Free for Children? Patient Cost-sharing and Health Care Utilization*

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March, 2018

Abstract: Understanding how patient responds to price is a key to the optimal design of health insurance. However, past studies are predominantly concentrated on the adults and elderly, and surprisingly little is known about children. We examine the effect of patient cost-sharing on health care utilization among children by exploiting newly collected data on drastic subsidy expansion in Japan, with more than 5,000 changes in subsidy status at municipality-age-time level. We find that free care for children significantly increases spending on outpatient care by 22–31%, with the arc-elasticity of –0.1 for all ages 7–14, which is smaller than the conventional estimate for adults. Interestingly, 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. Finally, we provide suggestive evidence that that increases mostly reflect low-value or costly care. Increases in outpatient visits do not translate to reduction in hospitalization by "avoidable" conditions nor reduction in mortality. Furthermore, we document that inappropriate use of antibiotics and costly off-hour visits increase. Taken together, we conclude that the benefit of such generous subsidy is limited at least in the short-run.

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

* The authors thank David Chan, Liran Einav, Martin Gaynor, Jon Kolstad, Chris Muris, Krishna Pendakur, and seminar participants at the Asian and Australasian Society of Labour Economics (AASLE) Inaugural Conference 2017, Keio University, Kyushu University, National University of Singapore, Osaka University, Simon Fraser University, Singapore Management University, Stanford Asian Health Policy Program Seminar, the University of Tokyo, for their suggestions. Shigeoka gratefully acknowledges the financial support from Abe fellowship. Masaki Takahashi provided excellent research assistance. All remaining errors are our own.
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1. Introduction

Childhood health has shown to influence both short and long-term socioeconomic outcomes and health, (Case *et al.* 2005; Currie 2009; Smith 2009), providing the ground for generous subsidy for child health care in public health insurance programs across the countries. 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, Netherland, Taiwan and Korea.[1] The lower out-of-pocket cost, however, may induces unnecessary consumption of medical services. In fact, in countries with the universal coverage, one of the few demand-side approaches on containing rising medical expenditure is to finetune the level of patient cost-sharing.[2]

Understanding how patient responds to price is a key to the optimal design of health insurance but past studies on patient cost-sharing are predominantly concentrated on adults and the elderly, and surprisingly little is known about children.[3] We have a couple of good reasons to believe that evidence from the adults and elderly may not be simply applicable to children. First, mothers, who may need to take their children to clines, may face higher opportunity cost than the elderly. Second, 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 view that the return from child health care is large as children tend to live longer.[4]

This paper examines the effect of patient cost-sharing on health care utilization among children. To do so, we newly hand-collected data on 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 years and for different age groups, we have more than 5,000 changes in subsidy status at municipality-age-time cell, which is the level of the variation for identification in our empirical analysis. This very unique variation in subsidy generosity combined with individual panel data enables us to estimate the age-specific price elasticity in a difference-in-difference framework. Furthermore, we examine the utilization patterns by type of visit

[1] Children and adolescents are exempt from cost sharing up to age 18 in Germany and Netherlands. Similarly, children below age 3 are exempt from payment of health care in Taiwan, and those below age 6 are subsidized in Korea.
[2] 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).
[3] See Baicker and Goldman (2011) for an excellent review of the literature on patient cost-sharing. 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.
[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).

and health status to investigate whether the increases in utilization mainly reflect beneficial or low-value care.

The unique institutional background in Japan offers a clean setting in identifying patient price responsiveness since the roles of insurers and medical providers are relatively limited. As for insurances, 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. As for medical providers, they cannot price discriminate patients as 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 actual transaction price which allows us to easily quantity the monetary values of (excess) utilization unlike the case in the US which is notorious for the complex price schedule.

Our paper contributes to the literature on demand for health care among children in several ways. First and most importantly, the extensive variation in subsidy status, which is always tied to the age of children, enable us to estimate age-specific price elasticity of 7–14 years-old children even at each age in month level. The price elasticity at various ages can be informative for policy makers to design 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 or not 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 when the policy maker considers implementing a policy with the opposite direction of price change. On the other hand, if such asymmetry does not exit, it is very useful for welfare analysis as the standard welfare calculation does not differentiate the direction of the price changes in their analysis. Despite the importance, the research design of past studies (such as randomized control trial or regression discontinuity design) precludes them from testing it as there is only single direction of price change.

Finally, our unique panel dataset which observe *both* outpatient and inpatient spending for the same individual in the same data 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 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 RAND Health Insurance Experiment (RAND HIE, hereafter) conducted in 1970s

(Newhouse 1993; Manning *et al.* 1987)—which randomly assign families to different patient cost-sharing level—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 findings on children's price responsiveness to health care. We find that reduced cost-sharing significantly increases utilization of outpatient care among children. When the municipal subsidy lowers the patient 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 percent in the absence of the subsidy. Similarly, total outpatient spending per month increases by 1.00–1.38 thousand JPY（roughly USD10.0–13.8）, which corresponds to 22–31% increase from the mean of 4.49 thousand JPY (roughly USD44.9). The overall arc-elasticity is relatively constant for both outcomes around –0.10 throughout ages 7–14, which is somewhat smaller than conventional estimate of –0.20 for non-elderly in RAND HIE (Keeler and Rolph 1988). Interestingly, we find little evidence of asymmetric responses, meaning that children respond to different directions of price changes in a similar magnitude in our setting. This finding implies that policy makers can reasonably employ existing elasticity estimates, including ours, regardless of the direction of the price change that they consider.

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 (974 million 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.

In the second part, we investigate whether subsidy-induced excess 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 how such reduction occurs. While this is always a challenging task—especially for children as the nature of diseases tends to be acute, we examine heterogeneity of utilization patterns from various dimensions and the short-run mortality rate 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 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 with the reduction in inpatient spending. Also, we find little impact on short-run child mortality while we need to interpret this result with considerable caution due to very low mortality rate among children of this age range in Japan. Taken together, we find little evidence of increases in "beneficial" care.

Then, we next turn to 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 generous subsidy by increasing physician visits outside of regular hours. In addition, the 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. This increase in off-hour visits may place a substantial burden on the workload of the physicians as well as increase the overall spending since the national fee schedule sets higher fees for off-hour visits 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 dies as a direct result of these infections in the United States (Centers for Disease Control and Prevention 2013). To our knowledge, no prior studies has investigated whether financial incentives, such as subsidy for child health care, increases 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 medical care most in the absence of generous subsidy. Taken individually, each piece of evidence is not sufficient to establish the existence of wasteful utilization. But taken together, 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-term benefit of such generous subsidy is at least far from clear.

This paper is most related to RAND HIE as our knowledge on price sensitivity to health care among the non-elderly still relies. Leibowitz *et al.* (1985) specifically analyzes children under age 13 and finds that, among others, the use of outpatient services decreased as cost-sharing increases. However, the study suffers from small sample size to identify the effect for some types of services (e.g., inpatient care).[6] Furthermore, it is nearly 40 years old, and thus changes in practice of medicine (e.g., reliance on

[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 for children.

[6] 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

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 non-experimental works are recent papers by Han *et al.* (2016) which examine the effect of cost-sharing at age 3 in Taiwan, and Nilsson and Paul (2015) which examine similar question for children in one region in Sweden.[7] Our study may arguably entail broader policy implications than all of these studies because of variety and number of variations in cost-sharing, wider coverage of ages, and detailed analysis on comprehensive health care utilization.

Finally, this paper also is related to large literature on health insurance and child healthcare utilization especially the studies on Medicaid in the US (e.g., Currie and Gruber 1996; Gruber and Dafny 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) like 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. Finally, the assignment of health plans often affects the entire family like RAND HIE.

The rest of the paper is organized as follows. Section 2 provides the institutional background in 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). Regardless of the insurer, people face the same fee schedule and benefits package both of which are set

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services thanks to insurance coverage, and hence less likely to become sick, the children who resides with parents may less likely to get cold.

[7] Han *et al.* (2016) exploit the copayment change at age 3 in Taiwan and find that 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 (2015) exploit the abolishment of copayments for outpatient care among children between 7 and 19 years in one region in Sweden. They find that children increased their number of visits to a doctor while the effect vary substantially by income, with children from low income families being three times as responsive as their more advantaged peers.

by national government.

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 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 often restrict the choices of medical providers through managed care. Third, patients cannot be price discriminated by physicians and hospitals since all fees paid to medical providers are solely based on the national fee schedule (i.e., Fee-For-Service). As a consequence, medical providers receive the same fee for the same service regardless of patient insurance type or age. 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 health care utilization comes from quantities instead of prices since there is no room for price shopping to search cheaper providers.

2.2. Patient cost-sharing

Patient cost-sharing—for which the beneficiary is responsible out of the 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 patient pays a 1% coinsurance above a threshold.[8] Unlike a typical health insurance plan in the United States, there is no deductible in Japan.

The nonlinearity imposed by the stop-loss, which 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 anticipate spending beyond the stop-loss may respond to true "shadow" price rather than "spot" price (Aron-Dine *et al.* 2015). However, this is unlikely in this setting for the following two reasons. First, only 0.067% person-month exceeds the stop-loss as the hospitalization—which is costly and main reason for reaching stop-loss—is very rare among children of this age group (only 0.28% person-month).

[8] The supplementary health insurance that covers the remaining out-of-pocket cost does not virtually exist in Japan probably because the stop-loss prevents the catastrophic income loss upon illness.

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 coming 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 type. It is called Medical Subsidy for Children and Infants (MSCI), and it has drastically expanded in last decade. Children who are eligible for subsidy at the age 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 level of cost-sharing) that each child faces. As detailed below, there is a large variation in the generosity of subsidy across municipalities and age over time, which allows us to identify its effect on health care utilization.

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 as discussed in Section 4.2.

For each municipality, we collected the following four information on subsidy for outpatient care from April 2005 to March 2015: 1) age till 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; 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 till which the subsidy is provided. Note that while the eligibility age is often expressed by the school grade (e.g. till the end of junior high school), we 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 school entry rule as well as very rare grade retention and advancement.[10] Second, the level and the form of subsidy (and thus cost-

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

[10] In Japan, 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

sharing) differ by municipality: the majority of municipalities fully subsidize child health care (i.e., coinsurance rate is reduced from the national level of 30% to 0%). Some municipalities reduce coinsurance rate to 10%, 15% or 20%, while other municipalities take the form of copayment, such as 200, 300, or 500 JPY (roughly 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 refund, they may prefer in-kind to refund because of time cost and/or credit constraint.[11] Finally, some municipalities impose income restrictions on eligibility for the subsidy. While we cannot identify the individuals who are ineligible for subsidy due to lack of income variable in our claims data, the fraction of municipalities with income restriction is very small in our data. In the empirical model, we include a dummy for income restriction.

One contribution of this paper is that we newly construct detailed information on subsidy at each municipality-age-time level (where both age and time are measured in *months*). Since information disaggregated at such a monthly level is not formally collected even by either prefectural or central government, we hand-collect through a variety of sources including municipality web page, local newspaper, and municipal ordinance.[12] Importantly, after collecting 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 record in small municipalities, we limit data collection to municipalities in the six largest prefectures in Japan which result 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 to these large prefectures is that municipalities are probably 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 used in this

in Japan (only 0.03% of children are exempted from the mandatory entry). Also, 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 almost identical to a birth cohort—that is, those born in April to those born in March next year. See Shigeoka (2015) for more details.

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

[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 total of 47 prefectures, and 1,719 municipalities in Japan as of January 2015.

study as explained in Section 3.3.[14] Note that this figure reflects the compositional changes of municipalities as the number of municipality increases at the later period in our claims data. Importantly, within the municipalities, the subsidy expansion is always monotonic—that is, there are *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 upper ages in the last decade. For example, none of the municipalities provide subsidy till age 15 (till 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 has expanded the eligibility age for national-level subsidy (i.e., 20% coinsurance rate) from age 3 to 6 (till the start of primary school). While Figure 1 clearly shows that all municipalities in our sample have already provided the subsidy till age 6 by April 2008, this national-level subsidy expansion eases the budgetary burden on municipalities as the 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).[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 low fertility rate, and combat recent increases in child poverty (Bessho 2012). We discuss potential endogeneity of subsidy expansions in Section 4.2.

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 corporation. Since parents of the children in the JMDC data work for large firms, our sample does not include children with extremely low household income like those who receive public assistance. Thus, the liquidity constraint is unlikely to explain the results described below. As of November 2015, the JMDC claims database contains more

[14] We also collected information on subsidy for *impatient* care. However, most municipalities had already covered inpatient care till 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 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 a *monthly* level data on both explanatory variable (subsidy level) and outcome variable (health care utilization).

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 are uniquely determined by municipality-age-time. The claims data report the monthly spending, including the months of no utilization.[16] Specifically, the claims data contain the year and month of the visit, and detailed information on the line-by-line medical services including diagnoses (ICD10), types of services, quantity of each service, and fees charged for each service. For example, when a prescription drug is dispensed, we have detailed information on year and month of prescription, the name of drug, ATC code, retail price, and quantity. The unit of claims data is monthly as the reimbursement to medical institutions occurs monthly in Japan. The enrollment and claims data are linked by 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, which allows us to 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 includes 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 of each municipality we hand-collected to JMDC, and then JMDC merges it with their insurance claims data in-house by municipality and year-month, and return it to us where municipality ID and patient ID are deidentified for confidential reason. Thus, we cannot examine the heterogeneity by the characteristics of the municipality (e.g., *average* household income or maternal education) as 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 kids).

As for the outcome variables, we mainly focus on two outpatient outcomes to save space: a dummy which takes one if there is at least one outpatient visit per month (an outpatient visit dummy, hereafter), and total monthly outpatient spending (outpatient spending, hereafter) measured in thousand JPY (roughly USD10). In fact, total outpatient spending in each month can be decomposed as:

$$\text{Outpatient spending} = 1[\text{Visit} > 0] \times (\text{Frequency of visits}|\text{Visit} > 0) \times \text{ Spending per visit} - [1]$$

[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.

Thus, an outpatient visit dummy ($1[\text{Visit} > 0]$) captures the extensive margin of health care utilization while the outpatient spending captures the total utilization. In the online appendix, we include the results from other outcomes.

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 ages 7–14 years-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 majority of the municipalities (81.3%) have already provided subsidy till age 6 (the start of primary school) at the beginning of our sample period, and also 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 years-old (one year wider on both sides of ages of interest) to identify the effect of patient cost-sharing at ages 7–14.[17]

Finally, we limit the sample to 165 municipalities which only have either 0% (full subsidy) or 30% (no subsidy) of coinsurance rate during our sample period for following four reasons. First, the transitions of "30% to 0%" and "0% to 30%" in cost-sharing are by far the top two variations in our data. Appendix Table A-1 lists all the combinations of transitions (including the changes from coinsurance to copayment or vice versa). These two variations accounts for 54.2% of all the transitions at municipality-age-time level, and as much as 70.0% at the person-month level. In fact, even after imposing such restriction, we still maintain as many as 5,438 changes in subsidy status at municipality-age-time cell, which is the level of the variation for identification in our empirical analysis.[18] Second and relatedly, unlike other transitions, these two variations are observed at entire age ranges. This point is extremely crucial for our purpose of estimating age-specific price elasticities across wide age ranges.

Third, as shown in Appendix Table A-1, the other cost-sharing often takes the form of copayment (e.g., 500 JPY/visit), and it is inherently difficult to make them comparable to coinsurance rate (e.g., 30%). One can do this by, for example, dividing copayments by total medical expenditures. However, estimating the counterfactual number of monthly visits unavoidably introduces measurement errors.[19]

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[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] These two types of transitions are followed by "500 JPY/visit to 30%" (9.1%), "30% to 200 JPY/visit" (7.0%), and "30% to 500 JPY/visit" (5.7%), where the number in parentheses are the share at municipality-age-time level, but these transitions do not always spread across the ages.

[19] We need to compute the counterfactual monthly visits for age a (in months), time t (in months), and living in municipality m. To do so, while we can keep two of them fixed, we need to relax the remaining one. For example, we may want to use the frequency of visits at age a and time t, but from other municipality m' or the frequency of visits at age a, municipality m, but from different time t'.

Because the effect of non-linear pricing on utilization is an important empirical question, we intend to investigate this further in our future research.[20] Fourth, it is easy to compare the asymmetric price sensitivity to the opposite directions of price changes, as detailed later. Importantly, in Appendix Table A-2, we show that the characteristics of children as well as their health care utilization are quite similar between 165 municipalities in our data and the remaining municipalities. This alleviates the concern that our sample municipalities are specific, and thus the results are not generalizable.

3.4. Descriptive statistics

Table 1 provides the summary statistics of selected variables in our sample at 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 *expiration* of the subsidy at certain age. At the individual level (Panel B), we have total of 63,590 individuals, and each individual is observed on average 36.2 months, and 21.8% of individuals experience at least one subsidy change. Concretely, 16.5% experience at least one subsidy expansion (from 30% to 0%) and 19.3% experience at least one expiration (from 0% to 30%) as detailed in the Section 4.1. Gender is well balanced (48.8% is 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-month 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 (roughly USD60.9) per month including zero-spending, and 14.95 thousand JPY (roughly USD149.5) conditional on at least one visit. 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 USD4065.2) than outpatient care.

The simple plots of raw data already reveal interesting patterns. Panel A of Figure 2 plot 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 visit dummy shows that the line with subsidy is always higher than the line without subsidy by 8–11 percentage points at any age ranges, while both age profiles is declining since 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 roughly 2 to 3 thousand JPY (USD20–

[20] A number of studies point out the possibility that there may be special psychological properties to zero price (e.g., Shampanie *et al.* 2007).

30) higher with the subsidy than without 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 a very rare event in this age group. In contrast to outpatient outcomes, we see no clear difference in inpatient admission 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 roughly 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 municipality and birth in months) experience own price schedule as long as they do not move across municipalities. Figure 3 illustrates one example of 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 cohort born in July 1998 (“younger” cohort, hereafter), and the dotted line for 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) till the beginning of primary school (roughly 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 national level of 30% coinsurance rate.

Suppose that in October 2007 the municipality expands the subsidy up to the end of junior high school (roughly age 15). Panel B draws the price schedules *after* subsidy expansion. The younger cohort (solid line) pays full 30% from age 6 years and 9 months to age 9 years and 2 months, a month before subsidy expansion in October 2007. Thanks to subsidy expansion, the cohort enjoys the free care from age 9 years and 3 months till age 15 years and 8 months when the cohort graduates from junior high school in March 2014. Then, once again the cohort pays full 30% after age 15 years and 9 months. On the other hand, the price schedule for older cohort (dotted line) is shifted by one month to the right as the cohort is one month older than 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 cohorts between 6 and 15 years old are benefited from the same subsidy expansion. As a result, each cohort uniquely experiences the subsidy expansion as well as expiration at different ages. This enables us to estimate the price elasticity for broad age ranges (ages 7–14) even at the monthly level. Price elasticities at different ages can be informative for the government to design more flexible cost-sharing schedule.

Second, we can investigate *asymmetric* responses to the direction of the price changes as our variation includes the price changes in both directions even *at the same age*. It is not a priori obvious whether elasticities are the same by the direction of price changes. On one hand, the elasticities can be smaller in price increase ("worse") than price decrease ("better") if the exposure to free care before the price increase leads to habit formation of visiting doctors, and hence the utilization does not decrease much even after the price increase. On the other hand, the elasticities can be larger in price increase ("worse") than price decrease ("better") if people respond more to the loss of subsidy ("worse") than the gain of subsidy ("better") due to loss aversion. Hence, this is ultimately an empirical question.

The research design of past studies (such as randomized control trial in RAND HIE or regression discontinuity design) do not allow them to test this question because there is only single direction of price change. Fortunately, in our setting, out of 5,438 changes in subsidy status at municipality-age-time cell, the directions of price changes are split nearly half: 2,505 changes (46.1%) are the expansion of the subsidy (labeled "better" or 30% to 0%) while 2,933 changes (53.9%) are the expiration of the subsidy (labeled "worse" or from 0% to 30%).[21]

4.2. Identification strategy

We exploit the unique variation in subsidy for child health care across municipality, age, and time combined with the longitudinal claims data in difference-in-difference framework. Specifically, our basic estimation equation is:

$$Y_{iamt} = \alpha + \sum_{a=85}^{179} \beta_a subsidized_{iamt} + \gamma X'_{mt} + \delta_a + \varphi_m + \pi_t + \theta_i + \varepsilon_{iamt} \quad \text{–[2]}$$

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

[21] Note that since we only allow for the municipalities to have either 0% or 30% of coinsurance rate *throughout* our sample period, the actual number of these two transitions is slightly smaller than those listed in Appendix Table A-1.

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. Also, 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 refund and a dummy takes one if there exists income restriction on eligibility for the subsidy while recognizing the lack of the statistical power to identify these effects (thus not the focus of the paper). Standard errors are clustered at the municipality to account for serial correlation in the error terms within the municipalities.

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:

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

where $1(Age\ A)$ is an indicator variable which takes one if the person is more than age A but less than age $A + 1$ (or equivalently $1(Age\ A) = 1(A \leq a < A + 1)$). We construct age in year dummies in this way so that age corresponds to school grade.[22] Our coefficients of interest are series of β_A (A= 7–14) which captures the average effect of subsidy within the age ranges.[23] Importantly, we still include δ_a at the monthly level to account for any age in month specific effects.

For our main analysis, we focus on the sample of non-movers (98.3% of the sample) who stay in the same municipality while they are observed in our data. The migration rate in our sample is lower than the actual migration since *intra*-municipality migration is not counted since the subsidy level is still the same.[24] Although we have very few movers in our data, 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 Section L, we estimate a discrete choice model that examines whether children (and their parents) migrate to a municipality that provides free child health care,

[22] For example, age 6, 12, and 15 corresponds to age just before start of primary school, the last year of primary school, the last year of junior high school in Japan, respectively.

[23] We abstract from whether this effect stems from the patient-induced demand, that is, children or mothers ask more care when 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.

[24] Furthermore, the inter-municipality migration is much more common before the children enter primary school, which is outside of our sample age ranges.

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 small number of inter-municipality migration. For non-movers, since φ_m and θ_i are identical, our final estimation equation is written as[25]:

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

In addition, we can exploit the two opposite directions of changes in subsidy status to examine the asymmetric price responses of children. Thus, a subsidized dummy in equation [4] is decomposed to two sets of dummies as:

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

where $better_{iamt}$ is an indicator equal to zero before subsidy is not available, and equal to one in all period after the subsidy is introduced, even if the subsidy expires. Similarly, $worse_{iamt}$ is an indicator variable equal to zero before subsidy expires, and equal to one in all years after the subsidy expires, even if the subsidy becomes available.[26] Because of the way that the indicators are defined, β_A^{Better} tests the effect of *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 subsidy, after the subsidy is expired at the age, relative to the period when the subsidy was available.

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. Since the level of variation for both age and time are measured in months and thus there are as many as 5,438 changes in subsidy statuses at municipality-age-time cell, it is difficult to imagine that such omitted variables are likely to influence our estimates. Nonetheless, it is still possible that the municipality with different pre-trend in utilization may implement the subsidy expansion at the different timing, which may bias our estimates. For example, if the municipalities in better business cycles are more likely to implement the subsidy expansion, while income effects simply increase utilization, our estimates can be upward biased.

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

[26] Currie *et al.* (2015) employ a similar strategy to examine the asymmetric effects of 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 subsidy status within the individual are less than or equal to two. Thus, in the analysis of 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).

To account for such concern, we take three approaches. First, we conduct the event-study that normalize the data to the timing of the subsidy changes, and examine whether there are any systematic differences in pre-trend between treated and control municipalities before the changes. Second, we add 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 as these fixed effects capture any municipality specific policy changes or events 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. Since we only exploit the *timing* of the changes in subsidy status in this specification, we can to some extent mitigate the concern that individuals in the treatment and control municipalities might be different.

5. Basic Results

5.1. Graphical evidence: 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 for any ages to increase statistical power. Then, we replace the subsidized dummy in estimation equation [4] by a series of dummy 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 event-study for an outpatient visit 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 y-axis are set the same within the panels so that two figures for opposite directions of subsidy changes are visually comparable.

There are a few important points to make from these graphs. First, there does not seem to show any pre-trend as the estimates are mostly close to zero before the changes in subsidy status. We are very reassured as this result addresses the concern that the municipalities that expand the subsidy may have 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 changes and behave strategically by delaying visits till the subsidy becomes available, and conversely rush to visit just before the subsidy is over. On one hand, the existence of the anticipatory utilization is rather surprising as nature of diseases for children tend to be acute. On the other hand, the fact that the magnitude is larger for subsidy expiration ("worse") than subsidy expansion ("better") indicates that at least a part of these visits is indeed acute because one cannot delay treatments too much till the expansion while one can more easily stockpile before expiration.[27] Also, the differential response can be behavioral in that mothers of children may react more to price increase rather than price decrease (loss aversion).[28] Importantly, as we include age and time FEs (both in months), this difference is not driven by a particular age or 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—seems 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.[29]

Finally and most importantly, the effect on utilization seem 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. In fact, Appendix Figure C-1 further expands the window of the event-study to $\pm$24 months (instead of $\pm$12 months) from the subsidy changes. It is reassuring that the estimates are relatively stable even 24 months (2 year) after the change in subsidy status. Technically, we can do the event-study for each age despite the loss of the statistical power. Appendix Figures C-2 shows the event-study at age 9 and 12, as examples. The fact that we observe opposite signs of changes in utilization for subsidy expansion and expiration even *at the same age* indicates that we are unlikely to capture something that is particular at that age (e.g., finishing primary school at age 12).

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

[28] Another potential explanation is information availability; 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 have at the maximum of 9 years to be aware of the end date of subsidy while a nearly 15-year old child have at the minimum of 1 month. Assuming the uniform distribution of children across ages, they have on average 4.5 years from the start of the subsidy expansion to realize the end date. On the other hand, while the subsidy expansion should be announced at least a few months (or even longer) in advance, children have less time to know the start date of expansion.

[29] 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.

5.2. Main results

Figure 5 demonstrates the graphical presentation of estimating equation [4] which plots β_A for each age (A=7–14) in the upper half, and the corresponding arc-elasticity from equation [5] in the lower half. Panels A and B present the results of an outpatient visit dummy, and outpatient spending respectively. Note that Appendix Figure A-3 plots the monthly estimates (β_a) instead of yearly estimates (β_A). Since monthly estimates are relatively stable within age in year (and statistically significant at 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 visit dummy are relatively stable across ages 7–14, and are statistically significant at the conventional level for any ages. With subsidy, the 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. The corresponding arc-elasticities presented at the bottom half range from –0.07 to –0.10.[30] While the considerable caution is necessary to compare the elasticities estimated across countries and time periods, the arc-elasticities we find are smaller than the gold-standard estimate of –0.17 to –0.31 for non-elderly 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 Section B for more detail on these elasticities and related comparisons with literature.[31]

Panel B of Figure 5 plots that 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 less precise than extensive margin documented above because of skewed right tail of spending. 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

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[30] If the congestion at medical institutions deter some demand of health care in order to avoid the waiting cost, our estimate on price elasticity can be lower bound. Unfortunately, we don't have any data on waiting time.

[31] The arc-elasticity for each age in year A is defined by $\varepsilon_A = \left(\frac{Q_{1A}-Q_{0A}}{Q_{0A}+Q_{1A}}\right) / \left(\frac{P_{1A}-P_{0A}}{P_{1A}+P_{0A}}\right) = \left(\frac{\beta_A}{Q_{0A}+Q_{1A}}\right) / \left(\frac{0-0.3}{0.3+0}\right) = -\frac{\beta_A}{Q_{0A}+Q_{1A}}$ where subscripts 0 and 1 denotes unsubsidized and subsidized, respectively. β_A are the estimates from the equation [4]. We report arc-elasticity because they are widely used and comparable to estimates from RAND HIE, in which the largest plan was also the free care plan. However, one can make an argument that when the starting price is zero, as in our case, price elasticity may not be well defined. Thus, as an alternative, we also report the *semi* arc-elasticity, which is defined by $\varepsilon_A^{semi} = \left(\frac{Q_{1A}-Q_{0A}}{Q_{0A}+Q_{1A}}\right) / \left(\frac{P_{1A}-P_{0A}}{2}\right) = \left(\frac{\beta_A}{Q_{0A}+Q_{1A}}\right) / \left(\frac{0-0.3}{2}\right) = \varepsilon_A / 0.15$. Thus, in our case, *semi* arc-elasticity is simply arc-elasticity divided by 0.15. For example, semi arc-elasticities are reported in Brot-Goldberg *et al.* (2017), and Nilsson and Paul (2015).

the denominator of arc-elasticity ($= -\beta_A/(Q_{0A} + Q_{1A})$)—is relatively stable across ages, arc-elasticities are largely governed by the sizes of β_A. Hence, arc-elasticities are also slightly declining in ages, which go from –0.12 at age 7 to –0.08 at age 14.

Since the total number of children aged 7–14 in Japan is roughly 8.8 million according to the Population Census in 2015 (Statistics Bureau 2015), the 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 (974 million USD).[32] 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 is financed by taxes (39%), premiums (49%), and out-of-pocket (12%) in Japan (Ministry of Health, Labour and Welfare 2014), the municipal subsidy has a substantial negative fiscal spillover on many stakeholders.

Table 2, which corresponds to Figure 5, also 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 income restriction on eligibility for the subsidy.[33] For outpatient spending, the signs of these coefficients are also as expected, but not statistically significant at the conventional level.

We subject these results to a series of robustness checks. In the interest of brevity, we leave the detailed descriptions of the exercises to Appendix Section D. Critically, the results in Table 2 on the causal effects of patient cost-sharing are robust across all specifications considered. In particular, 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 event-study before T=0 seems 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 health care utilization. We are reassured that the estimates are barely changed. We also collapse the data at municipality-age-time cells, which is the level of variation, to partially account for zero spending but the estimates are almost identical. As a separate exercise, we present 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.

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[32] We multiply each β_A by the number of children in the age A in 2015 and sum them up to calculate monthly spending. Then, we multiply it by 12 to convert to annual spending. The average exchange rate of 120.39 JPY/USD in March 2015 is used.

