Free for Children? Patient Cost-Sharing and Health Care Utilization[*]

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Abstract

Understanding how a patient responds to the price of health care is key to the optimal design of health insurance. However, past studies are predominantly concentrated on adults and the elderly, and surprisingly little is known concerning children. Exploiting over 5,000 variations in subsidies at the municipality-age-time level in Japan, we document a number of behavioral price responses among children. First, we find that free care for children significantly increases the outpatient spending by 22%–31%, with the price elasticities being considerably smaller than RAND Health Insurance Experiment for the nonelderly. Second, we do not find asymmetric responses to the price changes of opposite directions. Third, we find substantially larger price responses when small copayments are introduced to free care, indicating that demand is more elastic around a zero price ("zero-price" effects). Finally, we provide evidence suggesting that most increases in utilization reflect low-value or costly care. Increases in outpatient visits neither reduce hospitalization by "avoidable" conditions nor improve short- or medium-term health outcomes. Furthermore, inappropriate use of antibiotics and costly off-hour visits increase. Taken together, this study's results indicate that the benefit of such a generous subsidy is limited, at least in the short and medium term.

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Online Appendix HERE

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1. Introduction

Understanding how a patient responds to the price of health care 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 health care, and patient cost-sharing is often zero (free care).[1] For example, the federal government in the US regulates the share of cost paid by patients in the Children's Health Insurance Program (CHIP). Similarly, child health care is heavily subsidized in many 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]

We have 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. 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 health care utilization is often preventive and self-limiting, and hence potentially more discretionary (Leibowitz *et al.* 1987). Finally, mothers may perceive a higher return from child health care, as children are expected to live longer.[4]

This paper examines the effect of patient cost-sharing on health care utilization among children. To this end, we newly hand-collected data on the drastic expansion of subsidy for child health care in the last decade in Japan. We merge this information with individual-level monthly panel data on item-by-item health care utilization. Because municipalities expanded subsidies in different timing and covered different age groups, we have more than 5,000 changes in subsidy status at the municipality-age-time cell, which is the level of the variation for identification in our empirical analysis. This unique variation in subsidy generosity combined with individual panel data enables us to estimate a number of behavioral responses to the price of health care 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 a generous subsidy for child health care in public health insurance programs across the countries.

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

[3] Hossein and Gerard (2013) document that the cost-sharing for outpatient care has been increased between 2000 and 2010 among all the high-income countries examined in the study (UK, Germany, Japan, France, and the United States).

[4] Thus, mothers may seek health care regardless of prices or they are at least less willing to reduce their children's health care than to reduce their own (Leibowitz *et al.* 1987).

The unique institutional background in Japan offers a clean setting in identifying patient price responsiveness, since the roles of insurers and medical providers, which are also major players in health care 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. For medical providers, they cannot price discriminate patients as the physician and hospital are paid solely based on the same national fee schedule regardless of providers' and patients' types. Relatedly, our insurance claims data *inherently* include the actual transaction price, which allows us to easily quantify the monetary values of (excess) utilization, unlike the case in the US, which is notorious for having a complex price schedule.

Our paper contributes to the literature on demand for health care 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 7–14 year-old children even at each age in month level. The price elasticity at various ages can be informative for policy makers to design a more precise cost-sharing schedule.

Second, we can examine whether children *asymmetrically* respond to the price of health care since prices change in the opposite directions even *at the same age* in our setting. From a policy and welfare standpoint, understanding whether such asymmetry exists is crucial. On one hand, such asymmetry—if it exists—provides a cautionary note on applying the price sensitivity estimated from just one direction of price change (e.g., price increase) when the policy maker 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, the research design of past studies (such as randomized control trials or regression discontinuity designs) precludes them from testing it as there is only a single direction of price change.

Third, we also examine the effect of a very 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 also related to the "zero-price" effect, which argues that people substantially increase demand if the price is zero (Shampanier *et al.* 2007). 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, and Douven *et al.* (2017) present the same story for the demand for the health insurance.

Finally, we examine the utilization patterns by type of visit and health status to investigate whether the increases in utilization primarily reflect beneficial or low-value care. In particular, our unique panel dataset which observes *both* outpatient and inpatient spending for the same individual in the same dataset over time allows us to examine the possibility of the cross-price effect (or known as "offset" effect in health economics)—whether beneficial *outpatient* care prevents avoidable *inpatient* admissions. More generally, if patients cut the 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. To our knowledge, there is no study which examines the offset effects for children except for the RAND Health Insurance Experiment (RAND HIE, hereafter) conducted in 1970s (Newhouse 1993; Manning *et al.* 1987)—which randomly assigned families to different patient cost-sharing levels—but it lacks the statistical power to make any decisive conclusion (only 1,136 children).

The findings of this paper are divided into two parts. In the first part, we document the basic behavioral price responses to health care among children. We find that reduced patient cost-sharing significantly increases utilization of outpatient care. When the municipal subsidy lowers the cost-sharing from 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 19–25% increases) from the mean of 32% in the absence of the subsidy. Similarly, outpatient spending per month increases by 1.00–1.38 thousand JPY (10.0–13.8 USD), which corresponds to a 22–31% increase from the mean of 4.49 thousand JPY (44.9 USD). The overall semi-arc elasticities are relatively constant for both outcomes at approximately –0.60 throughout ages 7–14, which is considerably smaller than the conventional estimate of –2.11 and –2.26 that Brot-Goldberg *et al.* (2017) calculate from the RAND HIE for nonelderly (Keeler and Rolph 1988).

The 'back-of-the-envelope' calculation suggests that if the full subsidy is expanded to all the municipalities among children aged 7–14 in Japan, the annual outpatient spending increases by 117 billion JPY (1.17 billion USD). Importantly, this creates a substantial negative fiscal externality to many stakeholders: while the municipality is only responsible to cover 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, among others.

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 policy makers 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. The elasticities for a smaller copayment (200 JPY or 2 USD per visit) are much larger than those of a larger copayment (500 JPY or 5 USD per visit). These results are broadly consistent with the "zero-price" effect.

In the second part, we investigate whether subsidy-induced outpatient spending (moral hazard) largely reflects the increases in beneficial or low-value care. In fact, the recent work by Baicker *et al.*

(2015) suggest that welfare implications of moral hazard depend on the types of care that increase. While this is always a challenging task, especially for children, as the nature of diseases tends to be acute, we take the following two approaches to answer this question to the extent possible.

We start with investigating whether we can find any evidence of increases in “beneficial” care. We find that while subsidy increases 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 do not find any evidence of offset effects: substantial increases in outpatient spending do not accompany the reduction in inpatient spending. Additionally, we find little impact on short-run child mortality; however, we need to interpret this result with considerable caution due to the very low mortality rate among children of this age range in Japan. In addition to the short-run effect, we document that neither health care utilization nor health status after subsidy expiration is systematically associated with the length of free care during childhood, suggesting that subsidy-induced utilization does not seem to improve the health of subsidy beneficiaries, even in the medium term. Taken together, we find little evidence of increases in “beneficial” care.

Next, we examine whether we can find any evidence of low-value or costly care. First, we find that reduced cost-sharing substantially increases off-hour visits, validating the concern that children (and hence mothers) exploit the opportunity of free care by increasing physician visits outside of regular hours. In addition, the semi arc-elasticity for off-hour visits is much larger in magnitude than that of regular-hour visits for older aged children, indicating that off-hours visits seem to be more discretionary and less urgent than regular-hour visits. 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). In fact, antibiotic-resistant infections annually affect at least 2 million people, and 23,000 people die as a direct result of these infections in the United States (Centers for Disease Control and Prevention 2013). To our knowledge, no prior studies have investigated whether financial incentives, such as subsidy for child health care, increase inappropriate use of antibiotics for children.[5] Third, we find that healthier children are more price responsive to health care than sicker children. This result suggests that health care utilization by healthy children is more discretionary and relatively low-value. The result also indicates that it is not the sickly children but the healthy children who will cutback health care most in the absence of a generous subsidy.

[5] For example, Foxman *et al.* (1987) examine the impact of patient cost-sharing on inappropriate antibiotic use in RAND HIE but did not separately examine it for children. See also Currie *et al.* (2011, 2014), who examine the relationship between the inappropriate use of antibiotics and the supply-side financial incentives in China.

Taken individually, each piece of evidence is not sufficient to establish the existence of wasteful utilization. However, 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 the generous subsidy for child health care leads to the increases in unnecessary and costly visits, implying that short- or medium-term benefit of such a generous subsidy is at least limited among the children we examine

This paper is most related to RAND HIE which still serves as a gold standard in price sensitivity among the nonelderly.[6] Leibowitz *et al.* (1985) specifically analyze children under age 13 and find that, among others, the use of outpatient services decreased as cost-sharing increases. However, the study suffers from a small sample size to identify the effect for some types of services (e.g., inpatient care).[7] Furthermore, it is nearly 40 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 may not be directly applicable to the situation today, especially to countries other than the United States. A few notable exceptions from the nonexperimental works are recent papers by Han *et al.* (2016), which examine the effect of cost-sharing at age 3 in Taiwan, and by Nilsson and Paul (2018), which examine a similar question for children in one region in Sweden.[8] Our study arguably entails broader policy implications because of the richness in both varieties and number of changes in cost-sharing, wider coverage of age ranges, and comprehensive analysis on health care utilization.

Finally, this paper is also related to the large literature on health insurance and child health care utilization, especially the 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 papers 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 ours or RAND HIE. This distinction is important because the provision of health insurance entails large wealth effects, and thus these studies capture combined effects of price reduction and wealth effects.

The rest of the paper is organized as follows. Section 2 provides the institutional background in

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

[7] The study may also suffer from the assignment of health plans that affect the entire family, so that interaction with family members may confound children's own price sensitivity. For example, if the parents receive more medical services due to insurance coverage, and hence are less likely to become sick, children who reside with parents may be less likely to become sick as well.

[8] Han *et al.* (2016) exploit the copayment change at age 3 in Taiwan and find that a lower price significantly increases the utilization of outpatient care, especially low-value care at high-cost hospitals. Despite the increase in utilization, they find little impact on children's health. Nilsson and Paul (2018) exploit the abolishment of copayment for outpatient care among children between 7 and 19 years in one region in Sweden. They find that children increased their number of doctor visits, and children from low-income families are three times as responsive as their more advantaged peers.

subsidy to child health care. Section 3 describes the data, and Section 4 presents our identification strategy. Section 5 documents the basic findings on children's price responsiveness to health care, and Section 6 investigates whether the changes in utilization reflect beneficial or low-value care. Section 7 concludes.

2. Background

2.1. Health care system in Japan

We briefly provide the background of the Japanese health care system related to this study. Japan has a universal health insurance system since 1961, which is heavily regulated by the government. All citizens are obligated to enroll either in an employment-based insurance system or a residential-based insurance system (See, for example, 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. Here, note that inpatient care refers to hospital admissions with at least one overnight stay. 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 United States, where insurance companies restrict the choices of medical providers through managed care.

Third, patients cannot be price discriminated by medical providers since all fees paid to the providers are solely based on the national fee schedule (i.e., fee-for-service). Consequently, medical providers receive the same fee for the same service regardless of insurers. This prevents so-called "cost shifting" by the medical providers in the US (Cutler 1998), where they charge private insurers higher prices to offset losses from the beneficiaries of government-funded health insurance (e.g., Medicaid). Another important implication is that any changes in utilization come from quantities instead of prices since there is by default 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 populations: young children and the elderly. In particular, the cost-sharing is set at 20% for children below age 6. 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 United States, there is no deductible in Japan. Additionally, the supplementary private health insurance that covers the out-of-pocket cost virtually does not exist in Japan likely 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 "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 a 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 very similar. Second, the stop-loss is set monthly in Japan, unlike annually in RAND HIE and most health insurances in the United States. To the extent that illnesses are unpredictable, this shorter interval may make it harder 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.[9]

Importantly, many municipalities additionally provide a subsidy for child health care for those who live in the municipality regardless of their insurance types. It is called Medical Subsidy for Children and Infants (MSCI), and it has drastically expanded in last decade. Children who are eligible for subsidy receive an additional insurance card, and by showing the card at medical institutions they receive the discount. Crucially, we know from our claims data the municipality of their 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 the MSCI is set by each municipality, 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 visit (age). The variations in these three dimensions are the sources for 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) age until the subsidy is offered; 2) the level of patient cost-sharing (equivalently, the level of municipal subsidy); 3) whether the subsidy is refund or in-kind;

[9] In fact, even under an annual stop-loss in the US, people tend to respond myopically to spot price rather than future price (e.g., Keeler and Rolph 1988; Brot-Goldberg *et al.* 2017) while Aron-Dine *et al.* (2015) find that both spot and future price matter.

and 4) whether there are any household income restrictions for subsidy eligibility. We explain each component in detail below.

First, the generosity of subsidy is largely reflected 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 paper for convenience as the school grades are almost completely equivalent to age in Japan because of the strict enforcement of the school entry rule as well as very rare grade retention and advancement.[10] Second, the level and the form of subsidy (and thus cost-sharing) differ by municipality: the majority of municipalities fully subsidize child health care (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 the form of copayment such as 200, 300, or 500 JPY (2, 3 or 5 USD) per visit.

