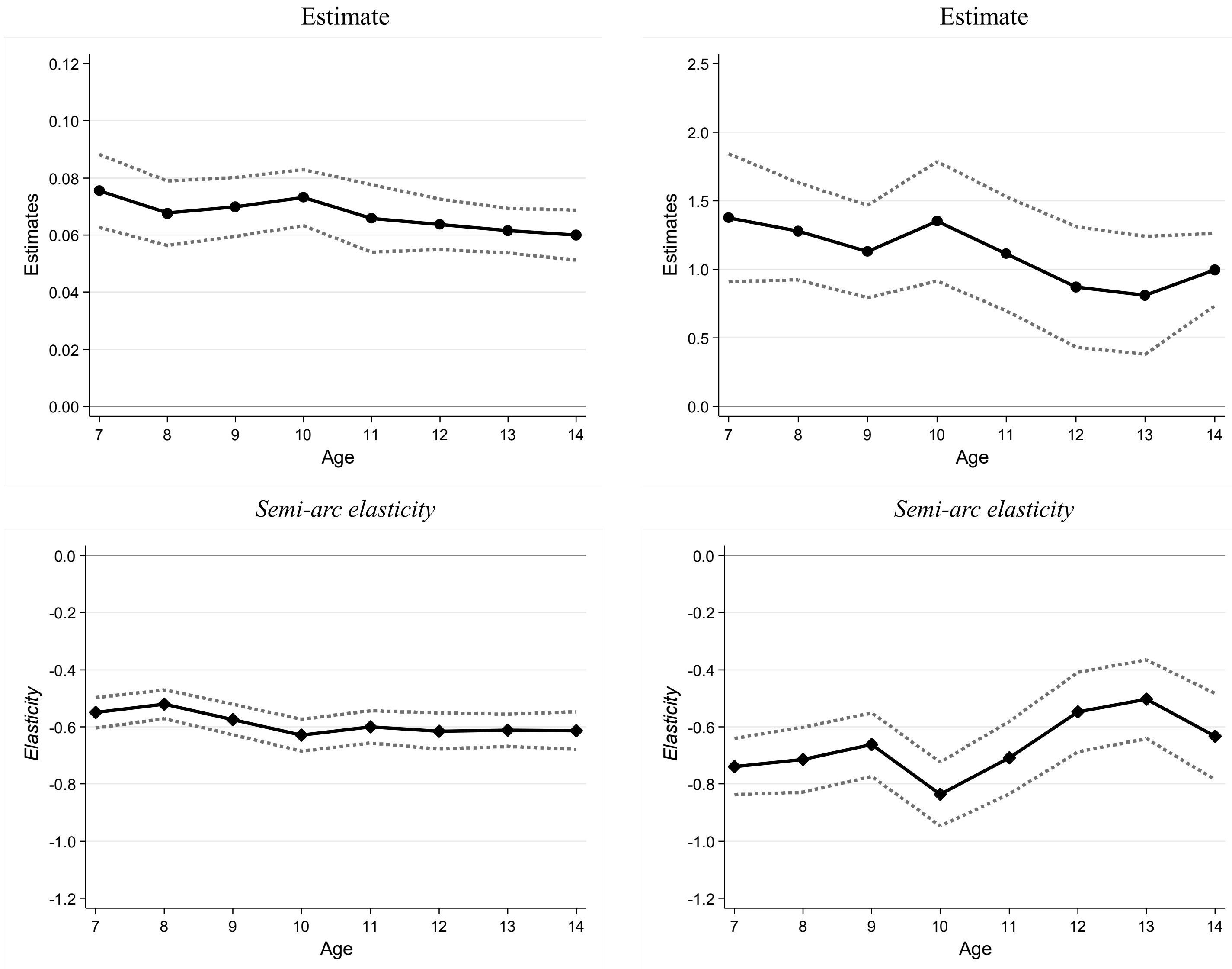
Better (subsidy expansion) Worse (subsidy expiration)



Notes: The main 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 1000 JPY (approximately 10 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 [3] 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

