BMJ Open

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Journal:	*BMJ Open*
Manuscript ID	bmjopen-2020-044549
Article Type:	Original research
Date Submitted by the Author:	11-Sep-2020
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Keywords:	Health policy < HEALTH SERVICES ADMINISTRATION & MANAGEMENT, HEALTH ECONOMICS, Quality in health care < HEALTH SERVICES ADMINISTRATION & MANAGEMENT

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Impact of Differential Copayment on Patient Healthcare Choice: Evidence from South Korean National Cohort Study

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Competing Interests Statement
The authors have no affiliations with or involvement in any organization or entity with any financial interest, or non-financial interest in the subject matter or materials discussed in this manuscript.

Word counts: 4,141

Acknowledgements
This work was supported by the Ministry of Science and ICT, Republic of Korea (NRF-2019R1F1A1055160) and SNU Business School, Seoul National University.

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Abstract

Objective We evaluate the impact of mild disease differential copayment policy aimed at reducing unnecessary patient visits to secondary/tertiary healthcare institutions in South Korea.

Design Retrospective study using difference-in-difference design

Setting Sample Research database provided by the Korean National Health Insurance Service, between 2010 and 2013.

Participants 206,947 patients who visited healthcare institutions to treat mild diseases during the sample period.

Methods A linear probability model with difference-in-difference approach was adopted to estimate the impact of differential copayment policy on patients' healthcare choices. The dependent variable was a binary variable denoting whether a patient visited primary healthcare or secondary/tertiary healthcare to treat her/his mild disease. Patients' individual characteristics were controlled with a fixed effect.

Results The policy significantly decreased the proportion of patients choosing secondary/tertiary healthcare over primary healthcare by 2.99 percent point. The policy effect was weaker by 14% in the low-income group compared to richer population, greater by 19% among the residents of Seoul metropolitan area than among people living elsewhere, and stronger among frequent healthcare visitors by 33% than among people who less frequently visit healthcare.

Conclusion The mild disease differential copayment policy of South Korea was successful in reducing unnecessary visits to secondary/tertiary healthcare institutions to treat mild diseases that can be treated well in primary healthcare.

Keywords: Differential copayment, Healthcare choice, Mild disease, Difference-in-difference, Primary healthcare

Strengths and limitations of this study

- The control group of this study provided the ideal counterfactual benchmark to precisely identify the policy's impact.
- Since the policy of this study pertains to mild diseases only, we could avoid the omitted variable problem due to unobserved disease severity.
- This study limited the subjects to similar pairs of mild diseases to construct a comparable control–treatment group setting.

INTRODUCTION

Excess demand for secondary and tertiary hospitals is a major healthcare challenge in many countries (e.g., China, Australia), resulting in overcrowding, safety, and inefficiency issues in public health.[1–3] The South Korean government has also recognized it as a major problem and taken steps to address it.[4–6] In 2011, the number of secondary/tertiary healthcare facilities was 319 (1.1%) and that of primary healthcare was 30,197 (98.9%). However, 4.7% of total patient visits to treat mild diseases were at secondary/tertiary facilities. Most of mild diseases can be treated well in primary healthcare. Nevertheless, substantial proportion of mild disease patients visit secondary/tertiary hospitals.[7] As patient visits to treat mild diseases increase, secondary/tertiary healthcare needs to allocate more resources to meet the demand, generating the inefficiency in attaining its main goal; to focus on severe or complicated cases.[9,10] Lee et al. reported that among the outpatient usage of secondary/tertiary hospitals, approximately 85% can be sufficiently treated in primary healthcare.[4]

A frequently used policy to tackle the overcrowding problems in secondary and tertiary healthcare by governments is strengthening the gatekeeping role of the primary healthcare sector.[11,12] In many countries (e.g., the United Kingdom and the Scandinavian countries), patients cannot directly access secondary or tertiary healthcare without referral from primary healthcare.[13] Similarly, in South Korea, treatment at secondary or tertiary healthcare requires a referral letter from a primary care doctor. However, referral letters are frequently written at a patient's request and do not always reflect an actual need for care from higher-level hospitals.[14] Since the referral has no expiration date, the patient no longer needs a new referral when she/he visits to treat different diseases at the same department of the same hospital later. All in all, the South Korean referral system has failed in the gatekeeping role.