Third, the payment of subsidy to patients can take the forms of either refund vs. in-kind (i.e., future vs. immediate reimbursement). While the amount of cost-sharing can be identical in two cases as long as the parents submit the required document for a refund, patients may prefer in-kind to refund because of the time cost and/or credit constraint.[11] Finally, some municipalities impose income restrictions on eligibility for the subsidy. While we cannot identify the ineligible individuals due to the lack of an income variable in our claims data, the fraction of municipalities with an income restriction is very small in our data. In the empirical model, we include a dummy for income restriction at municipality × year-month levels.

One contribution of this paper 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.[12] Importantly, 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

[10] In Japan, the School Education Law (SEL) obliges parents to send their children to primary schools as soon as their children turn six years of age before the school starting month, which is April. The school entry rule is strictly enforced in Japan (only 0.03% of children are exempted from the mandatory entry). Additionally, the Japanese educational system is known for its social promotion system, in which automatic promotion occurs from one grade to the next without grade retention and grade advancement. Consequently, a school cohort is nearly identical to a birth cohort—that is, those born in April to those born in March of the next year. See Shigeoka (2015) for more details.

[11] For example, suppose the municipal subsidy reduces the coinsurance rate from 30% to 10%. In the case of in-kind, patients only pay 10% at the medical institutions and no further action is necessary. In the case of refund, patients still pay the full 30% at the medical institutions, but then they are reimbursed the difference between the payment and coinsurance rate, which is 20%, after filing the required documents to the municipal office.

[12] While some prefectures collect such subsidy information once a year from all municipalities in the prefecture (e.g., every April), we need information on *monthly* basis for our identification strategy.

the six largest prefectures in Japan, which results in 323 municipalities.[13] According to national statistics, these six prefectures cover as much as 44.9% of children ages 0–15. While our data is not nationally representative, one benefit of restricting it 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 during April 2005–March 2015 among 165 municipalities, which are mainly used in this study as explained in Section 3.3.[14] Note that this figure reflects the compositional changes of municipalities as the number of municipalities increases at the later period in our claims data. Importantly, within the municipalities, the subsidy expansion is always monotonic— that is, there is *no* single municipality that *lowers* the maximum age during this period (April 2005–March 2015).

The graph clearly shows that the subsidy expanded rapidly to older ages in the last decade. For example, none of the municipalities provide the subsidy until age 15 (the end of junior high school) in April 2005, the beginning of the sample period. However, this number reaches nearly 80% in ten 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 age 3 to 6 (until the start of primary school). While Figure 1 clearly shows that all municipalities in our sample have already provided the subsidy until age 6 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 see the highest number of municipality-level subsidy expansions in April 2008 to ages above 6 (See Appendix Figure A-1 on the precise timing of all policy changes).[15]

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

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

[14] We also collected information on subsidy for *impatient* care. However, most municipalities had already covered inpatient care until age 15 (the end of junior high school), and thus there is not much variation in eligibility of subsidy for impatient care. In fact, while we examine the effect of subsidy for inpatient care on impatient spending, we do not detect any meaningful results (results available upon request). These results are consistent with RAND HIE which finds that children only respond to the price of outpatient care but not inpatient care (Newhouse 1993). Therefore, we focus on subsidy for outpatient care throughout this paper to save space.

[15] While the policy changes are rather concentrated in April, which is the start of the fiscal/school year in Japan, there are reasonable variations in the timing of implementation across months. This figure also demonstrates that we need *monthly* level data on both the explanatory variable (subsidy level) and outcome variable (health care utilization).

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 consists of administrative enrollment data and claims data. For each person, the enrollment data consist of patient ID, gender, age and the 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.[16] 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. For example, when a prescription drug is dispensed, we have detailed information on the year-month of the prescription, the name of the drug, ATC code, retail price, and quantity. 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 in the medium-run when children become adolescents. Additionally, we can test the possibility of the "offset" effect—whether beneficial *outpatient* care prevents avoidable *inpatient* admissions in the future. In contrast, the outpatient and inpatient data are often separated in the other settings. For example, hospital discharge data do not include information on office visits and prescription drugs. Relatedly, the claims data in Japan inherently 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 easily quantify the monetary values of (excess) utilization.

Our dataset is constructed in the following way. We provide the subsidy information we collected to JMDC, and then JMDC merges it with their insurance claims data in-house by municipality and year-month, and returns it to us with the municipality ID and patient ID de-identified for confidentiality

[16] 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.

reasons. Thus, we cannot 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 for 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

We impose the sample restriction in the following ways. Our data cover the period of 10 years between April 2005 and March 2015 (120 months). We focus on children of 7–14 year-old (96 months) since, as shown in Appendix Figure A-2, we do not have many observations without subsidy below age 7 and with subsidy above age 15. This happens because the majority of the municipalities (81.3%) have already provided subsidy until age 6 (the start of primary school) at the beginning of our sample period, and most of the municipalities do not provide subsidy beyond age 15 (the end of junior high school) at the end of our sample period. Therefore, we limit our sample to 6–15 year-old (one year wider on both sides of the ages of interest) to identify the effect of patient cost-sharing at ages 7–14.[17]

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 still maintain as many as 5,438 changes in subsidy status at the municipality-age-time cell, which is the level of the variation for identification in our empirical analysis.[18] Second and more importantly, these two price transitions are observed at entire age ranges. This point is crucial for our purpose of estimating 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.

Importantly, Appendix Table A-2 shows that the characteristics of children as well as their health

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

[18] Note that since we only allow for the municipalities to 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. These two types of transitions are followed by "500 JPY/visit to 30%" (6.5%), "30% to 500 JPY/visit" (4.0%), and "30% to 200 JPY/visit" (3.9%), where the number in the parentheses is the share at the year-month level, but these transitions do not always spread across the ages.

care 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 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 also use the full sample and exploit all the price variations to estimate such effects, recognizing the lack of statistical power on some 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. Importantly, as discussed in Section 4.1, the source of variation for identification does not simply come from the *expansion* of 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 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 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 provided in the form of 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 6.09 thousand JPY (60.9 USD) per month including zero-spending, and 14.95 thousand JPY (149.5 USD) conditional on at least one visit.[19] Importantly, out-of-pocket payment per visit is 2.23 thousand 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 when admitted (406.52 thousand JPY or 4065.2 USD) than outpatient care.

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 the 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

[19] 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).

the average health may improve at older ages. The graph on the right also demonstrates a similar pattern for outpatient spending: the mean outpatient spending is 2 to 3 thousand JPY (20–30 USD) higher with the subsidy than without the subsidy or by 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 impatient outcomes, which are aggregated in age in years as hospital admission is very rare. In contrast to outpatient outcomes, we see no clear difference in the inpatient dummy and inpatient spending with and without subsidy.

Appendix Table A-3 lists the major diagnosis groups in our sample by ICD10. The largest share comes from diseases of the respiratory system, which account for approximately one third of the all diagnoses. We also list the top 10 individual diagnoses at the ICD10 4-digit level. Importantly, the top ranked diagnoses tend to be acute such as acute bronchitis and acute upper respiratory infection, compared to the elderly who 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) at 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 just born one 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 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 full subsidy (i.e., 0% coinsurance rate) until the beginning of primary school (age 6). Since the school year starts in April in Japan, the younger cohort is 6 year 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 age 6 years and 9 months to age 9 years and 2 months, a month before the subsidy expansion in October 2007. Because of subsidy expansion, the cohort enjoys the free care from age 9 years and 3 months until age 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 age 15 years and 9 months. On the other hand, the price schedule for the older cohort (dotted line) is shifted by one month to the right as the cohort is one month older than the younger cohort at the entry of primary school, the subsidy expansion, and graduation from junior high school.

There are two important points to make from this simple illustration. First, any cohort between 6 and 15 years old benefited from the same subsidy expansion. As a result, each cohort uniquely experiences the subsidy expansion as well as the expiration at different ages. This enables us to estimate the price elasticity for broad age ranges (ages 7–14), technically, even at the monthly level. Price elasticities at different ages can be informative for the government to design a more flexible cost-sharing schedule.

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*. The 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 may not be the case. On one hand, the elasticities can be *smaller* in a price increase (labeled "worse" or 0%→30%) than in a price decrease (labeled "better" or 30%→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 health care, 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% seems not so much more inexpensive (of course, zero can be a special price which we briefly discuss in Section 5.5).[20] Thus, it is ultimately an empirical question.

The research design of past studies (such as 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. Fortunately, 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

[20] We view 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.

expansion of the subsidy ("better") while 2,933 changes are the expiration of the subsidy ("worse").

4.2. Identification strategy

We attempt to identify the effects of subsidy for child health care by exploiting the unique variation in subsidy across municipality, age, and time combined with the longitudinal claims data in a difference-in-difference framework.[21] Specifically, our basic estimation equation (ignoring the asymmetric price 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 health care 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 one 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, 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. Additionally, η_i is the individual FE, 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: a dummy that takes one if subsidy is in-kind rather than a refund, and a dummy takes one if there exists income restriction on subsidy eligibility, while recognizing the lack of the power to identify these effects (thus, not the focus of the paper). We estimate this equation using OLS. Standard errors are clustered at the municipality to account for serial correlation in the error terms within the municipalities. As discussed in Section 5.3, the estimates from alternative models such as one-part or two-part GLM are almost identical to 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) as below 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 which takes one if the person is more than age A but less than

[21] 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.

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 correspond to 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 schools).

For our main analysis, we focus on the sample of non-movers (98.3% of the sample) who stay in the same municipality. The migration rate in our sample is lower than the 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 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 into the sample hardly change the results because of the small number of inter-municipality migration. For non-movers, since φ_m and θ_i are identical, our final estimation equation is written as[22]:

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

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 the health care utilization. With over 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 the municipalities in a better financial situation are more likely to implement the subsidy expansion, while income effects simply increase utilization, our estimates can be upward biased.

To account for such a concern, we take three approaches. First, we conduct the 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 FEs (where time is measured in months), to examine the robustness of our baseline estimates. The latter specification is most stringent

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

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 only those 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.

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 three months before the change in subsidy status (T= –3). Note that 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 from 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 very reassured, as this result addresses the concern that the municipalities that expand the subsidy may have a different trend in health care 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 well-aware of the price changes and behave strategically by delaying or rushing visits. On one hand, the existence of the anticipatory utilization is rather surprising as the nature of diseases for children tends to be acute. 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 are indeed acute because one cannot delay treatments too much until the subsidy expands while one can more easily stockpile before the subsidy expires.[23] These differential responses can also be behavioral in that mothers of children may react more to a price increase rather than a price decrease due to utility loss from giving up the inexpensive service.[24] Importantly, as we include age and time FEs (both in months), this difference is not driven by a particular age or year-month effect such as the expiration of subsidy after graduation from primary school. In any case, since such anticipated effects—which may overstate our estimates—seem to be concentrated within two months from T=0, we exclude these four months of the data throughout the paper. 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 two months from T=0.[25]

Finally, and most importantly, 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.[26]

5.2. Main results

Figure 5 demonstrates the graphical presentation of the estimating equation [3] which plots β_A for each age (A= 7–14) 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 and are statistically significant at the conventional level for any age. With subsidy, the

[23] 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.
[24] Another potential explanation is awareness of information; that is, on average people may be more aware of subsidy expiration (“worse”) than subsidy expansion (“better”). Suppose the free care is expanded from age 6 to 15. Then, a 6-year-old child had at the maximum 9 years to be aware of the end date of the subsidy while a nearly 15-year old child had at the minimum of 1 month. Assuming the uniform distribution of children across ages, children have on average 4.5 years to realize the end date of the subsidy. On the other hand, while the subsidy expansion should be announced at least a few months (or even longer) in advance, children have arguably less time to know the start date of the subsidy.
[25] To the extent that the *net* change in utilization around the changes in subsidy status is positive, the excess mass of anticipatory utilization (such as delayed treatment) can be potentially viewed as a particular form of moral hazard (Cabral 2017). If so, the estimates and the corresponding elasticities without removing the data may provide the upper bound.
[26] We further expand the window of the event-study to $\pm$24 months (instead of $\pm$12 months) from the subsidy changes. The estimates are relatively stable even 24 months (2 years) after the change in subsidy status (not shown).

probability of seeing a doctor at least once a month increases by 6–8 percentage points higher than the probability without subsidy. This translates into 19–25% increases from 0.32, the mean without subsidy among ages 7–14.[27]

The corresponding semi-arc elasticities presented at the bottom half range from –0.52 to –0.63.[28] Here, considerable caution is necessary to compare the elasticities estimated across countries and time periods, and as noted by Aron-Dine *et al*. (2013), the price elasticity may not be simply captured by a single price as the price is often nonlinear. Nonetheless, these numbers are considerably smaller than –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 and –0.88 at age 20 by Nilsson and Paul (2018) in one region in Sweden.[29] See Appendix B for more details on the derivation of these elasticities.

Panel B of Figure 5 plots the estimates of outpatient spending. While the outpatient spending is arguably of the 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 are slightly declining as one gets older: with subsidy, the outpatient spending increases by 1.38 thousand JPY (13.8 USD) per month at age 7, and by 0.998 thousand JPY (9.98 USD) per month at age 14 than those without subsidy. These estimates correspond to 18–31% increases from 4.49 thousand JPY, which is the mean value for ages 7–14 without subsidy. Since the mean outpatient spending—which is the denominator of the semi-arc elasticity ($= -\beta_A/(0.15 \times (Q_{0A} + Q_{1A})$)—is relatively stable across ages, semi-arc elasticities are largely governed by the sizes of β_A. Hence, semi-arc elasticities are also slightly declining in ages, which decrease from –0.74 at age 7 to –0.63 at age 14. To save space, all the corresponding tables of these figures are reported in the Online Appendices throughout the paper.[30]

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

[28] 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 only reflect 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.