[33] This result is consistent with Zhong (2011) which shows that immediate reimbursement increases health care utilization compared to future reimbursement in China. Despite its importance, the effect of reimbursement method on utilization is understudied in health economics.

In Appendix Section E, we also report the results on frequency of outpatient visits and the outpatient spending per visit as outcomes. Interestingly, while both spending and frequency of visits increase, the spending *per visit* is also positive and statistically significant at the conventional level. This result suggests that the increases in total outpatient spending are driven by both the increases in the frequency *per se* and spending per visit. A simple decomposition indicates that the increases in frequency of visits accounts for 70.2–81.0% of increases in total outpatient spending.[34]

In Appendix Section F, we also examine each type of medical services: the medication accounts more than half the share of total spending (54.1%), followed by consultation fees (18.4%), laboratory tests (17.2%), and non-surgical procedure (5.3%).[35] The consultation fees—which are charged in each visit and thus is 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 non-surgical procedures, are more price sensitive. This result is consistent with our finding that not only the frequency of visits but also the spending per visit increases. Interestingly, the medication is not as price sensitive as other service categories.

5.3. Asymmetric responses

In this subsection, we investigate whether children asymmetrically respond to price of health care by exploiting the unique variation of price changes in opposite directions. Figure 6 demonstrates the graphical presentation of estimating equation [5] 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] Panels A and B report the results of an outpatient visit dummy, and outpatient spending, respectively. Table 3 corresponds to Figure 6.

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 1% level at any age ranges. 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, we do not

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[34] We simply calculate the percentage increase from the mean for both frequency and spending per visit, and calculate the fraction of the first one divided by the sum of the two.

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

[36] For the analysis of asymmetric responses, we instead report semi *point*-elasticity instead of *arc*-elasticity as we exactly know the starting quantity, and also the direction of the price changes. Specifically, the semi *point*-elasticity for each direction of price changes are 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 [5].

find much difference in semi point-elasticity for outpatient spending.[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 even if the policy maker wants to implement the policy with the opposite direction of price change. Also, 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 [4] without asymmetry hereafter.

6. Beneficial or low-value care

The remaining important question is whether the increased outpatient utilization due to lower price (moral hazard) largely reflect 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. While this is always a challenging task—especially for children as the nature of diseases tends to be acute, we examine heterogeneity of utilization patterns from various dimensions (such as type of visit and health status) and short-term child mortality to answer this question to the extent possible. We fully recognize that subsidy-induced utilization should include some aspects of both essential and non-essential care, as documented in RAND HIE (Newhouse 1993; Manning *et al.* 1987). Thus, we focus on the relatively extreme cases of utilization patterns to tackle this question.

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 prevents avoidable inpatient admissions and reduces short-term mortality.

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 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

[37] For outpatient spending, while the magnitude of 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.

risks of hospitalization by either preventing the onset of an illness or condition (e.g., asthma).[38]

We employ the list of ACSC from Gadomski *et al.* (1998) that specifically focus on children.[39] Appendix Table G-1 provides the lists of the ACSC with corresponding ICD10 codes, and the fraction of each ACSCs 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 7 plots the estimates from equation [4] when outcome is an outpatient visit dummy for (i) any ACSC, (ii) severe ENT infections, and (iii) asthma.[40] Panel A shows that outpatient visits for these diagnoses increase when subsidized, and all the estimates are statistically significant at the conventional level. Table 4 reports the estimates that are plotted in Figure 7. 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. The corresponding arc-elasticities presented in Appendix Figure G-1 are roughly –0.1. Since nearly 40 percent of the diagnoses for this age group are ACSCs, the arc-elasticities are very similar to overall arc-elasticities shown in Figure 5.

These results at a glance seems consistent with the literature that people are not only price sensitive to non-essential 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 past studies could not examine whether such seemingly beneficial care indeed lead to better health of patients or prevent avoidable hospital admissions. Here, one big advantage of our insurance claims data is that it includes the information for both outpatient and inpatient care from the same individual over time unlike most existing datasets that only capture either outpatient care or inpatient care. Thus, we can directly examine whether such increases in preventive care at outpatient setting indeed lead to the reduction in hospitalization.

Panel B in Figure 7 plots the estimates from equation [4] where the outcomes is an *inpatient* admission dummy while the explanatory variables are subsidy status for *outpatient* care as before.[41] We do not see any declines in the hospitalization associated with any ACSC or other individual ACSCs. Since the hospitalization 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

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[38] Nonetheless, we estimate equation [4] by the broad diagnosis groups as indicated in Online Appendix Table A-2. We do not find much heterogeneity except for the "Injury, poisoning and certain other consequences of external causes" which shows slightly smaller arc-elasticity as these conditions may be more urgent and less discretionary (results available upon request).

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

[40] Unfortunately, insurance claims data in Japan 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.

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

even statistically significant at some ages in case of asthma. Furthermore, since there is a possibility that the benefit of preventive care may emerge with a lag, we also estimate a variant of equation [4] where the explanatory variables are lagged outpatient subsidy dummies in a simple dynamic model. We find little evidence of any lagged effects either (results available upon request).

6.1.2. Offset effects

More generally, we can examine whether *outpatient* spending replaces *inpatient* spending—widely known as “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), 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 in statistical power due to 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 [4] by inpatient dummy or inpatient spending while 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.[42]

Panel A of Figure 8 plots the estimates on the probability of hospitalization, and Panel B plots the estimates on inpatient spending. Table 5 shows that out of 16 estimates (8 for each age for each outcome), none of the estimates except for one are statistically significant at the conventional level. Also,

[42] 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 inpatient admission 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).

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, despite the large increases in the outpatient visits and spending documented so far.

We also confirm that directions of price changes do not matter: we do not find evidence that reduced use of outpatient services due to subsidy expiration leads to increased demand for impatient services nor evidence that increased use of outpatient services due to subsidy expansion leads to reduced demand for impatient services. Furthermore, we find little evidence of lagged effects (all results available upon request). These results echo with 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 child mortality

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

Here instead of simple OLS, we account for the interval-censored nature of the mortality data in 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.[44]

[43] The mortality dummy takes one if either 1) enrollment data indicate death as the reason for drop-out from the data, or 2) claims data indicate death as a result of treatment at the medical institutions. We recognize that our data 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 hospital (Ministry of Health, Labour and Welfare 2010).

[44] 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.

Appendix Figure H-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 ages 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 captures the underlying baseline hazard more flexibly. Appendix Table H-1 shows that the estimates are very similar. Thus, at least in the short-run, we find little evidence on reduction in child mortality while we need to interpret this with considerable caution due to very low child mortality rate.[45]

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 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 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 visits. While this issue has been repeatedly raised in the media, there is no formal analysis.[46]

We divide the visits into three categories: regular-hour visits, off-hour visits, and midnight/holiday visits. Under the national fee schedule, additional fees for off-hour visits and midnight/holiday visits are charged on top of the fees for regular-hour visits, and thus from the billing information we know the timing of the outpatient visit within a day.[47] Appendix Table I-1 provides the list of billing codes for these non regular-hour visits and corresponding additional fees. As a benchmark, fees for regular-hour visits during the sample period are roughly 2.8 and 0.7 thousand JPY (roughly USD28 and 7) for the first visit and revisits, respectively. Appendix Table I-1 shows the *additional* fees charged for off-hour visits—which are typically 0.85 and 0.65 thousand JPY (roughly USD8.5 and 6.5) for the first visit and

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[45] Recent studies find some positive effect of Medicaid introduction (Goodman-Bacon 2016) and expansion on *long-term* health outcomes (Brown *et al*. 2016; Miller and Wherry forthcoming; Wherry *et al*. forthcoming). Note that their studies are likely to find the larger impacts as the focus is on the provision of health insurance (extensive margin), and the targeted population is more disadvantaged.

[46] Municipalities have indeed 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).

[47] 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 in the holiday. We combine midnight and holidays visits as fees for these two types of visits are set higher than other visits by national fee schedule.

revisits, respectively—are relatively high, making these visits costly. Moreover, the additional fees for midnight/holiday visits are set much higher than that 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 regular-hour visit.

Figure 9 plots the estimates (β_A) for regular-hour visits (for references), off-hour visits, and midnight/holiday visits. The corresponding estimates are reported in Table 6. The outcome here is the frequency of visits by each type of visit. Since the majority of the visits are regular-hour visits (89.1% of total visits), Panel A shows the similar pattern as our baseline estimates reported in Figure 5, and the arc-elasticities are stable around –0.1 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 generous subsidy increases burden on the workload of physicians by inducing children to make off-hour visits. The arc-elasticity is also increasing in age, ranging from as low as –0.076 at age 7 to –0.184 at age 14. Importantly, the arc-elasticity for off-hour visits is much larger in magnitude than that 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 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 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 I-2 and Table I-2 report the estimates on outpatient spending (instead of frequency of visits) and find similar results.[48]

In sum, 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.[49] 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 for these visits. We did not find evidence, however, that the subsidy increases midnight/holiday visits when the health care resources (e.g., physicians and nurses) are most scarce.

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

[49] It is certainly possible that additional cost of off-hour visits may be partially offset by the opportunity cost of working mothers who may need to leave the 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.

6.2.2. Inappropriate use of antibiotics

Another concern of generous subsidy for children is that it may increase the inappropriate use of medications. Since more than half of the outpatient spending is occupied by medication and related expenses (54.1%), this is a valid concern worth investigating. Specially, 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 dies 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.[50] Appendix Table J-1 presents the list with 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 zeros.

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 the equation [4] 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 10 (and Table 7) shows that the subsidy increases the spending on antibiotics by 0.009 to 0.020 thousand JPY, which is 17–38% off the mean. Similarly, the frequency of antibiotics prescriptions increases by 0.039 to 0.070 (20–36% off the mean) in Panel B. Thus, our results suggest that 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.[51] One

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[50] See Appendix Section J for the details on creating the list.

[51] A few papers examine the heterogeneity in price responsiveness by the patient health status. For example, RAND HIE finds no difference between healthier and sicker patients (Manning *et al.* 1987). More recently, Chandra *et al.* (2014) examine the same question among the low-income non-elderly population in Massachusetts. Their findings are rather mixed. 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, which contrasts our results. Fukushima *et al.*

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, 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 the child is observed at different timing in the claims data. Then, we divide children into two types (i.e., sicker or healthier) by the median spending for each age (in years) and the subsidy status at the first entry to data. 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 the last row in Table 8). 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 group.

Figure 11 (and Table 8) shows that health status does indeed affect children's response to cost sharing. Specifically, the healthier children are much more price sensitive than that of the sicker children. As for an outpatient visit dummy, while the arc-elasticities for the sick range from –0.055 to –0.075, those for the healthy range from –0.12 to –0.16, which is substantially larger in magnitude than that for the sick at any ages. While it is a bit noisier, the same story holds for the outpatient spending. These results shows 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 are more discretionary and relatively low-value. In Appendix Section K, we experiment with different windows (9, and 12 months) to calculate the patient health status and find qualitatively similar results across the windows.[52,53]

(2016) examine the elderly in Japan and find that, similar to ours, the healthier elderly are more price responsive than the sicker elderly.

[52] 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).

[53] 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

7. Conclusion

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 the adults and elderly, and surprisingly little is known about children. In this paper, we examine the effect of patient cost-sharing on health care utilization among children by exploiting more than 5000 regional and over-time variations on subsidy availability. Importantly, the eligibility for subsidy is always tied to the age of children, allowing 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 arc-elasticity is relatively stable around –0.1 across the age ranges we examine. While the considerable caution is needed to compare the elasticities estimated across countries or time periods, the arc-elasticity for children estimated in this paper is somewhat smaller than that of RAND HIE for non-elderly in the US, and Shigeoka (2014) and Fukushima *et al.* (2016) for the elderly in Japan.

We further examine the utilization patterns from various dimensions to understand whether changes in utilization largely reflect beneficial or low-value care. We show that the increases in outpatient visits do not translate to clear benefits in the form of reduction in hospitalization by "avoidable" conditions nor reduction in short-run mortality. We also document that the subsidy has some negative effects by increasing inappropriate use of antibiotics and costly off-hour visits. Furthermore, the healthier children—measured by the prior spending—is 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. But taken together, the weight of the evidence supports the notion that the aggressive expansion of child health care subsidy may lead to the increases of the low-value and costly outpatient visits.

This paper is subject to a couple of limitations. The biggest limitation is that we cannot investigate both short-term as well as long-term health outcomes except for short-run mortality. Since health is stock, the better access to preventive care during childhood may translate into the improvement in the long-run health, 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. This may be especially important in case of young children as their decision making is heavily influenced by mothers. To the best of our knowledge, such monthly data with age,

clinics rather than large hospitals, these visits are probably less serious and more discretionary, providing another suggestive evidence of increases in non-essential care.

municipality of residence, health care utilization, and any household characteristics, do not exist in Japan—due mainly to the lack of individual identifier in Japan such as social security number in the US—but this definitely leaves an avenue for future research.

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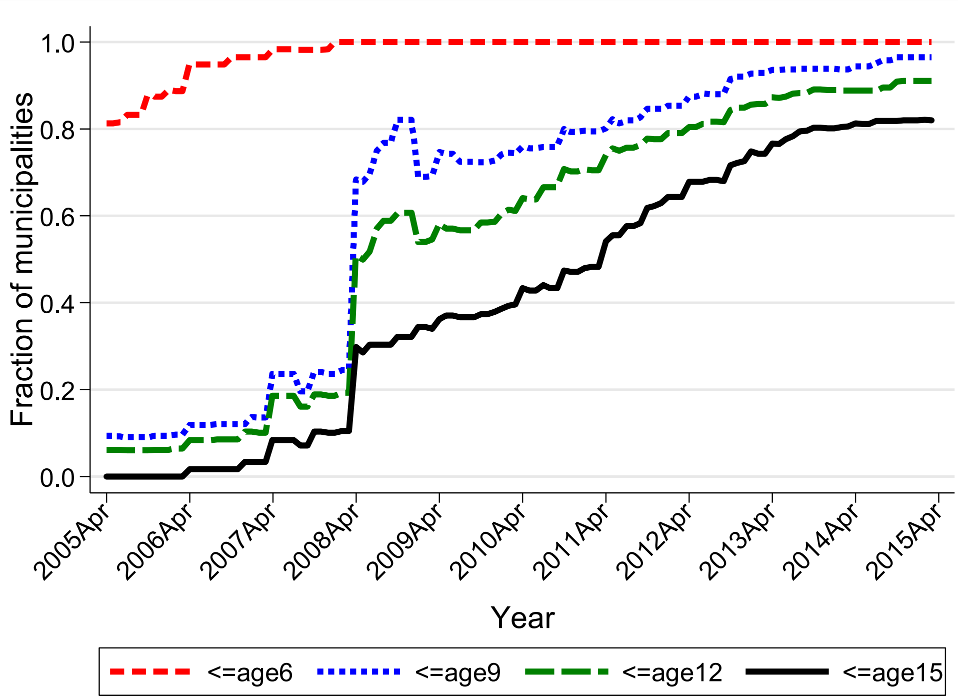
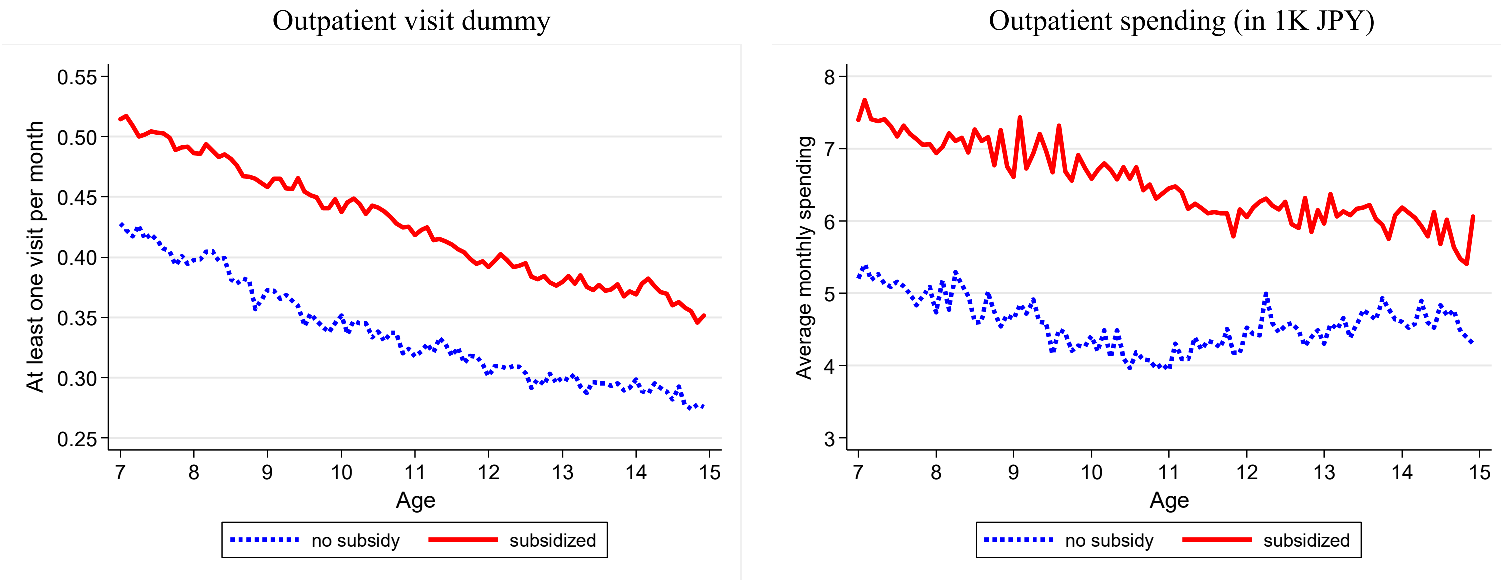
Figure 1: Time series of maximum age fully covered by child care subsidy



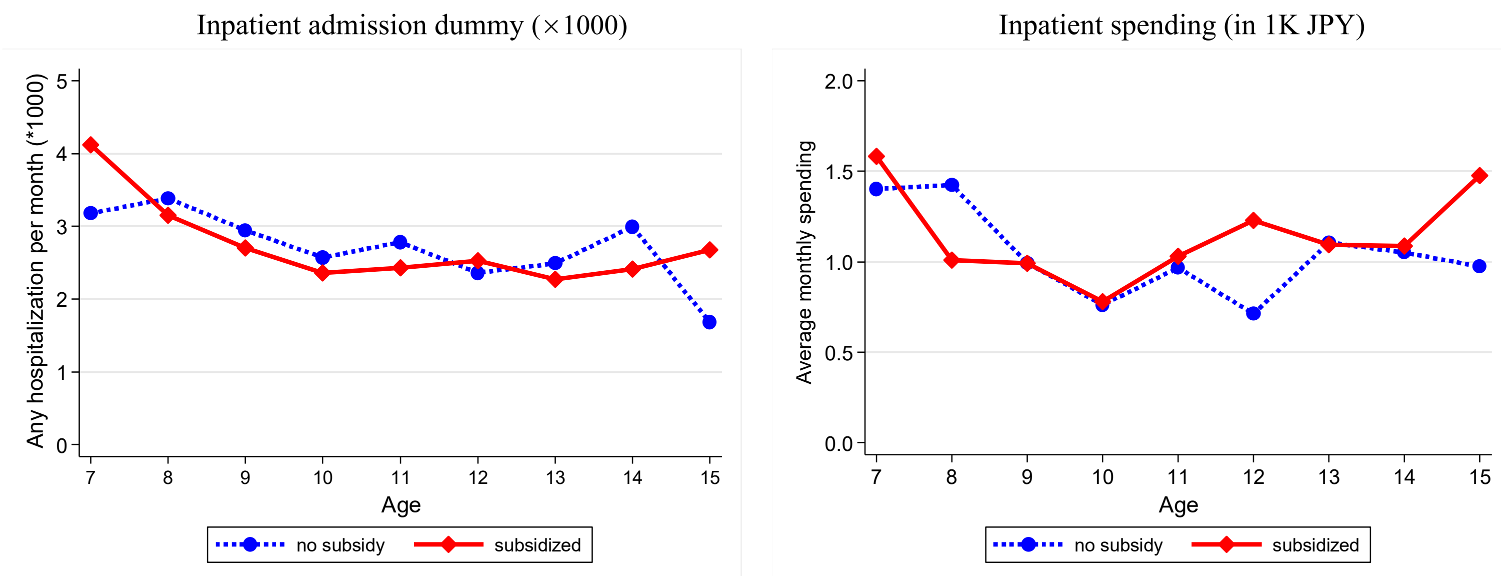
Notes: The data is unbalanced monthly panel where the unit of observation is municipality. There are total of 165 municipalities. Note that this figure reflects the compositional changes of municipalities as the number of municipality increases at the later period in our claim data. Importantly, within the municipalities, the subsidy expansion is always monotonic—that is, there are no single municipality that *lowers* the maximum age during this period (April 2005–March 2015). The spike in April 2008 is explained by the fact that the central government has expanded the eligibility age for national-level subsidy (i.e., 20% coinsurance rate) from age 3 to 6 (till the start of primary school). While Figure 1 clearly shows that all municipalities in our sample have already provided the subsidy till age 6 by April 2008, this national-level subsidy expansion eases the budgetary burden on municipalities as the 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: Age profiles of utilizations by subsidy status

A. Outpatient care (monthly)



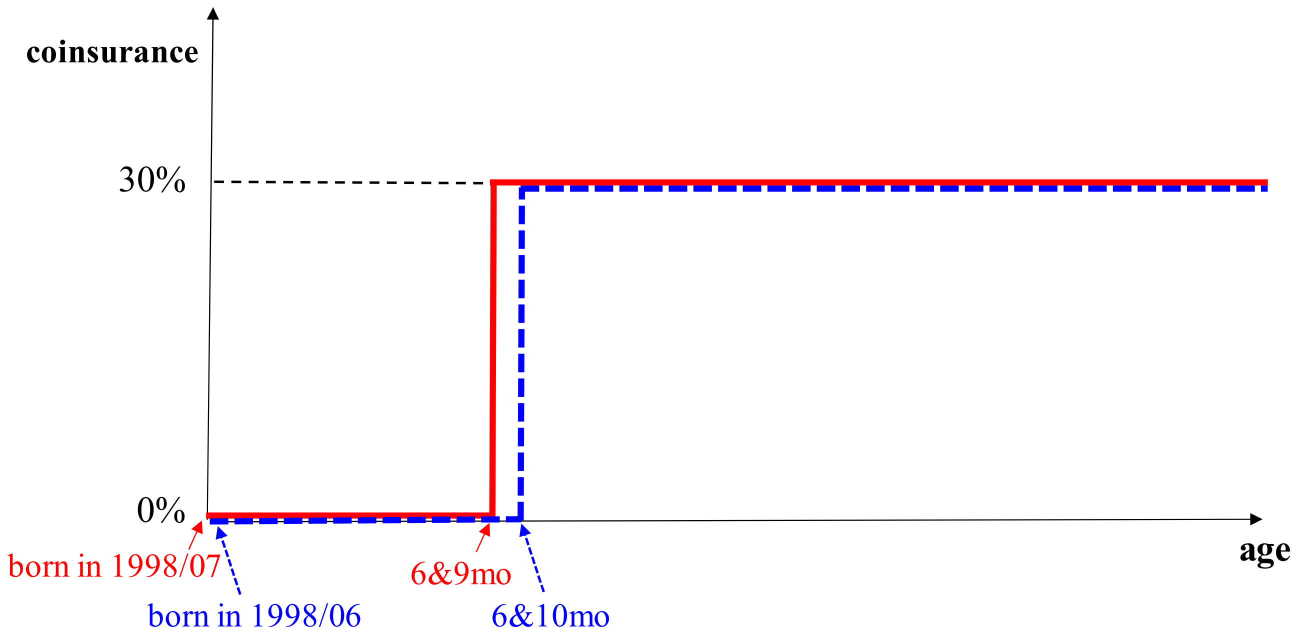
B. Inpatient care (yearly)



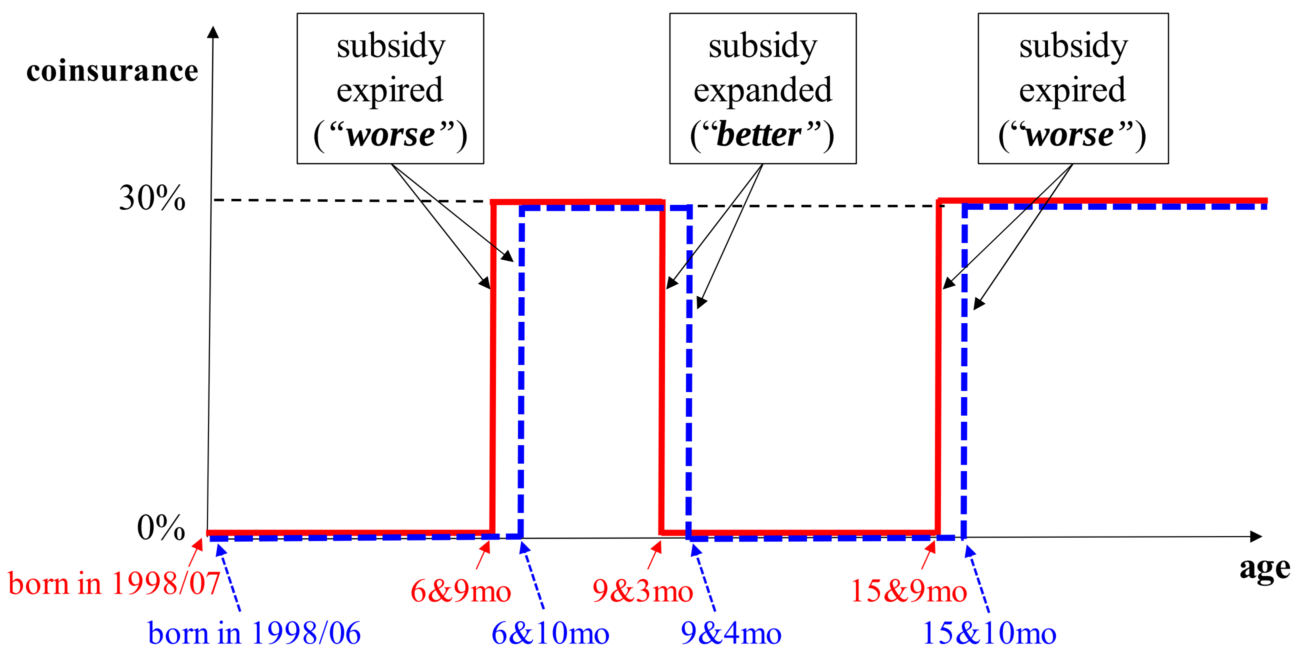
Notes: Panel A plots the monthly mean of outpatient outcomes, and Panel B plots the yearly mean of inpatient outcomes as inpatient admission is a rare event. An outpatient visit dummy takes one if there is at least one outpatient visit per month, and an inpatient admission dummy takes one if there is at least one hospitalization per month (×1000). The 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 (roughly USD10). The dotted lines are age profiles of utilization without subsidy (30% coinsurance, labeled "no subsidy"), and the solid lines are age profiles of utilization with subsidy (0% coinsurance, labeled "subsidized").