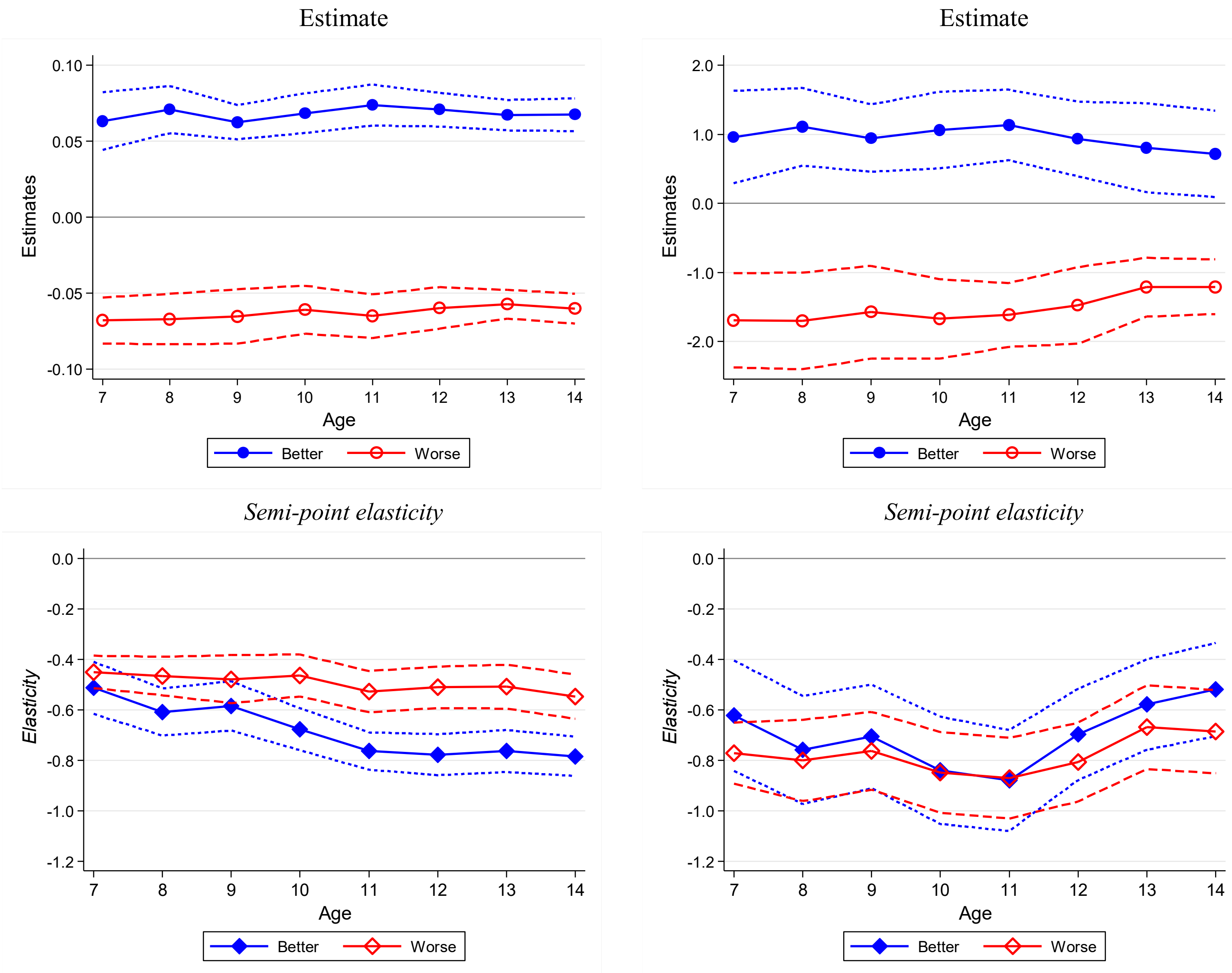
A. Outpatient dummy | **B. Outpatient spending** (in 1000 JPY)



Notes: The main 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 1000 JPY (approximately 10 USD). The upper half plots β_A for each age (A=7–14) from estimating equation [3], and the bottom half plots the corresponding semi-arc elasticity (See Online Appendix B for details). 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 C-1.