[29] For the 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 the arc elasticities of –0.12 and –0.08 for regular and emergency outpatient care among 3-year-old children in Taiwan. See Appendix Table A-4 for comparisons with the related literature.

[30] Appendix Table C-1, which corresponds to Figure 5, shows that the estimates on a dummy for in-kind payment, and a dummy for income restrictions take the expected sign: in-kind payment increases the outpatient visits by 4.7 percentage points, which is more than half the size of the estimates in patient cost-sharing from 30% to 0%, and decrease by 2.0 percentage points when there is an income restriction on eligibility for the subsidy. For outpatient spending, the signs of these coefficients are also as expected, but not statistically significant at the conventional level. These results are consistent with Zhong (2011) who shows that immediate reimbursement increases health care

Since the total number of children aged 7–14 in Japan was approximately 8.8 million in 2015 (Statistics Bureau 2015), a 'back-of-the-envelope' calculation suggests that—if the free care is expanded to all the municipalities in Japan—the annual outpatient spending increases by 117 billion JPY (1.17 billion USD).[31] It should be noted that although municipalities bear 30% of the subsidy-induced spending, the remaining 70% of increased spending should be borne by others. Because the universal health coverage in Japan is financed by taxes (39%), premiums (49%), and out-of-pocket (12%) (Ministry of Health, Labour and Welfare 2014), the municipal subsidy has a substantial negative fiscal externality on many stakeholders including the central government and insurers.

5.3. Robustness checks

We subject these results to a series of robustness checks. In the interest of brevity, we leave the detailed descriptions of the exercises to the 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 than children in municipalities with subsidy changes. Since the estimates in the event-study before T=0 seem to be reasonably smooth and close to zero, this does not seem to be a serious concern. Nonetheless, we add the time-by-municipality FEs (where time is measured in months) to account for the time-varying municipality characteristics that can 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 only exploit the timing of the changes. While the estimates are noisier due to a 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 OLS estimates. Again, for the

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utilization compared to future reimbursement in China. Despite its importance, the effect of the reimbursement method on utilization is understudied in health economics.

[31] We multiply each β_A by the number of children in the age A in 2015 and sum them to calculate monthly spending. Then, we multiply it by 12 to convert to annual spending. The exchange rate of 100 JPY/USD is used throughout the paper.

ease the computational burden for 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 magnitudes to those of outpatient spending.[32] 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 expensive and intensive margins.[33]

In Appendix F, we also 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%).[34] 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.

5.4. Asymmetric price responses

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

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

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. Similarly, $worse_{iamt}$ is an indicator variable equal to zero before the subsidy expires, and equal to one in all years after the subsidy expires,

[32] In addition to OLS, we also estimate the count 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 each other.

[33] Since both outpatient spending and frequency of outpatient visits increase by similar magnitudes, the spending *per* visit has not changed (not shown).

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

even if the subsidy becomes available.[35] Because of the way that 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 subsidy, after the subsidy is expanded, relative to the period when the subsidy was not available; on the other hand, β_A^{worse} tests the effect of *increases* in cost-sharing on utilization, relative to individuals in other municipalities with the subsidy, after the subsidy is expired at the age, relative to the period when the subsidy was available.

Figure 6 demonstrates the graphical presentation of estimating equation [4], which plots β_A^{better} and β_A^{worse} for each age (A=7–14) in the upper half and the corresponding semi-point elasticity in the lower half.[36] Here, we instead report semi-*point* elasticity instead of semi-*arc* elasticity as we exactly know the starting price as well as the direction of the price changes. Panels A and B report the results of an outpatient dummy and outpatient spending, respectively.

Two things are worthy of mentioning. First, the estimates take completely opposite signs for opposite directions of changes in subsidy status, reassuring 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.[37] Since we are eventually interested in the overall spending, we conclude that there is little evidence of asymmetric price responses.

The nonexistence of the 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. Additionally, it is very useful for welfare analysis as the standard welfare calculation does not differentiate the direction of the price changes. Since we see little asymmetry in price responsiveness for our baseline results, we focus on the estimates from equation [3] without asymmetry hereafter.

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[35] 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, in the analysis of the asymmetric price response, we remove 921 individuals (1.45%) who 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 available upon request).

[36] 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.

[37] For outpatient spending, while the magnitude of 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. Thus, the resulting semi-point elasticity is similar in both directions.

5.5. 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 the 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. This section exploits these variations to see 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 fully recognize the lack of statistical power to gain meaningful estimates for some outcomes.

Specifically, we expand the basic equation [3] to further interact the subsidy dummies with a dummy for each price level C ($1(price = C)$) to allow for separate estimates for each price level C and each age A (β_A^C). The estimation equation is:

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

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 the 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 only report 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 (the 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 full cost (30%), 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 full cost ($\beta^{30\%}$) 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.[38] The bottom half of Figure 7 plots the semi-arc elasticities for three price changes. Interestingly, while the elasticities for two copayment levels are noisy and have wide confidence intervals, the semi-arc elasticities for the smaller copayment (ε^{200}) is substantially larger than those of the larger copayment (ε^{500}). This happens 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 figure shows that the semi-arc elasticities for the larger copayment (ε^{500}) are still larger than that of full cost ($\varepsilon^{30\%}$).[39]

These results potentially have two implications for the literature. First, these results are broadly consistent with the "zero-price" effect, which argues that people might be particularly sensitive to the price of zero. For example, Shampanier *et al.* (2007) show that reducing the price from a small positive price to a zero price discontinuously boosts the demand for the product in the lab, and Douven *et al.* (2017) present the same story for the demand for the health insurance. The underlying idea is that people highly perceive the benefits associated with free products because of utility loss due to having to give up the free product (Shampanier *et al.* 2007; Hossain and Saini 2015). Our results imply that at approximately the price of zero, a small positive price may have a large behavioral effect on demand for health care utilization.

Second, while the context is different, our results echo the recent argument that representing elasticities by a single number can be potentially problematic (Aron-Dine *et al.* 2013). Past literature provides such evidence by exploiting the variations in a nonlinear budget set induced by deductible and stop-losses (e.g., Ellis 1986; Cardon and Hendel 2001; Dalton 2014; Kowalski 2015). Our study utilizes the variation in cost-sharing across municipalities and ages to examine the (static) nonlinear price effects.[40] Our results indicate that depending on the choice of two price points, the semi-arc elasticities can be substantially different.

Again, we should view these results with caution as the results are not fully robust to other outcomes. In fact, Appendix G presents the same estimates from equation [5] when outcomes are the

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[38] 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. In a similar vein, 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 also 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.

[39] 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 suppliers to provide excess care (supplier-induced demand) under copayment than coinsurance.

[40] Some studies further examine the *dynamic* price effects, that is, whether a patient responds to spot or future prices (e.g., Keeler and Rolph 1988; Aron-Dine *et al.* 2015; Einav *et al.* 2015; Brot-Goldberg *et al.* 2017).

frequency of outpatient visits and outpatient spending. While the estimates for the frequency of visits show qualitatively similar patterns as the visit dummy, the estimates on the spending are substantially noisier for making any conclusive statements.[41] For the analysis below, 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 due to lower price (moral hazard) reflects beneficial or low-value care. In fact, the recent work by Baicker *et al.* (2015) suggest that welfare implications of quantity changes depend on how they occur. This is always a challenging task—especially for children, as the nature of diseases tends to be acute. Additionally, we fully recognize that subsidy-induced utilization should include some aspects of both essential and non-essential care, as documented in RAND HIE (Manning *et al.* 1987; Newhouse 1993). Nonetheless, we take the following two approaches to answer this question to the extent possible. The first approach is to examine whether we can find any evidence of increases in "beneficial" care (Section 6.1). The second approach is to examine whether we can find any evidence of low-value or costly care (Section 6.2). If we find little evidence of the first, and the ample evidence of the second, the weight of the evidence supports the notion that the generous subsidy for child health care leads to the increases in unnecessary and costly visits.

6.1. Evidence of "beneficial" care

We start with 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. Additionally, we examine whether childhood subsidy reduces health care utilization and improves health outcomes when they become adolescents.

6.1.1. Ambulatory Care Sensitive Conditions (ACSC)

We begin our analysis by examining whether outpatient care prevents avoidable inpatient admissions. Instead of looking at broad disease categories or choosing them to be arbitrary, one useful set of preventive care is the utilization for so-called Ambulatory Care Sensitive Conditions (ACSC) or "avoidable conditions"—diagnoses for which timely and effective outpatient care can help to reduce the

[41] Our results on outpatient spending are not model sensitive. In fact, Appendix Figure F-2 shows that the estimates are very similar across different models (OLS, one-part GLS, and two-part GLS).

risks of hospitalization by preventing the onset of an illness or condition (e.g., asthma).[42]

We employ the list of ACSC from Gadomski *et al.* (1998) that specifically focuses on children.[43] Appendix Table G-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% belongs to ACSC, which verifies the acute nature of diseases for children. Among the list of 17 ACSCs, severe ENT infections (56.9%) and asthma (31.5%) stand out and account for nearly 90% of total ACSCs.

Therefore, to save space, Figure 8 plots the estimates from equation [3] when outcome is an outpatient dummy for (i) any ACSC, (ii) severe ENT infections, and (iii) asthma.[44] Panel A shows that outpatient visits for these 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 ages 7–14 where the mean without subsidy is 0.11.

These results at a glance seem consistent with the literature that people are not only price sensitive 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 of the 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 it includes information for both outpatient and inpatient care from the same individual over time unlike most existing datasets that only capture either outpatient or inpatient care. Thus, we can directly examine whether such increases in preventive care at the outpatient setting indeed lead to the 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.[45] We do not see any declines in the hospitalization associated with any ACSC or other individual ACSCs. Since the hospitalizations among children are 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 case of asthma. Since the benefit of preventive care may emerge with a lag, we also estimate a variant of equation [3], where the explanatory variables are lagged outpatient subsidy dummies in a simple dynamic model. We find little evidence of any lagged effects

[42] 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 available upon request).

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

[44] Unfortunately, insurance claims data in Japan lists all the ICD10 that are diagnosed in the month instead of diagnosis for each visit, making it difficult to examine the other outcomes such as spending and frequency of visits by ICD10.

[45] 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.

either (results available upon request).

6.1.2. Offset effects

More generally, we can examine whether *outpatient* spending replaces *inpatient* spending—widely known as the "offset" effect in health economics. On one hand, if the outpatient visit is preventive and beneficial in that it leads to the detection and successful treatment of a condition that would have otherwise resulted in hospitalization, it will *decrease* inpatient care use. On the other hand, if the outpatient visits lead to a referral to a specialist for additional examination and invasive treatment for a condition that would have otherwise resolved itself in time (self-limiting) or simply increase the chance of detecting serious health problems, it will *increase* 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.

Whether outpatient care is a substitute or complement for inpatient care is an important but unsettled question in health economics. Overall, RAND HIE does not find the evidence of "offset" effects (Newhouse 1993). Some studies report that outpatient and inpatient care are rather 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 which examines the cross-price effects for child health care except for RAND HIE which lacks statistical power due to a 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] by an inpatient dummy or inpatient spending while the explanatory variable is still the subsidy for outpatient care. In this way, we can investigate whether the change in subsidy for outpatient care has any impact on impatient care.[46]

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 for one are statistically significant at the conventional level (see Appendix Table J-1). Additionally, the estimates are mostly positive albeit statistically insignificant, which imply that the generous subsidy for outpatient care does not seem to reduce the utilization of inpatient care and, if

[46] Following Kaestner and Sasso (2015), we also directly estimate the effect of outpatient spending on inpatient use in the instrumental variable approach. To account for potential endogeneity of outpatient spending, we instrument outpatient spending by outpatient subsidy. Specifically, we estimate:
$inpatient\ care_{iamt} = \alpha + \sum_{A=7}^{14} \beta_A \{outpatient\ spending_{iamt} \times 1(Age\ A)\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt}$ where $\{outpatient\ spending_{iamt} \times 1(Age\ A)\}$ are instrumented by $\{subsidized_{iamt} \times 1(Age\ A)\}$. The outcome is either inpatient spending or an inpatient dummy. In this sense, the results presented in Figure 8 are the reduced-form estimates. We find that the estimates β_A are always positive as in Figure 8, but they are not precisely estimated (results available upon request).

anything, increases the hospitalization, despite the large increases in the outpatient visits and spending documented so far. These results echo the findings on ACSC in the previous subsection. Of course, hospitalization is just one short-term indicator of worsening health, and we cannot rule out the possibility of long-term positive return on health. At least in our data, we do not find any evidence of "offset" effects on inpatient care despite the substantial increases in outpatient care.

6.1.3. Short-term mortality

Finally, we examine whether the outpatient subsidy improves the health outcomes of subsidy beneficiaries in the short-run (in this subsection) as well as medium-run (in next subsection).

Here, we investigate whether subsidy affects the most drastic health outcome: short-term (cotemporaneous) mortality. We fully 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.[47] Nonetheless, we still examine the mortality for the sake of completeness as we believe that the mortality 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:

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

where $Pr(death = 1)_{iat}$ takes one if a 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 FE, month FE as well as a female dummy.[48]

Appendix Figure K-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 are 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 K-1 shows that

[47] 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 may not capture all the 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 die at a hospital (Ministry of Health, Labour and Welfare 2010).

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

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 with considerable caution due to a very low child mortality rate.