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

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



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

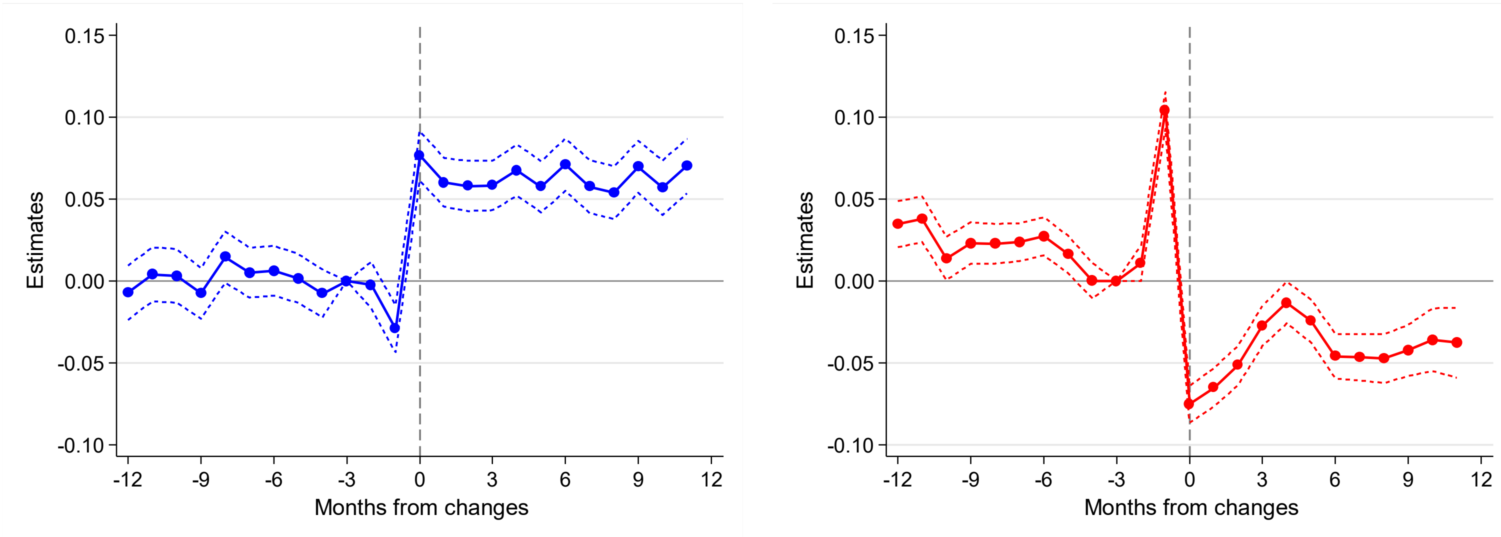


Notes: The solid line is the price schedule for cohort born in July 1998 ("younger" cohort) while that of dotted line is the price schedule for cohort born in June 1998 ("older" cohort"), a month before the previous cohort. In this example, the subsidy expansion from age 6 (before the start of primary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

Figure 4: Event study

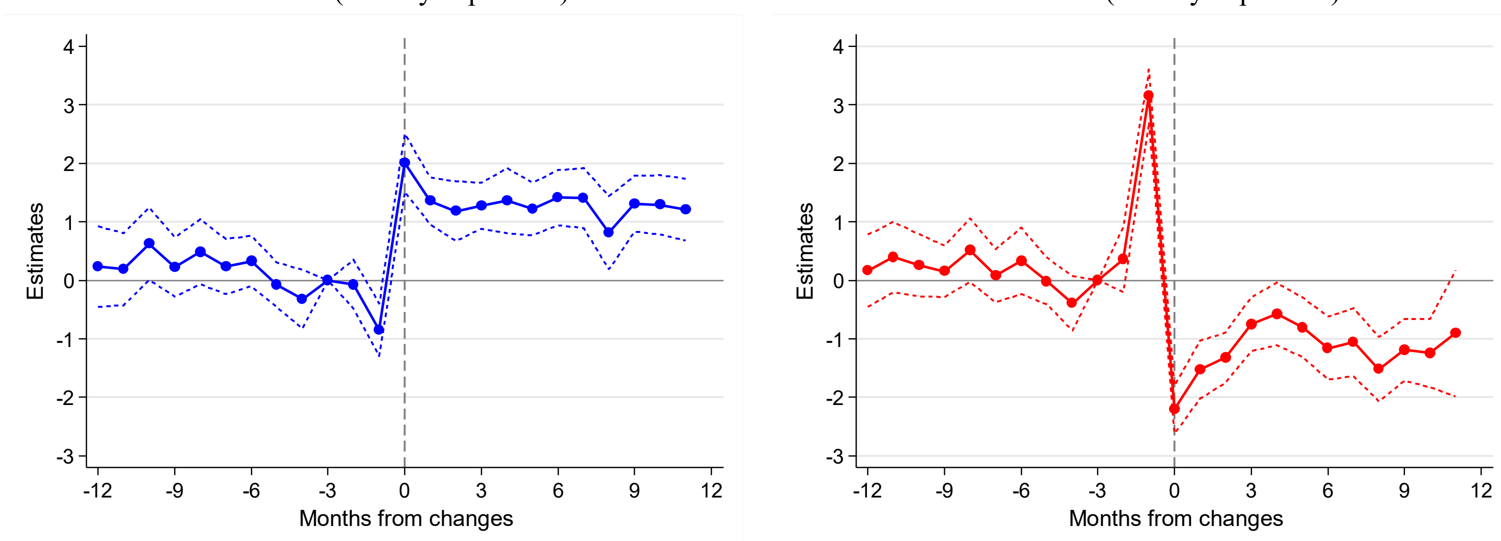
A. Outpatient visit dummy

Better (subsidy expansion) | Worse (subsidy expiration)



B. Outpatient spending (in 1K JPY)

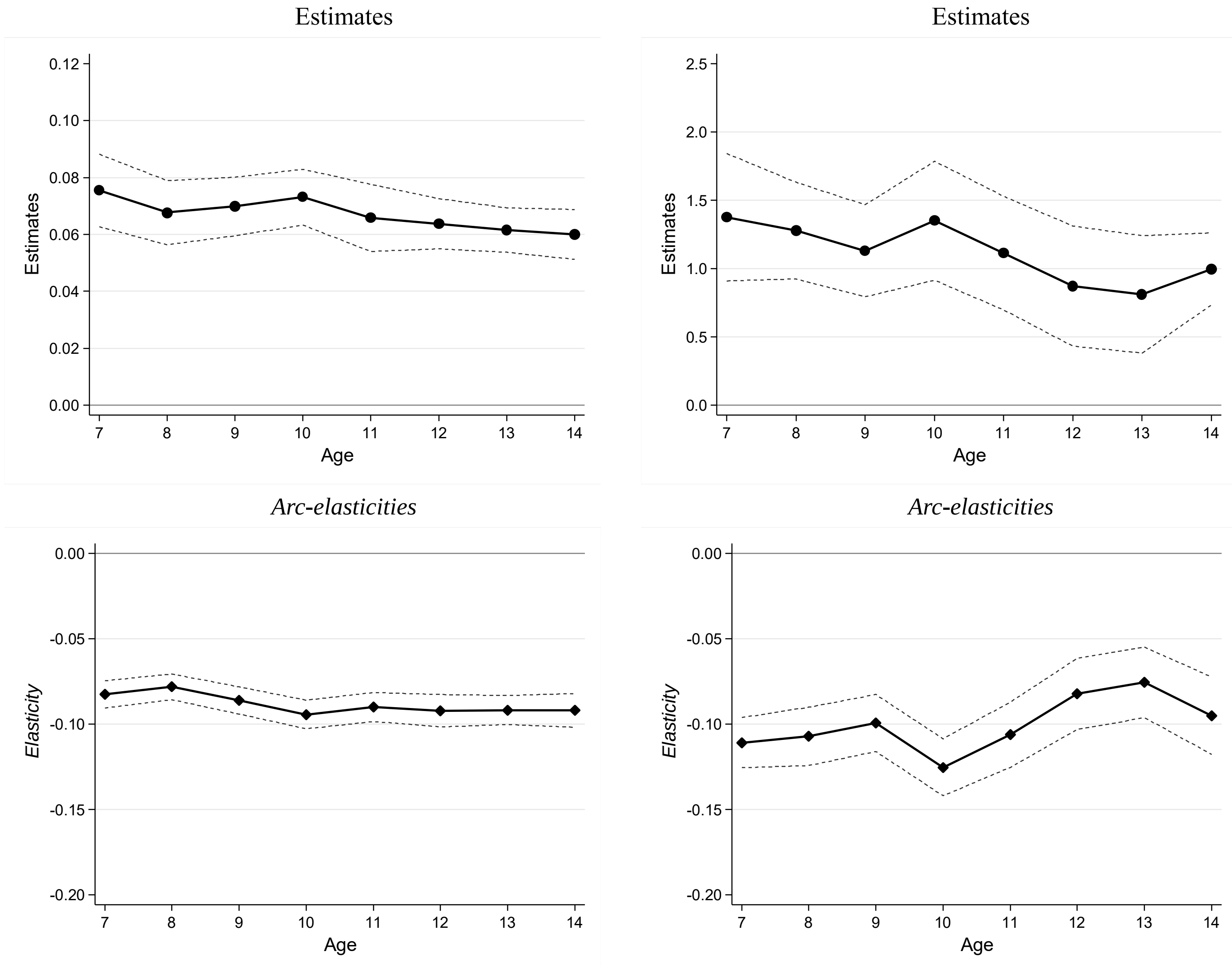
Better (subsidy expansion) | Worse (subsidy expiration)



Notes: An outpatient visit 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 (roughly USD10). “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 [4] where the subsidized dummy is replaced by a series of dummy 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 subsidy changes (T= –3). The observations within two months from the subsidy changes of the opposite direction are excluded from the sample to account for anticipatory utilization. Note that the scales of y-axis are set the same within the panels so that two figures for opposite directions of subsidy changes are visually comparable.

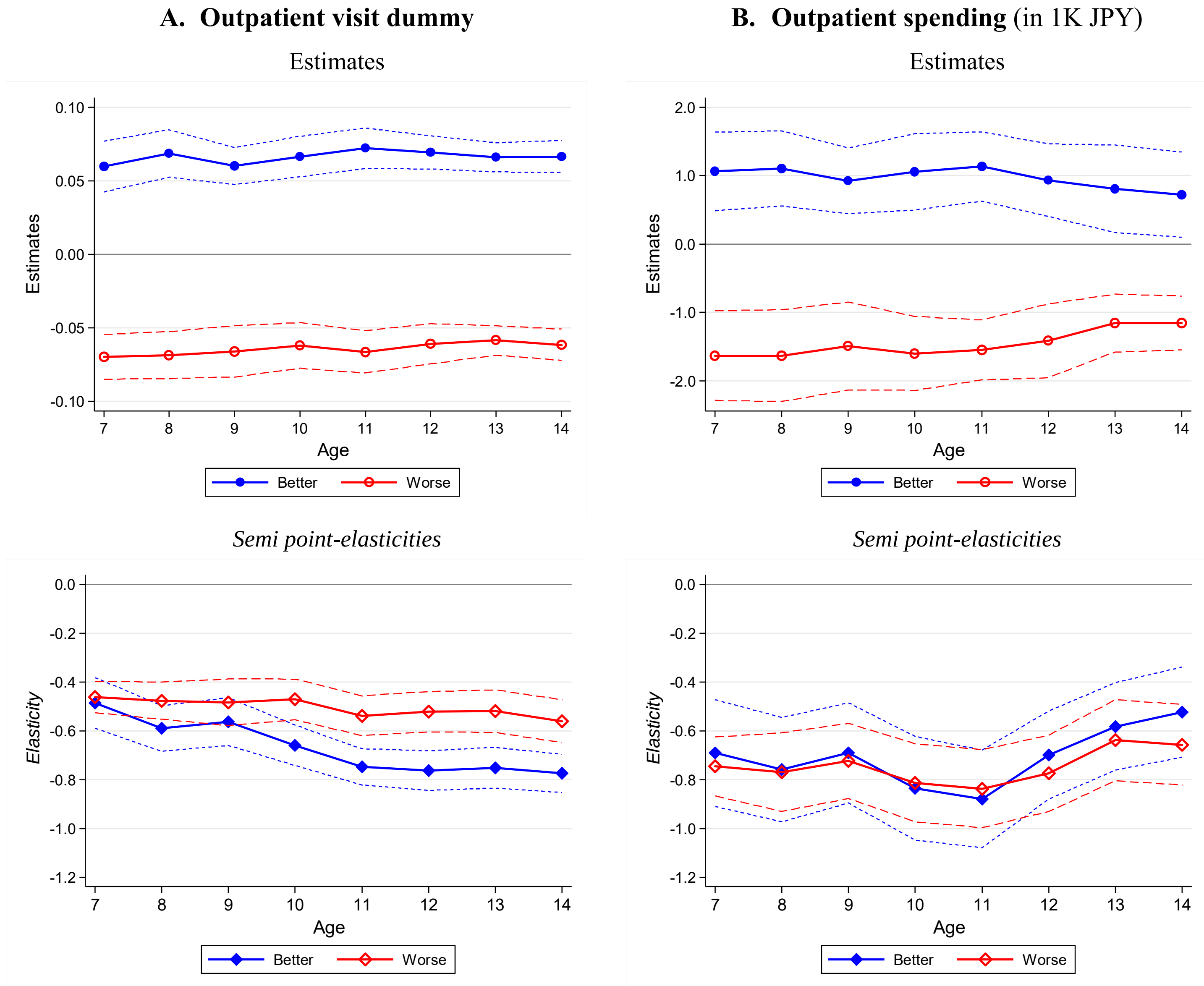
Figure 5: Basic results

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



Notes: An outpatient visit 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 (roughly USD10). 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 arc-elasticities. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Note that the scales of *y*-axis on the arc-elasticities are set the same so that two elasticities are visually comparable.

Figure 6: Asymmetric responses

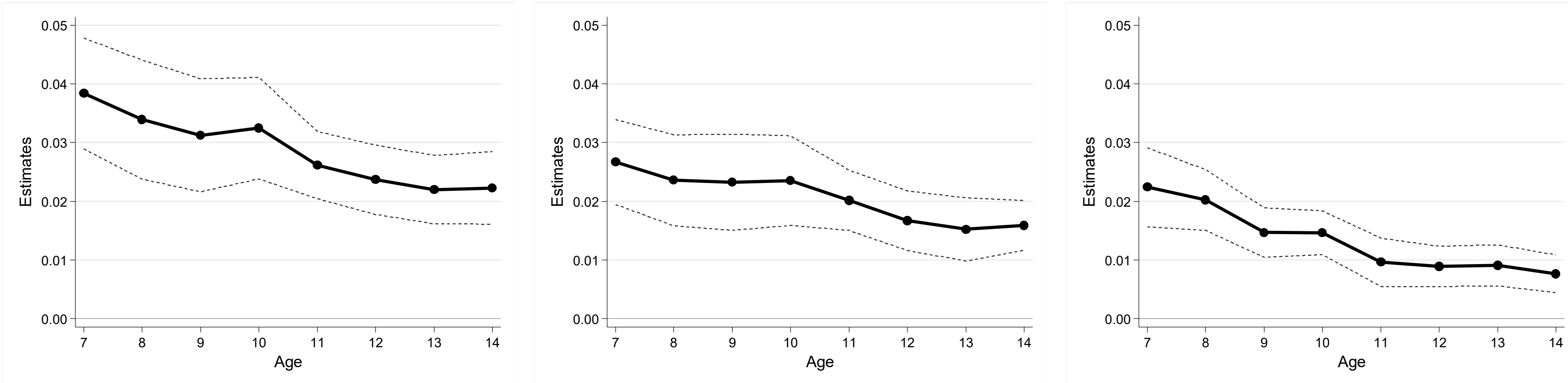


Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). "Better" indicates the subsidy expansion which lowers the price of health care from 30% to 0%, and "Worse" indicates subsidy expiration that raises the price from 0% to 30%. The dotted lines are the 95th confidence intervals. The standard errors clustered at municipality level are used for estimates, and the bootstrapped standard errors clustered at municipality with 200 repetitions are used for the semi point-elasticity. The observations within two months from subsidy 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.

Figure 7: Ambulatory Care Sensitive Conditions (ACSC)

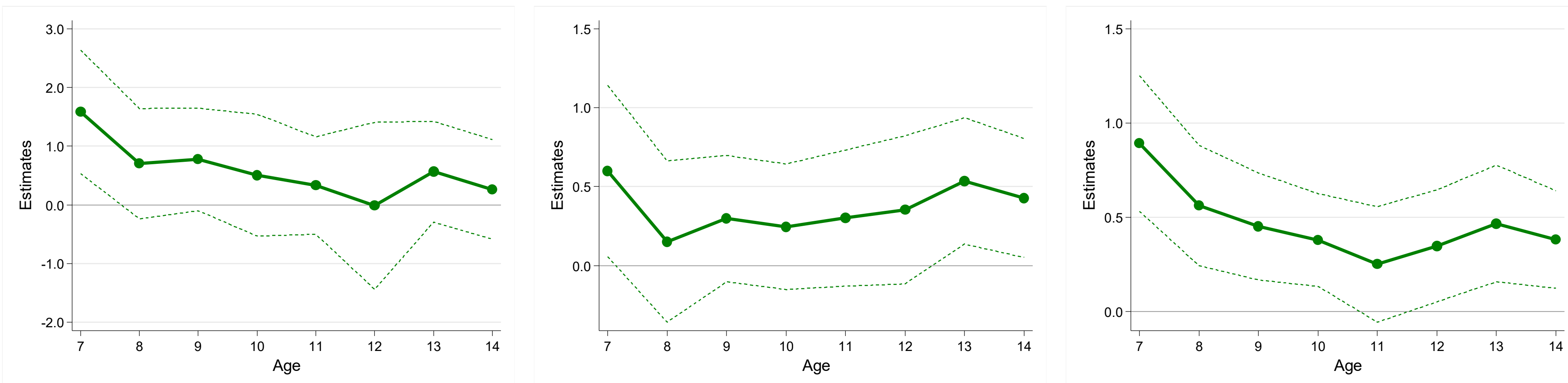
A. *Outpatient* visit dummy

(i) Any ACSC | (ii) ENT | (iii) Asthma



B. *Inpatient* admission dummy (×1000)

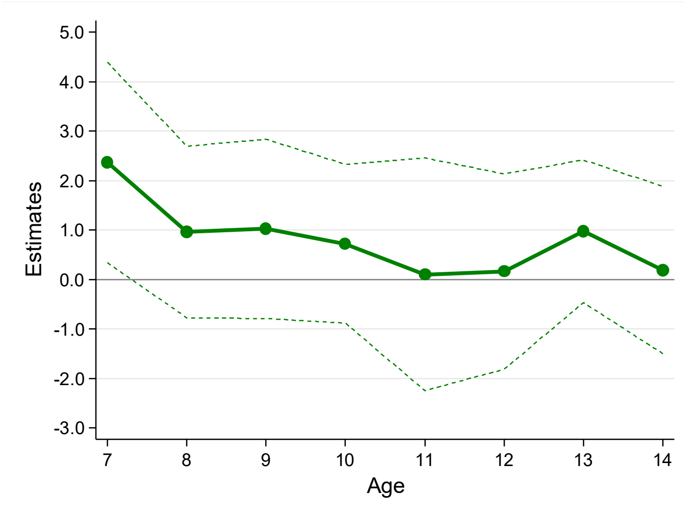
(i) Any ACSC | (ii) ENT | (iii) Asthma



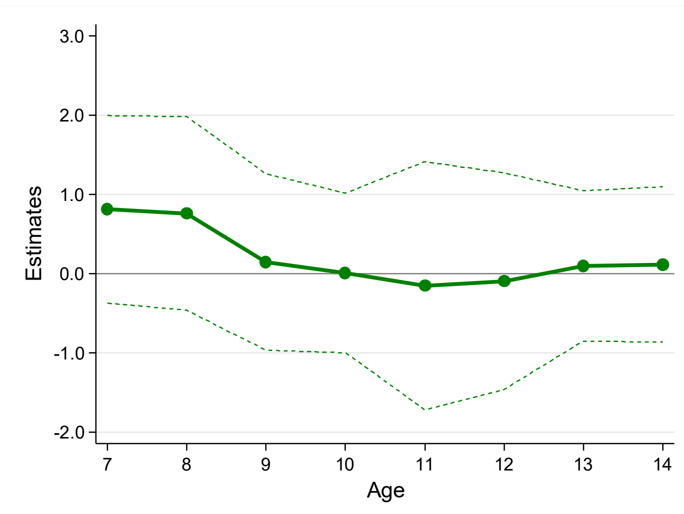
Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month, and an inpatient admission dummy takes one if there is at least one hospitalization per month (×1000). See Online Appendix G for the list of ACSC and summary statistics. The dotted lines are the 95th confidence intervals and the standard errors clustered at municipality level are used to construct them. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. ENT stands for Ear, Nose, and Throat. The corresponding arc-elasticities are presented in Online Appendix G.

Figure 8: Offset effects

A. *Inpatient* admission dummy (×1000)



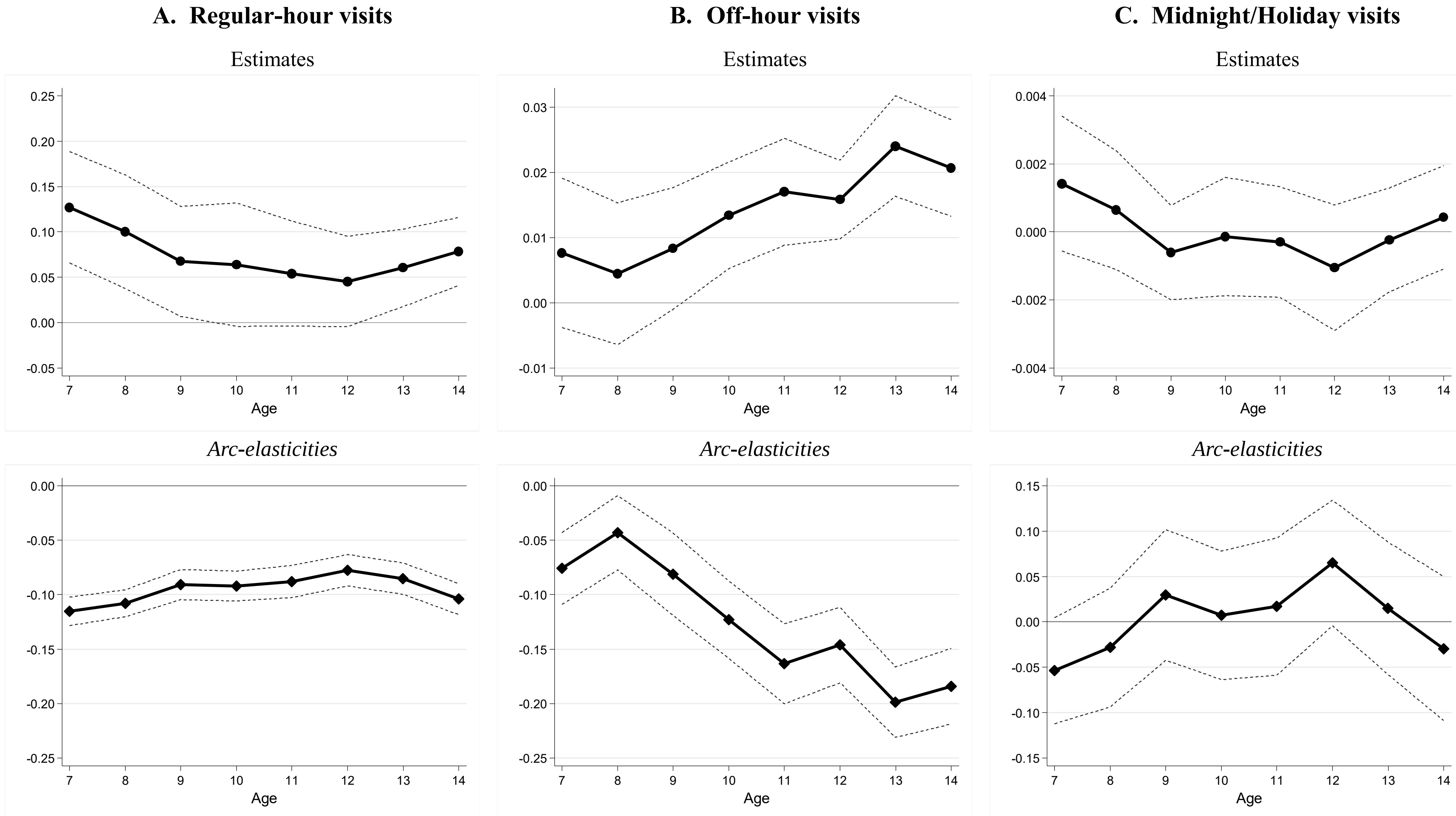
B. *Inpatient* spending (in 1K JPY)



Notes: An inpatient admission 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 (roughly USD10). The dotted lines are the 95th confidence intervals derived from standard errors clustered at municipality level. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization.

Figure 9: By time of visits

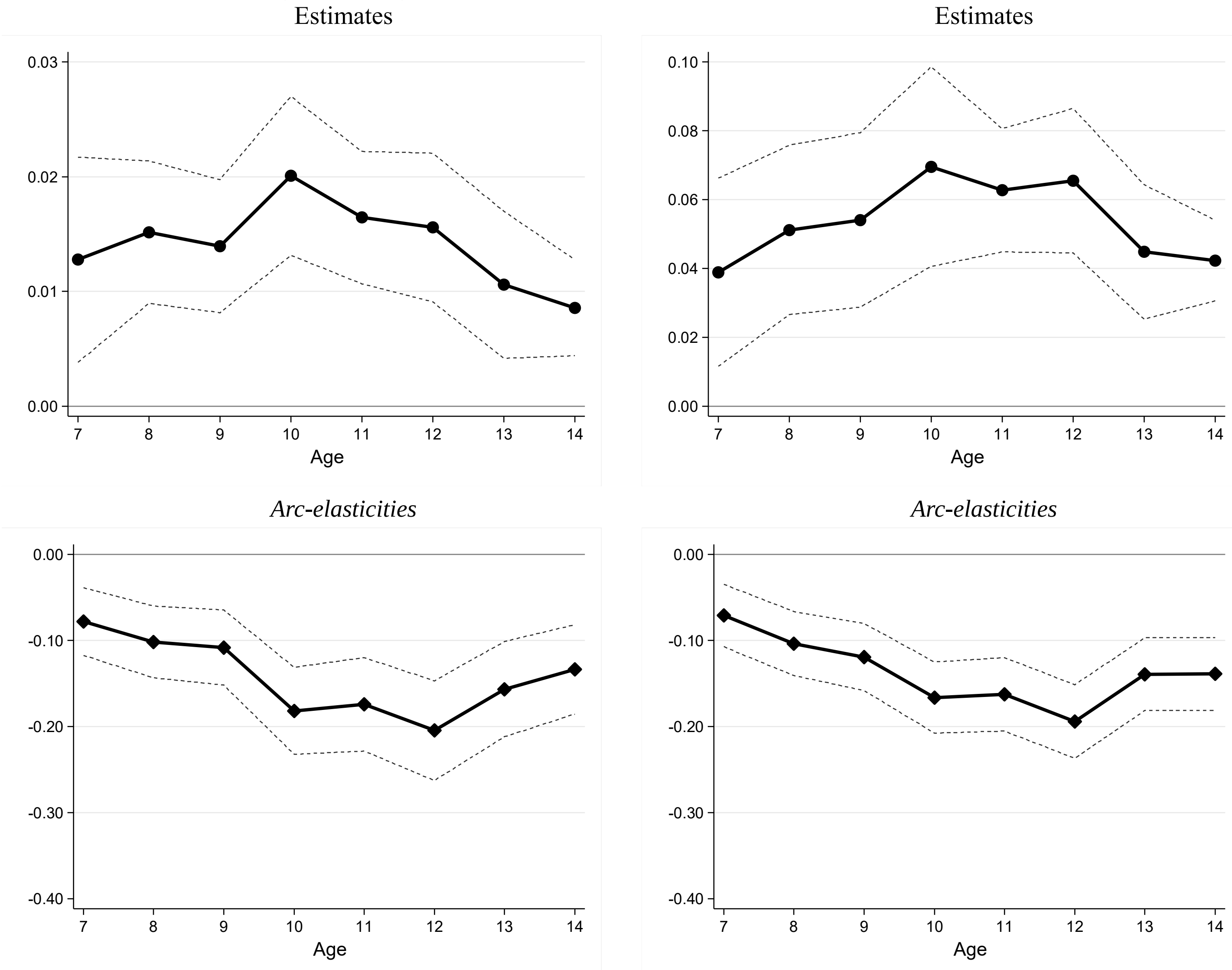
Outcome: Frequency of outpatient visits



Notes: The frequency of outpatient visits is the number of outpatient visits per month. See Online Appendix H 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 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 arc-elasticities.

Figure 10: Inappropriate use of antibiotics

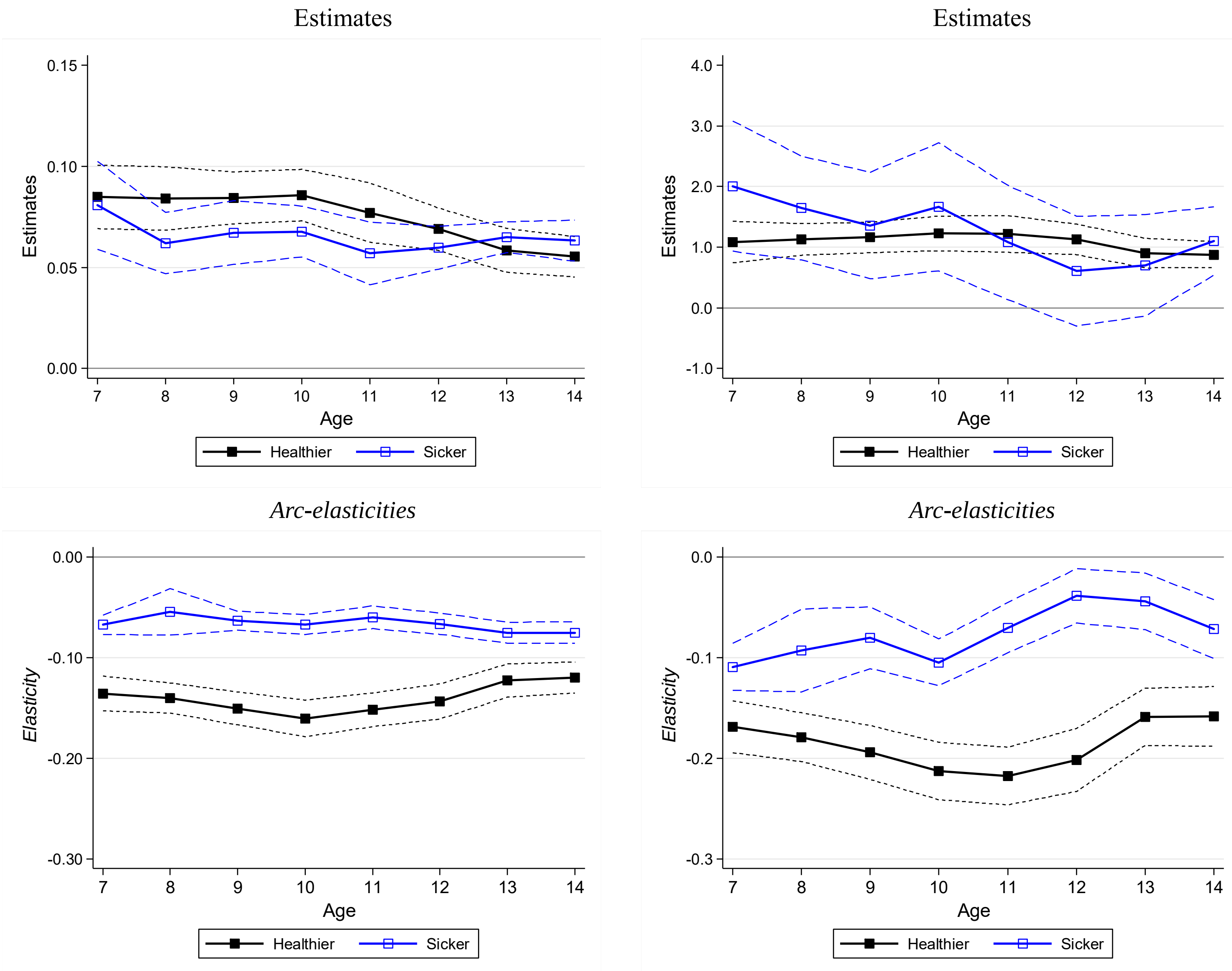
A. Outpatient spending on antibiotic drugs (in 1K JPY) **B. Frequency of antibiotics prescriptions**



Notes: The outcome is monthly outpatient spending on antibiotics considered as inappropriate measured in thousand JPY (roughly USD10) in Panel A and the number of prescriptions for the antibiotic per month in Panel B. See Online Appendix J for the list of ICD10 codes and the summary statistics for inappropriate use of antibiotics. 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 arc-elasticities. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization.