Another approach used to mitigate the excess demand problem is that of differential copayment.[20–22] In fact, the Korean government implemented a mild disease differential copayment policy in 2011. In this study, we empirically examined the impact of this differential copayment policy on patients' healthcare choices (i.e., primary vs. secondary/tertiary) using detailed and representative individual-level data provided by the Korea National Health Insurance Services (KNHIS) and a difference-in-difference approach. The policy increased patients' cost sharing for prescribed medications by 33% or 67% if they were used to treat one of 52 selected mild diseases in a secondary or tertiary healthcare institution, respectively. The rationale was that since the selected 52 diseases were mild ones that could be treated well in a primary healthcare, the extra

cost would discourage patients from visiting secondary/tertiary healthcare institutions to treat these diseases.

The results from previous studies on the effects of differential copayment policies have been mixed.[21,23,24] Moreover, some study results should be interpreted with caution because many of them used aggregate measures (e.g., annual number of visits, total expenditures) without controlling for potential confounding effects. Huang and Tung investigated if elderly Taiwanese patients' hospital tier choices have changed due to differential user charge using simple statistical tests (chi-square test, ANOVA, Scheffé test).[23] They found that the impact was too small to be practically significant. Rosen et al. investigated the effect of differential copayment on specialist visits in Israel using the difference-in-difference approach where they assigned medical beneficiaries who are exempted from the cost sharing as the control group.[25] They found that the differential copayment policy failed to restrain visits to specialist physicians. As they noted, however, there were systematic differences between treatment group (non-medical beneficiaries) and control group (medical beneficiaries) and potential confounding was not ruled out. There have been a few empirical studies that investigated the effect of differential copayment policy of South Korea but they had the same limitations as the above cited papers – namely, no rigorous handling of the confounding effects.[19,26,27]

Our study had several noteworthy strengths. First, we used a quasi-experimental setting with the difference-in-difference approach to precisely measure the policy's impact. When the focal differential copayment policy in South Korea was implemented in 2011, it applied to 52 mild diseases; this was extended to 100 diseases in 2018, adding 48 mild diseases. We measured the impact of the policy by focusing on the initial implementation (sample period January 2011–December 2012). Specifically, we constructed a set of treatment observations by selecting patient visits for the treatment of mild diseases selected from the set specified in 2011 (we refer to these as "treatment diseases") during the sample period. To construct a set of control observations, we selected patient visits whose purpose was to treat "control diseases" during the same sample period; the selected control diseases were similar to the treatment diseases (both belonged to the same middle-level categories in Korean Standard Classification of Diseases) and had been newly added in the 2018 extension. Consequently, our control observation provided the ideal counterfactual benchmark to precisely identify the policy's impact.

Previous studies have looked at the impact of healthcare policies applied to wide variety of diseases for which patients' condition severity may also vary widely (e.g., cancer) but remain unobserved by researchers.[28] In such cases, omitted disease severity becomes a critical challenge

in empirical estimation of a policy's effect. In contrast, our study examined a policy on mild diseases with only small variations in severity. The fact that the policy we studied covered only mild diseases allowed us to circumvent the omitted variable problem due to unobserved severity.

The aim of this study is to examine whether the Korean mild disease differential copayment policy of 2011 had a significant impact on patients' healthcare choices for the treatment of mild diseases.

DATA

This study used the Sample Research Data Base provided by the KNHIS, which provides mandatory social health insurance to all Koreans.[29,30] The 14-year cohort Sample Research Data Base includes socioeconomic and demographic variables (e.g., gender, residential area, income level) and detailed information on medical treatments (e.g. medical diagnosis, type of medical facilities visited) for approximately 1 million people (2.2% of the total population) collected from 2002 to 2013. Recorded diagnoses follow the Korean Standard Classification of Diseases-6 (KCD-6) code, which is a slightly modified version of International Statistical Classification of Diseases and Related Health Problems, 10th revision (ICD-10).[32]

On October 1, 2011, the Korean government implemented a differential copayment policy under which patients' cost sharing for prescribed medications used to treat one of 52 specified mild diseases increased if they were issued from secondary hospital (33% higher cost) or a tertiary hospital (67% higher cost); cost sharing for the same prescriptions issued at primary healthcare remained the same. In 2018, this policy was extended to include additional 48 mild diseases. From the initial 52 diseases and the 48 new diseases, we select adjacent diseases that shares a KCD-6 code at the middle classification level. After selecting "control diseases" and "treatment diseases" (**table 1**, **Online supplementary material**), we collected patient records of healthcare visits to treat the selected diseases between 2011 to 2012. Since we mainly examined the type of healthcare patients visited (i.e. primary vs. secondary/tertiary) and the impact of the focal policy on this decision, we included initial visits to treat mild diseases in our sample but subsequent visits to treat the same disease in the same hospital were excluded. Also, we only included patients younger than 65 years old since seniors (65+) are subject to a different cost sharing and insurance system. For the same reason, patients at the lowest income level (i.e., medical aid beneficiaries) were also excluded from the analysis. As a main empirical approach, we used a difference-in-difference method with patient fixed effect (we will provide more details in the next section). To this end, we included patients with two or more healthcare visits - specifically, at least one visit before the policy and one visit after