6.1.4. Medium-term utilization and health outcomes

In this subsection, we explore the effect of subsidy during childhood on later life health care utilization and health outcomes. It is possible that a generous subsidy during childhood increases beneficial care and makes children healthy which, in turn, reduces health care utilization and improves health outcomes when they become adolescents.

Here, we exploit the fact that in almost all the municipalities, subsidy expires at age 15 (the end of junior high school). Furthermore, the duration of subsidy that children receive before age 15 differs by municipalities. Thus, we can relate the length of the free care (full subsidy) until age 15 with the health care utilization and health outcomes *after* the subsidy expiration at age 15. In this way, we can cleanly identify the medium-run effects of subsidy, if not the long run.[49]

Specifically, for the cohorts who enter the primary school (age 6) in April 2005—the start of our sample period—we observe 9 years of the subsidy information (ages 6-15), as well as one year of utilization after the subsidy expires at age 15 (as our sample precisely spans 10 school years from April 2005 to March 2015). Combining the information on the maximum age of subsidy eligibility in April 2005, we can construct the entire history of subsidy exposure from ages 0–15 for each individual, assuming that there is no inter-municipality migration before age 6. We have a total of 2,932 individuals.[50] The average length of free care between ages 0–15 is 11.37 years (SD= 1.67) with the minimum of 5 years and the maximum of full 15 years.

This approach is in the same spirit as the recent studies that relate the Medicaid eligibility during childhood on later life utilization and health outcomes (e.g., Wherry *et al.* forthcoming; Brown *et al.* 2016). Unlike these studies that rely on the assumption that children were living in the same state before they appeared in the data, our study has an advantage in that for each individual we observe the entire history of access to subsidy. As a result, we can measure the exact length of free care even in months without measurement errors.

With this set-up, Panel A of Figure 10 plots the relationship between the length of free care and

[49] One big challenge in analyzing the contemporaneous effects of subsidy on utilization is that it is difficult to isolate the potential health benefit of subsidy from the increased access that results from lower price due to the subsidy. Thus, even if an individual's health improves as a result of subsidy, the access effect may dominate in the short run, leading to higher utilization of medical services. Since we examine health care utilization and health outcomes *after* the subsidy expiration, our results are free from availability effects.

[50] These cohorts are born between April 1998 and March 1999 (12 birth month cohorts). Among 2,955 individuals, we exclude 23 individuals (0.87%) who live in municipalities that provide subsidy even after age 15.

total spending measured in thousand JPY (approximately 10 USD)—which is the sum of the monthly outpatient and inpatient spending—during age 16 after the subsidy expiration (i.e., after the graduation from junior high school). To be consistent with the analysis so far, we exclude two months of the utilization right after the subsidy expiration to account for the intertemporal substitution, and thus we observe 10 months of utilization (including these two months does not change the results). 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 do not find that more years of subsidy exposure during childhood decrease utilization after subsidy expiration. The slope is even slightly positive, 0.061 (p-value= 0.818), although it is far from statistically significant, and economically very small, indicating that a one-year increase in subsidy exposure increases the total spending by as small as 0.061 thousand JPY (or 0.61USD) per month.[51] We also examine outpatient and inpatient care separately, but the results are qualitatively very similar (not shown).[52]

One might argue that utilization may not fully capture the health status of children, if for example, children form a habit of seeing a doctor due to longer subsidy exposure. To account for this concern, we next examine whether childhood subsidy improves health status after subsidy expiration by looking at the occurrence of serious chronic health problems during age 16. 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 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".[53] Appendix Table L-1 provides the list of pediatric CCCs and the descriptive statistics. At the person-month level (N= 29,320), 2.4% of an outpatient visit is associated with one of the CCCs. At the individual level (N= 2,932), 8.7% of individuals are diagnosed with one of the CCCs in 10 months.

Panel B of Figure 10 plots the relationship between the length of free care and an outcome that

[51] We formally run the specification where outcome is the total spending per month and dependent variables are the length of the free care as well as birth year-month fixed effects (12 cohort fixed effects) and a dummy for female while standard errors are clustered at municipality to account for serial correlation within the municipalities. The results are essentially the same (results available upon request).

[52] Since we know the full history of subsidy exposure, we also investigate whether the free care at different ages has any differential impacts on later life utilization. Specifically, we break the total length of free care by each age, but none of the estimates are statistically and economically significant (results available upon request).

[53] 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 health care 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.

takes one if any visits/admissions in 10 months are diagnosed with any CCCs. Again, we do not see any discernable pattern. The slope is again economically very small (0.0021) and far from statistically significant (p-value= 0.569). Our results contrast with the recent studies on Medicaid that find some positive effect of Medicaid eligibility on long-term health outcomes.[54] 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 where universal coverage guarantees the minimum access to health care, 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. Off-hour visits

One concern of a generous subsidy is that children (and hence mothers) exploit the opportunity by increasing off-hour visits outside of regular hours because additional fees for off-hour visits are also subsidized. This may place a substantial burden on the workload of the physicians as well as increase medical spending as the fees for off-hour 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 repeatedly raised in the media, there is no formal analysis.[55]

We divide the visits into three categories: 1) regular-hour visits, 2) off-hour visits, and 3) midnight/holiday visits. Under the national fee schedule, additional fees for off-hour 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.[56] Appendix Table M-1 provides the list of billing codes for these off-hour visits and midnight/holiday visits and corresponding

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

[55] Municipalities have indeed been concerned that subsidy for child health care may increase unnecessary off-hour visits. See, for example, an article from the leading newspaper in Japan (Nikkei 2017).

[56] For example, suppose the regular hours of a clinic are registered from 9 am to 5 pm. Then, any visits outside of the regular hours are either off-hour visits or midnight/holiday visits. 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—which are between 5 pm to 10 pm and 6 am to 9 am—are considered to be the off-hour visits. Holiday visits are visits on the holiday. We combine midnight and holidays visits as fees for these two types of visits that are set higher than other visits by the national fee schedule.

additional fees. As a benchmark, fees for regular-hour visits during the sample period are approximately 2.8 and 0.7 thousand JPY (28 and 7 USD) for the first visit and revisits, respectively. The additional fees charged for off-hour visits—which are typically 0.85 and 0.65 thousand 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 off-hour visits. Note that the medical institutions can charge only one billing code from the list for each visit on top of the fee for a regular-hour visit if any.

Figure 11 plots the estimates (β_A) for regular-hour visits (for references), off-hour 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 as 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 shows that the costly off-hour visits—which account for 8.4% of total visits—also increase due to subsidy. The estimates are slightly increasing in age and are statistically significant at least above age 9. These results validate the concern that a generous subsidy increases the burden on the workload of physicians by inducing children to make off-hour visits. The semi-arc elasticities are also increasing in age, ranging from as low as –0.51 at age 7 to –1.23 at age 14. Importantly, the semi-arc elasticities for off-hour visits are much larger in magnitude than those of regular-hour visits at older ages. This indicates that at least at the older ages, off-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. In contrast, we do not see any increases in midnight/holiday visits (which only accounts for 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 visits at midnight or holidays are indeed very serious cases, and thus children and mothers are less price elastic for these unavoidable visits, which seems plausible. Appendix Figure M-1 reports the estimates on outpatient spending (instead of frequency of visits) and find similar results.[57]

In summary, the subsidy for child health care seems to increase not only the regular-hour visits but also costly off-hour visits, which may increase not only the cost but also the workload of physicians.[58] From the policy standpoint, the subsidy by the *municipal* government partially undoes the effort of the *national* government to discourage costly off-hour visits for non-serious reasons by setting higher fees

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

[58] It is certainly possible that the additional cost of off-hour 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 the regular hours in the absence of subsidy. Ultimately, the availability of free off-hour visits may affect the labor supply of parents. Unfortunately, since our claims data do not include any parental information, we cannot investigate such possibilities.

for these visits. We do not find evidence, however, that the subsidy increases midnight/holiday visits when the health care resources (e.g., physicians and nurses) are most scarce.

6.2.2. Inappropriate use of antibiotics

Another concern of a generous subsidy for children is that it may increase the inappropriate use of medications. Since more than half of the 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, the antibiotic-resistant infections annually affect at least 2 million people, and 23,000 people die as a direct result of these infections in the United States (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 N for the details on creating the list. Appendix Table N-1 presents the list with the corresponding ICD10 as well as summary statistics of antibiotic usage. For example, antibiotics use for children with bronchitis and asthma are considered inappropriate. Even without subsidy, roughly 20% of the children diagnosed for any of these diseases are prescribed with 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 0.24 thousand JPY (Column 6), and the frequency of antibiotics prescriptions is 0.94 per person-month (Column 7) without subsidy. Both numbers are far from zero.

It can be problematic if the subsidy increases the number of children in these diagnoses who are prescribed with antibiotics. To investigate this possibility, we estimate equation [3] where the outcome is the interaction of being diagnosed as any of these diseases and total spending on antibiotics in Panel A, and frequency of antibiotics prescriptions in Panel B. Panel A of Figure 12 shows that the subsidy increases the spending on antibiotics by 0.009 to 0.020 thousand JPY, which is 17–38% from the mean. Similarly, the frequency of antibiotics prescriptions increases by 0.039 to 0.070 (20–36% from the mean) in Panel B. Thus, our results suggest that a generous subsidy seems to increase the inappropriate use of antibiotics, potentially leading to more antibiotic-resistant infections and adverse effects.

6.3. Price responsiveness by health status

Finally, we examine whether the effect of cost-sharing varies by patient health status.[59] One might expect that as Manning *et al.* (1987) conjectured, 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 allows 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 (See Online Appendix O for the details). Using prior spending as an indicator for health status has been used in previous studies (e.g., Dranove *et al.* 2003). Without the subsidy, the probability of having at least one outpatient visit in a month is 44% for the sick, which is substantially higher than that for the healthy (20%), as expected (see Appendix Table O-1). Similarly, the sicker children spend on average 6.89 thousand JPY per month, which is more than three times higher than that of the healthy children (2.04 thousand JPY per month). We estimate the model separately for each type.

Figure 13 shows that health status does indeed affect children's response to cost-sharing. Specifically, the healthier children are much more price sensitive than the sicker children. 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 the outpatient spending. These results show that sicker children are not the ones who forgo treatments most in the absence of subsidy. Conversely, our results indicate subsidy-induced medical spending for the healthier children is more discretionary and relatively low-value. The result also indicates that it is not the sickly children but the healthy children who will cutback medical care most in the absence of a generous subsidy. In Appendix O, we experiment with different windows (9 and 12 months) to calculate the patient health status and

[59] A few papers examine the heterogeneity in price responsiveness by the patient health status but the evidence is mixed. A seminal work by RAND HIE finds no difference between healthier and sicker patients (Manning *et al.* 1987). More recently, Chandra *et al.* (2014) find lower price elasticities among individuals with chronic illness and with higher levels of prior spending among the low-income non-elderly population in Massachusetts. Fukushima *et al.* (2016) examine the elderly in Japan and find that, similar to ours, the sicker elderly are less price responsive than the healthier elderly. In contrast, Brot-Goldberg *et al.* (2017) documents that the sickest quartile of consumers reduces spending most in response to the introduction of high-deductible health plan in the US.

find qualitatively similar results across the windows.[60,61]

7. Conclusions

Understanding the price responsiveness to health care is a central question in health economics and the fundamental issue for the optimal design of health insurance. However, past studies on price elasticities are predominantly concentrated on adults and the elderly, and surprisingly little is known regarding children. In this paper, we examine the effect of patient cost-sharing on health care utilization among children by exploiting more than 5,000 regional and over-time variations on subsidy availability. Importantly, the eligibility for subsidy is always tied to the age of children, enabling us to estimate the price elasticity of children at each age from 7 to 14.

We find that the reduction in cost-sharing from 30% (national level) to 0% (free) increases the outpatient spending by 22–31%. The semi-arc elasticities are relatively stable at approximately –0.6 across the age ranges we examine. While considerable caution is needed to compare the elasticities estimated across countries or time periods, the elasticities for children estimated in this paper 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, implying that policy makers can reasonably employ existing elasticity estimates, regardless of the direction of the price changes. Additionally, 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 hospitalizations 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

[60] We also examine the price elasticities by gender. While the raw outpatient spending is always higher for boys than girls at any ages, the price elasticities are very similar across gender (results available upon request).

[61] While we have limited information on supply-side, JMDC data categorizes medical providers into four types: public hospitals, teaching hospitals, other hospitals, and clinics (similar to office visits in the US). In Japan, hospitals are defined as medical institutions with 20 or more beds, and clinics are those with less than 20 beds. There are no for-profit hospitals in Japan in the sense that medical institutions are prohibited from issuing a bond. Since the same national fee is applied to all the medical providers, the sole incentive for patients to visit hospital rather than clinics is because people tend to believe that hospital is of higher quality while the waiting time is longer, and making appointment is harder. Most outpatient visits are at clinics (77.1% of spending and 89.5% of frequency of visits), and thus we combine all the hospital visits into one category. Not surprisingly, we find that almost all the increases in outpatient spending come from clinics (results available upon request). Since people have much easier access to small clinics rather than large hospitals, these visits are probably less serious and more discretionary, providing another suggestive evidence of increases in non-essential care.

and costly off-hour visits. Furthermore, the healthier children—measured by prior spending—are much more price sensitive than the sickly children, which appears to indicate that the subsidy induces discretionary health care utilizations.