Figure 11: Price responsiveness by health status

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



Notes: An outpatient visit 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 (roughly USD10). We determine each child's 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 for each age (in years) and the subsidy status at the first entry to data. See the main text for details. 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 arc-elasticities. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Note that the scales of *y*-axis on the arc-elasticities are set the same so that two elasticities are visually comparable.

Table 1: Summary statistics

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. Overall (N= 2,303,335 person-month)				
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 visit dummy	40.7%	0.49	0	1
Outpatient spending (in 1K JPY)	6.09	25.33	0	9,336
Outpatient spending (outpatient visit >0)	14.95	37.98	0.08	9,336
N of outpatient visits	0.83	1.46	0	34
N of outpatient visits (outpatient visit >0)	2.04	1.65	0	34
Inpatient admission dummy	0.28%	0.05	0	1
Inpatient spending (in 1K JPY)	1.15	35.24	0	6,084
Inpatient spending (inpatient admission >0)	406.52	523.57	5.23	6,084

Notes: The outpatient and inpatient spending are measured in thousands JPY (roughly 10USD).

Table 2: Basic results

	A. Outpatient visit dummy				B. Outpatient spending (in 1K JPY)			
	(1)		(2)		(3)		(4)	
	Estimate	(SE)	*Arc-elasticities*	[SE]	Estimate	(SE)	*Arc-elasticities*	[SE]
Subsidized ×								
Age7	0.075***	(0.006)	*-0.083****	[0.005]	1.376***	(0.236)	*-0.111****	[0.008]
Age8	0.068***	(0.006)	*-0.078****	[0.004]	1.278***	(0.179)	*-0.107****	[0.009]
Age9	0.070***	(0.005)	*-0.086****	[0.004]	1.131***	(0.171)	*-0.099****	[0.009]
Age10	0.073***	(0.005)	*-0.094****	[0.004]	1.350***	(0.221)	*-0.125****	[0.009]
Age11	0.066***	(0.006)	*-0.090****	[0.004]	1.113***	(0.211)	*-0.106****	[0.009]
Age12	0.064***	(0.004)	*-0.092****	[0.004]	0.872***	(0.223)	*-0.082****	[0.010]
Age13	0.062***	(0.004)	*-0.092****	[0.004]	0.811***	(0.218)	*-0.076****	[0.009]
Age14	0.060***	(0.004)	*-0.092****	[0.004]	0.998***	(0.134)	*-0.095****	[0.010]
In-kind	0.047***	(0.014)			0.440	(0.388)		
Income restriction	-0.020**	(0.009)			-0.561	(0.372)		
R-squared	0.23				0.51			
N	2,205,647				2,205,647			
N of Individual	63,530				63,530			
Mean wo subsidy	0.32				4.49			

Notes: An outpatient visit dummy takes one if an individual makes at least one outpatient visit per month and zero otherwise. The outpatient spending is total monthly spending on outpatient care measured in thousands JPY (roughly 10USD). All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the arc-elasticities, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 3: Asymmetric responses

	A. Outpatient visit dummy				B. Outpatient spending (in 1K JPY)			
	(1)		(2)		(3)		(4)	
	Estimate	(SE)	*Semi point-elasticity*	[SE]	Estimate	(SE)	*Semi point-elasticity*	[SE]
Subsidized ×								
Age7 × Better	0.060***	(0.009)	*-0.485****	[0.053]	1.064***	(0.291)	*-0.691****	[0.112]
Age8 × Better	0.069***	(0.008)	*-0.590****	[0.048]	1.106***	(0.278)	*-0.759****	[0.109]
Age9 × Better	0.060***	(0.006)	*-0.562****	[0.050]	0.925***	(0.244)	*-0.691****	[0.105]
Age10 × Better	0.067***	(0.007)	*-0.659****	[0.042]	1.056***	(0.283)	*-0.836****	[0.109]
Age11 × Better	0.072***	(0.007)	*-0.748****	[0.038]	1.135***	(0.257)	*-0.880****	[0.102]
Age12 × Better	0.069***	(0.006)	*-0.763****	[0.041]	0.936***	(0.269)	*-0.700****	[0.092]
Age13 × Better	0.066***	(0.005)	*-0.751****	[0.043]	0.809**	(0.325)	*-0.582****	[0.091]
Age14 × Better	0.067***	(0.006)	*-0.775****	[0.040]	0.724**	(0.315)	*-0.524****	[0.094]
Subsidized ×								
Age7 × Worse	-0.070***	(0.008)	*-0.462****	[0.033]	-1.633***	(0.333)	*-0.746****	[0.062]
Age8 × Worse	-0.068***	(0.008)	*-0.476****	[0.039]	-1.633***	(0.340)	*-0.769****	[0.082]
Age9 × Worse	-0.066***	(0.009)	*-0.483****	[0.049]	-1.493***	(0.326)	*-0.723****	[0.079]
Age10 × Worse	-0.062***	(0.008)	*-0.471****	[0.042]	-1.601***	(0.274)	*-0.813****	[0.082]
Age11 × Worse	-0.066***	(0.007)	*-0.538****	[0.042]	-1.551***	(0.222)	*-0.837****	[0.081]
Age12 × Worse	-0.061***	(0.007)	*-0.522****	[0.042]	-1.416***	(0.274)	*-0.774****	[0.080]
Age13 × Worse	-0.058***	(0.005)	*-0.519****	[0.044]	-1.157***	(0.214)	*-0.639****	[0.085]
Age14 × Worse	-0.061***	(0.005)	*-0.560****	[0.045]	-1.155***	(0.200)	*-0.657****	[0.084]
In-kind	0.050***	(0.014)			0.452	(0.395)		
Income restriction	-0.020**	(0.009)			-0.731**	(0.346)		
R-squared	0.23				0.51			
N	2,144,756				2,144,756			
N of Individual	62,609				62,609			
Mean wo subsidy	0.32				4.49			

Notes: An outpatient visit dummy takes one if an individual makes at least one outpatient visit per month and zero otherwise. The outpatient spending is total monthly spending on outpatient care measured in thousands JPY (roughly 10USD). “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%. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the semi point-elasticity, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 4: Ambulatory Care Sensitive Conditions (ACSC)

	A. Any ACSC				B. ENT				C. Asthma			
	(1)		(2)		(3)		(4)		(5)		(6)	
	An *outpatient* dummy		An *inpatient* dummy ($\times$1000)		An *outpatient* dummy		An *inpatient* dummy ($\times$1000)		An *outpatient* dummy		An *inpatient* dummy ($\times$1000)	
	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)
Subsidized $\times$												
Age7	0.038^{***}	(0.005)	0.159^{***}	(0.053)	0.027^{***}	(0.004)	0.060^{**}	(0.028)	0.022^{***}	(0.003)	0.089^{***}	(0.018)
Age8	0.034^{***}	(0.005)	0.070	(0.048)	0.024^{***}	(0.004)	0.015	(0.026)	0.020^{***}	(0.003)	0.056^{***}	(0.016)
Age9	0.031^{***}	(0.005)	0.077^{*}	(0.044)	0.023^{***}	(0.004)	0.030	(0.020)	0.015^{***}	(0.002)	0.045^{***}	(0.014)
Age10	0.032^{***}	(0.004)	0.051	(0.053)	0.024^{***}	(0.004)	0.025	(0.020)	0.015^{***}	(0.002)	0.038^{***}	(0.012)
Age11	0.026^{***}	(0.003)	0.033	(0.042)	0.020^{***}	(0.003)	0.030	(0.022)	0.010^{***}	(0.002)	0.025	(0.016)
Age12	0.024^{***}	(0.003)	-0.002	(0.072)	0.017^{***}	(0.003)	0.035	(0.024)	0.009^{***}	(0.002)	0.035^{**}	(0.015)
Age13	0.022^{***}	(0.003)	0.056	(0.044)	0.015^{***}	(0.003)	0.054^{***}	(0.020)	0.009^{***}	(0.002)	0.047^{***}	(0.016)
Age14	0.022^{***}	(0.003)	0.026	(0.043)	0.016^{***}	(0.002)	0.043^{**}	(0.019)	0.008^{***}	(0.002)	0.038^{***}	(0.013)
In-kind	0.030^{**}	(0.015)	0.180^{*}	(0.095)	0.021^{***}	(0.005)	0.053^{***}	(0.017)	0.010	(0.015)	0.058^{**}	(0.025)
Income restriction	-0.008	(0.008)	-0.185^{***}	(0.059)	-0.011	(0.007)	-0.060^{**}	(0.027)	0.000	(0.004)	-0.079^{**}	(0.035)
R-squared	0.24		0.14		0.16		0.06		0.35		0.16	
N	2,205,647		2,205,647		2,205,647		2,205,647		2,205,647		2,205,647	
N of Individual	63,530		63,530		63,530		63,530		63,530		63,530	
Mean wo subsidy	0.11		1.04		0.08		0.32		0.04		0.25	

Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month, and an inpatient admission dummy takes one if there is at least one hospitalization per month ($\times$1000). See Online Appendix G for the list of ACSC and summary statistics. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For even-numbered columns, we also control for a dummy that takes one if the municipality also offers subsidy for inpatient care. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. ENT stands for Ear, Nose, and Throat. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 5: Offset effects

	***Inpatient* dummy (×1000)**		***Inpatient* spending** (in 1K JPY)	
	(1)		(2)	
	Estimate	(SE)	Estimate	(SE)
Subsidized ×				
Age7	2.365**	(1.026)	0.814	(0.599)
Age8	0.958	(0.879)	0.761	(0.618)
Age9	1.023	(0.918)	0.147	(0.564)
Age10	0.721	(0.812)	0.009	(0.510)
Age11	0.104	(1.191)	-0.151	(0.792)
Age12	0.160	(1.001)	-0.094	(0.692)
Age13	0.974	(0.730)	0.098	(0.481)
Age14	0.193	(0.857)	0.116	(0.497)
In-kind	2.984***	(1.109)	0.316	(0.539)
Income restriction	-2.423***	(0.757)	-1.306***	(0.453)
R-squared	0.12		0.20	
N	2,205,647		2,205,647	
N of Individual	63,530		63,530	
Mean	2.41		1.00	

Notes: An inpatient admission 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 (roughly USD10). All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. We also control for a dummy that takes one if the municipality also offers subsidy for inpatient care. The standard errors clustered at the municipality level are reported in parenthesis. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 6: By time of visits

Outcome: Frequency of outpatient visits

	A. Regular-hour visits				B. Off-hour visits				C. Midnight/Holiday visits			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Estimate	(SE)	*Arc-elasticity*	[SE]	Estimate	(SE)	*Arc-elasticity*	[SE]	Estimate	(SE)	*Arc-elasticity*	[SE]
Subsidized ×												
Age7	0.169***	(0.028)	*-0.115****	[0.007]	0.008	(0.006)	*-0.076****	[0.017]	0.001	(0.001)	*-0.054**	[0.030]
Age8	0.148***	(0.030)	*-0.108****	[0.006]	0.004	(0.006)	*-0.043***	[0.017]	0.001	(0.001)	*-0.028*	[0.033]
Age9	0.115***	(0.027)	*-0.091****	[0.007]	0.008*	(0.005)	*-0.081****	[0.019]	-0.001	(0.001)	*0.030*	[0.037]
Age10	0.110***	(0.031)	*-0.092****	[0.007]	0.013***	(0.004)	*-0.123****	[0.018]	-0.000	(0.001)	*0.007*	[0.036]
Age11	0.096***	(0.026)	*-0.088****	[0.008]	0.017***	(0.004)	*-0.163****	[0.019]	-0.000	(0.001)	*0.017*	[0.039]
Age12	0.079***	(0.023)	*-0.078****	[0.007]	0.016***	(0.003)	*-0.146****	[0.018]	-0.001	(0.001)	*0.065**	[0.035]
Age13	0.082***	(0.020)	*-0.085****	[0.007]	0.024***	(0.004)	*-0.199****	[0.017]	-0.000	(0.001)	*0.015*	[0.037]
Age14	0.096***	(0.018)	*-0.104****	[0.007]	0.021***	(0.004)	*-0.184****	[0.018]	0.000	(0.001)	*-0.029*	[0.040]
In-kind	0.035	(0.053)			0.031	(0.019)			-0.001	(0.004)		
Income restriction	-0.051	(0.032)			-0.005	(0.010)			-0.001	(0.002)		
R-squared	0.28				0.14				0.04			
N	2,205,647				2,205,647				2,205,647			
N of Individual	63,530				63,530				63,530			
Mean wo subsidy	0.401				0.023				0.006			
Share	89.1%				8.4%				2.5%			

Notes: The frequency of outpatient visits is the number of outpatient visits per month. See Online Appendix I for the list of billing codes for the urgent visits (off-hour visits and midnight/holiday visits) and the corresponding fees that are additionally charged on top of fees for regular-hour visits. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the semi point-elasticity, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 7: Inappropriate use of antibiotics

	A. Outpatient spending on antibiotics (in 1K JPY)				B. Frequency of antibiotics prescriptions			
	(1)		(2)		(3)		(4)	
	Estimate	(SE)	*Arc-elasticities*	[SE]	Estimate	(SE)	*Arc-elasticities*	[SE]
Subsidized ×								
Age7	0.013***	(0.005)	*-0.078****	[0.020]	0.039***	(0.014)	*-0.071****	[0.019]
Age8	0.015***	(0.003)	*-0.102****	[0.021]	0.051***	(0.012)	*-0.104****	[0.019]
Age9	0.014***	(0.003)	*-0.108****	[0.022]	0.054***	(0.013)	*-0.119****	[0.020]
Age10	0.020***	(0.004)	*-0.182****	[0.026]	0.070***	(0.015)	*-0.166****	[0.021]
Age11	0.016***	(0.003)	*-0.174****	[0.028]	0.063***	(0.009)	*-0.163****	[0.022]
Age12	0.016***	(0.003)	*-0.205****	[0.030]	0.066***	(0.011)	*-0.194****	[0.022]
Age13	0.011***	(0.003)	*-0.157****	[0.028]	0.045***	(0.010)	*-0.139****	[0.022]
Age14	0.009***	(0.002)	*-0.134****	[0.026]	0.042***	(0.006)	*-0.139****	[0.022]
In-kind	0.018**	(0.009)			0.087*	(0.052)		
Income restriction	-0.006	(0.005)			-0.029*	(0.017)		
R-squared	0.08				0.09			
N	2,205,647				2,205,647			
N of Individual	63,530				63,530			
Mean wo subsidy	0.052				0.193			

Notes: The outcome is monthly outpatient spending on antibiotics considered as inappropriate in Panel A and the number of prescriptions for the antibiotic per month in Panel B. See Online Appendix J for the list of ICD10 codes and the summary statistics for inappropriate use of antibiotics. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the arc-elasticities, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 8: Price responsiveness by health status

A. Outpatient visit dummy

	Healthier				Sicker			
	(1)		(2)		(3)		(4)	
	Estimate	(SE)	*Arc-elasticities*	[SE]	Estimate	(SE)	*Arc-elasticities*	[SE]
Subsidized ×								
Age7	0.085***	(0.008)	*-0.136****	[0.009]	0.081***	(0.011)	*-0.067****	[0.005]
Age8	0.084***	(0.008)	*-0.140****	[0.008]	0.062***	(0.008)	*-0.055****	[0.012]
Age9	0.084***	(0.006)	*-0.150****	[0.008]	0.067***	(0.008)	*-0.063****	[0.005]
Age10	0.086***	(0.006)	*-0.160****	[0.009]	0.068***	(0.006)	*-0.067****	[0.005]
Age11	0.077***	(0.007)	*-0.152****	[0.009]	0.057***	(0.008)	*-0.060****	[0.006]
Age12	0.069***	(0.005)	*-0.143****	[0.009]	0.060***	(0.005)	*-0.066****	[0.005]
Age13	0.058***	(0.006)	*-0.123****	[0.008]	0.065***	(0.004)	*-0.075****	[0.005]
Age14	0.055***	(0.005)	*-0.120****	[0.008]	0.063***	(0.005)	*-0.075****	[0.005]
In-kind	0.029	(0.034)			0.052***	(0.016)		
Income restriction	-0.021**	(0.010)			-0.006	(0.015)		
R-squared	0.15				0.22			
N	998,107				994,982			
N of Individual	26,097				26,076			
Mean wo subsidy	0.20				0.44			
Mean w subsidy	0.32				0.56			

Notes: An outpatient visit dummy takes one if an individual makes at least one outpatient visit per month and zero otherwise. We determine each patient's health status by the outpatient spending in the first 6 months after one is observed in the claim data. Then, we divide enrollees into two types (i.e., sicker or healthier) by the median spending for each age (in years) and the subsidy status. See the main text for the details. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the arc-elasticities, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

B. Outpatient spending per month (in 1K JPY)

	Healthier				Sicker			
	(1)		(2)		(3)		(4)	
	Estimate	(SE)	*Arc-elasticities*	[SE]	Estimate	(SE)	*Arc-elasticities*	[SE]
Subsidized ×								
Age7	1.085***	(0.173)	*-0.169****	[0.013]	2.007***	(0.544)	*-0.109****	[0.012]
Age8	1.128***	(0.132)	*-0.179****	[0.012]	1.645***	(0.434)	*-0.093****	[0.021]
Age9	1.160***	(0.127)	*-0.194****	[0.014]	1.357***	(0.444)	*-0.080****	[0.016]
Age10	1.228***	(0.145)	*-0.213****	[0.015]	1.665***	(0.534)	*-0.105****	[0.012]
Age11	1.219***	(0.153)	*-0.218****	[0.015]	1.078**	(0.477)	*-0.070****	[0.013]
Age12	1.130***	(0.128)	*-0.202****	[0.016]	0.606	(0.458)	*-0.039****	[0.014]
Age13	0.902***	(0.123)	*-0.159****	[0.015]	0.700*	(0.423)	*-0.044****	[0.014]
Age14	0.877***	(0.109)	*-0.159****	[0.015]	1.105***	(0.286)	*-0.072****	[0.015]
In-kind	0.521	(0.589)			0.371	(0.656)		
Income restriction	-0.444	(0.381)			-0.629	(0.565)		
R-squared	0.28				0.52			
N	998,107				994,982			
N of Individual	26,097				26,076			
Mean wo subsidy	2.04				6.89			
Mean w subsidy	3.70				9.84			

Notes: The outpatient spending is total monthly spending on outpatient care measured in thousands JPY (roughly 10USD). We determine each patient's health status by the outpatient spending in the first 6 months after one is observed in the claim data. Then, we divide enrollees into two types (i.e., sicker or healthier) by the median spending for each age (in years) and the subsidy status. See the main text for the details. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the arc-elasticities, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

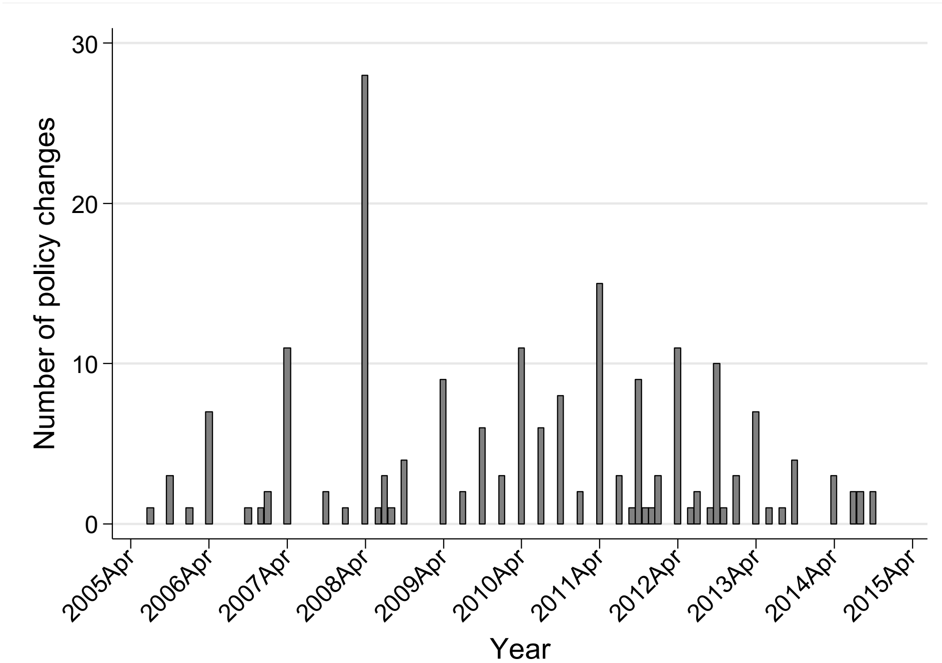
Online Appendix

(Not for Publication)

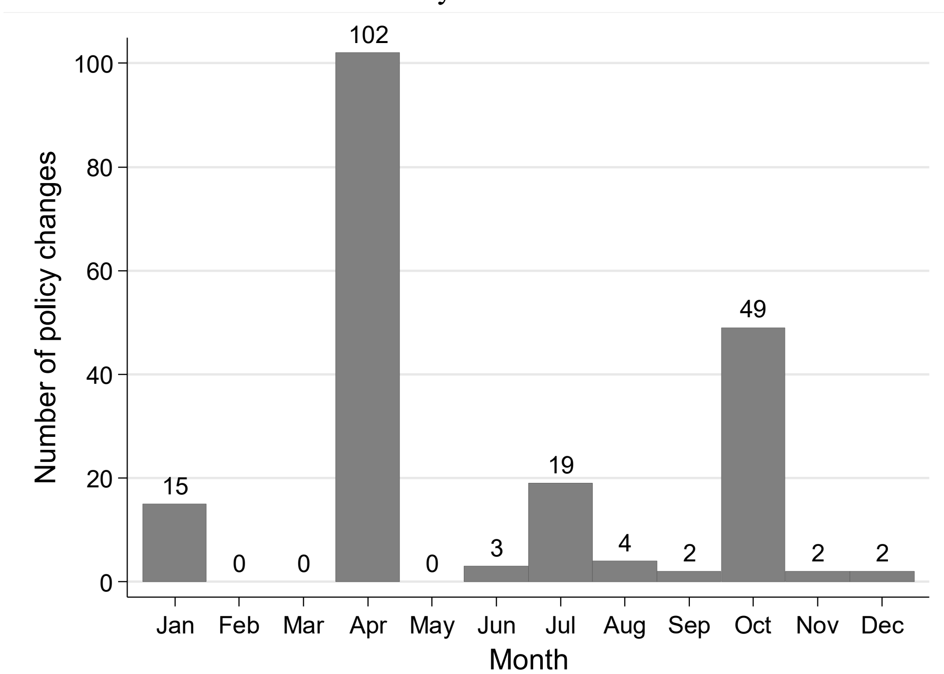
Appendix A: Additional figures and tables

Figure A-1: Timing of the subsidy expansions

A. By year-month

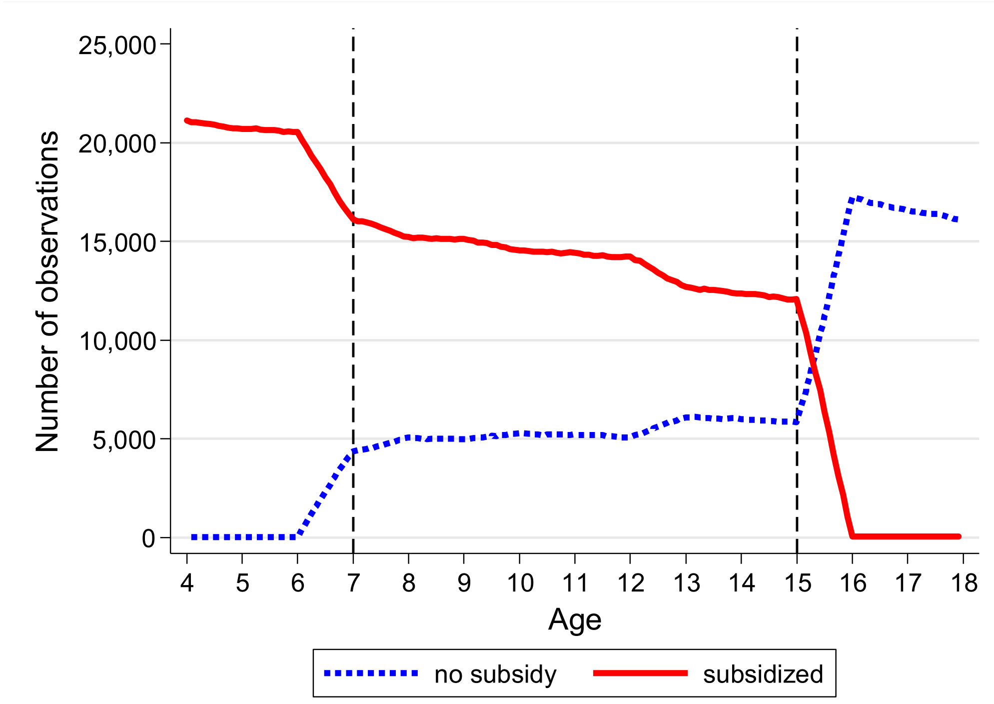


B. By month



Notes: The total number of municipalities is 165. The data spans from April 2005 to March 2015 (10 years). There are total of 198 subsidy expansions for child health care.

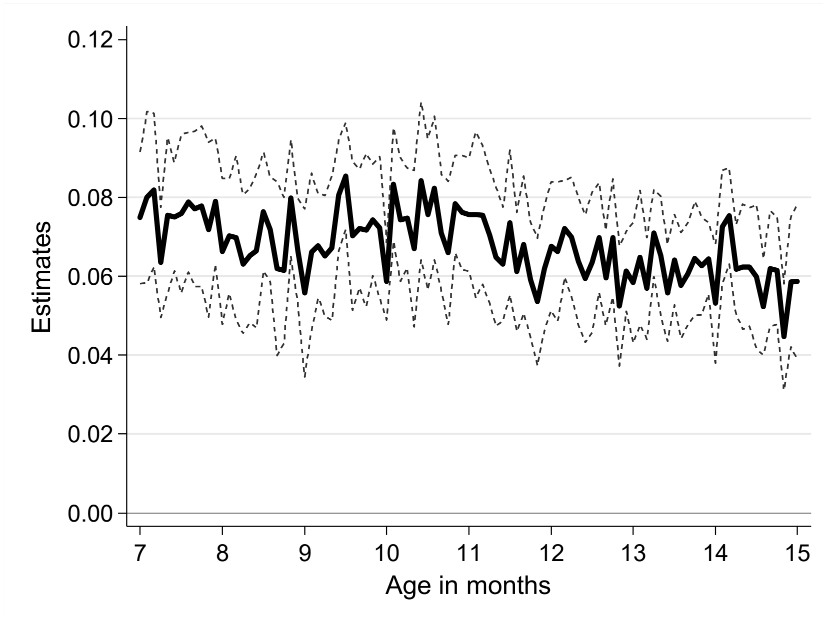
Figure A-2: The number of observations by subsidy status



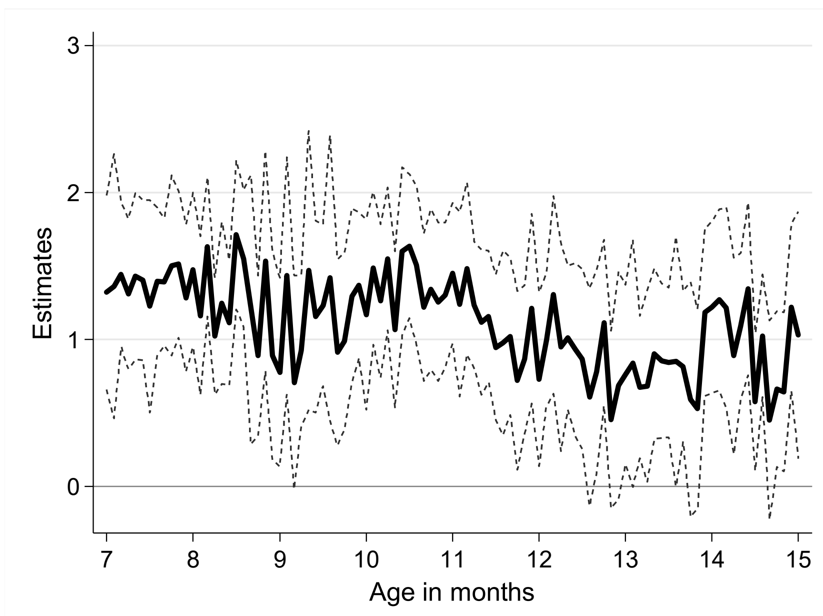
Notes: The two vertical dotted lines indicates the ages of children focused in this paper since we do not have many observations without subsidy below age 7 and with subsidy above age 15. This happens because majority of the municipalities (81.3%) have already provided subsidy till age 6 (the start of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

Figure A-3: Basic results (monthly)

A. Outpatient visit dummy



B. Outpatient spending (in 1K JPY)



Notes: An outpatient visit 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 (roughly USD10). The estimates β_a (where a is age in months) are plotted. The dotted lines are the 95th confidence intervals and the standard errors clustered at municipality level are used to construct them. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization.

Table A-1: List of changes in subsidy status

		Mun-time-age cell		Year-month	
Before change	*After* change	N	*%*	N	*%*
30%	0%	3,308	*29.5%*	14,166	*38.4%*
0%	30%	2,768	*24.7%*	11,685	*31.6%*
500 JPY/visit	30%	1,021	*9.1%*	2,489	*6.7%*
30%	200 JPY/visit	786	*7.0%*	1,365	*3.7%*
30%	500 JPY/visit	643	*5.7%*	1,425	*3.9%*
200 JPY/visit	20%	471	*4.2%*	969	*2.6%*
200 JPY/visit	0%	432	*3.9%*	865	*2.3%*
200 JPY/visit	30%	331	*3.0%*	460	*1.2%*
300 JPY/visit	30%	258	*2.3%*	479	*1.3%*
10%	30%	243	*2.2%*	702	*1.9%*
30%	300 JPY/visit	158	*1.4%*	308	*0.8%*
10%	0%	149	*1.3%*	413	*1.1%*
0%	10%	125	*1.1%*	422	*1.1%*
300 JPY/visit	200 JPY/visit	118	*1.1%*	265	*0.7%*
30%	10%	118	*1.1%*	247	*0.7%*
0%	20%	51	*0.5%*	65	*0.2%*
15%	0%	51	*0.5%*	217	*0.6%*
15%	30%	37	*0.3%*	105	*0.3%*
30%	20%	36	*0.3%*	46	*0.1%*
30%	15%	33	*0.3%*	144	*0.4%*
0%	200 JPY/visit	27	*0.2%*	30	*0.1%*
0%	15%	17	*0.2%*	32	*0.1%*
200 JPY/visit	300 JPY/visit	14	*0.1%*	14	*0.0%*
500 JPY/visit	20%	8	*0.1%*	8	*0.0%*
300 JPY/visit	0%	1	*0.0%*	1	*0.0%*
20%	0%	1	*0.0%*	1	*0.0%*
Total		11,205	*100%*	36,923	*100%*

Notes: This table lists all combinations of transitions in price cost-sharing in our original data. In this paper, we focus on the first two transitions. 200, 300 and 500 JPY are roughly USD2, 3, and 5, respectively.