the policy. Note that patients with only one visit are canceled out in the fixed effect estimation. Key descriptive statistics of the selected samples are provided in **table 1.**

		Treatment disease	Control disease
		[B35.2-6, 8, 9] Dermatophytosis [H60.1, 3, 5, 8, 9] Otitis externa [J20.9] Acute bronchitis, unspecified [J30.0-4] Vasomotor and allergic rhinitis [J31.1, 2] Nasopharyngitis and pharyngitis [K52.2, 3, 8, 9] Other noninfective gastroenteritis and colitis [L20.8, 9] Atopic dermatitis [L23.8,9] Allergic contact dermatitis [M50.9] Cervical disc disorder, unspecified [M54.8, 9] Dorsalgia [M77.8, 9] Other enthesopathies [M79.1, 4, 6, 8, 9] Neuralgia and neuritis, unspecified [S63.6, 7] Dislocation, sprain and strain of joints and ligaments (hand) [S93.5, 6] Dislocation, sprain and strain of joints and ligaments (ankle/foot)	[B35.0, 1] Dermatophytosis [H60.0] Abscess of external ear [J20.0-2] Acute bronchitis [J31.0] Chronic rhinitis [K52.1] Toxic gastroenteritis and colitis [L20.0] Besnier's prurigo [L23.0-7] Allergic contact dermatitis [M50.3] Other cervical disc degeneration [M54.0, 1-6] Dorsalgia [M77.2, 3, 5] Other enthesopathies [M79.2] Neuralgia and neuritis, unspecified [S63.5] Sprain and strain of wrist
Variable	Categories of variable	Patients in treatment group (n=201,256)	Patients in control group (n=5,691)
Gender	Male	44.1%	42.6%
	Female	55.9%	57.4%
Age	Age_group1 (<20)	35.6%	14.4%
	Age_group2 (20's)	12.0%	8.8%
	Age_group3 (30's)	17.1%	17.8%
	Age_group4 (40's)	15.4%	23.8%
	Age_group5 (50's)	14.8%	26.2%
	Age_group6 (60's)	5.2%	9.1%
Income	Low (1–2 decile)	11.7%	12.9%
	Middle (3–8 decile)	55.4%	53.4%
	High (9–10 decile)	33.0%	33.8%
Residential area	Seoul-metro. area	55.9%	55.1%
	Other areas	44.1%	44.9%
Differential copayment policy (visit)	Pre-policy (1/1/2010 –9/30/2011)	Count: 394,316 (secondary/tertiary: 5.0%, primary: 95.0%)	Count: 6,452 (secondary/tertiary: 3.0%, primary: 97.0%)
	Post-policy (10/1/2011 –12/31/2012)	Count: 307,920 (secondary/tertiary: 4.0%, primary: 96.0%)	Count: 6,113 (secondary/tertiary: 5.0%, primary: 95.0%)

Table 1. Selected Diseases and Descriptive Statistics

Korean Standard Classification of Diseases-6 (KCD-6) code is shown in brackets. Detailed information on the selected disease is provided in online supplementary material.

ETHICS STATEMENT

The study protocol was approved by the Institutional Review Board of the Korea Advanced Institute of Science and Technology(KH2018-94). Informed consent was waived by the board.

METHODOLOGY

Our dataset has an unbalanced panel structure and the unit of analysis is a patient-visit. We adopted a linear regression model with patient-level fixed effect in our analysis. Accordingly, cluster standard errors were used in all inferences; standard errors were clustered at individual patient level.[33] The dependent variable was whether the afflicted patients selected primary healthcare or secondary/tertiary healthcare in their visit to treat the focal diseases; thus, it is represented as a binary dummy variable (1 if secondary/tertiary healthcare was chosen and 0 if primary healthcare was chosen). This modeling approach is categorized as a linear probability model (LPM), where the estimated dependent variable can be interpreted as the probability of visiting secondary/tertiary healthcare rather than primary healthcare. [34–36]

We applied a difference-in-difference (DID) approach to measure the impact of the differential copayment policy. This method has been widely applied in previous studies to measure the impact of policies because it eliminates the effects of unobservable external factors by using control observations as counterfactuals.[23,37,38] Before applying the difference-in-difference approach, we checked the validity of our control observations by performing a parallel trend test to check whether the treatment and control observations followed the same pattern before the differential copayment policy and confirmed that they had the same trend (**Online supplementary material**).