Taken individually, each piece of empirical evidence may 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 health care subsidy may lead to the increases of the low-value and costly outpatient visits. Importantly, our results contrast with the studies on Medicaid in the US that document the positive effect of Medicaid eligibility on both the short- and long-term utilization and health outcomes. In our setting, where universal coverage guarantees a minimum access to health care, the additional generous subsidy does not seem to have any meaningful positive impacts, at least among the utilization measures and health outcomes observed in our data.

This paper 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 the long-run health beyond the ages of our sample, which can potentially justify the generous subsidy for child health care. Another important limitation is that our insurance claims data do not include basic parental characteristics such as income and education.[62] 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, health care utilization, and any household characteristics, do not exist in Japan—due mainly to the lack of an individual identifier in Japan, such as the social security number in the US—which leaves an avenue for future research.

[62] Surprisingly, there has been relatively little research on the impacts of patient cost-sharing by income and Baicker and Goldman (2011) indeed write that "while there is a lot of speculation that the poor have more elastic demand, there is little evidence." One notable exception is Nilsson and Paul (2018) which document that children from low-income families respond more strongly to copayment. To the extent that household income is negatively correlated with the magnitude of price elasticity, our estimates from the children of relatively richer family serves as the lower bound of the price elasticity among general children.

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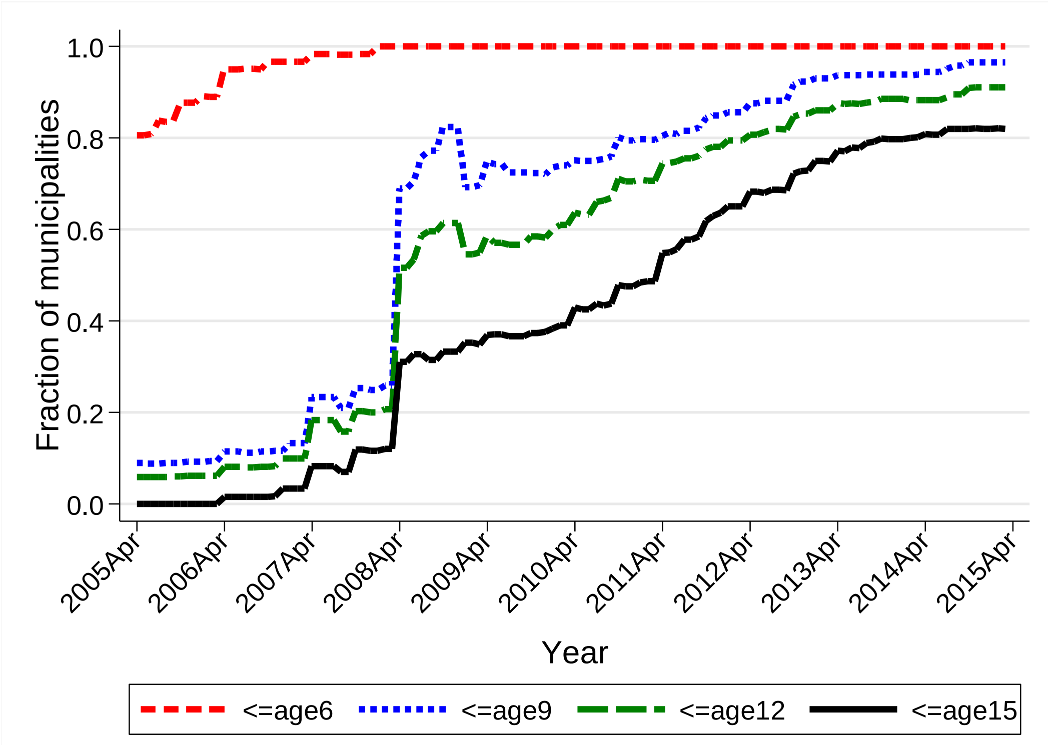
Figure 1: Time series of maximum age fully covered by child care subsidy

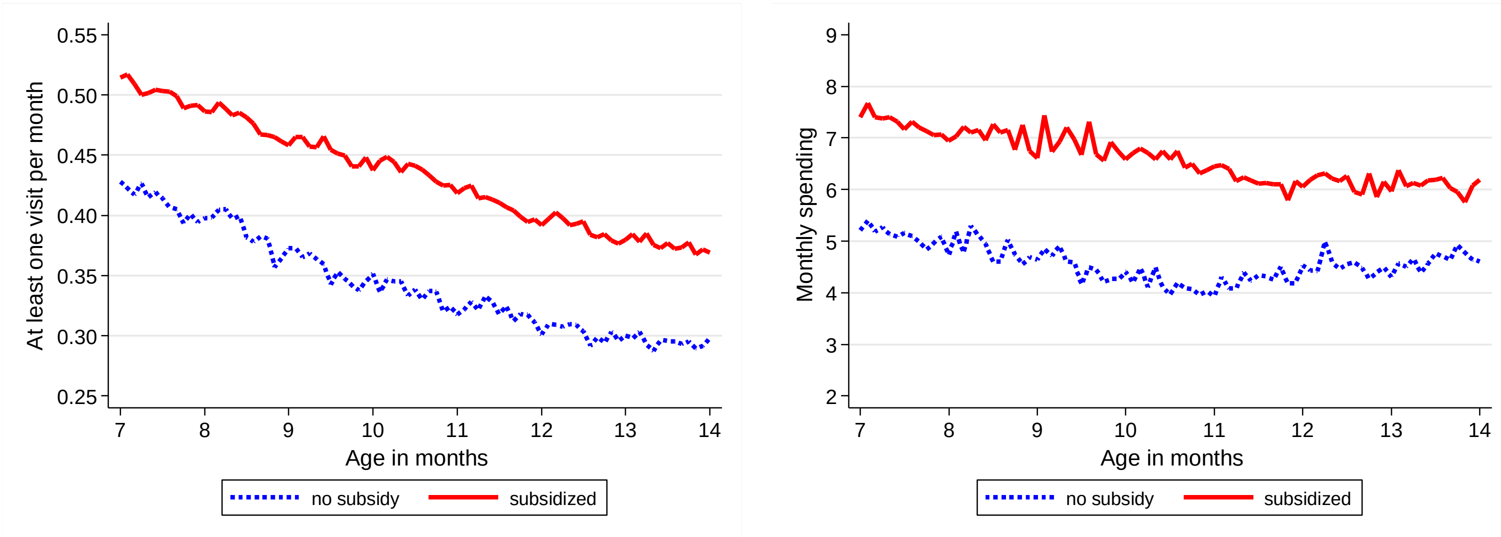


Notes: The data is unbalanced monthly panel where the unit of observation is municipality. There are total of 165 municipalities which are mainly used in this study (the main sample). Note that this figure reflects the compositional changes of municipalities as the number of municipalities increases at the later period in our claim data. Importantly, within the municipalities, the subsidy expansion is always monotonic—that is, there is no single municipality that *lowers* the maximum age of subsidy eligibility during our sample 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 have already provided the subsidy until age 6 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 see the highest number of municipality-level subsidy expansions in April 2008 to ages higher than age 6 (See Appendix Figure A-1 on the precise timing of all policy changes).

Figure 2: Utilization with or without subsidy by age

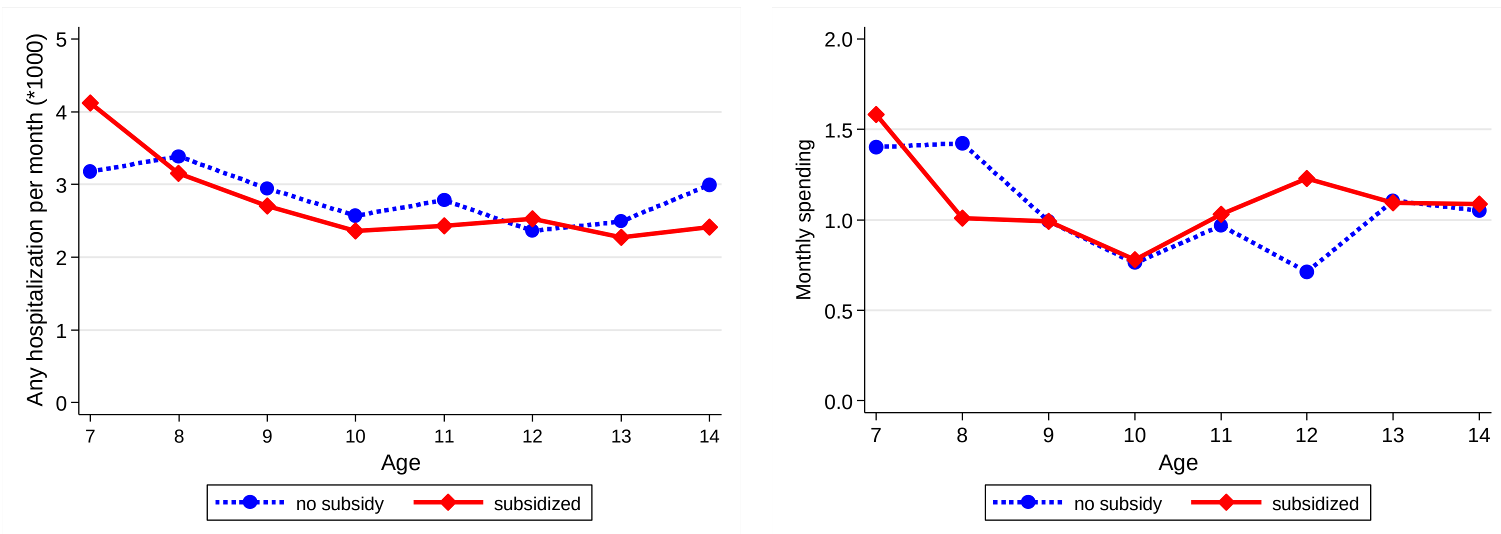
A. Outpatient care (monthly)

Outpatient dummy | Outpatient spending (in 1K JPY)



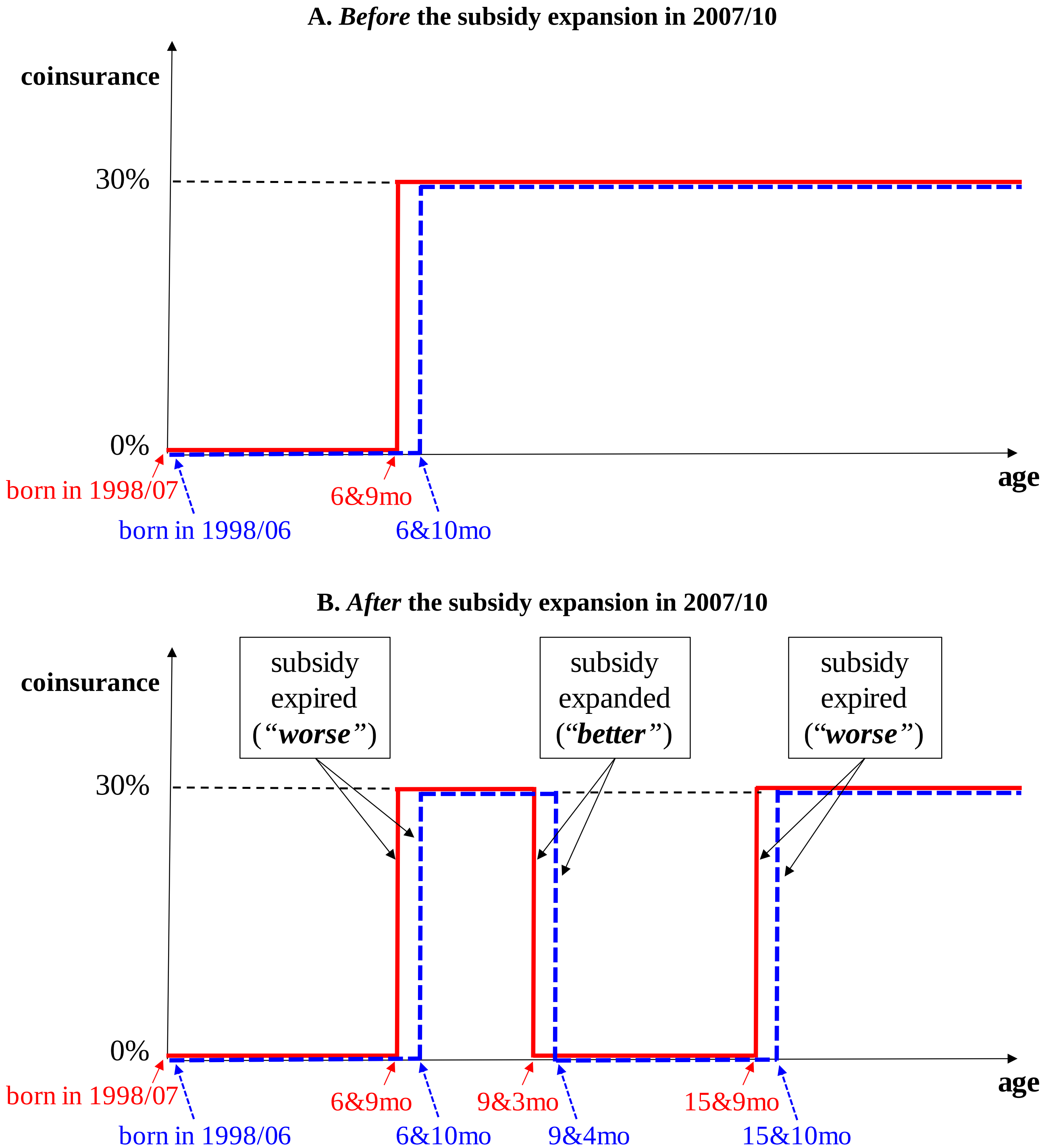
B. Inpatient care (yearly)

Inpatient dummy (×1000) | Inpatient spending (in 1K JPY)



Notes: The main sample is used. 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 the inpatient spending is monthly spending on inpatient care, both of which are measured in thousand 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

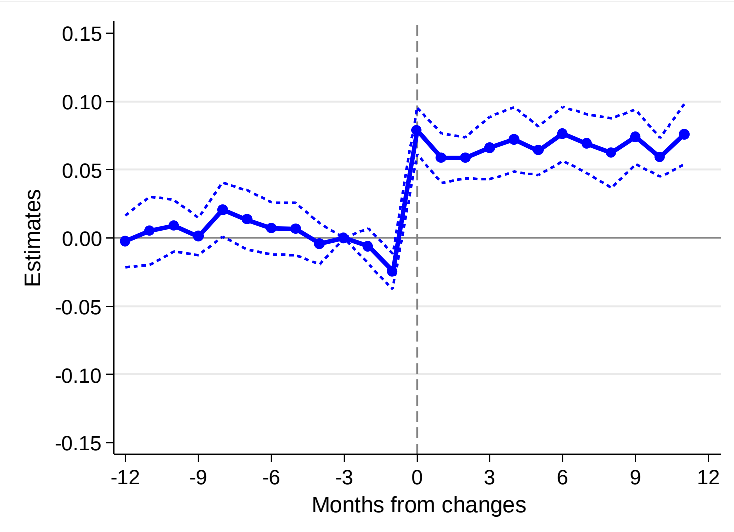


Notes: The solid line is the price schedule for cohort born in July 1998 (the "younger" cohort) while that of dotted line is the price schedule for cohort born in June 1998 (the "older" cohort"), a month before the previous cohort. In this example, the subsidy expansion from age 6 (the beginning of primary school) to 15 (the end of junior high school) occurs in October 2007.