Table A-2: Sample selection

Variable	In our sample (1)	Not in our sample (2)	Dif (3)=(1)-(2)
Characteristics			
Female	0.49	0.49	0.00
	[0.50]	[0.50]	(0.01)
Age (in years)	11.83	12.06	-0.23
	[2.86]	[2.71]	(0.15)
Utilization			
Outpatient visit dummy	0.32	0.32	0.01
	[0.47]	[0.46]	(0.01)
Outpatient spending (in 1K JPY)	4.47	4.20	0.27
	[21.56]	[16.40]	(0.20)
N of outpatient visits	0.62	0.57	0.05***
	[1.26]	[1.17]	(0.01)
Inpatient admission dummy ($\times$1000)	2.39	2.65	-0.26
	[48.81]	[51.42]	(0.26)
Inpatient spending (in 1K JPY)	0.98	1.28	-0.30
	[33.26]	[40.22]	(0.22)
N	660,697	301,005	
N of individuals	24,429	11,846	

Notes: The sample is limited to person-month observations *without* subsidy. Columns (1) and (2) report the means of variables in the far-left column for in our sample and not in our sample, respectively. The standard deviations are in brackets. Column (3) reports the difference in means between Columns (1) and (2) with standard errors clustered at the municipality in parentheses. The outpatient and inpatient spending are measured in thousands JPY (roughly 10USD). Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table A-3: List of diagnosis group and ICD10

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

Appendix B: Elasticities

B.1 Arc-elasticity

For our basic estimate that does not distinguish the asymmetry of price changes, we report the arc-elasticity. Our basic estimation equation [4] from the main text is repeated here as follows:

$$Y_{iamt} = \alpha + \sum_{A=7}^{14} \beta_A \{subsidized_{iamt} \times 1(Age\ A)\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt} \quad \text{–[B1]}$$

Then, the arc-elasticity for each age in year A is defined by

$$\textit{arc-elasticity}_A = \left(\frac{Q_{1A} - Q_{0A}}{Q_{0A} + Q_{1A}}\right) / \left(\frac{P_{1A} - P_{0A}}{P_{1A} + P_{0A}}\right) = \left(\frac{\beta_A}{Q_{0A} + Q_{1A}}\right) / \left(\frac{0 - 0.3}{0.3 + 0}\right) = -\frac{\beta_A}{Q_{0A} + Q_{1A}} \quad \text{–[B2]}$$

where Q_{1A}, Q_{0A} are quantity of health care utilization for subsidized (denoted by 1), and unsubsidized (denoted by 0), and P_{1A}, P_{0A} are defined in the same way for prices. β_A are the estimates from the first equation. We report arc-elasticity because they are widely used and comparable to estimates from RAND HIE, in which the largest plan was also the free care plan. However, one can make an argument that when the starting price is zero, as in our case, price elasticity may not be well defined. Thus, as an alternative, we also report the *semi* arc-elasticity, which is defined by

$$\textit{semi arc-elasticity}_A = \left(\frac{Q_{1A} - Q_{0A}}{Q_{0A} + Q_{1A}}\right) / \left(\frac{P_{1A} - P_{0A}}{2}\right) = \left(\frac{\beta_A}{Q_{0A} + Q_{1A}}\right) / \left(\frac{0 - 0.3}{2}\right) = \textit{arc-elasticity}_A / 0.15 \quad \text{–[B3]}$$

Thus, in our case, *semi* arc-elasticity is simply arc-elasticity divided by 0.15. For example, *semi* arc-elasticities are reported in Brot-Goldberg *et al.* (2017), and Nilsson and Paul (2015).

B.2 Semi *point*-elasticity

For the analysis of asymmetric responses, we instead report semi *point*-elasticity instead of *arc*-elasticity as we exactly know the starting quantity, and also the direction of the price changes. Again, the equation [5] from the main text is repeated here:

$$Y_{iamt} = \alpha + \sum_{A=7}^{14} \beta_A^{Better} \{subsidized_{iamt} \times better_{iamt} \times 1(Age\ A)\} + \sum_{A=7}^{14} \beta_A^{Worse} \{subsidized_{iamt} \times worse_{iamt} \times 1(Age\ A)\} + \gamma X'_{mt} + \delta_a + \pi_t + \theta_i + \varepsilon_{iamt} \quad \text{–[B4]}$$

Then, the semi *point*-elasticity for each direction of price changes are defined as:

$$\textit{semi point-elasticity}_A^{Better} = \left(\frac{Q_{1A} - Q_{0A}}{Q_{0A}}\right) / (P_{1A} - P_{0A}) = -\left(\frac{\beta_A^{Better}}{Q_{0A}}\right) / 0.3 \quad \text{–[B4]}$$

$$\textit{semi point-elasticity}_A^{Worse} = \left(\frac{Q_{0A} - Q_{1A}}{Q_{1A}}\right) / (P_{0A} - P_{1A}) = \left(\frac{\beta_A^{Worse}}{Q_{1A}}\right) / 0.3 \quad \text{–[B5]}$$

where β_A^{Better} and β_A^{Worse} are estimates from equation [B4] above. For arc-elasticity and semi point-elasticity, the standard errors clustered at municipality are obtained by bootstrapping with 200 repetitions.

References:

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

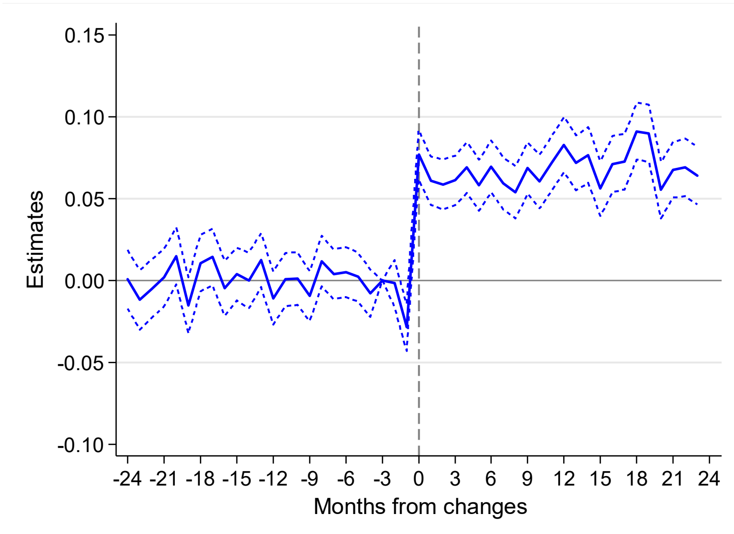
Nilsson, Anton, and Alexander Paul. (2015) "The Effect of Copayments on Children's and Adolescents' Use of Medical Care." *Unpublished manuscript*.

Appendix C: Event-study

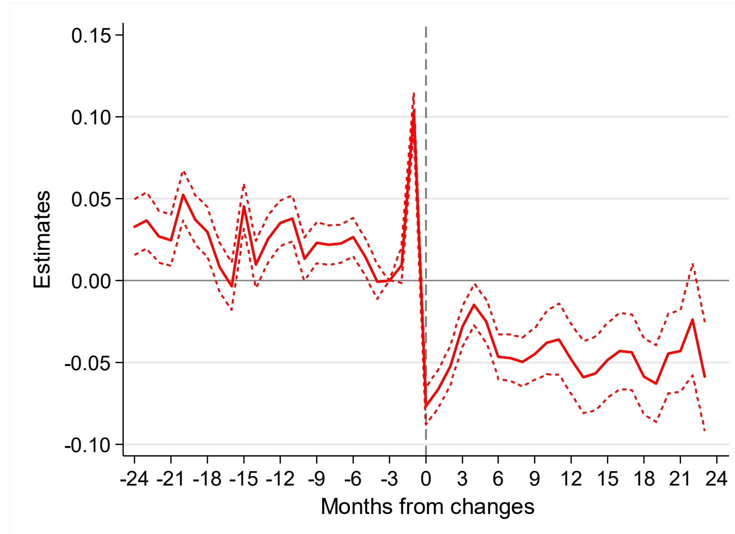
Figure C-1: Event study (±24 months)

A. Outpatient visit dummy

Better (subsidy expansion)

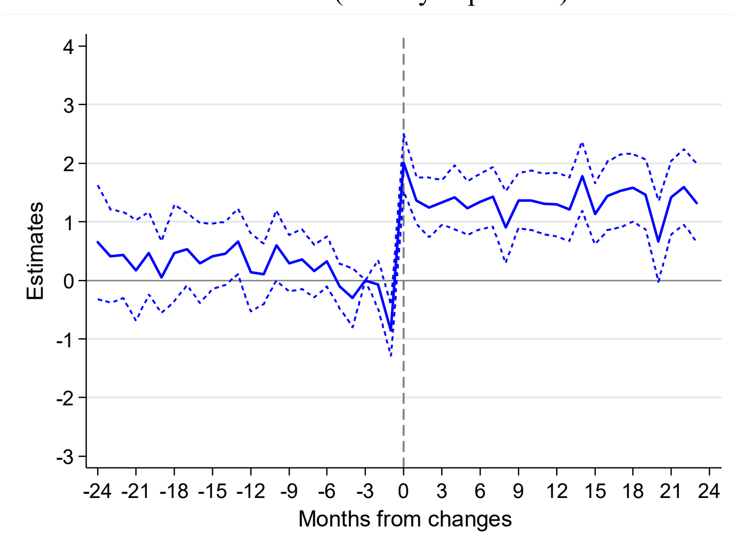


Worse (subsidy expiration)

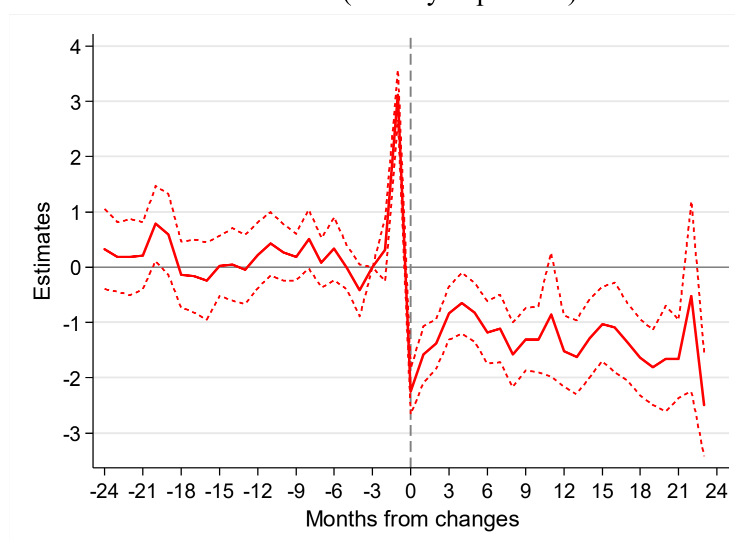


B. **Outpatient spending** (in 1K JPY)

Better (subsidy expansion)



Worse (subsidy expiration)



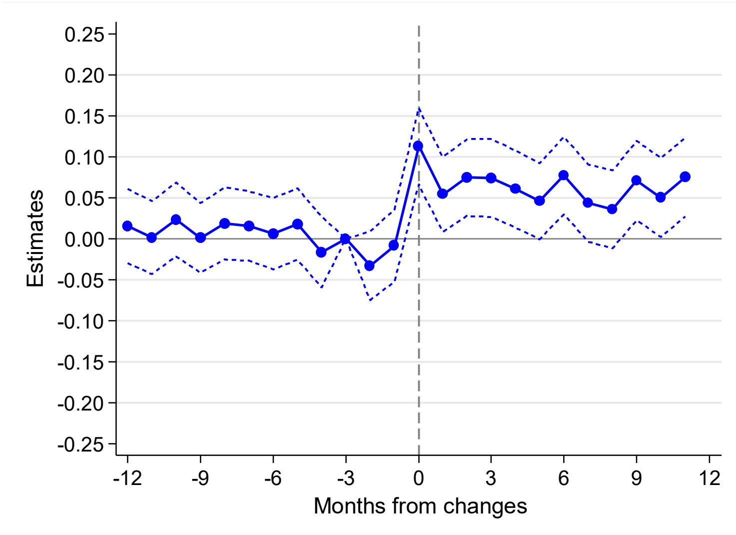
Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). "Better" indicates the subsidy expansion which lowers the price of health care from 30% to 0%, and "Worse" indicates subsidy expiration that raises the price from 0% to 30%. The solid lines plot the estimates from a variant of estimation equation [4] where the subsidized dummy is replaced by a series of dummy for each month ranging from 12 months prior to the change in subsidy status to 24 months after the change in subsidy status (T= –24 to +23, where $T = 0$ is the change). The dotted lines are the 95th confidence interval derived from standard errors clustered at municipality level. The reference month is 3 months before the subsidy changes ($T = –3$). The observations within two months from the subsidy changes of the opposite direction are excluded from the sample to account for anticipatory utilization. Note that the scales of *y*-axis are set the same within the panels so that two figures for opposite directions of subsidy changes are visually comparable.

Figure C-2: Event study at selected ages

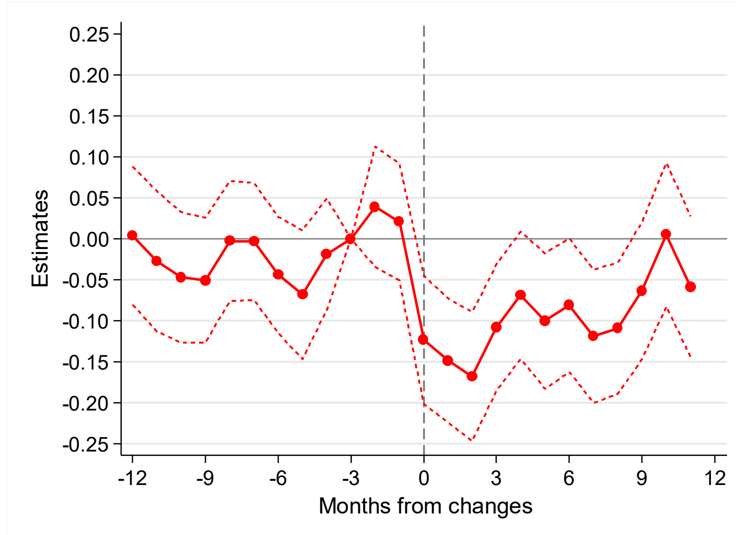
(i) Age 9

A. Outpatient visit dummy

Better (subsidy expansion)

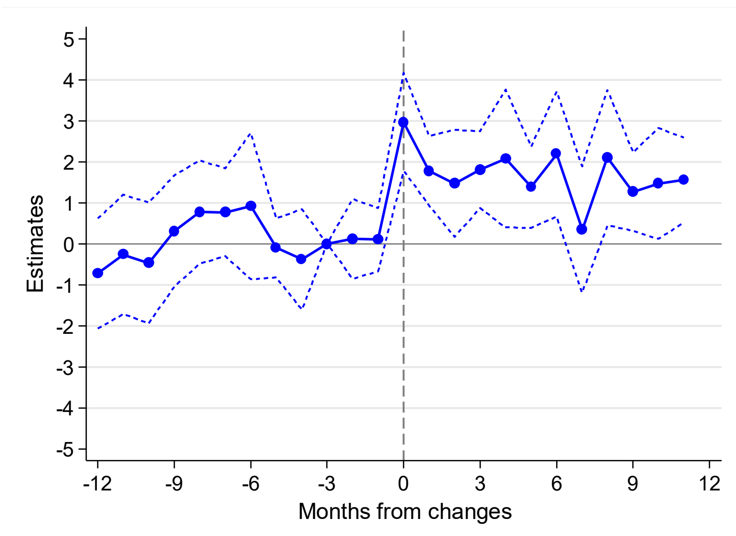


Worse (subsidy expiration)

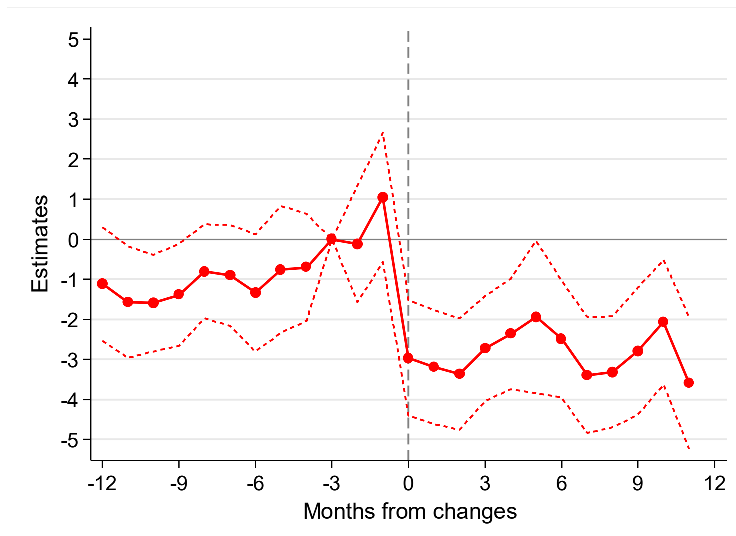


B. Outpatient spending (in 1K JPY)

Better (subsidy expansion)



Worse (subsidy expiration)

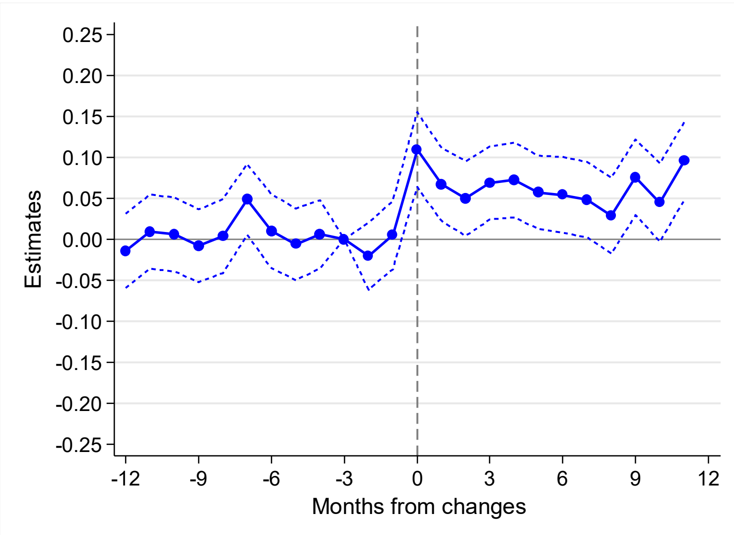


Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). "Better" indicates the subsidy expansion which lowers the price of health care from 30% to 0%, and "Worse" indicates subsidy expiration that raises the price from 0% to 30%. The solid lines plot the estimates from a variant of estimation equation [4] where the subsidized dummy is replaced by a series of dummy 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) at any ages between 9 and 0 months and 9 and 11 months. The dotted lines are the 95th confidence interval derived from standard errors clustered at municipality level. The reference month is 3 months before the subsidy changes (T = –3). The observations within two months from the subsidy changes of the opposite direction are excluded from the sample to account for anticipatory utilization. Note that the scales of y-axis are set the same within the panels so that two figures for opposite directions of subsidy changes are visually comparable.

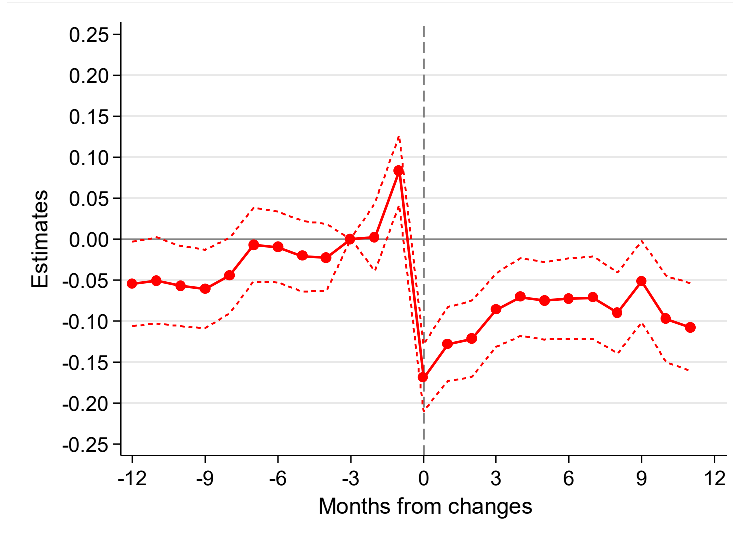
(ii) Age 12

A. Outpatient visit dummy

Better (subsidy expansion)

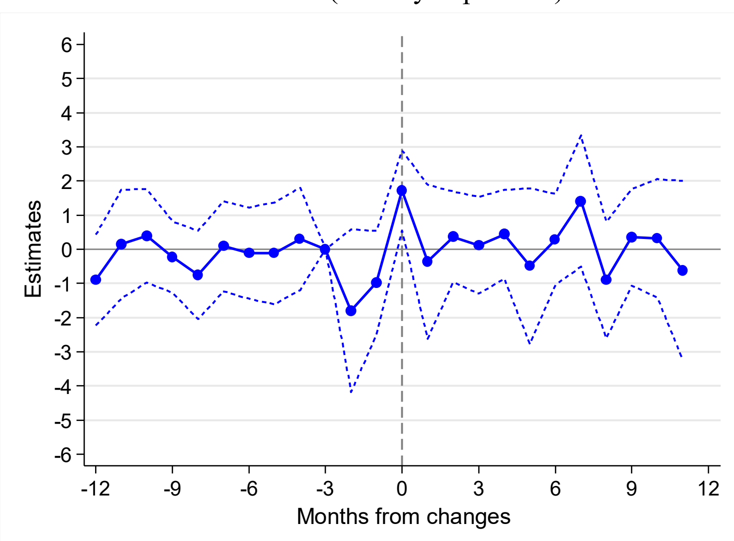


Worse (subsidy expiration)

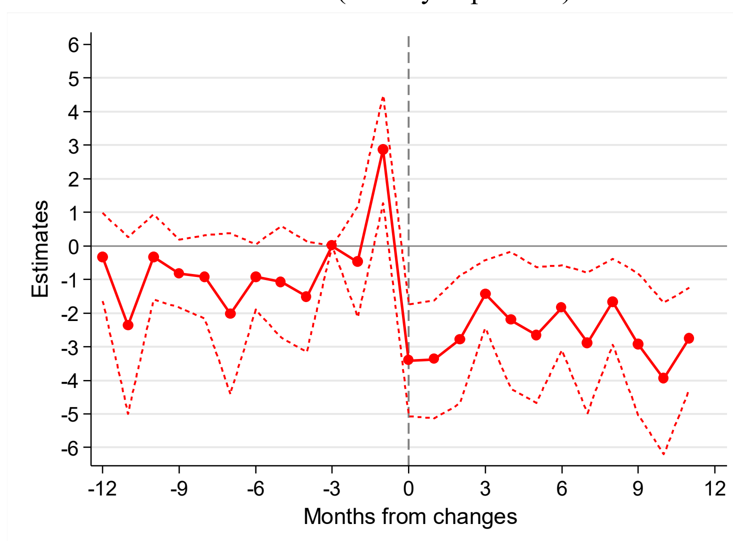


B. Outpatient spending (in 1K JPY)

Better (subsidy expansion)



Worse (subsidy expiration)



Notes: An outpatient visit 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 (roughly USD10). "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 [4] where the subsidized dummy is replaced by a series of dummy 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) at any ages between 12 and 0 months and 12 and 11 months. The dotted lines are the 95th confidence interval derived from standard errors clustered at municipality level. The reference month is 3 months before the subsidy changes (T = –3). The observations within two months from the subsidy changes of the opposite direction are excluded from the sample to account for anticipatory utilization. Note that the scales of y-axis are set the same within the panels so that two figures for opposite directions of subsidy changes are visually comparable.

Appendix D: Robustness Checks

We subject the main results in Table 2 (and Figure 5) to a series of robustness checks. Critically, these results on the causal effects of patient cost-sharing to be robust across all specifications considered. Figure D-1 plots the estimates of key robustness checks together with our baseline estimates from Figure 5. The corresponding results are summarized in Tables C-1. For the ease the comparison, Columns (1) present the estimates from our baseline model of equation [4].

First, we address the potential concern that our control group—namely municipalities without changes in subsidy—exhibits a different time trend than municipalities with subsidy. For example, if the municipalities in better business cycles are more likely to implement the subsidy expansion, while income effects simply increase utilization, our estimates can be upward biased. Since the estimates in event-study before T=0 seems to be reasonably smooth and close to zero, this does not seem to be a serious concern. Nonetheless, we conduct several robustness checks to address this concern. Specifically, we add the municipality specific time trend in Column (2) and time-by-municipality FEs (where time is measured in months) in Column (3) to account for the time-varying municipality characteristics that are correlated with both the expansion of the subsidy and the health care utilization. The latter is especially stringent as these fixed effects capture any municipality specific policy change (e.g., income transfer or other subsidies) or event (outbreak of influenza) in a particular month. We are reassured that row (i) in Figure D-1 shows that the estimates are almost unchanged.

Another way to account for the potential endogeneity of subsidy expansion is to restrict the sample to only those children which experienced at least one change in subsidy status, thereby, dropping children which remain either subsidized or unsubsidized throughout the sample period. Importantly, this identification strategy only exploits the *timing* of the changes in subsidy status. In this way, we can to some extent mitigate the concern that individuals in the treatment and control groups are different. Row (ii) in Figure D-1 shows that the estimates are somewhat noisier due to smaller sample but are qualitatively similar.

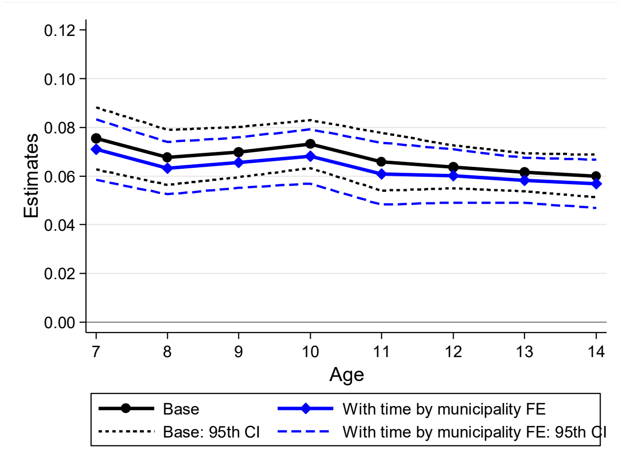
Third, we collapse the data at municipality-age-time cells, which is the level of variation, to partially account for zero spending at the person-month level. Then, we estimate analogous to equation [4] where the number of observations in each cell is used as a weight. Row (iii) in Figure D-1 presents the estimates. It is reassuring that the estimates from the cell level analysis yields almost identical results to those from underlying individual micro data.

As a separate exercise, Figure D-2 and Table 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.