Figure 6: Asymmetric price responses

A. Outpatient dummy | **B. Outpatient spending** (in 1000 JPY)

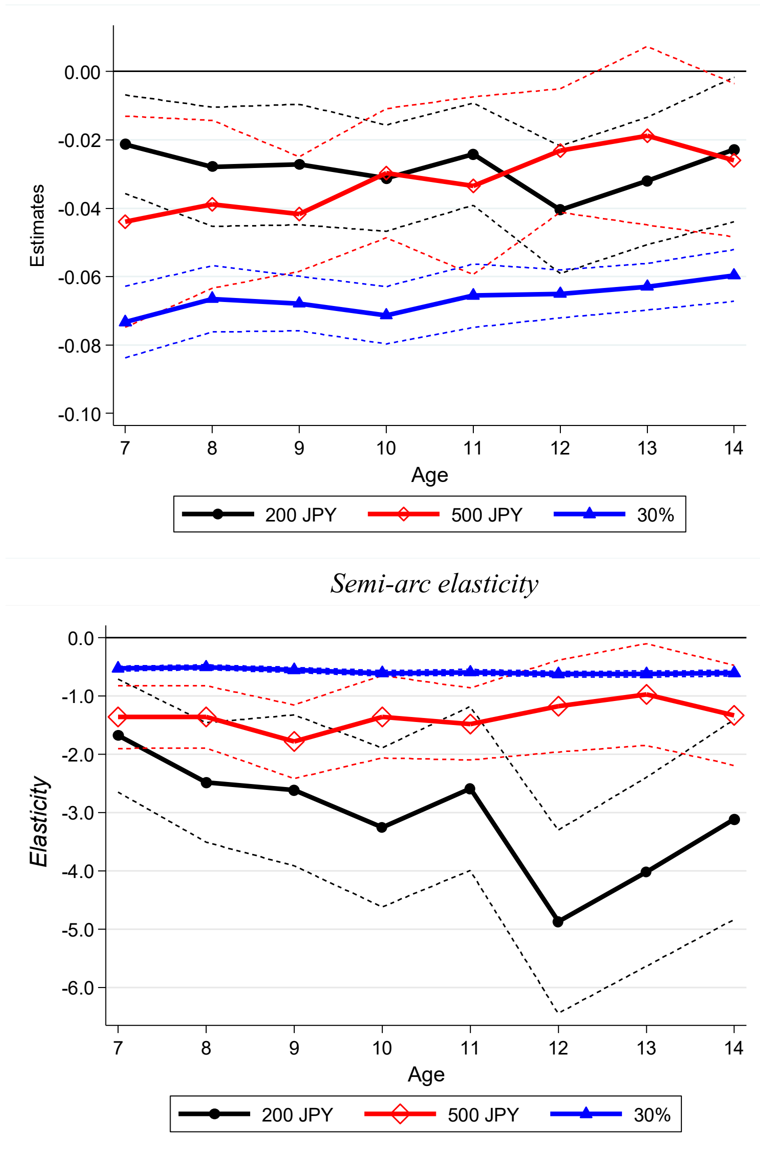


Notes: The main sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in 1000 JPY (approximately 10 USD). "Better" indicates the subsidy expansion which lowers the price of healthcare from 30% to 0%, and "Worse" indicates subsidy expiration that raises the price from 0% to 30%. The upper half plots β_A^{better} and β_A^{worse} for each age (A=7–14) from estimation equation [4], and the bottom half plots the corresponding semi-point elasticity (See Online Appendix Section B for details). 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-point elasticity. The observations within 2 months from the price changes are excluded from the sample to account for anticipatory utilization. The scales of y-axis on the semi-point elasticity are set the same so that two elasticities are visually comparable. The corresponding table is found in Online Appendix Table H-1.

Figure 7: Effect of small copayment

Outcome: Outpatient dummy

Estimate

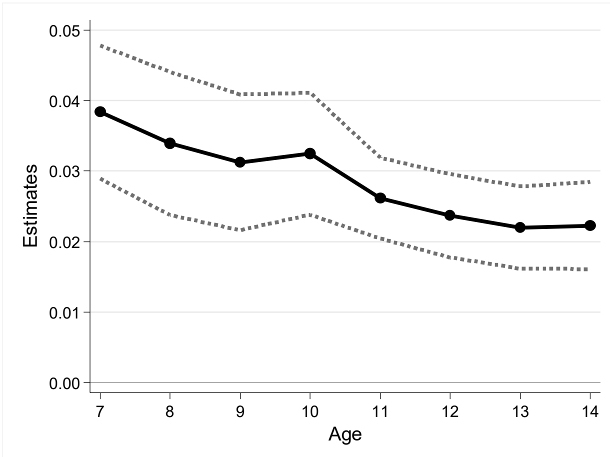


Notes: The full sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month. The upper half plots β_A^C for each age (A=7–14) and three price levels (C= 200 JPY/visit, 500 JPY/visit, and 30%) from estimating equation [5], and the bottom half plots the corresponding semi-arc elasticity (See Online Appendix B for details). The control group is the individuals who live in municipality with free care (C= 0%). 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 I-1.