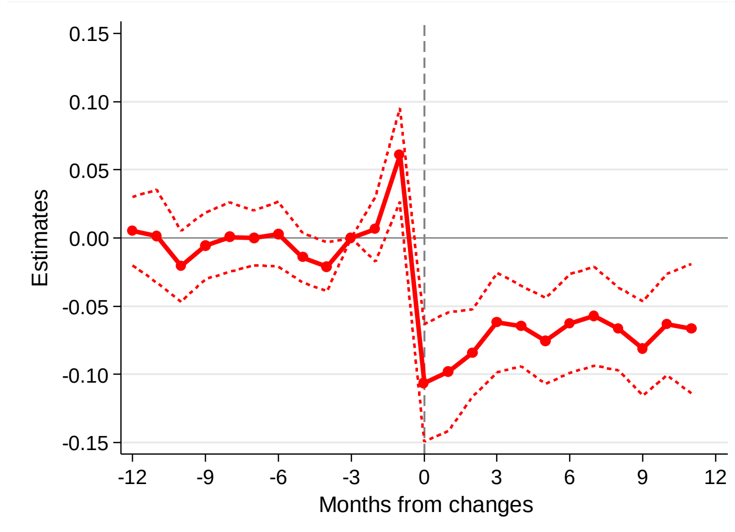
Figure 4: Event study

A. Outpatient dummy

Better (subsidy expansion)

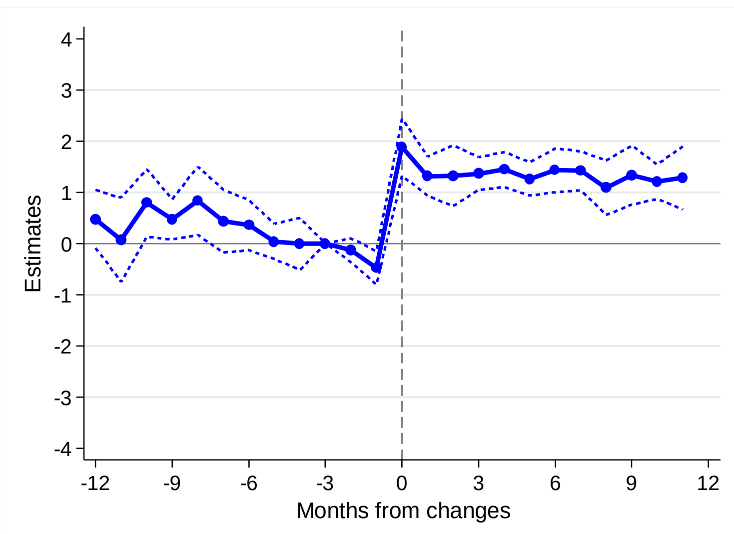


Worse (subsidy expiration)

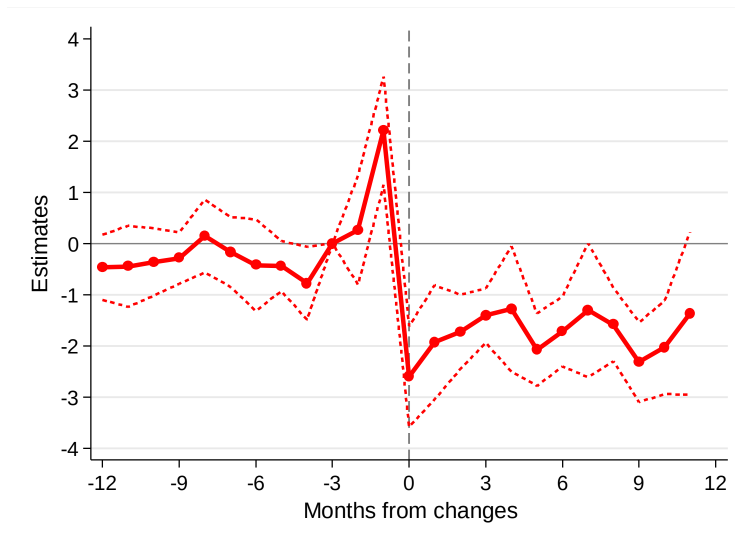


B. Outpatient spending (in 1K JPY)

Better (subsidy expansion)

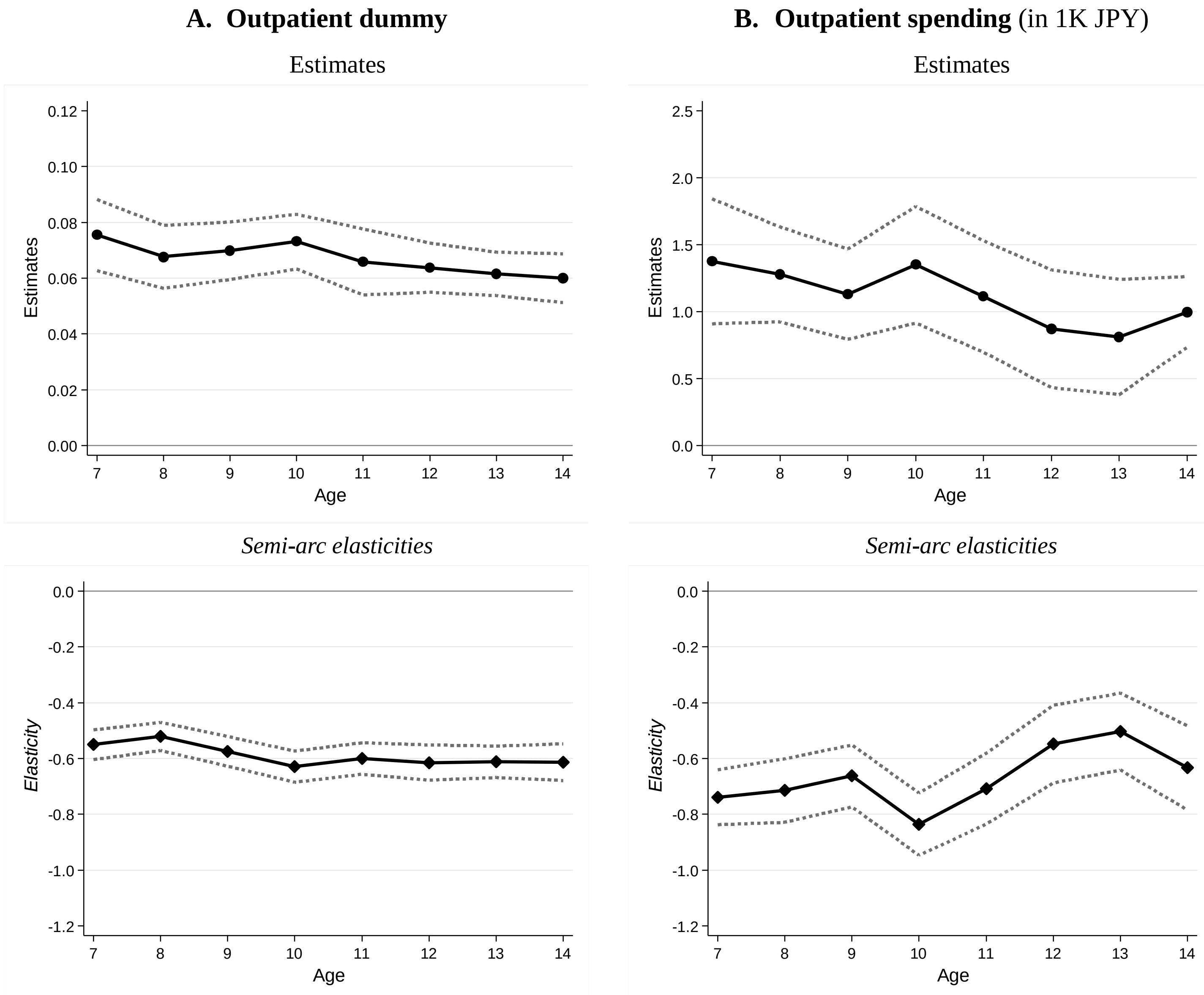


Worse (subsidy expiration)



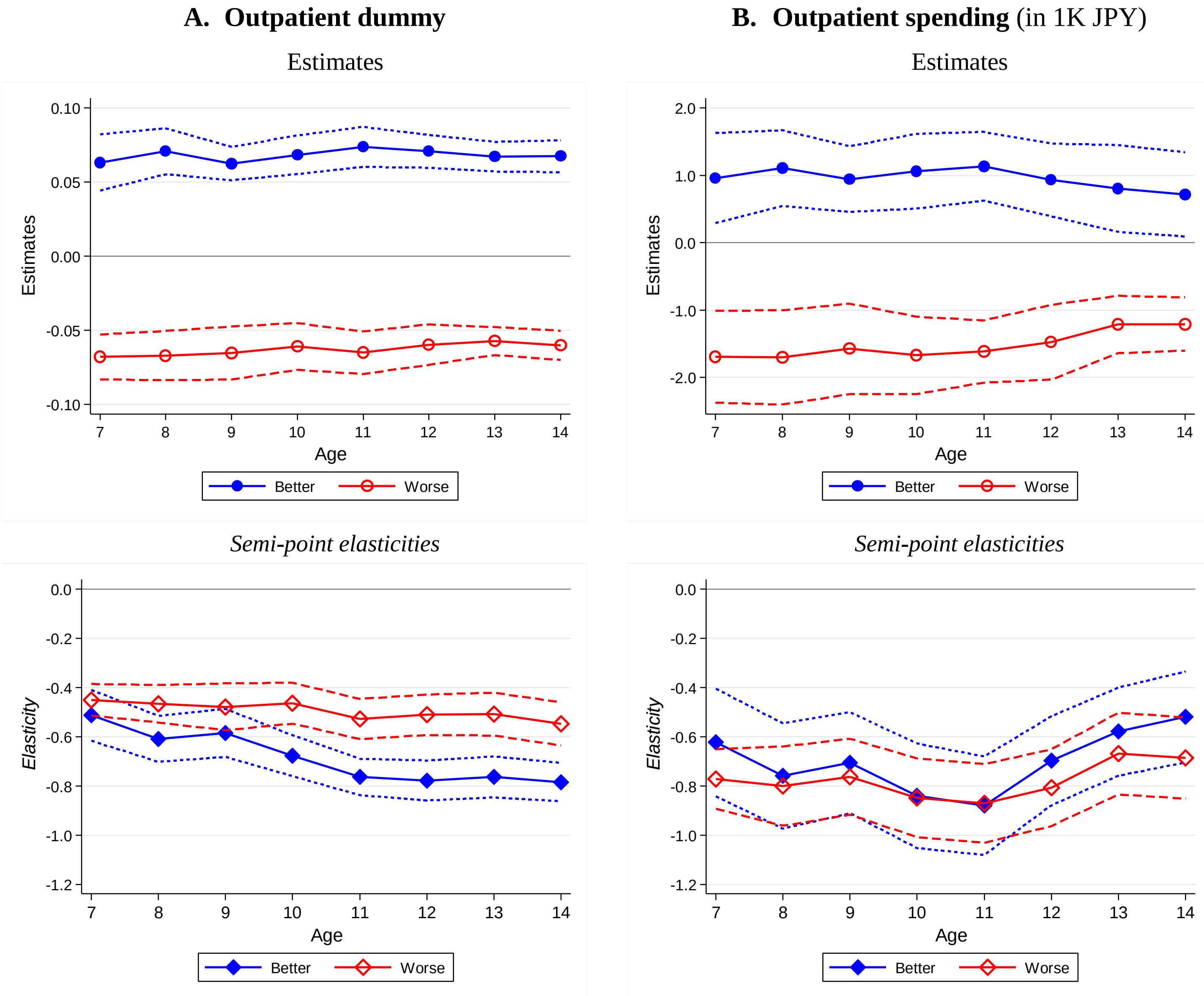
Notes: The main sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in thousand JPY (approximately 10 USD). “Better” indicates the subsidy expansion which lowers the price of health care 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 a series of dummies for each month ranging from 12 months prior to the change in subsidy status to 12 months after the change in subsidy status (T= –12 to +11, where T= 0 is the change). 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). Note that the scales of y-axis are set the same within the panels so that two figures for opposite directions of price changes are visually comparable.