Next, we defined the *"Treat"* dummy variable as 1 if a patient visit was to treat one of the treatment diseases and 0 if it was to treat one of the control diseases. We defined the *"Post"* dummy variable as 1 if the visit occurred after the policy implementation and 0 if it occurred before. We also included month dummy variables to capture time trends and/or seasonal variations in the dependent variable. To account for the differences in selected mild diseases, disease dummies are included. The policy impact was measured using the coefficient of the interaction of the terms *"Treat"* and *"Post."*

Furthermore, we used a triple-difference linear probability model (TD LPM) to investigate how the policy impact varied with key demographic features (i.e., gender, income group, and residential area). To this end, we included demographic variables such as gender, income

(low/middle/high), and residential area as categorical dummy variables. With respect to residential area, we looked at whether the patients lived in the Seoul metropolitan area or not; this was of interest because about 25 million people (50% of the country's population) live currently in the Seoul metropolitan area, where healthcare facilities and resources are highly concentrated.[39]

As stated, we define "*Treat*" variable based on whether the patient visit was to treat treatment disease or control disease. Accordingly, a patient can serve as treatment group in a visit but as control group in another visit. In contrast, in Model 3, we select patients who belong to only one group during the entire sample period and perform TD LPM analysis using them. Note that the assignment of treatment vs. control is at the individual patient level in this model.

Next, considering data imbalance and demographic difference between treatment and control observations, we used propensity score matching (PSM) to make treatment observations and control observations comparable with respect to various observed variables and then performed TD LPM analysis (Model 4).[40–42] Specific details regarding our PSM process is provided in **Online supplementary material**.

The specifications for the above-stated models are provided below.

DID with fixed effect LPM (Model1)

$$Y_{it} = \alpha_i + \beta_T \cdot Treat_{it} + \beta_P \cdot Post_{it} + \beta_{DID} \cdot Treat_{it} \times Post_{it} + \tau_{Month} + \delta_{Disease} + u_{it} \quad \text{Eq. 1}$$

TD LPM with fixed effect (Model2, Model3, Model4)

$$\begin{aligned} Y_{it} = \alpha_i + \beta_T \cdot Treat_{it} + \beta_P \cdot Post_{it} + \beta_{DID} \cdot Treat_{it} \times Post_{it} + \beta_{Male} \cdot Male_i \times Treat_{it} \times Post_{it} \\ + \beta_{LI} \cdot LowInc_i \times Treat_{it} \times Post_{it} + \beta_{HI} \cdot HighInc_i \times Treat_{it} \times Post_{it} \\ + \beta_{Metro} \cdot Metro_i \times Treat_{it} \times Post_{it} + \tau_{Month} + \delta_{Disease} + u_{it} \end{aligned} \quad \text{Eq. 2}$$

where i and t denote patient and healthcare visit, respectively, and Y_{it} is a binary indicator variable which takes the value of one if secondary/tertiary healthcare is visited by i at t and zero otherwise (i.e. primary healthcare visit). α_i is a patient-fixed effect which account for patient-specific characteristics in healthcare choice. τ_{Month} and $\delta_{Disease}$ are month- and disease-fixed effects to account for seasonality, time trend and disease-specific variations. $Male_i$, $LowInc_i$, $HighInc_i$, and $Metro_i$ are indicator variables denoting whether i is a male or not, i belongs to the low-income group or not, the high-income group or not, and i resides in the Seoul-metro area or not, respectively. u_{it} is an idiosyncratic error.

After the above-mentioned main model analyses, we conducted additional analyses to check robustness and to obtain nuanced implications regarding the policy effect. First, we expanded our sample period from 2011-12 to 2010-13. Through this, we investigated whether the effect of differential copayment policy lasted for extended period of time. Second, we divided our sample patients into two groups based on the count of healthcare visits and investigated whether the policy impact varied with the visit frequency.