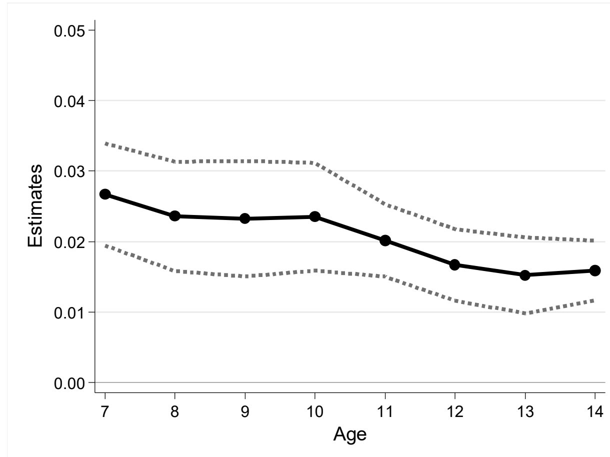
Figure 8: Ambulatory Care Sensitive Conditions (ACSC)

A. *Outpatient* dummy

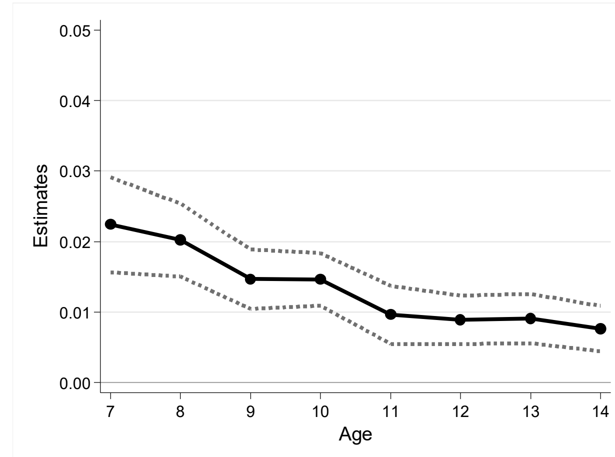
(i) Any ACSC



(ii) ENT

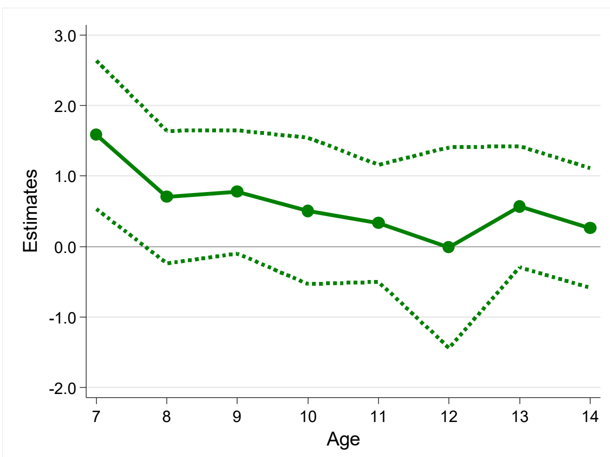


(iii) Asthma

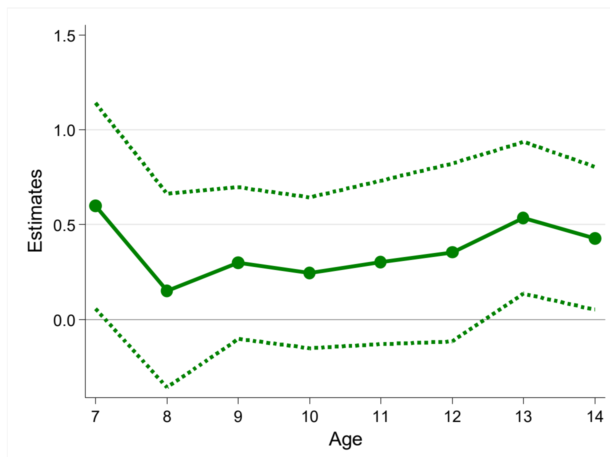


B. *Inpatient* dummy ($\times$1000)

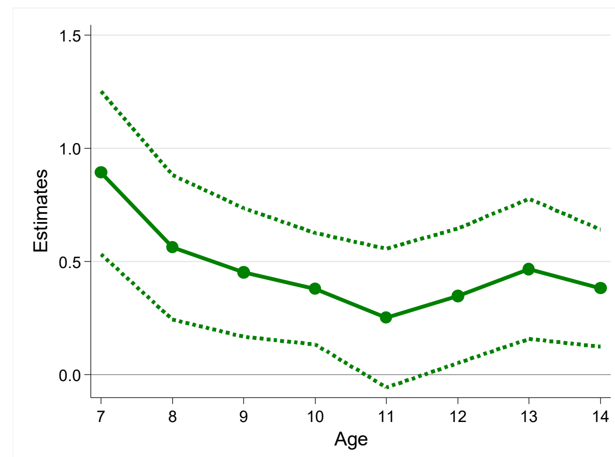
(i) Any ACSC



(ii) ENT



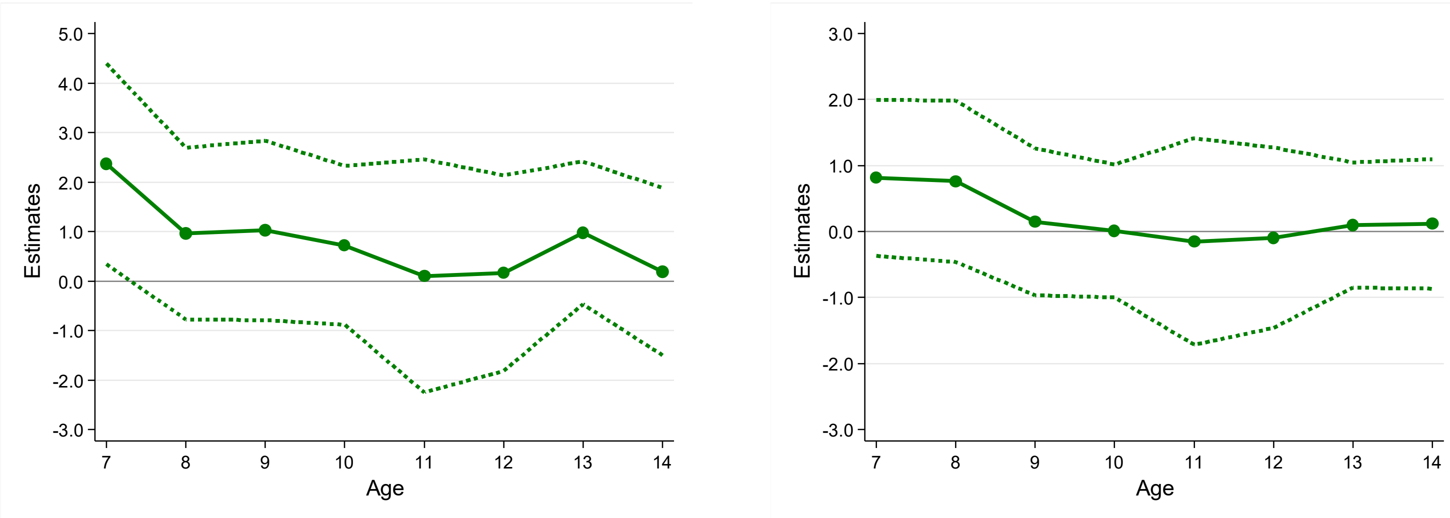
(iii) Asthma



Notes: The main sample is used. 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 ($\times$1000). The estimates β_A for each age (A=7–14) from estimating equation [3] are plotted. See Online Appendix Table J-1 for the list of ACSC and summary statistics. 