Figure 5: Basic results



Notes: The 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 thousand 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 elasticities (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 elasticities. The observations within two 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

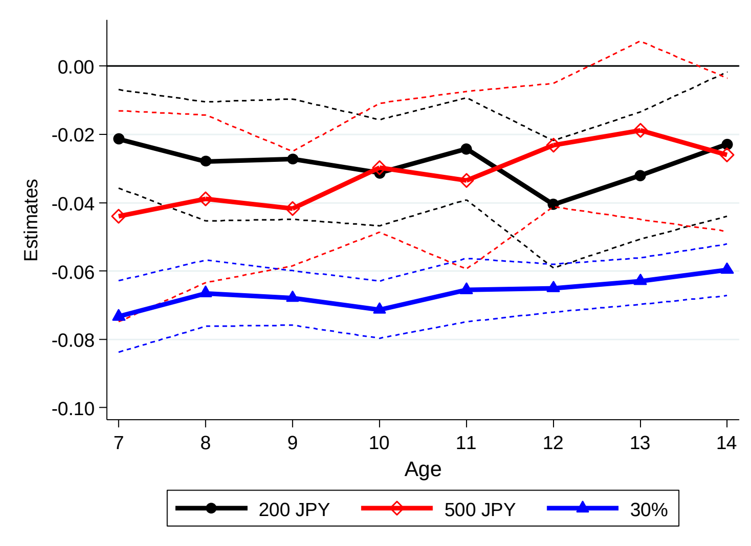


Notes: The main sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in thousand JPY (approximately 10 USD). "Better" indicates the subsidy expansion which lowers the price of health care 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 elasticities (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 elasticities. The observations within two months from the price changes are excluded from the sample to account for anticipatory utilization. Note that 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 F-1.

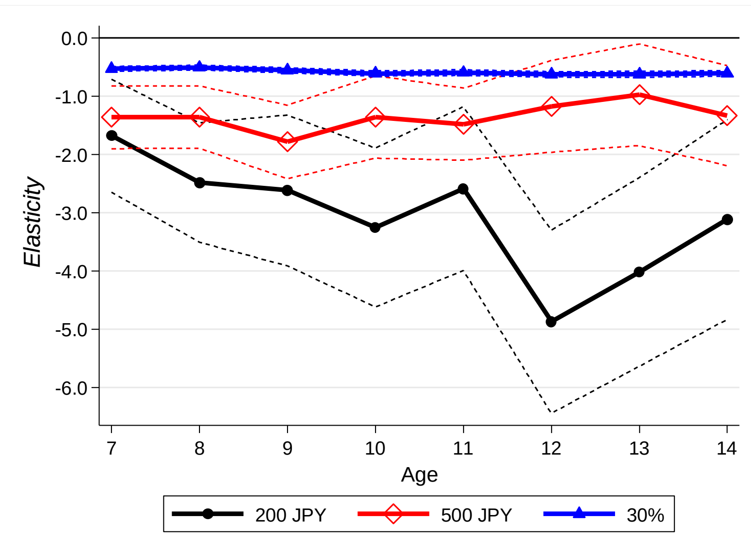
Figure 7: Effect of small copayment

Outcome: Outpatient dummy

Estimates

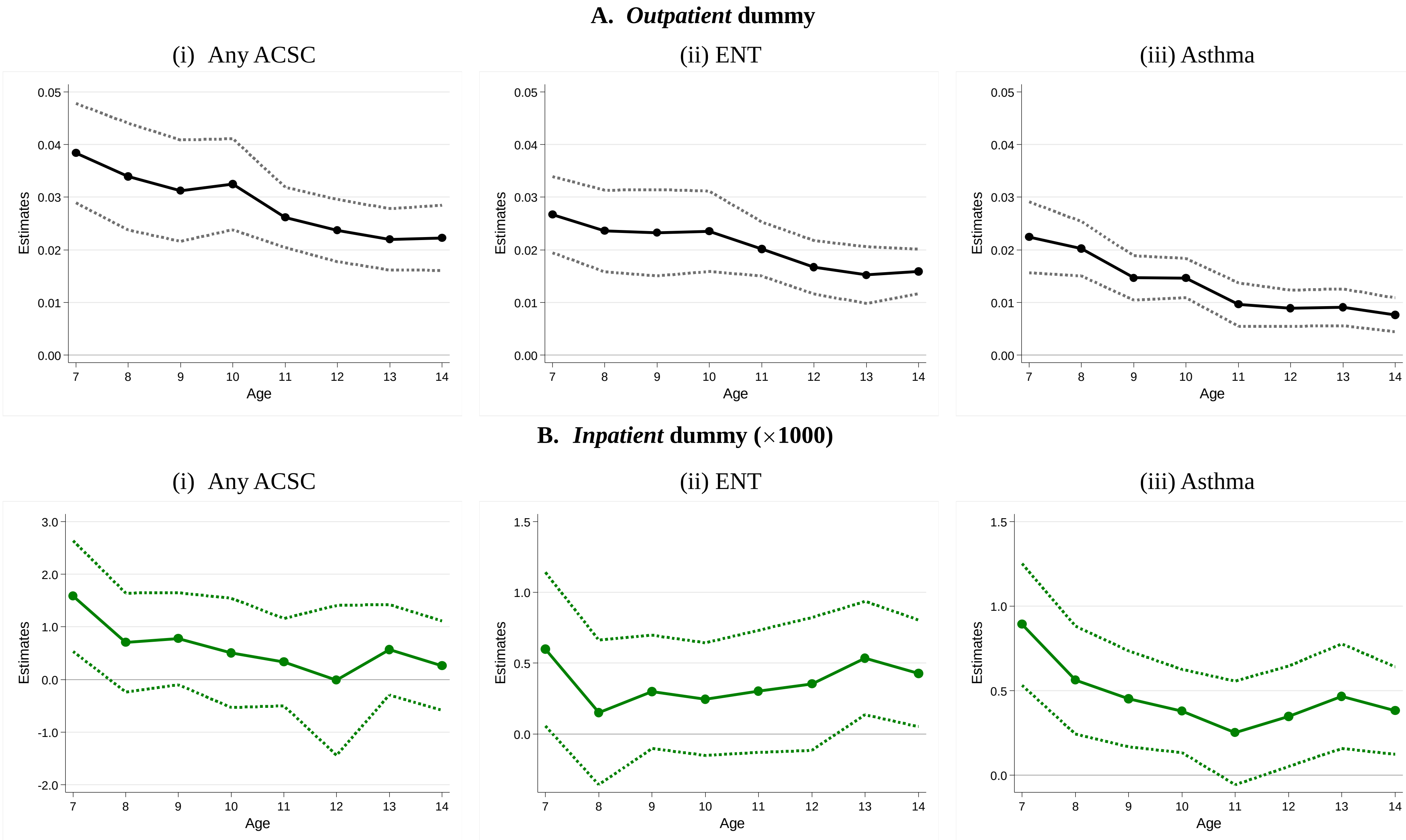


Semi-arc elasticities



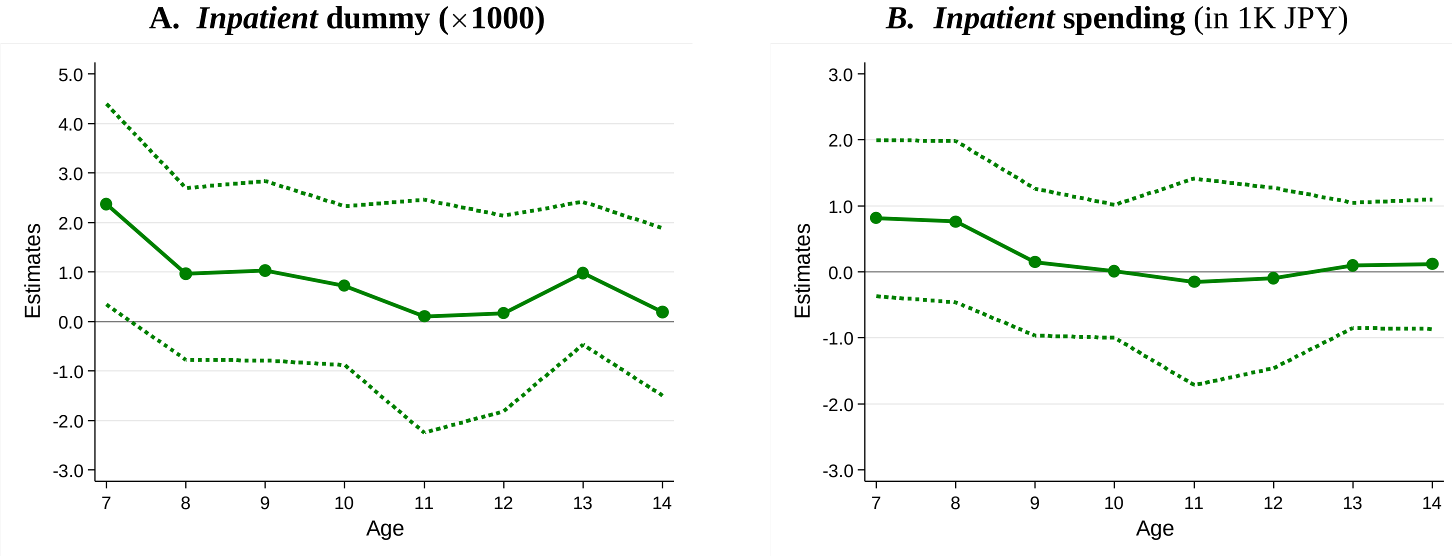
Notes: The full sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month. The upper half plots β_A^C for each age (A=7–14) and three price levels (C= 200 JPY/visit, 500 JPY/visit, and 30%) from estimating equation [5], and the bottom half plots the corresponding semi-arc elasticities (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 elasticities. The observations within two months from the price changes are excluded from the sample to account for anticipatory utilization. The corresponding table is found in Online Appendix Table G-1.

Figure 8: Ambulatory Care Sensitive Conditions (ACSC)



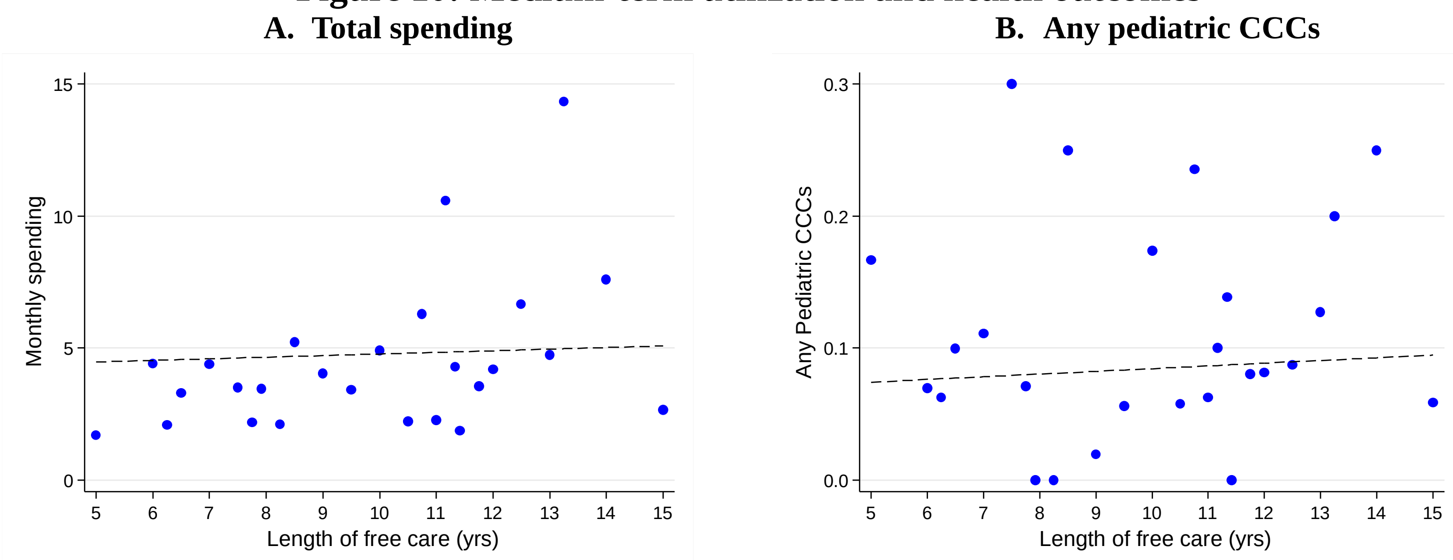
Notes: The main sample is used. An outpatient dummy takes one if there is at least one outpatient visit per month, and an inpatient dummy takes one if there is at least one hospitalization per month (×1000). The estimates β_A for each age (A=7–14) from estimating equation [3] are plotted. See Online Appendix I 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 two 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 I-2.