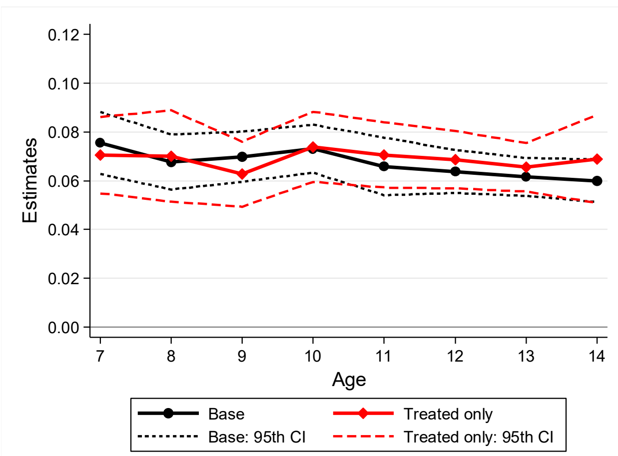
Figure D-1: Robustness checks (Estimates only)

A. Outpatient visit dummy

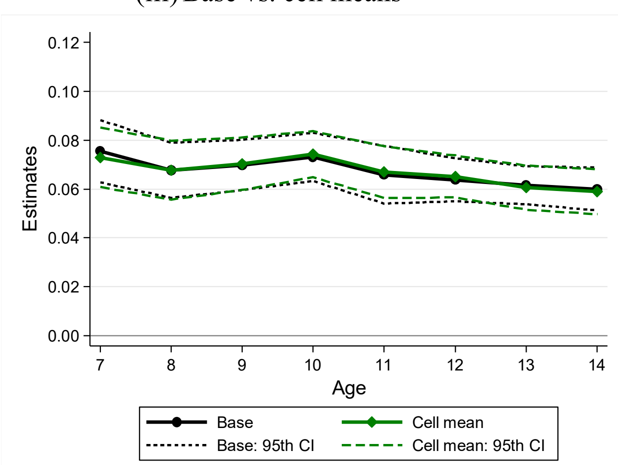
(i) Base vs. With time by municipality FE



(ii) Base vs. Treated only

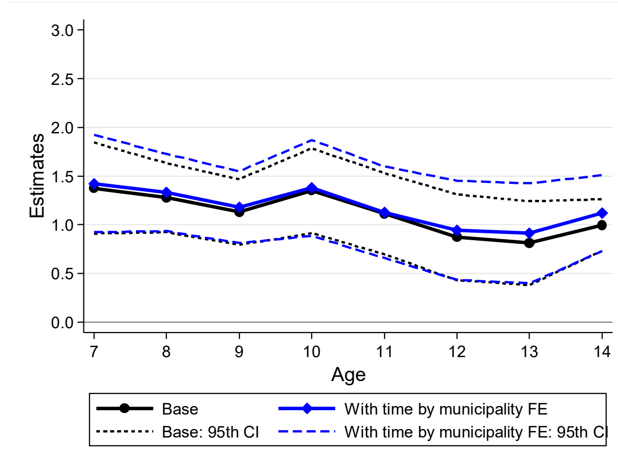


(iii) Base vs. cell means

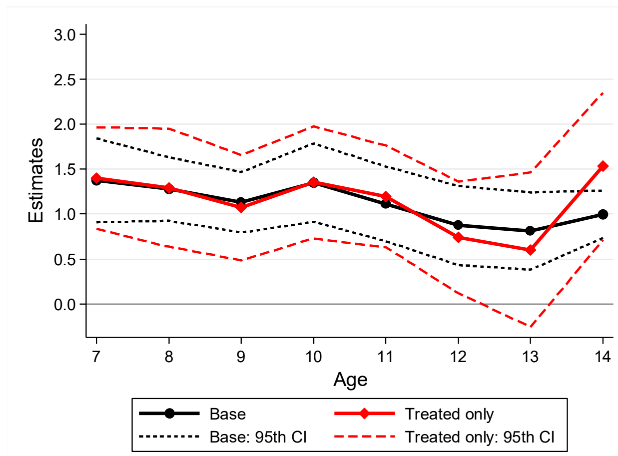


B. Outpatient spending (in 1K JPY)

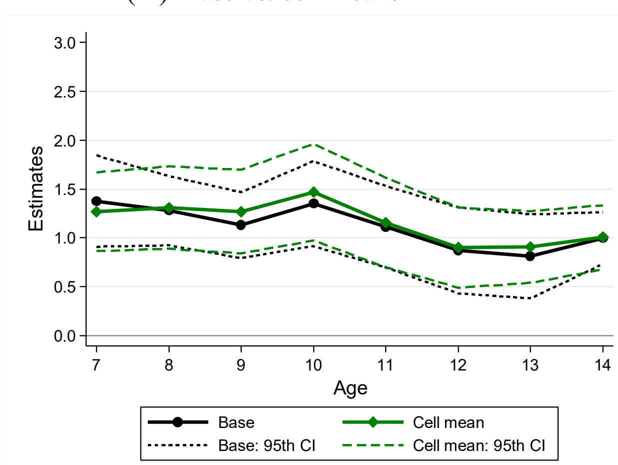
(i) Base vs. With time by municipality FE



(ii) Base vs. Treated only



(iii) Base vs. cell means



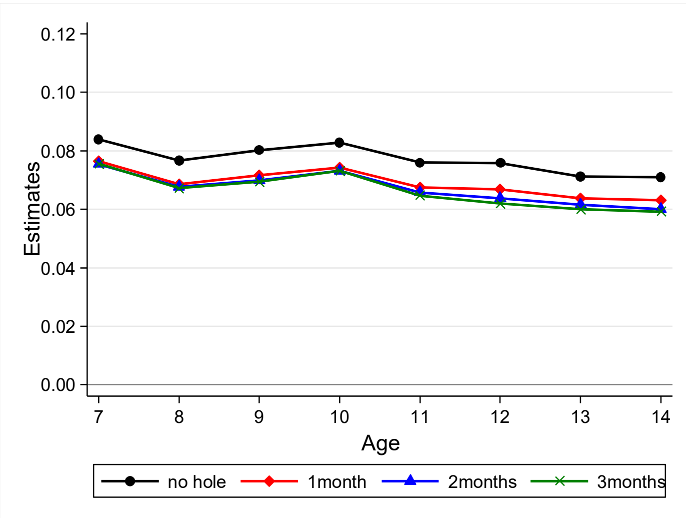
Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). The dotted lines are the 95th confidence intervals derived from standard errors clustered at municipality level except for 2) where standard errors clustered at individual level. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Along with our baseline estimates, (i) reports the estimates which include time by municipality FE, (ii) reports the estimates from the sample limited to those individuals which experienced at least one change in subsidy status, and (iii) reports the estimates from cell means where the cell is defined by municipality-age-time and the number of observations in each cell is used as a weight.

Figure D-2: Sizes of "donut" holes and the estimates/elasticities

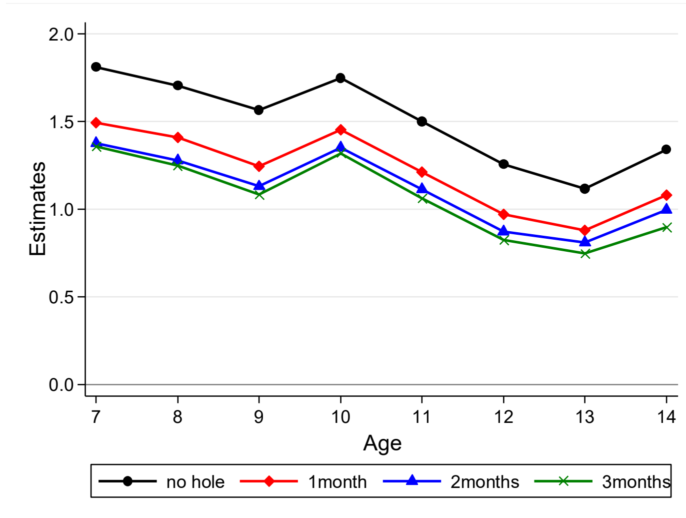
A. Outpatient visit dummy

B. Outpatient spending (in 1K JPY)

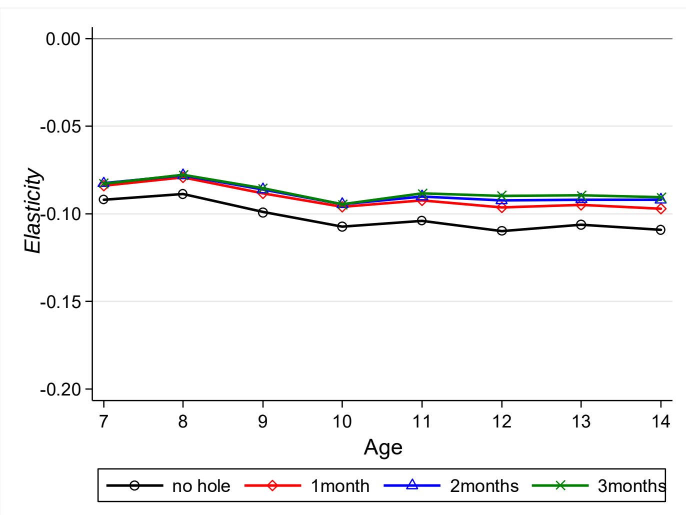
Estimates (A)



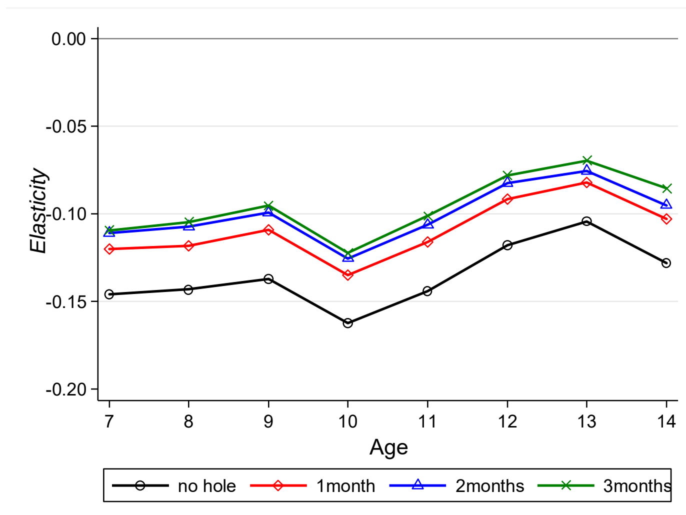
Estimates (B)



Arc-elasticities (A)



Arc-elasticities (B)



Notes: An outpatient visit 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 (roughly USD10). The lines plot the estimates from the sample where the observations within one, two, and three months from subsidy changes are excluded along with the estimates of no exclusion ("no hole").

Table D-1: Robustness checks

(i) Outcome: Outpatient visit dummy

	Baseline (Col. 1 Table 2)		With municipality-time trend		With municipality-time FE		Among only treated		Cell	
	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)
	(1)		(2)		(3)		(4)		(5)	
Subsidized ×										
Age7	0.075***	(0.006)	0.070***	(0.006)	0.071***	(0.006)	0.070***	(0.008)	0.073***	(0.006)
Age8	0.068***	(0.006)	0.064***	(0.005)	0.063***	(0.005)	0.070***	(0.009)	0.068***	(0.006)
Age9	0.070***	(0.005)	0.066***	(0.005)	0.065***	(0.005)	0.063***	(0.007)	0.070***	(0.005)
Age10	0.073***	(0.005)	0.070***	(0.005)	0.068***	(0.006)	0.074***	(0.007)	0.074***	(0.005)
Age11	0.066***	(0.006)	0.063***	(0.006)	0.061***	(0.006)	0.071***	(0.007)	0.067***	(0.005)
Age12	0.064***	(0.004)	0.062***	(0.005)	0.060***	(0.006)	0.069***	(0.006)	0.065***	(0.004)
Age13	0.062***	(0.004)	0.061***	(0.004)	0.058***	(0.005)	0.065***	(0.005)	0.061***	(0.005)
Age14	0.060***	(0.004)	0.060***	(0.005)	0.057***	(0.005)	0.069***	(0.009)	0.059***	(0.005)
In-kind	0.047***	(0.014)	0.054***	(0.019)	0.017	(0.031)	0.040**	(0.019)	0.054***	(0.016)
Income restriction	-0.020**	(0.009)	-0.015*	(0.008)	-0.014	(0.009)	-0.017*	(0.009)	-0.017**	(0.008)
R-squared	0.23		0.23		0.24		0.19		0.30	
N	2,205,647		2,205,647		2,204,496		862,211		465,241	
N of Individual	63,530		63,530		63,502		13,892		-	
Age FE	X		X		X		X		X	
Time FE	X		X		X		X		X	
Individual FE	X		X		X		X		-	
Municipality-Time trend			X							
Municipality-Time FE					X					

Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month. For ease of comparison, Column (1) replicates the estimates from Column (1) in Table 2. Columns (2) and (3) add municipality-specific trend and municipality-time (in months) FE, respectively. Column (4) reports the estimates from the sample limited to those individuals which experienced at least one change in subsidy status. Column (5) reports the estimates from cell means where the cell is defined by municipality-age-time and the number of observations in each cell is used as a weight. The standard errors clustered at the municipality level are reported in parenthesis. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

(ii) Outcome: Outpatient spending (in 1K JPY)

	Baseline (Col. 3 Table 2)		With municipality-time trend		With municipality-time FE		Among only treated		Cell	
	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)
	(1)		(2)		(3)		(4)		(5)	
Subsidized ×										
Age7	1.376***	(0.236)	1.452***	(0.216)	1.422***	(0.253)	1.401***	(0.285)	1.267***	(0.204)
Age8	1.278***	(0.179)	1.366***	(0.172)	1.331***	(0.201)	1.293***	(0.333)	1.310***	(0.213)
Age9	1.131***	(0.171)	1.215***	(0.147)	1.180***	(0.186)	1.073***	(0.297)	1.269***	(0.216)
Age10	1.350***	(0.221)	1.430***	(0.189)	1.378***	(0.249)	1.353***	(0.315)	1.468***	(0.251)
Age11	1.113***	(0.211)	1.184***	(0.191)	1.128***	(0.237)	1.196***	(0.287)	1.157***	(0.231)
Age12	0.872***	(0.223)	0.943***	(0.212)	0.944***	(0.257)	0.741**	(0.314)	0.900***	(0.208)
Age13	0.811***	(0.218)	0.889***	(0.196)	0.912***	(0.260)	0.602	(0.435)	0.907***	(0.186)
Age14	0.998***	(0.134)	1.069***	(0.124)	1.120***	(0.197)	1.531***	(0.414)	1.006***	(0.167)
In-kind	0.440	(0.388)	0.815***	(0.305)	0.405	(0.792)	0.097	(0.336)	0.399	(0.363)
Income restriction	-0.561	(0.372)	-0.519*	(0.269)	-0.366	(0.288)	-0.501	(0.439)	-0.297	(0.617)
R-squared	0.51		0.51		0.51		0.54		0.56	
N	2,205,647		2,205,647		2,204,496		862,211		465,241	
N of Individual	63,530		63,530		63,502		13,892		-	
Age FE	X		X		X		X		X	
Time FE	X		X		X		X		X	
Individual FE	X		X		X		X		-	
Municipality-Time trend			X							
Municipality-Time FE					X					

Notes: The outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). For ease of comparison, Column (1) replicates the estimates from Column (1) in Table 2. Columns (2) and (3) add municipality-specific trend and municipality-time (in months) FE, respectively. Column (4) reports the estimates from the sample limited to those individuals which experienced at least one change in subsidy status. Column (5) reports the estimates from cell means where the cell is defined by municipality-age-time and the number of observations in each cell is used as a weight. The standard errors clustered at the municipality level are reported in parenthesis. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. *** p<0.01, ** p<0.05, * p<0.10

Table D-2: The size of "donut" holes and corresponding estimates/elasticity

(i) Outcome: Outpatient visit dummy

	No exclusion		±1month excluded		±2months excluded		±3months excluded		No exclusion	±1month excluded	±2months excluded	±3months excluded
	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	*Arc-elasticities*			
	(1)		(2)		(3)		(4)		(5)	(6)	(7)	(8)
Subsidized ×												
Age7	0.084***	(0.005)	0.077***	(0.006)	0.075***	(0.006)	0.076***	(0.007)	*-0.106*	*-0.097*	*-0.095*	*-0.095*
Age8	0.077***	(0.004)	0.069***	(0.005)	0.068***	(0.006)	0.067***	(0.007)	*-0.098*	*-0.088*	*-0.086*	*-0.086*
Age9	0.080***	(0.004)	0.072***	(0.005)	0.070***	(0.005)	0.069***	(0.006)	*-0.105*	*-0.093*	*-0.091*	*-0.090*
Age10	0.083***	(0.005)	0.074***	(0.005)	0.073***	(0.005)	0.073***	(0.005)	*-0.109*	*-0.097*	*-0.095*	*-0.095*
Age11	0.076***	(0.005)	0.067***	(0.006)	0.066***	(0.006)	0.065***	(0.006)	*-0.101*	*-0.089*	*-0.086*	*-0.085*
Age12	0.076***	(0.004)	0.067***	(0.004)	0.064***	(0.004)	0.062***	(0.005)	*-0.100*	*-0.087*	*-0.083*	*-0.081*
Age13	0.071***	(0.004)	0.064***	(0.004)	0.062***	(0.004)	0.060***	(0.004)	*-0.092*	*-0.082*	*-0.078*	*-0.076*
Age14	0.071***	(0.004)	0.063***	(0.004)	0.060***	(0.004)	0.059***	(0.005)	*-0.090*	*-0.079*	*-0.074*	*-0.073*
In-kind	0.041***	(0.013)	0.042***	(0.013)	0.047***	(0.014)	0.051***	(0.014)				
Income restriction	-0.021***	(0.008)	-0.018**	(0.008)	-0.020**	(0.009)	-0.017	(0.011)				
R-squared	0.23		0.23		0.23		0.23					
N	2,303,335		2,253,851		2,205,647		2,158,881					
N of Individual	63,590		63,570		63,530		63,362					

Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. The standard errors clustered at the municipality level are reported in parenthesis. Column (1) does not exclude any observations, while Columns (2)–(4) exclude the observations within one, two, and three months from the both sides of subsidy changes to account for anticipatory utilization. Columns (5)–(8) report the arc-elasticities that corresponds to estimates from Columns (1)–(4). Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

(ii) Outcome: Outpatient spending (in 1K JPY)

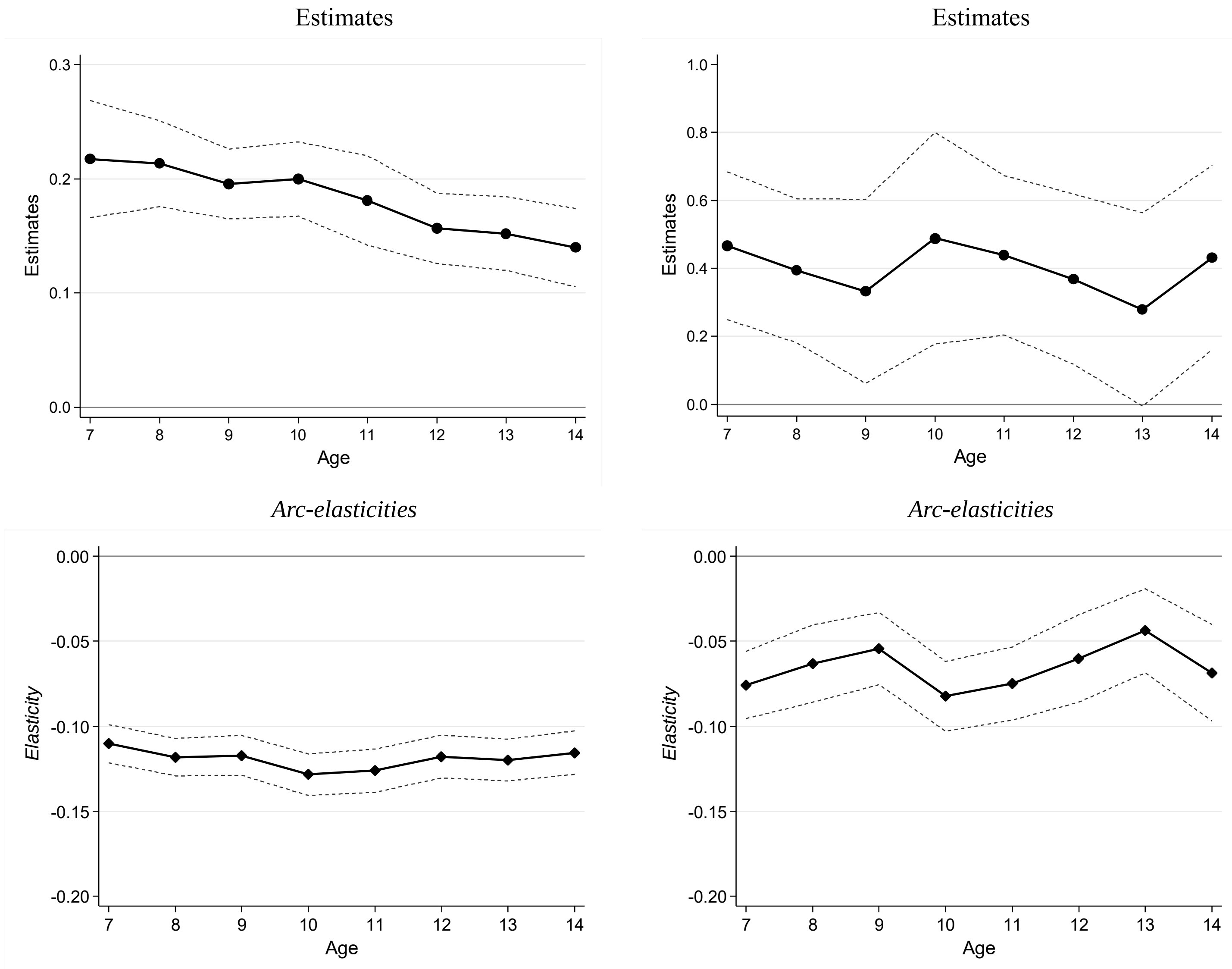
	No exclusion		±1month excluded		±2months excluded		±3months excluded		No exclusion	±1month excluded	±2months excluded	±3months excluded
	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	*Arc-elasticities*			
	(1)		(2)		(3)		(4)		(5)	(6)	(7)	(8)
Subsidized ×												
Age7	1.812***	(0.192)	1.493***	(0.221)	1.376***	(0.236)	1.357***	(0.270)	*-0.158*	*-0.131*	*-0.121*	*-0.117*
Age8	1.705***	(0.164)	1.410***	(0.175)	1.278***	(0.179)	1.249***	(0.199)	*-0.153*	*-0.126*	*-0.115*	*-0.110*
Age9	1.565***	(0.155)	1.243***	(0.163)	1.131***	(0.171)	1.083***	(0.196)	*-0.142*	*-0.112*	*-0.102*	*-0.097*
Age10	1.749***	(0.200)	1.453***	(0.217)	1.350***	(0.221)	1.319***	(0.246)	*-0.160*	*-0.131*	*-0.121*	*-0.118*
Age11	1.502***	(0.192)	1.214***	(0.208)	1.113***	(0.211)	1.062***	(0.229)	*-0.137*	*-0.108*	*-0.099*	*-0.094*
Age12	1.255***	(0.186)	0.970***	(0.201)	0.872***	(0.223)	0.824***	(0.253)	*-0.109*	*-0.083*	*-0.074*	*-0.070*
Age13	1.117***	(0.192)	0.881***	(0.205)	0.811***	(0.218)	0.748***	(0.240)	*-0.094*	*-0.072*	*-0.066*	*-0.061*
Age14	1.341***	(0.112)	1.080***	(0.117)	0.998***	(0.134)	0.899***	(0.166)	*-0.113*	*-0.088*	*-0.080*	*-0.073*
In-kind	-0.385	(0.661)	0.422	(0.354)	0.440	(0.388)	0.470	(0.435)				
Income restriction	-0.564*	(0.317)	-0.553	(0.356)	-0.561	(0.372)	-0.626	(0.401)				
R-squared	0.51		0.51		0.51		0.51					
N	2,303,335		2,253,851		2,205,647		2,158,881					
N of Individual	63,590		63,570		63,530		63,362					

Notes: The outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. The standard errors clustered at the municipality level are reported in parenthesis. Column (1) does not exclude any observations, while Columns (2)–(4) exclude the observations within one, two, and three months from subsidy changes to account for anticipatory utilization. Columns (5)–(8) report the arc-elasticities that corresponds to estimates from Columns (1)–(4). Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Appendix E: Other outcomes

Figure E-1: Other outcomes

A. Frequency of outpatient visits **B. Outpatient spending per visit** (in 1K JPY)



Notes: The frequency of outpatient visits is the number of outpatient visits per month. The outpatient spending per visit measured in thousand JPY (roughly USD10) is the monthly outpatient spending divided by the number of outpatient visits per month. 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 arc-elasticities. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization.

Table E-1: Other outcomes

	A. Frequency of outpatient visits				B. Outpatient spending *per* visit (in 1K JPY)			
	(1)		(2)		(3)		(4)	
	Estimate	(SE)	*Arc-elasticities*	[SE]	Estimate	(SE)	*Arc-elasticities*	[SE]
Subsidized ×								
Age7	0.217***	(0.026)	*-0.110****	[0.006]	0.467***	(0.110)	*-0.076****	[0.010]
Age8	0.213***	(0.019)	*-0.118****	[0.006]	0.393***	(0.107)	*-0.063****	[0.012]
Age9	0.196***	(0.016)	*-0.117****	[0.006]	0.332**	(0.137)	*-0.054****	[0.011]
Age10	0.200***	(0.016)	*-0.128****	[0.006]	0.488***	(0.158)	*-0.082****	[0.010]
Age11	0.181***	(0.020)	*-0.126****	[0.006]	0.438***	(0.119)	*-0.075****	[0.011]
Age12	0.157***	(0.016)	*-0.118****	[0.006]	0.368***	(0.127)	*-0.06****	[0.013]
Age13	0.152***	(0.016)	*-0.120****	[0.006]	0.279*	(0.144)	*-0.044****	[0.013]
Age14	0.140***	(0.017)	*-0.116****	[0.007]	0.431***	(0.137)	*-0.069****	[0.014]
In-kind	0.072	(0.051)			0.141	(0.190)		
Income restriction	-0.083***	(0.026)			-0.141	(0.208)		
R-squared	0.28				0.44			
N	2,205,647				2,205,647			
N of Individual	63,530				63,530			
Mean wo subsidy	0.62				2.59			

Notes: The frequency of outpatient visits is the number of outpatient visits per month. The outpatient spending per visit measured in thousand JPY (roughly USD10) is the monthly outpatient spending divided by the number of outpatient visits per month. All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the arc-elasticities, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Appendix F: By service categories

Given we find the substantial increases in outpatient utilization, it is natural to ask which types of medical services are driving the results. To do so, we group the medical services into six broad categories: medication, consultation fees, laboratory tests, non-surgical procedure, surgical procedure, and others. The last row in Table F-1 shows that the medication is by far the largest share in outpatient spending that accounts more than half (54.1%). Note here that medication includes fees not only for medicine itself but also related to prescribing and dispensing medications, including fees at the pharmacy. Consultation fees (18.4%), laboratory tests (17.2%), and non-surgical procedure (5.3%) are next three categories, and combined with medication, these four categories account for 95% of total outpatient spending. The remaining categories are “others” (3.4%) and “surgical procedure” (1.6%). Thus, in what follows, we focus on these four main service categories.

Figure F-1 plots the event study separately for these four service categories on outpatient spending. As expected, we see the abrupt changes in utilization at the time of subsidy changes for all service categories examined. In addition, as consistent with intuition, the magnitudes of anticipatory spending seem to large in mediation and laboratory tests, suggesting that these medical services can be more easily timed (stockpiled) than other services such as non-surgical procedure.

Figure F-2 provides the graphical presentation of our difference-in-difference estimates from equation [4]. Table F-1 is the corresponding table. The consultation fees—which are charged in each visit and thus is closely related to the frequency—are least price sensitive. On the other hand, the medical services related to the treatment intensity such as laboratory tests and non-surgical procedures are more price sensitive.[1] This result is consistent with our finding that not only the frequency of visits but also the spending per visit increases. Interestingly, the medication is not as price sensitive as other service categories. We also find that brand-name drugs and generics are similarly price sensitive (results available upon request).

References:

Lee, David, and Frank Levy. (2012) “The Sharp Slowdown in Growth of Medical Imaging: An Early Analysis Suggests Combination of Policies Was the Cause.” *Health Affairs* 31(8): 1–9.

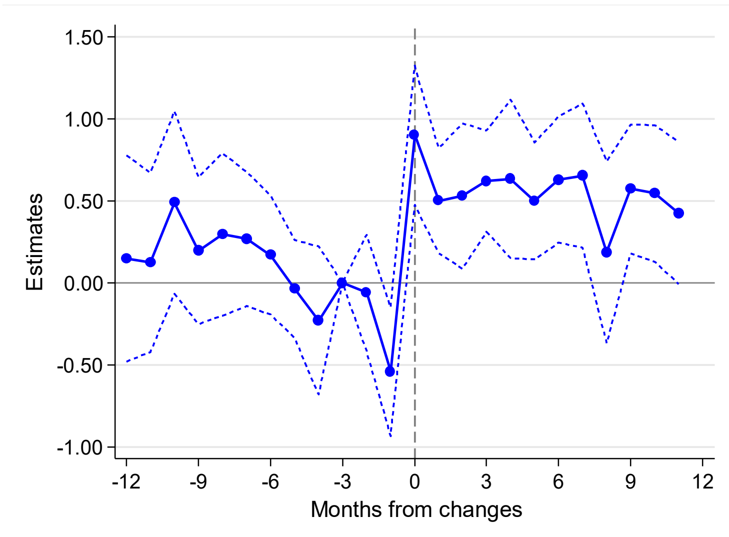
[1] Laboratory test include imaging, which is often identified as having unproven medical value (e.g., Lee and Levy 2012). When imaging is separately examined, we also find the statistically significant increase as well (results available upon request).