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. ENT stands for Ear, Nose, and Throat. The corresponding table is found in Online Appendix Table J-2.

Figure 9: Offset effects

A. *Inpatient* dummy (×1000) **B. *Inpatient* spending** (in 1000 JPY)

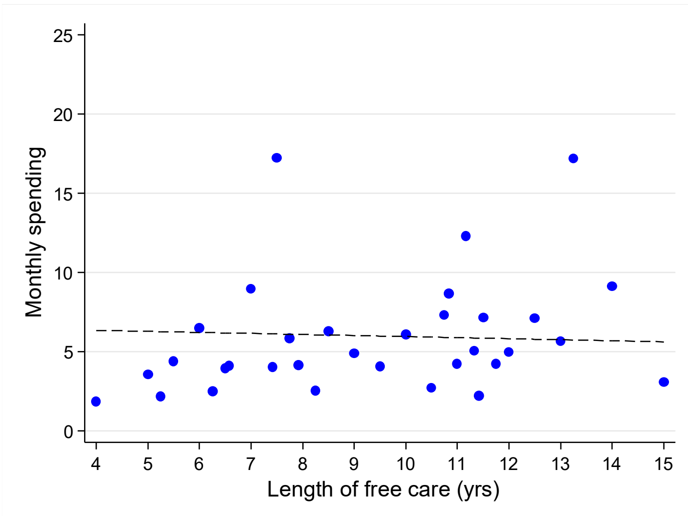


Notes: The main sample is used. An inpatient dummy takes one if there is at least one hospitalization per month (×1000), and inpatient spending is the monthly spending on inpatient care measured in 1000 JPY (approximately 10 USD). The estimates β_A for each age (A=7–14) from estimating equation [3] are plotted. 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. The corresponding table is found in Online Appendix Table K-1.