Figure 9: Offset effects



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 thousand 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 two months from the price changes are excluded from the sample to account for anticipatory utilization. The corresponding table is found in Online Appendix Table J-1.

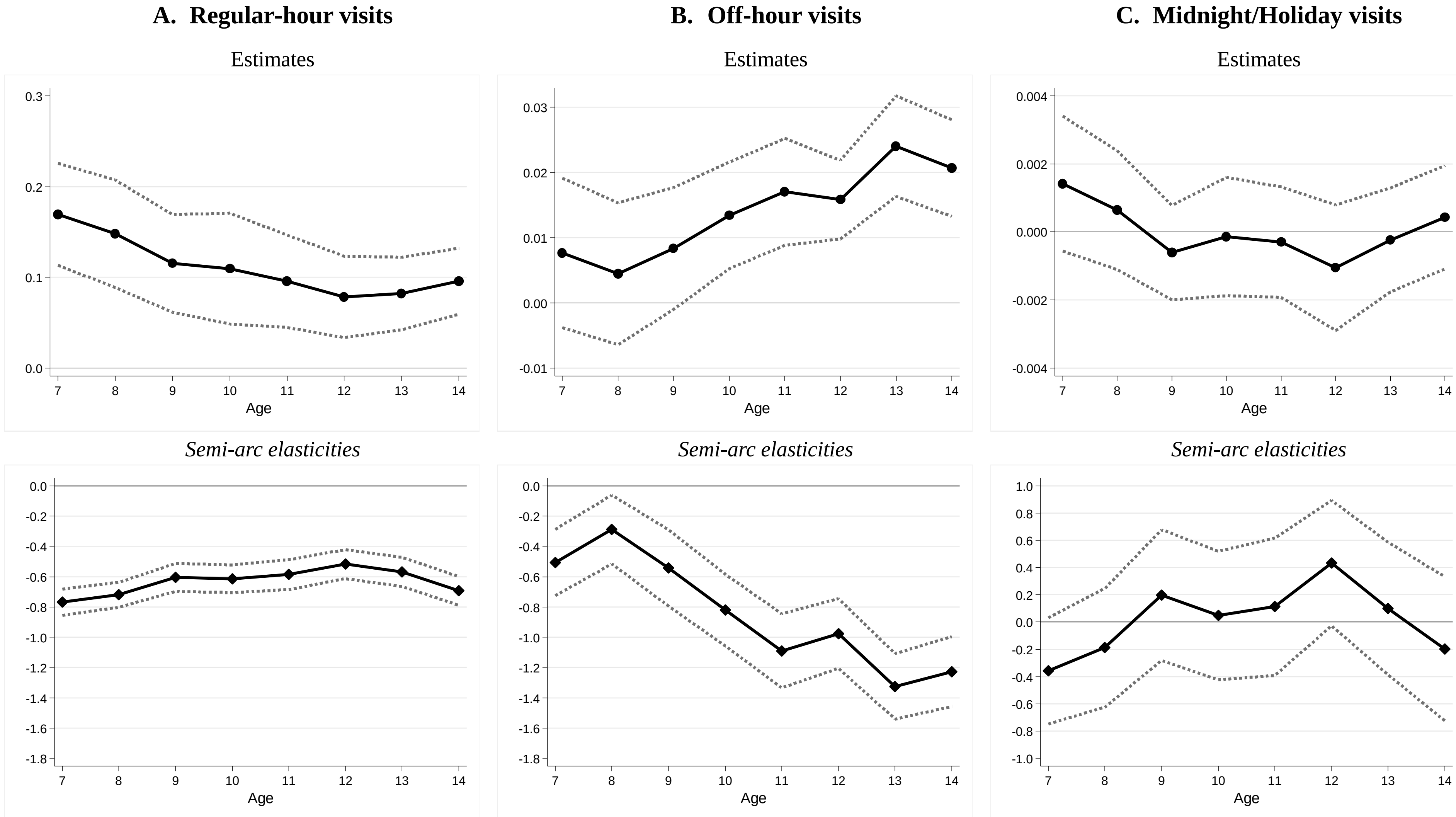
Figure 10: Medium-term utilization and health outcomes



Notes: The sample is limited to the cohorts who enter the primary school (age 6) in April 2005—the start of our sample period—for whom we observe 9 years of the subsidy information (ages 6–15), as well as one year of utilization after the subsidy expires at age 15 (see the main text for the details). We have a total of 2,932 individuals. Each dot presents the means of the outcomes by each birth year-month cohort×municipality. Since the observations within two months right after subsidy expiration at age 15 are excluded from the sample to account for anticipatory utilization, the outcomes are observed for 10 months during age 16. The dotted line is the predicted values of weighted least square regressions where weight is the number of observations in each dot. The x-axis is the length of free care (in years) between ages 0–15. The mean length is 11.37 years (SD=1.67) with the minimum of 5 years and the maximum of 15 years. The y-axis is the health care utilization at age 16 after subsidy expiration at age 15. The outcome in Panel A is total spending measured in thousand JPY (approximately 10 USD), which is the sum of the monthly outpatient and inpatient spending. The outcome in Panel B is a dummy that take one if any visits/admissions in 10 months are diagnosed with any pediatric complex chronic conditions (CCCs) from Feudtner *et al.* (2014). See Online Appendix K for the list of CCCs. The slope is 0.061 (p-value= 0.818) for panel A, and 0.0021 (p-value= 0.569) for panel B, both of which are far from statistically significant.

Figure 11: By time of visits

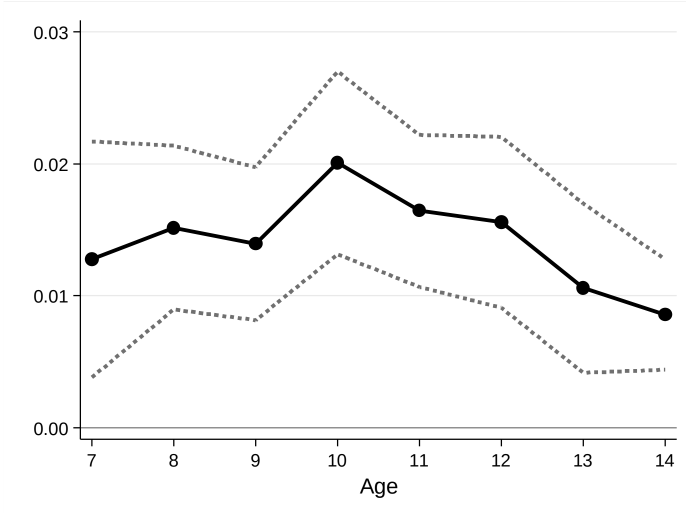
Outcome: Frequency of outpatient visits



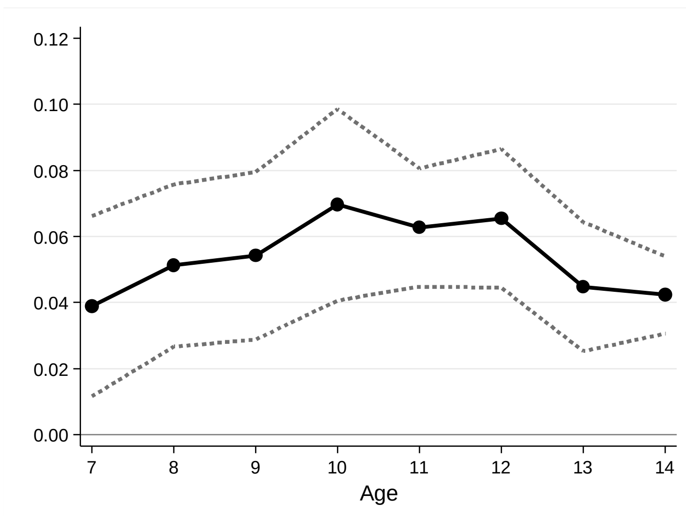
Notes: The main sample is used. The frequency of outpatient visits is the number of outpatient visits per month. See Online Appendix M which provides the list of billing codes for off-hour and midnight/holiday and corresponding fees that are additionally charged on top of fees for regular-hour visits. 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 elasticities (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 elasticities. The corresponding table is found in Online Appendix Table M-2.

Figure 12: Inappropriate use of antibiotics

A. Outpatient spending on antibiotic drugs
(in 1K JPY)



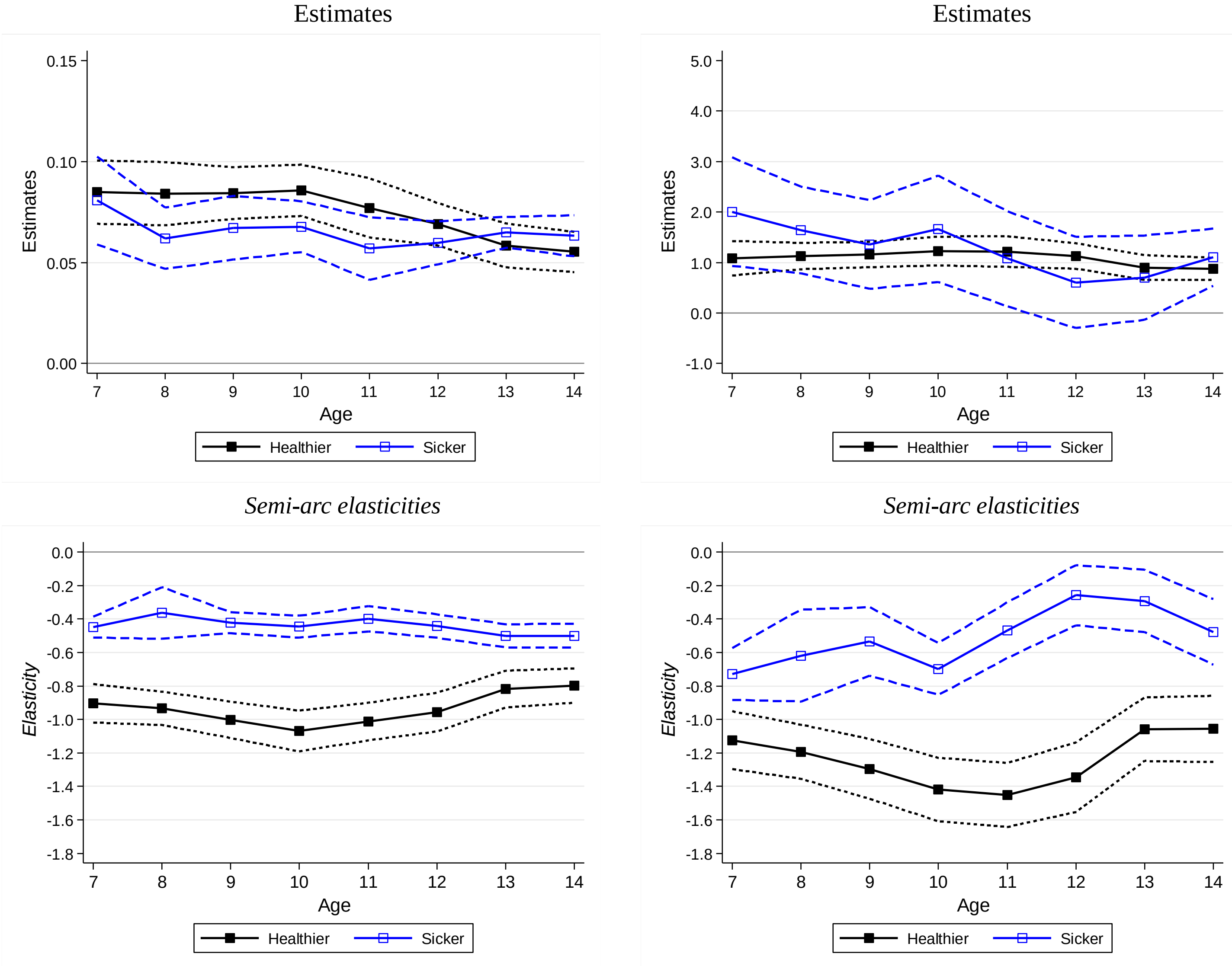
B. Frequency of antibiotics prescriptions



Notes: The main sample is used. The outcome is monthly outpatient spending on antibiotics considered as inappropriate measured in thousand JPY (approximately 10 USD) in Panel A and the number of prescriptions for the antibiotic per month in Panel B. See Online Appendix N for the list of ICD10 codes and the summary statistics. 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 two months from the price changes are excluded from the sample to account for anticipatory utilization. The corresponding table is found in Online Appendix Table N-2.

Figure 13: Price responsiveness by health status

A. Outpatient dummy | **B. Outpatient spending** (in 1K JPY)



Notes: The main sample is used. An outpatient dummy in Panel A takes one if there is at least one outpatient visit per month, and outpatient spending in Panel B is the monthly spending on outpatient care measured in thousand 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 elasticities, separately for two types of children grouped by initial health status. See Online Appendix B for details on derivation of semi-arc elasticities. We determine each child's initial health status by the outpatient spending in the first 6 months since the child is observed in the claim data. 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) in the first 6 months of observations. 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 elasticities. The observations within two 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-1.

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

		Mun-time-age cell		Year-month	
Before change	*After* change	N	*Share*	N	*Share*
30%	0%	3,623	*30.6%*	15,472	*39.7%*
0%	30%	2,790	*23.6%*	11,814	*30.3%*
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 total number and the share is based on the total number of price changes (not just by top 10 combinations). In this paper, we mainly focus on the first two price transitions. 200, 300 and 500 JPY are roughly 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 thousands JPY (roughly 10USD).