Figure F-1: Event study by service categories

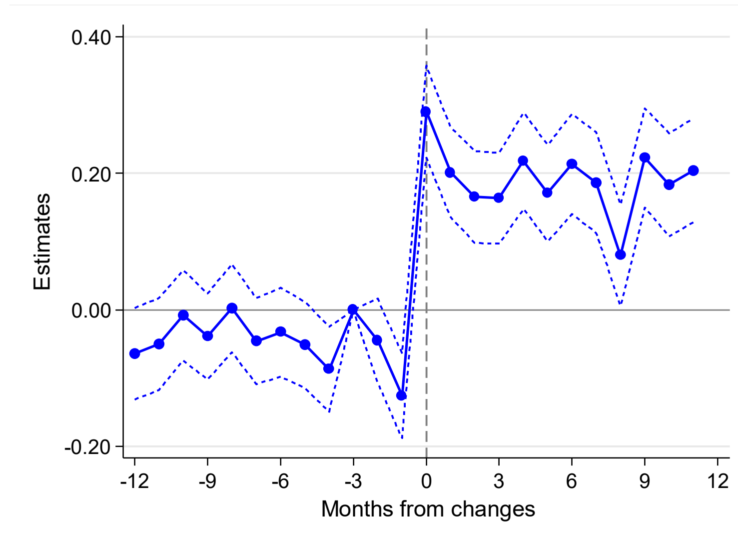
(Outpatient spending (in 1K JPY)

(i) Better (subsidy expansion)

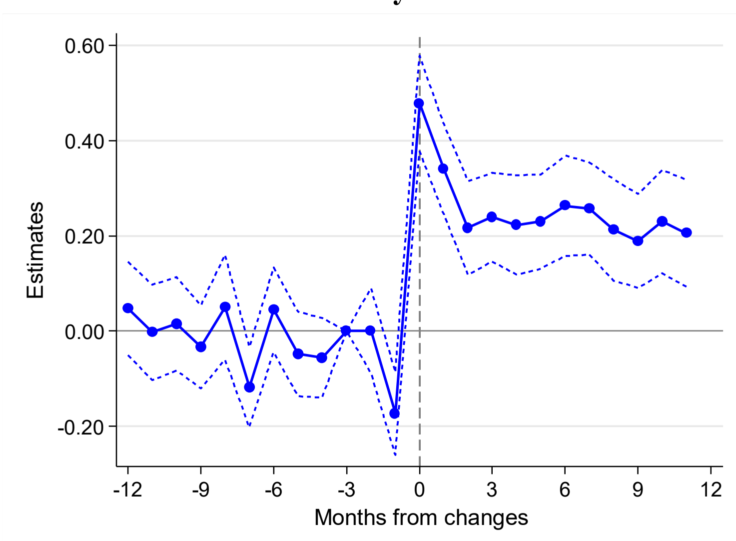
A. Medication



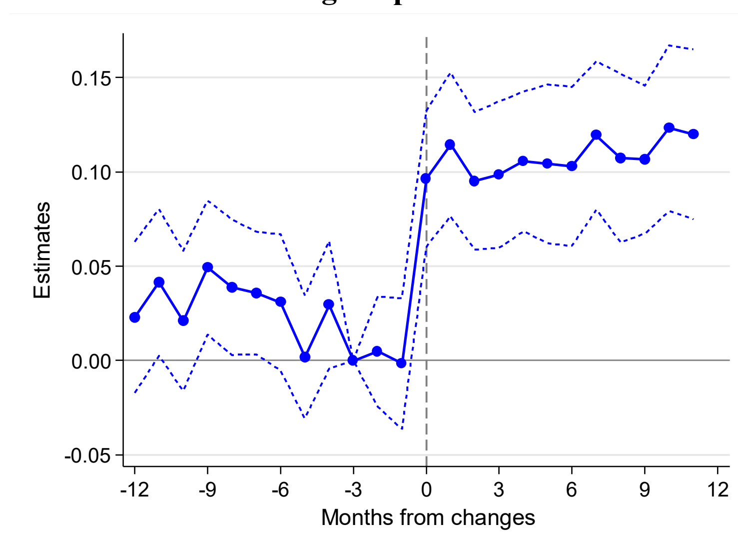
B. Consultation fees



C. Laboratory tests



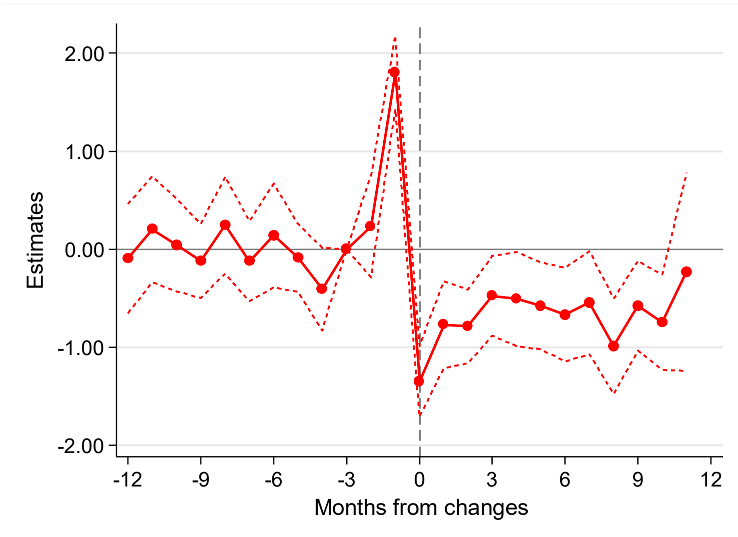
D. Non-surgical procedure



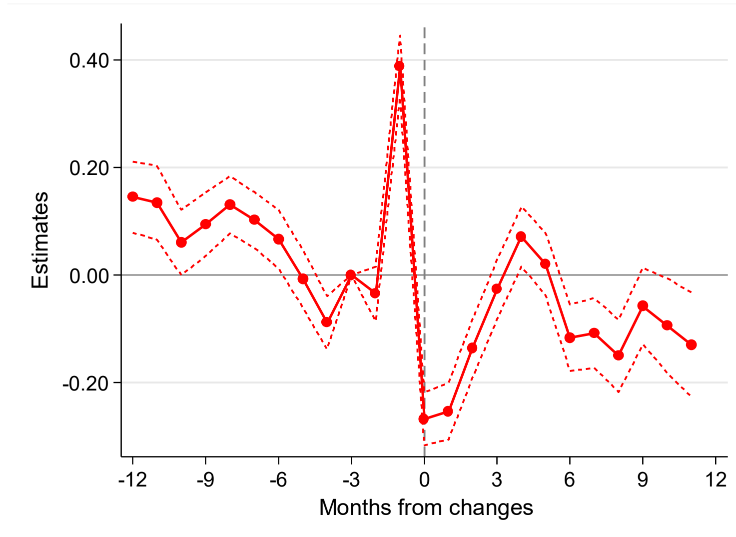
Notes: The outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). "Better" indicates the subsidy expansion which lowers the price of health care from 30% to 0%. The solid lines plot the estimates from a variant of estimation equation [4] where the subsidized dummy is replaced by a series of dummy 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) at age between 12 and 0 months and 12 and 11 months. The dotted lines are the 95th confidence interval derived from standard errors clustered at municipality level. The reference month is 3 months before the subsidy changes (T= –3). The observations within two months from the subsidy changes of the opposite direction are excluded from the sample to account for anticipatory utilization.

(ii) Worse (subsidy expiration)

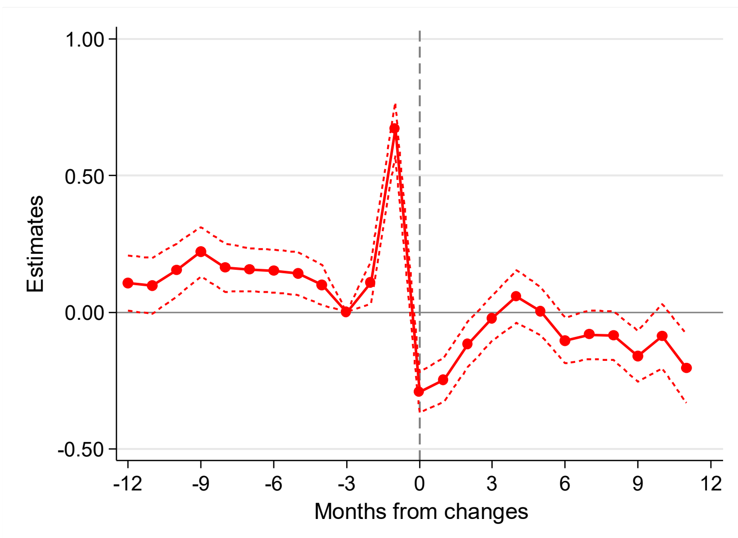
A. Medication



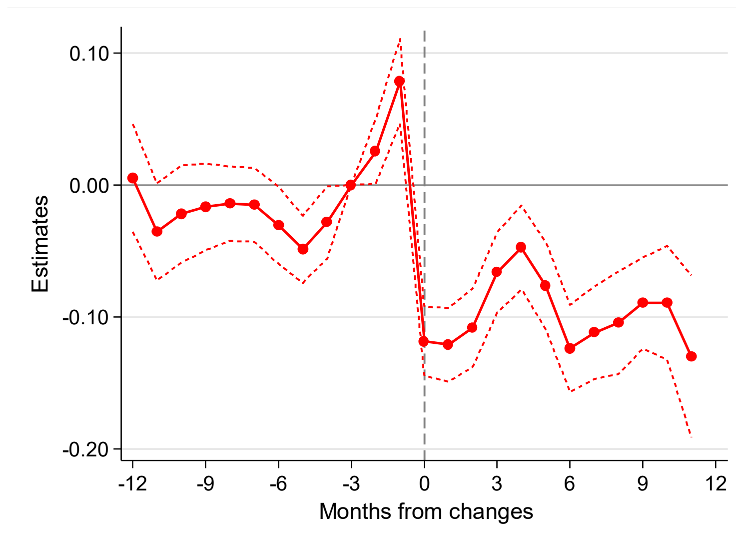
B. Consultation fees



C. Laboratory tests



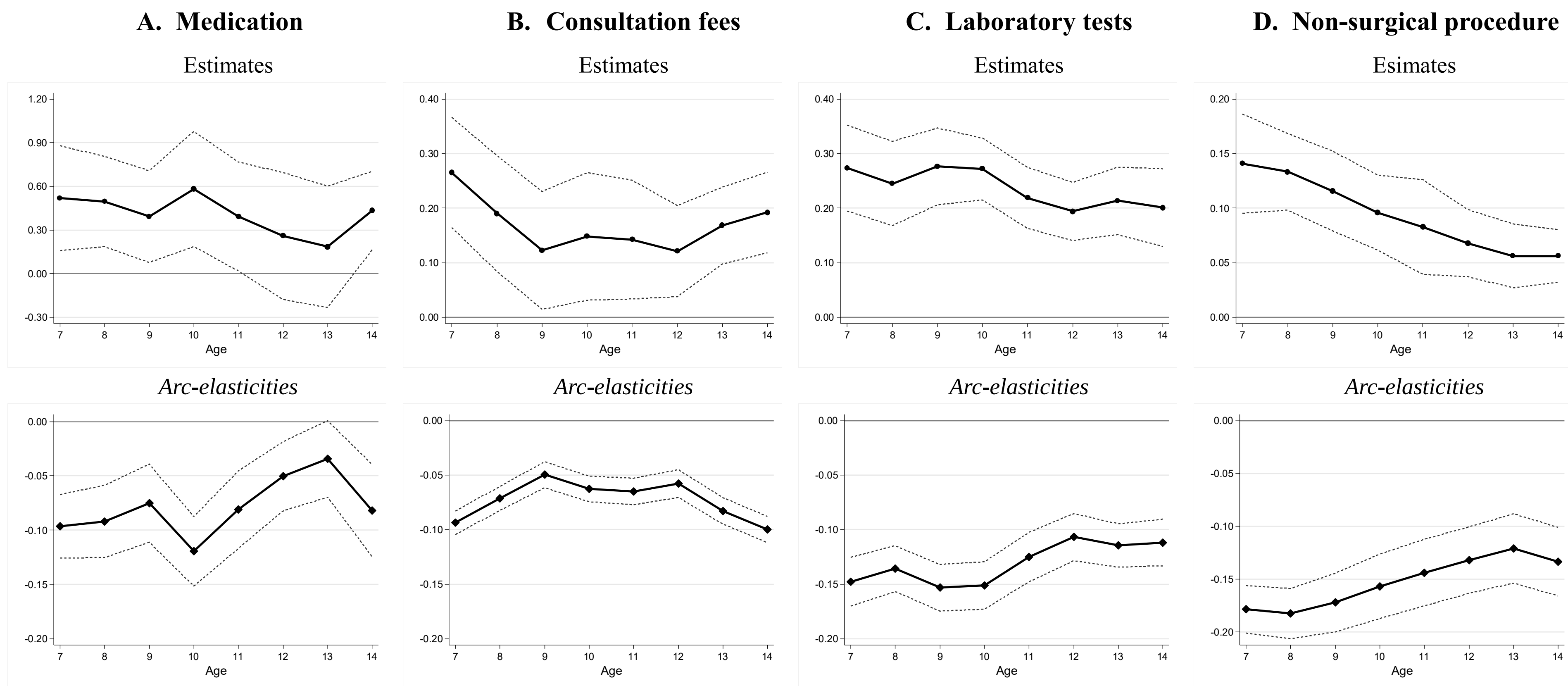
D. Non-surgical procedure



Notes: The outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). "Worse" indicates subsidy expiration that raises the price from 0% to 30%. The solid lines plot the estimates from a variant of estimation equation [4] where the subsidized dummy is replaced by a series of dummy 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) at age between 12 and 0 months and 12 and 11 months. The dotted lines are the 95th confidence interval derived from standard errors clustered at municipality level. The reference month is 3 months before the subsidy changes (T= –3). The observations within two months from the subsidy changes of the opposite direction are excluded from the sample to account for anticipatory utilization.

Figure F-2: By service categories

Outcome: Outpatient spending (in 1K JPY)



Notes: Outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). 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 arc-elasticities. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Note that the scales of *y*-axis on the bottom half are set the same so that elasticities across service categories are visually comparable.

Table F-1: By service categories

Outcome: Outpatient spending per month (in 1K JPY)

	A. Medication		B. Consultation Fees		C. Laboratory tests		D. Non-surgical procedure	
	Estimate	*Arc - elasticity*	Estimate	*Arc - elasticity*	Estimate	*Arc - elasticity*	Estimate	*Arc - elasticity*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subsidized ×								
Age7	0.518^{***}	*-0.096^{***}*	0.265^{***}	*-0.094^{***}*	0.274^{***}	*-0.148^{***}*	0.141^{***}	*-0.178^{***}*
	(0.182)	*[0.015]*	(0.051)	*[0.006]*	(0.040)	*[0.011]*	(0.023)	*[0.011]*
Age8	0.496^{***}	*-0.092^{***}*	0.190^{***}	*-0.071^{***}*	0.245^{***}	*-0.136^{***}*	0.133^{***}	*-0.183^{***}*
	(0.157)	*[0.017]*	(0.054)	*[0.006]*	(0.039)	*[0.011]*	(0.018)	*[0.012]*
Age9	0.392^{**}	*-0.075^{***}*	0.123^{**}	*-0.050^{***}*	0.277^{***}	*-0.153^{***}*	0.116^{***}	*-0.172^{***}*
	(0.160)	*[0.018]*	(0.055)	*[0.006]*	(0.036)	*[0.011]*	(0.019)	*[0.014]*
Age10	0.582^{***}	*-0.119^{***}*	0.148^{**}	*-0.063^{***}*	0.272^{***}	*-0.151^{***}*	0.096^{***}	*-0.157^{***}*
	(0.201)	*[0.016]*	(0.059)	*[0.006]*	(0.029)	*[0.011]*	(0.017)	*[0.016]*
Age11	0.392^{**}	*-0.081^{***}*	0.142^{**}	*-0.065^{***}*	0.219^{***}	*-0.125^{***}*	0.083^{***}	*-0.144^{***}*
	(0.190)	*[0.018]*	(0.055)	*[0.006]*	(0.028)	*[0.012]*	(0.022)	*[0.016]*
Age12	0.258	*-0.050^{***}*	0.121^{***}	*-0.058^{***}*	0.194^{***}	*-0.107^{***}*	0.068^{***}	*-0.132^{***}*
	(0.221)	*[0.016]*	(0.042)	*[0.006]*	(0.027)	*[0.011]*	(0.016)	*[0.016]*
Age13	0.184	*-0.034^{*}*	0.168^{***}	*-0.083^{***}*	0.213^{***}	*-0.115^{***}*	0.056^{***}	*-0.121^{***}*
	(0.211)	*[0.018]*	(0.036)	*[0.006]*	(0.031)	*[0.01]*	(0.015)	*[0.017]*
Age14	0.434^{***}	*-0.082^{***}*	0.192^{***}	*-0.100^{***}*	0.201^{***}	*-0.112^{***}*	0.056^{***}	*-0.133^{***}*
	(0.137)	*[0.022]*	(0.037)	*[0.006]*	(0.036)	*[0.011]*	(0.012)	*[0.017]*
In-kind	0.239		0.152^{**}		0.014		0.068	
	(0.279)		(0.076)		(0.071)		(0.043)	
Income restriction	-0.165		-0.030		-0.123^{***}		-0.103^{***}	
	(0.269)		(0.100)		(0.044)		(0.025)	
R-squared	0.53		0.25		0.12		0.17	
N	2,205,647		2,205,647		2,205,647		2,205,647	
N of Individual	63,530		63,530		63,530		63,530	
Mean wo subsidy	2.13		0.72		0.68		0.21	
Share	54.1%		18.4%		17.2%		5.3%	

Notes: The outpatient spending is total monthly spending on outpatient care measured in thousands JPY (roughly 10USD). All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the arc-elasticities, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. The remaining service categories are "Other" (3.4%), and "Surgery" (1.6%). Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Appendix G: Ambulatory Care Sensitive Conditions (ACSC)

Table G-1: List of ACSC

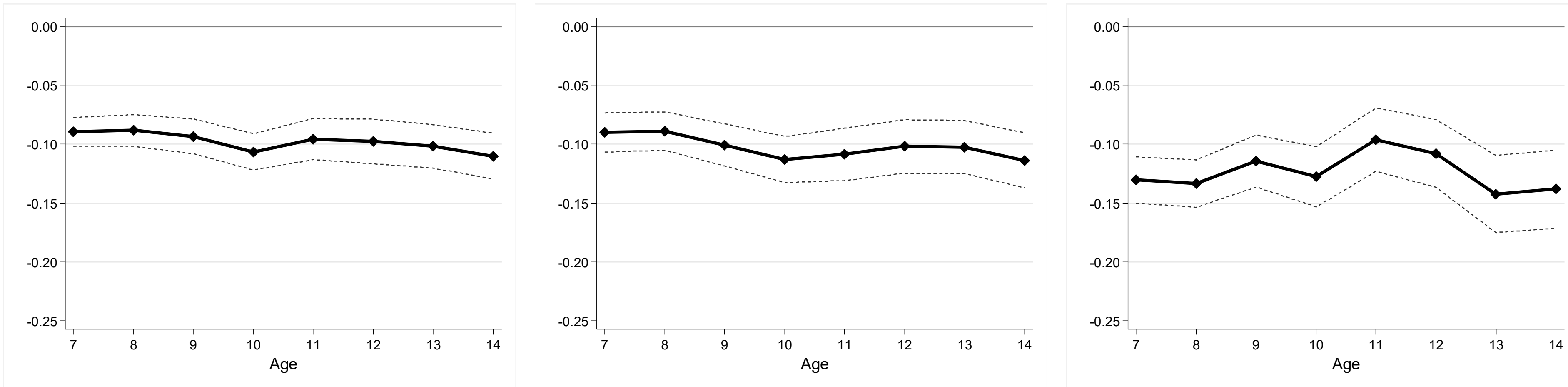
Name of diseases	A dummy for disease		Share among ACSC (%)	ICD-10
	Uncond. Mean	Cond. on visit Mean		
Congenital syphilis	0.000	0.000	0.0%	A50.0-A50.9
Immunization preventable conditions	0.002	0.005	1.0%	A35, A36, A37, A80, G00
Grand mal status and other epileptic convulsions	0.005	0.012	2.4%	G40, G41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

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

Figure G-1: Arc-elasticities for Ambulatory Care Sensitive Conditions (ACSC)

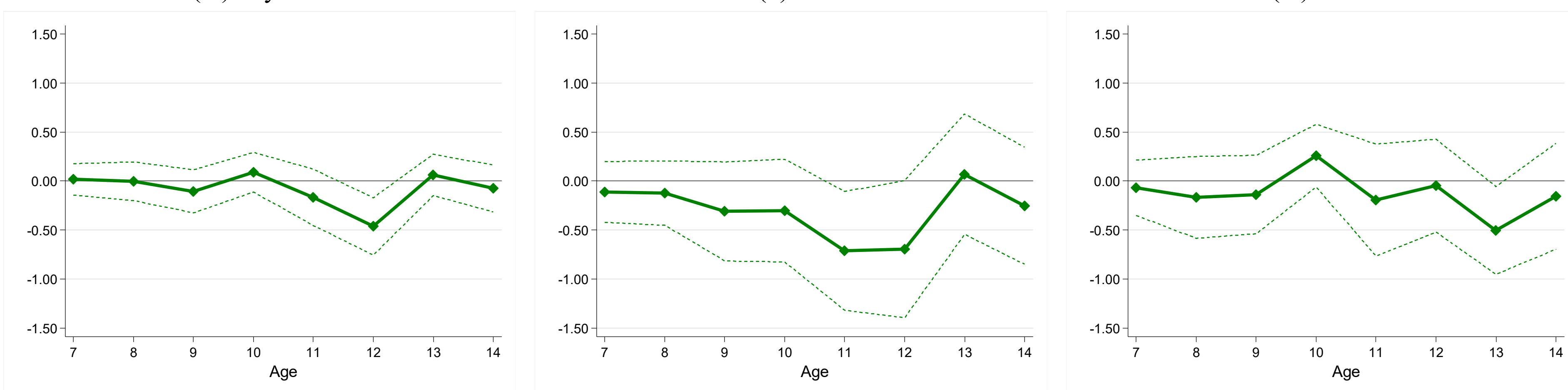
A. *Outpatient* visit dummy

(iv)Any ACSC (v) ENT (vi) Asthma



B. *Inpatient* admission dummy (×1000)

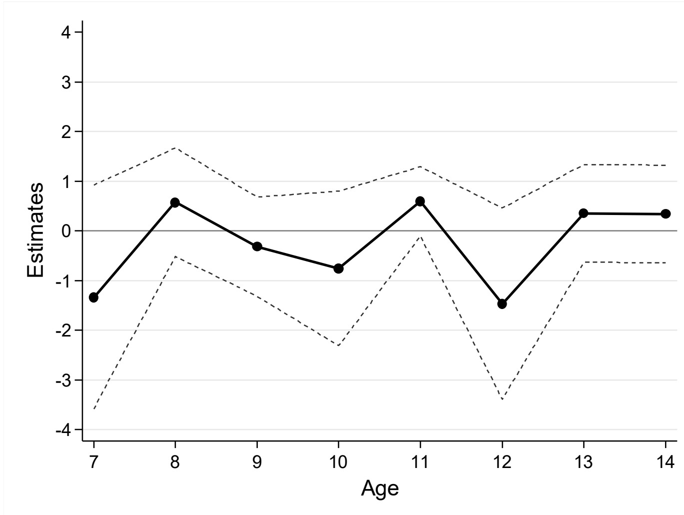
(iv)Any ACSC (v) ENT (vi) Asthma



Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month, and an inpatient admission dummy takes one if there is at least one hospitalization per month (×1000). See Table G-1 for the list of ACSC and summary statistics. The dotted lines are the 95th confidence intervals and bootstrapped standard errors clustered at municipality with 200 repetitions are used to construct them. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. ENT stands for Ear, Nose, and Throat. See Figure 7 in the main text for the corresponding estimates from equation [4].

Appendix H: Child Mortality

Figure H-1: Child Mortality



Notes: The estimates come from complementary log-log regression model where the baseline hazard is the log in age in months. The dotted lines are the 95th confidence intervals derived from standard errors clustered at municipality level. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization.

Table H-1: Child Mortality

Baseline hazard	*Log in age in months*		*Linear in age in months*		*Each dummy for age in years*	
	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)
	(1)		(2)		(3)	
Subsidized ×						
Age7	-1.336	(1.149)	-1.312	(1.132)	-1.433	(1.129)
Age8	0.576	(0.559)	0.613	(0.570)	0.195	(0.628)
Age9	-0.317	(0.510)	-0.275	(0.539)	-0.150	(0.541)
Age10	-0.757	(0.795)	-0.716	(0.799)	0.210	(0.923)
Age11	0.591*	(0.356)	0.624*	(0.364)	0.694	(0.506)
Age12	-1.469	(0.984)	-1.448	(0.984)	-1.582*	(0.916)
Age13	0.348	(0.502)	0.353	(0.504)	0.420	(0.675)
Age14	0.342	(0.500)	0.327	(0.498)	0.568	(0.566)
N	2,205,647		2,205,647		2,205,647	
N of Individual	63,530		63,530		63,530	
N of deaths	68		68		68	

Notes: The estimates come from complementary log-log regression models with different baseline hazard indicated at the first rows. The standard errors clustered at the municipality level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10

Appendix I: By time of visits

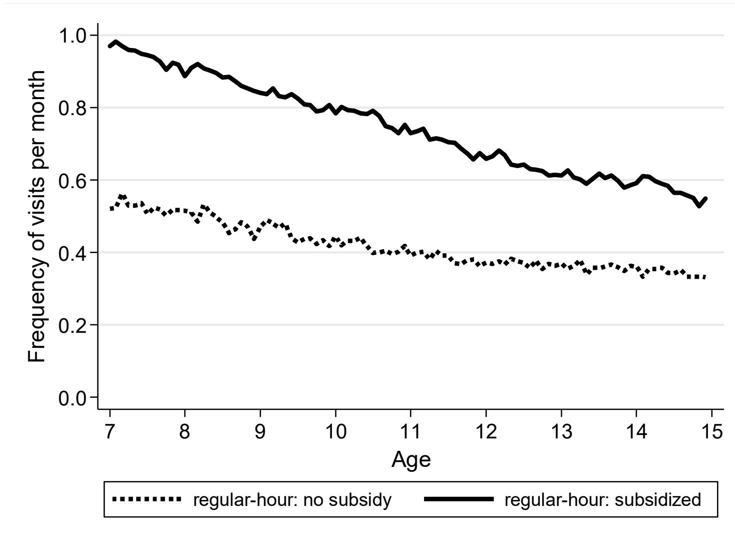
Table I-1: List of billing codes for off-hour and midnight/holiday visits and corresponding additional fees charged

Billing code #	Type of visit	Timing of visit	Additional fee charged (in 1K JPY)	Number of times charged (frequency)
Off-hour visits				
111000570	first visit	off-hour	0.85	4,329
111000870	first visit	off-hour (*)	2.30	5,527
111012470	first visit	night/early morning	0.50	53,083
111700870	first visit	off-hour (*)	2.30	5
112001110	revisit	off-hour	0.65	2,411
112001410	revisit	off-hour (*)	1.80	70
112006470	revisit	off-hour	0.65	256
112006770	revisit	off-hour (*)	1.80	1,986
112015570	revisit	night/early morning	0.50	62,423
Midnight/Holiday visits				
111000670	first visit	holiday	2.50	16,406
111000770	first visit	midnight	4.80	2,343
112001210	revisit	holiday	1.90	930
112001310	revisit	midnight	4.20	59
112006570	revisit	holiday	1.90	1,143
112006670	revisit	midnight	4.20	506

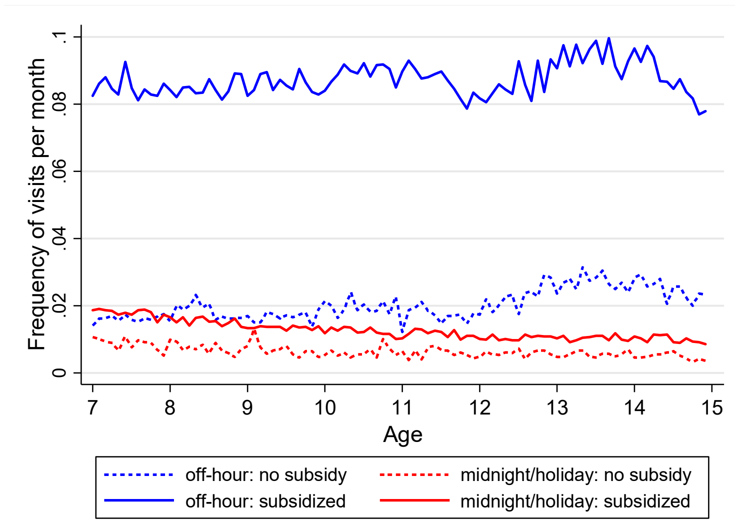
Notes: The fees listed on the far right are the fees charged additionally to the fees for regular-hour visits. As a benchmark, the fees for regular-hour visits during the sample period are roughly 2.8 for first visit, and 0.7 for revisits measured in thousands JPY (roughly USD10). (*) is applied to specific medical institutions. The unit of additional fee is in thousand JPY.
Source: From Japan Federation of Democratic Medical Institutions (2015)

Figure I-1: Age profiles of frequency of visits by time of visits and subsidy status

A. Regular-hour visits



B. Off-hour and midnight/holiday visits

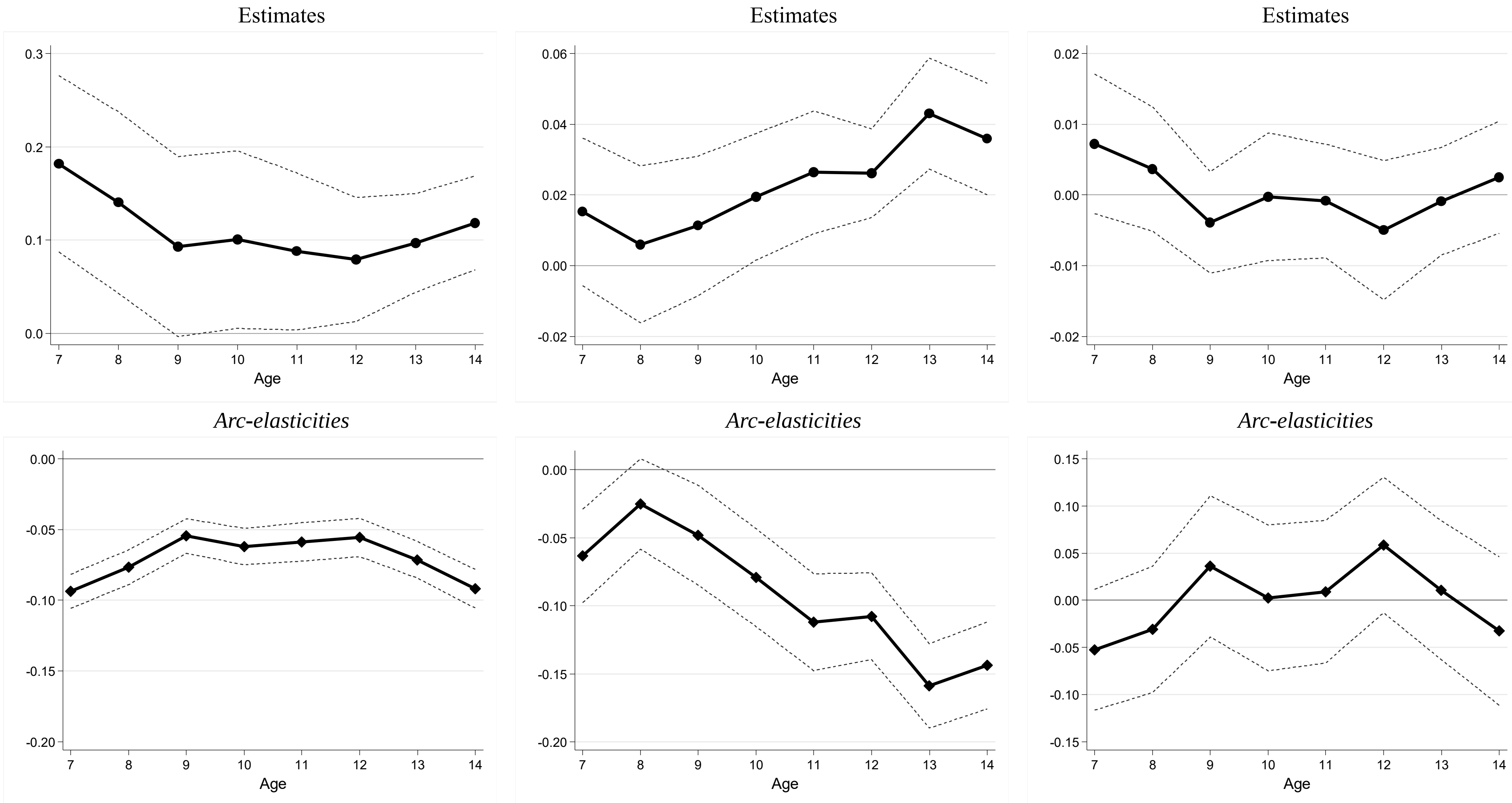


Notes: Panel A plots the monthly frequency of visits for regular-hour visits, and Panel B plots the monthly frequency of visits for off-hour and midnight/holiday visits. The dotted lines are age profiles of utilization without subsidy (labeled "no subsidy"), and the solid lines are age profiles of utilization with subsidy (labeled "subsidized").