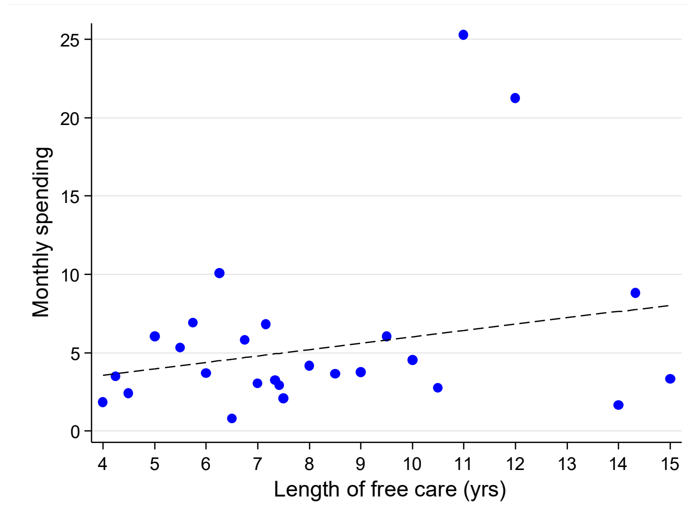
Figure 10: Medium-term utilization and health outcomes

A. Average monthly spending (in 1000 JPY)

(i) Age 16

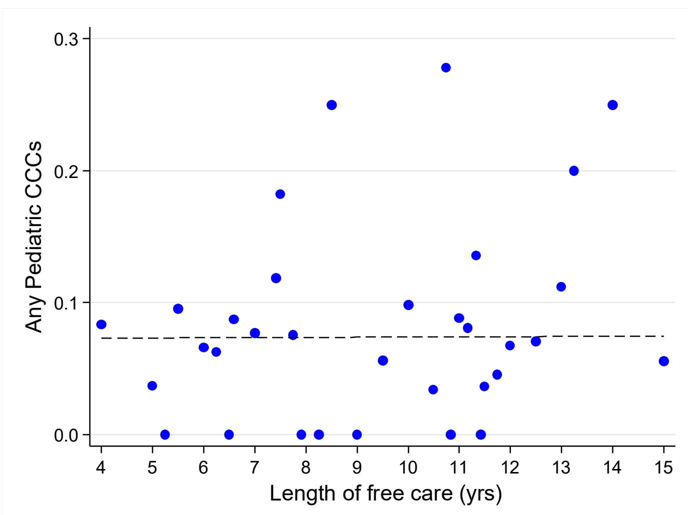


(ii) Age 18

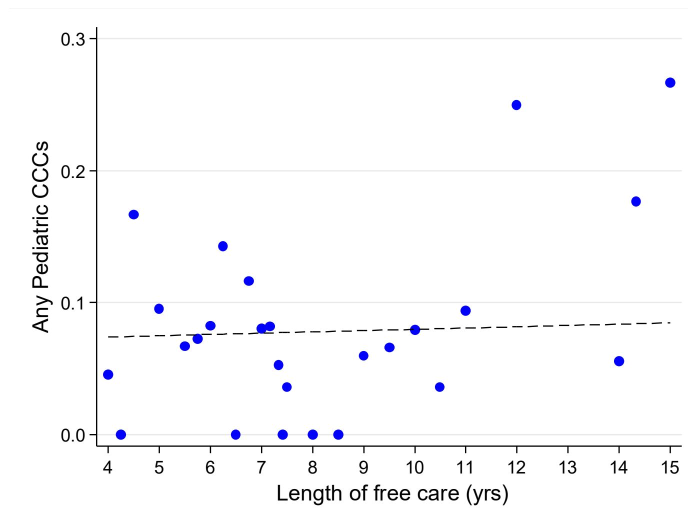


B. Any pediatric CCCs

(i) Age 16



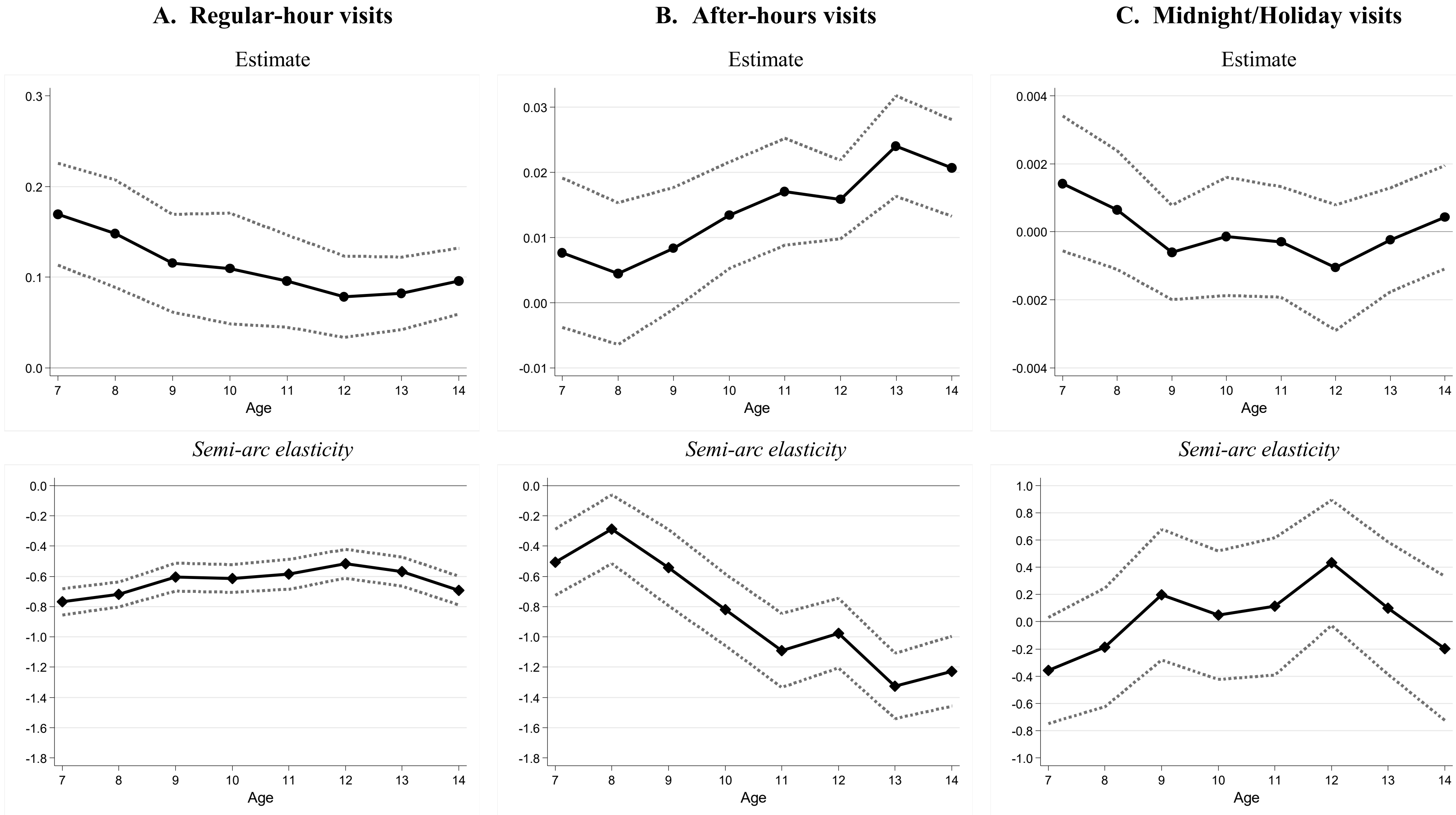
(ii) Age 18



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 with SD of 2.09 (2.06). The minimum and maximum are 4 years and the full 15 years, respectively, for both samples. The *y*-axis in Panel A is average monthly spending measured in 1000 JPY (approximately 10 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 M for the list of 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. For Panel A, the slope for the samples aged 16 (18) years are –0.066 (0.406) with p-values of 0.760 (0.341), 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= 0.957 and 0.674).