Figure I-2: By time of visits

Outcome: Outpatient spending (in 1K JPY)

A. Regular-hour visits **B. Off-hour visits** **C. Midnight/Holiday visits**



Notes: The outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). See Table H-1 which provides the list of billing codes for these urgent visits and corresponding fees that are additionally charged on top of fees for regular-hour visits. 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 arc-elasticities. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization.

Table I-2: By time of visits

Outcome: Outpatient spending (in 1K JPY)

	A. Regular-hour visits				B. Off-hour visits				C. Midnight/Holiday visits			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Estimate	(SE)	*Arc-elasticity*	[SE]	Estimate	(SE)	*Arc-elasticity*	[SE]	Estimate	(SE)	*Arc-elasticity*	[SE]
Subsidized ×												
Age7	0.182***	(0.048)	*-0.094****	[0.006]	0.015	(0.011)	*-0.063****	[0.018]	0.007	(0.005)	*-0.053*	[0.033]
Age8	0.140***	(0.049)	*-0.077****	[0.006]	0.006	(0.011)	*-0.025*	[0.017]	0.004	(0.004)	*-0.031*	[0.034]
Age9	0.093*	(0.049)	*-0.055****	[0.006]	0.011	(0.010)	*-0.048***	[0.019]	-0.004	(0.004)	*0.036*	[0.038]
Age10	0.101**	(0.048)	*-0.062****	[0.007]	0.019**	(0.009)	*-0.079****	[0.018]	-0.000	(0.005)	*0.003*	[0.039]
Age11	0.088**	(0.043)	*-0.059****	[0.007]	0.026***	(0.009)	*-0.112****	[0.018]	-0.001	(0.004)	*0.009*	[0.039]
Age12	0.079**	(0.034)	*-0.056****	[0.007]	0.026***	(0.006)	*-0.108****	[0.016]	-0.005	(0.005)	*0.059*	[0.037]
Age13	0.097***	(0.027)	*-0.071****	[0.007]	0.043***	(0.008)	*-0.159****	[0.016]	-0.001	(0.004)	*0.011*	[0.038]
Age14	0.118***	(0.025)	*-0.092****	[0.007]	0.036***	(0.008)	*-0.144****	[0.016]	0.003	(0.004)	*-0.033*	[0.040]
In-kind	0.080	(0.057)			0.052	(0.032)			-0.004	(0.019)		
Income restriction	-0.029	(0.055)			-0.008	(0.020)			-0.002	(0.009)		
R-squared	0.21				0.10				0.04			
N	2,205,647				2,205,647				2,205,647			
N of Individual	63,530				63,530				63,530			
Mean wo subsidy	0.509				0.057				0.032			
Share	85.1%				9.5%				5.4%			

Notes: The outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). All the regressions include age (in months) FE, time (in month) FE, and individual FE. In-kind is a dummy that takes one if the municipality offers the subsidy in the form of in-kind instead of refund, and income restriction is a dummy that takes one if the municipality imposes income restriction for subsidy eligibility. For the estimates, the standard errors clustered at the municipality level are reported in parenthesis. For the semi point-elasticity, the bootstrapped standard errors with 200 repetitions clustered at municipality level are reported in brackets. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Appendix J: Inappropriate use of antibiotics

Fleming-Dutra *et al.* (2016) divide the diagnoses (with corresponding ICD9-CM) into three tiers by the degree of appropriateness of antibiotics use. Specifically, Tiers 1, 2, and 3 are diagnostic categories where antibiotic use is always indicated, is occasionally indicated, and not indicated at all, respectively. For example, antibiotic prescription is considered appropriate for pneumonia because the diagnosis almost always warrants antibiotic therapy (Tier 1), while antibiotics for bronchitis and asthma are considered inappropriate because children with these conditions should not receive antibiotics (Tier 3).

In this paper, we focus on Tier 3 children who should not receive the antibiotics. When a patient has multiple diagnoses in a month, a priority is given to Tier 1 diagnoses, then Tier 2 diagnoses, then finally Tier 3 diagnoses so that a patient at each month is assigned to mutually exclusive tiers. Specifically, we assign a patient to Tier 1 when the patient has any diagnosis in Tier 1 in the month and to Tier 2 when the patient has any diagnosis in Tier 2 but not Tier 1, and the rest to Tier 3. In this way, Tier 3 only includes the patients for whom antibiotics should not be recommended at all since none of the diagnoses include the ones from Tier 1 and Tier 2. Table I-1 presents the list of Tier 3 diagnoses with corresponding ICD10 (which we convert from ICD9-CM) as well as summary statistics of antibiotic usage. The summary statistics for Tier and Tier 2 children are available upon request.

Table J-1: Summary statistics of inappropriate antibiotic use

Name of diagnosis	ICD 10	Fraction of the diagnosis (1)	Unconditional: Antibiotics use (dummy) (2)	Unconditional: Spending on antibiotics (in 1K JPY) (3)	Unconditional: Freq. of antibiotics prescriptions (4)	Conditional on having the diagnosis: Antibiotics use (dummy) (5) = (2)/(1)	Conditional on having the diagnosis: Spending on antibiotics (in 1K JPY) (6) = (3)/(1)	Conditional on having the diagnosis: Freq. of antibiotics prescriptions (7) = (4)/(1)
All		**0.218**	**0.041**	**0.053**	**0.205**	**0.19**	**0.24**	**0.94**
By diagnosis:								
Asthma, allergy	J30, J44, J45, T784	0.058	0.015	0.021	0.084	0.27	0.36	1.45
Bronchitis, bronchiolitis	J20, J21, J40	0.035	0.023	0.032	0.118	0.66	0.90	3.38
Influenza	J09, J10, J11	0.019	0.007	0.009	0.034	0.37	0.48	1.80
Non-suppurative otitis media	H65, H68, H69	0.002	0.001	0.001	0.004	0.27	0.38	1.73
Viral pneumonia	J12	0.000	0.000	0.000	0.000	0.33	0.50	2.34
Viral upper respiratory infection	J00, J04, J05, J06, R05	0.033	0.017	0.022	0.087	0.53	0.67	2.65
Other respiratory conditions	All remaining respiratory conditions (J00-J99) not coded above and R060-R064, R068-R069, R042, R048, R049, R093	0.000	0.000	0.000	0.000	0.26	0.30	1.78
All other codes not listed elsewhere	All other codes not listed elsewhere	0.168	0.019	0.024	0.095	0.11	0.14	0.56

Notes: The spending on antibiotics is measured in thousand JPY (roughly USD10). The list of ICD10 codes comes from Fleming-Dutra *et al.* (2016) eTable “2. Diagnostic categories by tier with corresponding ICD-9CM code”.

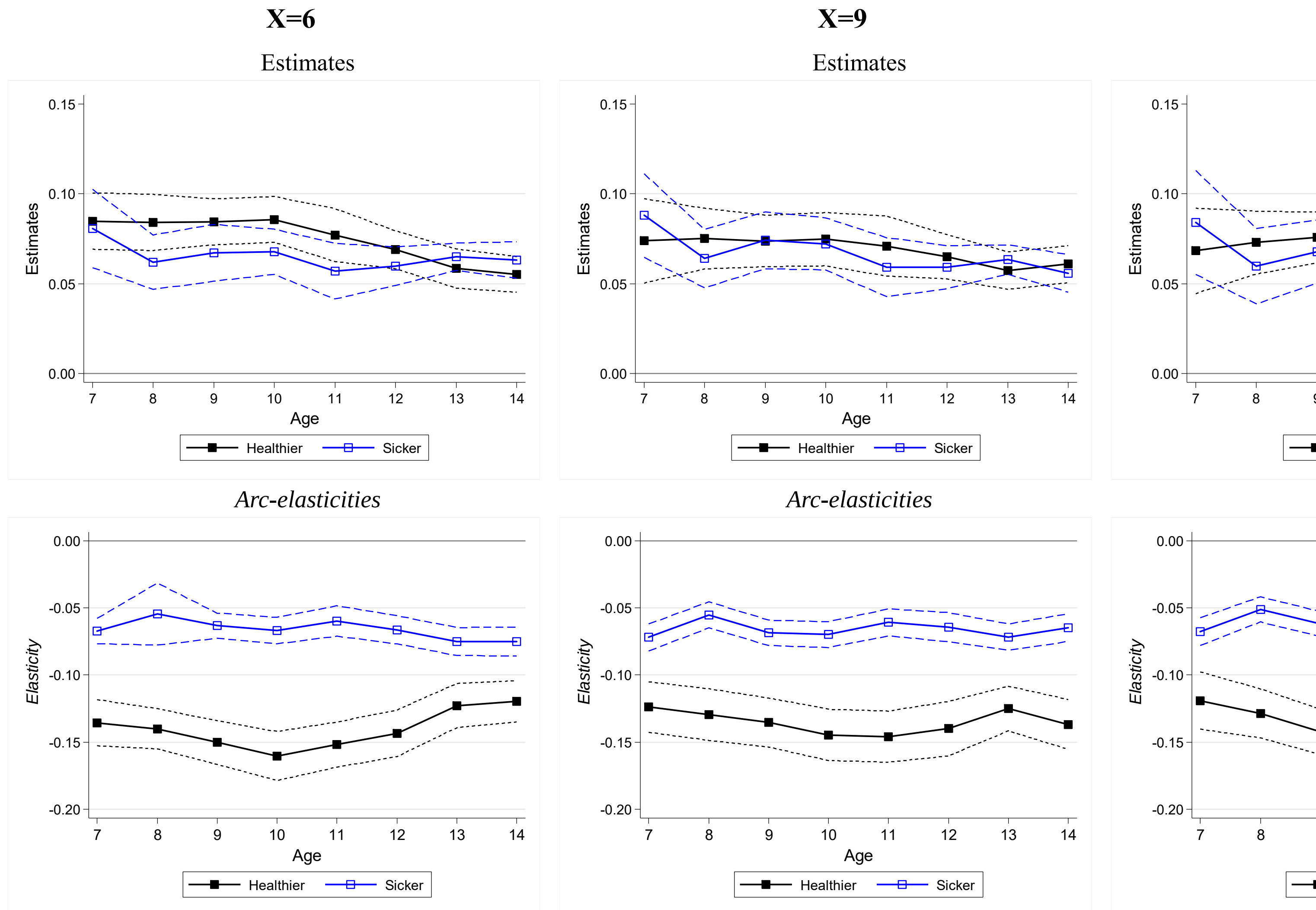
Appendix K: Price responsiveness by health status

In the main text, we determine each child's health status by the outpatient spending in the first 6 months since the child is observed in the claims data. Then, we divide children into two types (i.e., sicker or healthier) by the median spending for each age (in years) and the subsidy status at the first entry to data. There are two main complications in defining the patient health status by using the initial spending. First, each child shows up in the claims data at different ages. Second, the subsidy status may change during these months. To avoid the second issue, we focus on the individuals whose subsidy status does not change during the spell. For the first issue, we calculate the average spending separately for each age and subsidy status combination (10 years of ages groups×2 subsidy status). Then, we define those above the median of corresponding age and subsidy status as "sick" and those less than median as "healthy".

We also experiment with different windows (X= 9, and 12) to calculate the average spending. The benefit of taking longer spell is that we may be able to capture the health status with more accuracy while the cost is that we may lose more observations as we impose the restriction that the subsidy status does not change during the spell (at X=6, we still maintain 90% of the total observations). Figure K-1 shows that the arc-elasticities are qualitatively similar across different Xs.

Figure K-1: Price responsiveness by health status

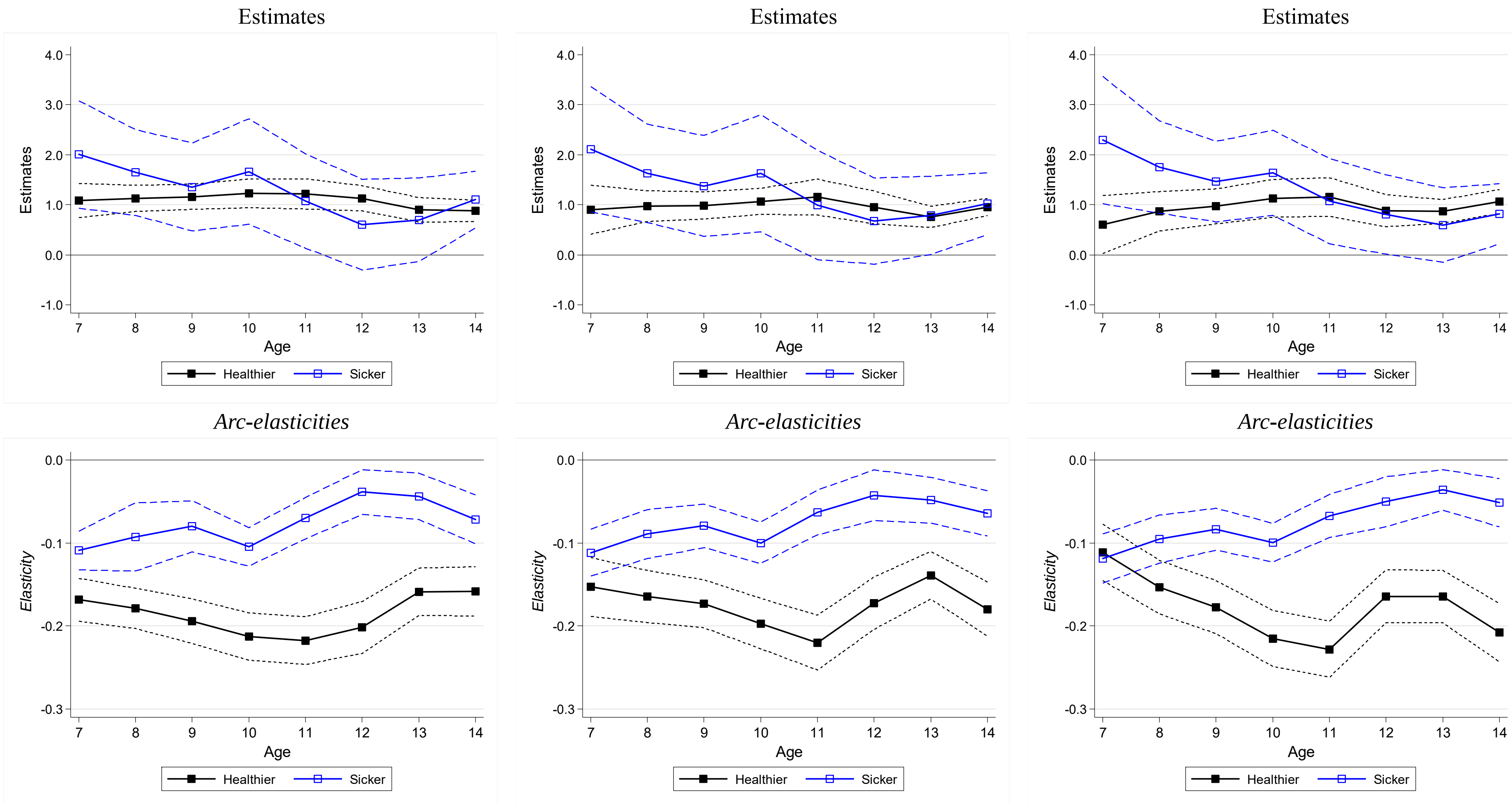
(i) Outpatient visit dummy



Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month. We determine each child's health status b first X months (X= 6, 9 and 12) since one is observed in the claim data. Then, we divide children into two types (i.e., sicker or health each age (in years) and the subsidy status. The standard errors clustered at municipality level are used for estimates, and the bootstrap municipality with 200 repetitions are used for the arc-elasticities. The observations within two months from subsidy changes are exclu for anticipatory utilization. Note that the scales of y-axis on the arc-elasticities are set the same so that they are visually comparable.

(ii) Outpatient spending (in 1K JPY)

X=6 **X=9** **X=12**



Notes: The outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). We determine each child's health status by the outpatient spending in the first *X* months (*X*= 6, 9 and 12) since one is observed in the claim data. Then, we divide children into two types (i.e., sicker or healthier) by the median spending for each age (in years) and the subsidy status. 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 arc-elasticities. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Note that the scales of *y*-axis on the arc-elasticities are set the same so that they are visually comparable.

Appendix L: Inter-municipality migration

In the main text, we focus on the children who do not move across municipalities as there are only 1,079 such children which account for only 1.7% of total children (63,530 vs. 64,609). The migration rate in our sample is lower than the actual migration since intra-municipality migration is not counted as migration since the subsidy level is the same. However, if a family with very sick children is more likely to move to more generous municipality, our estimates —which may fail to control for the time-varying unobserved health conditions—can be potentially biased. We think that this is very unlikely for a couple of reasons. First, the migration rate is declining function of age of children and is already low by age 7 as parents tend to move before their children enter primary school. Second, there may be many other municipality characteristics than subsidy generosity for child health care that may affect the decision of migration such as other childrearing support, availability of daycare, and quality of school in the districts. Nonetheless, we include those who move across municipalities into the sample and estimate the equation [4]. Figure K-1 compares the estimates with and without movers. It is reassuring that estimates are very similar.

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

$$Pr(Y_{iat} = m) = F(\textstyle\sum_{A=7}^{14} \beta_A \{subsidized_{amt} \times 1(Age\ A)\} + \delta_m + \varepsilon_{iat}) \quad \text{–[L1]}$$

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

For the second question, we further interact the series of subsidy dummies with the proxy for health status—the average outpatient spending for six months just before the month of move (denoted by $prior\ spending_{iat-1}$ below)[2]:

$$Pr(Y_{iat} = m) = F(\textstyle\sum_{A=7}^{14} \beta_A \{subsidized_{amt} \times 1(Age\ A)\} + \sum_{A=7}^{14} \gamma_A \{subsidized_{amt} \times 1(Age\ A) \times prior\ spending_{iat-1}\} + \delta_m + \varepsilon_{iat}) \quad \text{–[L2]}$$

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

The graph on the left in Figure K-2 demonstrates the graphical presentation of estimating equation [L1] which plots β_A for each age (A=7–14). Even though the estimates are quite noisy, β_A are mostly negative and are not

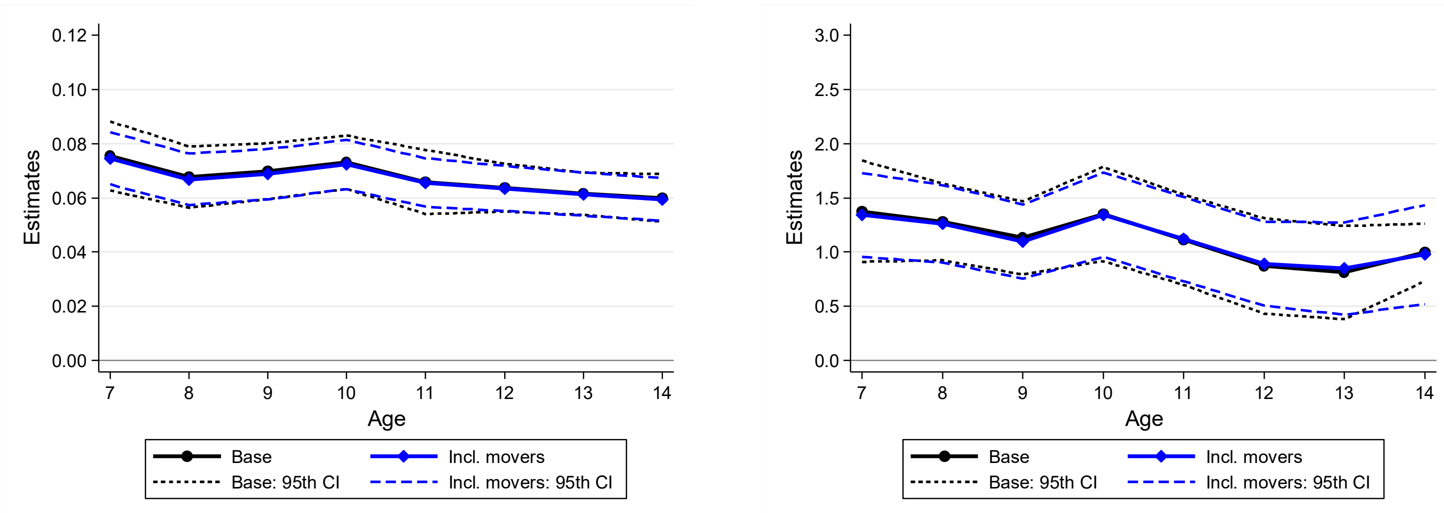
[2] We experiment the length of prior months to calculate the average prior spending from X months (X= 3,6,9 and 12) but the estimates are very similar. The benefit of taking longer span to compute the average spending is that we may be able to capture the health status with more accuracy while the cost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

statistically significant at the conventional level (See Table K-2 for the estimates). Thus, these results at least do not support that children are more likely to choose the municipality with the subsidy for her/his age.

The graph on the right in Figure K-2 demonstrates the graphical presentation of estimating equation [L2] which plots β_A in the upper half, and γ_A in the lower half. Again, β_A are not statistically significant and mostly negative. Furthermore, γ_A are close to zero, and far from statistically significant, suggesting that sickly children are no more likely to choose the municipality with subsidy. Taken together, we do not find any evidence of selective inter-municipality migration at least in the current setting.

Figure L-1: Baseline vs. including movers

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

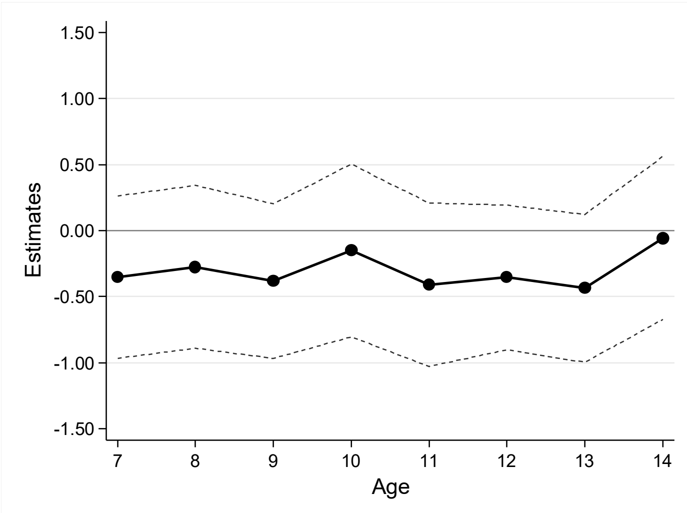


Notes: An outpatient visit dummy takes one if there is at least one outpatient visit per month, and outpatient spending is the monthly spending on outpatient care measured in thousand JPY (roughly USD10). The dotted lines are the 95th confidence intervals derived from standard errors clustered at individual level. The observations within two months from subsidy changes are excluded from the sample to account for anticipatory utilization. Along with our baseline estimates, we report estimates from the sample that include inter-municipality movers (1.7%).

Figure L-2: Selective inter-municipality migration

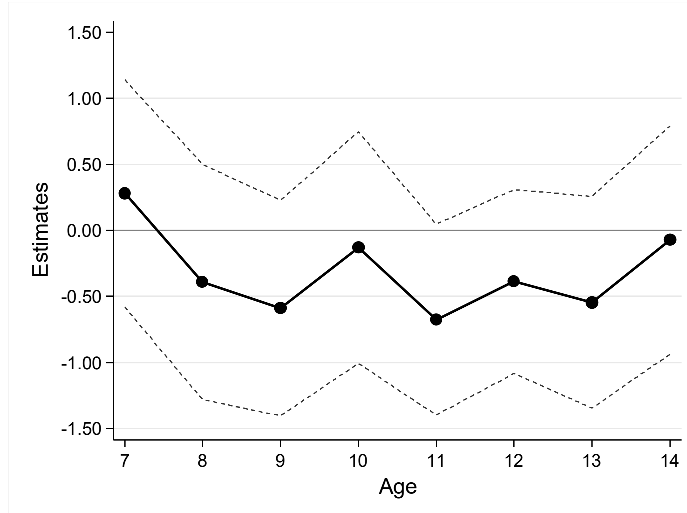
Equation [L1]

Plot of β_A

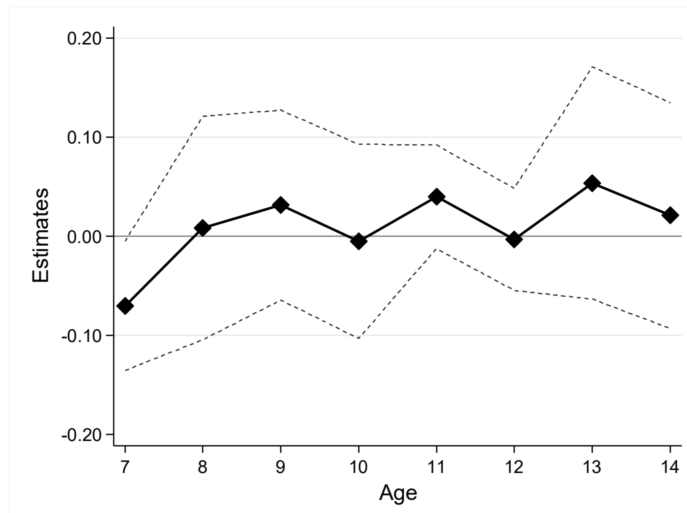


Equation [L2]

Plot of β_A



Plot of γ_A



Notes: The estimates from conditional logit model are plotted. The dotted lines are the 95th confidence intervals derived from standard errors clustered at individual level. The graph on the left plots β_A from estimating equation [L1] while the graphs on the right plot β_A in the upper half, and γ_A in the lower half from estimating equation [L2].

Table L-1: Base vs. including movers

	A. Outpatient visit dummy				B. Outpatient spending (in 1K JPY)			
	Baseline (Col. 3 Table 2)		Including movers		Baseline (Col. 3 Table 2)		Including movers	
	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)
	(1)		(2)		(3)		(4)	
Subsidized ×								
Age7	0.075***	(0.006)	0.074***	(0.005)	1.376***	(0.236)	1.351***	(0.196)
Age8	0.068***	(0.006)	0.067***	(0.005)	1.278***	(0.179)	1.257***	(0.181)
Age9	0.070***	(0.005)	0.069***	(0.005)	1.131***	(0.171)	1.092***	(0.174)
Age10	0.073***	(0.005)	0.072***	(0.005)	1.350***	(0.221)	1.340***	(0.197)
Age11	0.066***	(0.006)	0.065***	(0.005)	1.113***	(0.211)	1.107***	(0.197)
Age12	0.064***	(0.004)	0.064***	(0.004)	0.872***	(0.223)	0.895***	(0.196)
Age13	0.062***	(0.004)	0.062***	(0.004)	0.811***	(0.218)	0.857***	(0.216)
Age14	0.060***	(0.004)	0.059***	(0.004)	0.998***	(0.134)	0.972***	(0.230)
In-kind	0.047***	(0.014)	0.050***	(0.018)	0.440	(0.388)	0.532	(0.326)
Income restriction	-0.020**	(0.009)	-0.021***	(0.008)	-0.561	(0.372)	-0.705**	(0.298)
R-squared	0.23		0.23		0.51		0.51	
N	2,205,647		2,252,600		2,205,647		2,252,600	
N of Individual	63,530		64,609		63,530		64,609	
Mean wo subsidy	0.32		0.32		4.49		4.49	

Notes: An outpatient visit 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 (roughly USD10). For ease of comparison, Columns (1) and (3) replicate the estimates from Columns (1) and (3) in Table 2. Columns (2) and (4) report estimates from the sample that include inter-municipality movers (1.7%). The standard errors clustered at the municipality level are reported in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table L-2: Selective inter-municipality migration

	No interaction with prior spending	Interaction with prior spending
	(1)	(2)
Subsidized ×		
Age7	-0.352	0.281
	(0.313)	(0.439)
Age8	-0.273	-0.391
	(0.315)	(0.455)
Age9	-0.382	-0.587
	(0.298)	(0.417)
Age10	-0.150	-0.131
	(0.334)	(0.448)
Age11	-0.410	-0.675*
	(0.316)	(0.369)
Age12	-0.354	-0.388
	(0.279)	(0.355)
Age13	-0.437	-0.544
	(0.285)	(0.409)
Age14	-0.055	-0.074
	(0.315)	(0.440)
Age7 × Prior spending		-0.070**
		(0.033)
Age8 × Prior spending		0.008
		(0.058)
Age9 × Prior spending		0.032
		(0.049)
Age10 × Prior spending		-0.005
		(0.050)
Age11 × Prior spending		0.040
		(0.027)
Age12 × Prior spending		-0.003
		(0.026)
Age13 × Prior spending		0.054
		(0.060)
Age14 × Prior spending		0.021
		(0.058)
In-kind	2.597***	-0.891
	(0.451)	(0.755)
Income restriction	-0.484***	-0.188
	(0.164)	(0.318)
N	161,703	126,344
N of move	1,179	920
N of Individual	1,052	816

Notes: The estimates from conditional logit model are reported. Column (1) reports the results from estimating equation [L1] while column (2) results are from estimating equation [L2]. The standard errors clustered at individual level are reported in parentheses. "Prior spending" is the average outpatient spending for six months just before the month of move. The sample is limited to a month when children moves across municipalities. The number of moves is slightly larger here as the observations within two months from subsidy changes are not excluded.