Figure 11: By time of visits

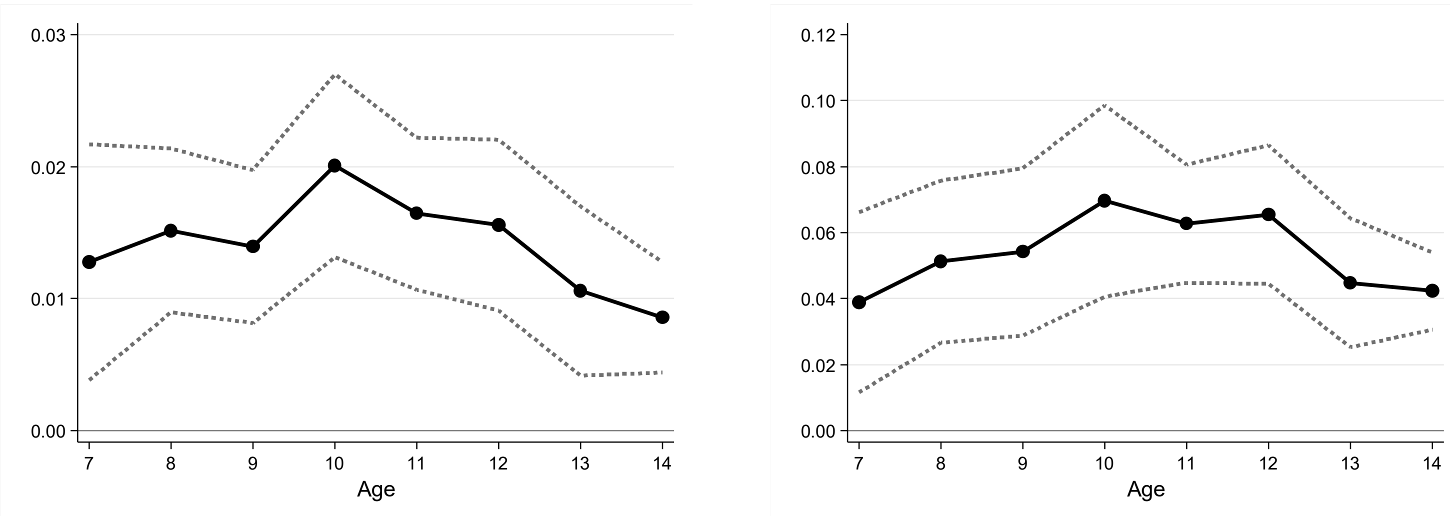
Outcome: Frequency of outpatient visits



Notes: The main sample is used. The frequency of outpatient visits is the number of outpatient visits per month. See Online Appendix M which provides the list of billing codes for after-hours and midnight/holiday visits and corresponding fees that are additionally charged on top of fees for regular-hour visits. The upper half plots β_A for each age (A=7–14) from estimating equation [3], and the bottom half plots the corresponding semi-arc elasticity (See Online Appendix B for details). 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 corresponding table is found in Online Appendix Table N-2.

Figure 12: Inappropriate use of antibiotics

A. Outpatient spending on antibiotic drugs (in 1000 JPY) **B. Frequency of antibiotics prescriptions**



Notes: The main sample is used. The outcome is monthly outpatient spending on antibiotics measured in 1000 JPY (approximately 10 USD) in Panel A and the number of prescriptions for the antibiotic per month in Panel B. See Online Appendix Table O-1 for the list of diagnoses for which antibiotics are not recommended. The estimates β_A for each age (A=7–14) from estimating equation [3] are plotted. 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. The corresponding table is found in Online Appendix Table O-2.

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

		Municipality-age-time 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%*
500 JPY/visit	30%	1,029	*8.7%*	2,516	*6.5%*
30%	200 JPY/visit	855	*7.2%*	1,502	*3.9%*
30%	500 JPY/visit	706	*6.0%*	1,556	*4.0%*
200 JPY/visit	0%	535	*4.5%*	1,050	*2.7%*
200 JPY/visit	20%	475	*4.0%*	981	*2.5%*
200 JPY/visit	30%	331	*2.8%*	460	*1.2%*
300 JPY/visit	30%	260	*2.2%*	482	*1.2%*
10%	30%	249	*2.1%*	712	*1.8%*
Total		11,205	*100%*	36,923	*100%*

Notes: This table lists top 10 combinations of transitions in patient cost-sharing. See Appendix Table A-1 for the complete list. The share are based on all price changes (not just by top 10 combinations). In this study, we mainly focus on the first two price transitions. 200, 300 and 500 JPY are approximately USD2, 3, and 5, respectively.

Table 2: Summary statistics (main 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	6.09	25.33	0	9,336
Outpatient spending (outpatient spending >0)	14.99	38.04	0.26	9,336
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	2.23	4.63	0.07	229
Inpatient dummy	0.28%	0.05	0	1
Inpatient spending	1.15	35.24	0	6,084
Inpatient spending (inpatient spending >0)	406.52	523.57	5.23	6,084

Notes: Outpatient spending, inpatient spending, and out-of-pocket (OOP) payment are all measured in 1000 JPY (approximately 10USD).