Patient and Public Involvement: No patient involved

RESULTS

Difference-in-Difference Analysis Using LPM

While the observations from descriptive statistics supported the effectiveness of the policy, we formally examined this after controlling for other effects such as unobserved patient-level characteristics, seasonal trend, and disease-specific characteristics using the proposed models. After confirming common trend between treatment and control group (see **Online supplementary material**), the proposed fixed-effect LPM (Eq. 1) was estimated. Note that we used the within-estimator to handle the patient-level fixed effect.[43] The first column (Model 1) of **table 2** presents the estimation result. Here, the coefficient of "*Treat"* indicates the estimated mean difference in the probability of selecting secondary/tertiary healthcare between the treatment and control observations prior to policy implementation. The coefficient of *"Post"* indicates the estimated change in the probability after policy implementation in the control observations. We captured the effect of the policy through the interaction of *"Post"* and "*Treat"* represented as *"Post x Treat".*

In Model 1, the coefficient of *"Treat"* (-0.3722) was not statistically significant, indicating that there is no significant difference between treatment and control observations in choosing secondary/tertiary healthcare over primary healthcare. In contrast, the coefficient of *"Post"* (0.0235) was positive and significant, indicating that the proportion of secondary/tertiary healthcare visits among the control diseases increased after policy implementation. More importantly, the DID term related to the policy effect (*"Post x Treat"*) was negative and significant (-0.0299). This indicates that the Korean differential copayment policy of 2011 reduced patients' probability of visiting secondary/tertiary healthcare by 2.99 percent point.

Heterogeneous Policy Effect: Triple Difference Analysis

After verifying the effectiveness of the policy, we conducted additional analyses to examine the heterogeneity of the policy effect among different demographic groups. To this end, we added

triple interaction terms between *"Post×Treat"* and dummies for gender, income, and residential area (Eq. 2). The estimation results are reported in **table 2** (Model 2).

In Model 2, the coefficient of *"Post×Treat"* is highly significant and negative (-0.0267), indicating that the policy decreased the probability of choosing secondary/tertiary healthcare instead of primary healthcare among patients in the reference group (i.e. female, middle income, non-Seoul metropolitan area). The coefficient of *"Post×Treat×LowInc"* (0.0037) was statistically significant, while the coefficient of *"Post×Treat×HighInc"* (-0.0005) was not statistically significant. This indicates that the differential copayment policy had a smaller impact among people in the low income group than among the others in the middle/high-income group in decreasing the probability of choosing secondary/tertiary over primary healthcare. Specifically, the effect was weaker by 14% in the low income group. Similarly, the coefficient of *"Post×Treat×Metro"* (-0.0052) was significant and negative, revealing that the impact of the differential copayment policy was greater by 19% among the residents of Seoul metropolitan area than among people living elsewhere.

Exclusion of Patients Who Have Both Treatment and Control Observations

We defined treatment and control observations based on the disease – whether the disease is influenced by the policy or not. Accordingly, a patient can have both treatment and control observations. We selected the patients who visited healthcare due to treatment diseases only or control diseases only. As a result, our assignment of samples to treatment and control groups was not varying within a patient. Note that the "*Treat*" variable became time invariant and was absorbed in the fixed effect term.

Model 3 in **table 2** represents the estimation results from this model. Overall, the main findings from Model 3 are highly consistent with those from Model 1 and Model 2. The coefficient of *"Post×Treat"* is highly significant and negative (-0.0270), indicating that the policy decreased the probability of choosing secondary/tertiary healthcare instead of primary healthcare among patients in the reference group (i.e. female, middle income, non-Seoul metropolitan area). The coefficient of *"Post×Treat×LowInc"* (0.0035) was positive and statistically significant, while the coefficient of *"Post×Treat×HighInc"* (-0.0005) was not. Specifically, the effect was weaker by 13% in the low-income group. The coefficient of *"Post×Treat×Metro"* (-0.0052) reveals that the effect of the differential copayment policy was stronger by 19% among the residents of the Seoul metropolitan area than that of the patients living elsewhere.

Propensity Score Matching

Next, we performed a propensity score matching (PSM) to remove confounding from observable variables and then estimated the TD model (Eq.2) using the matched sample. We provide details on our PSM procedure in **Online supplementary material**. Model 4 in **table 2** reports the estimation results. Overall statistical significance of the model decreased since we lost many observations in the matching process (from 714,801 observations to 37,626 observations). However, we still found marginally significant (10% level) main effect of the differential copayment policy (-0.0149). Also, almost all estimated coefficients have the same sign and similar magnitude as the estimates from the other models. We conclude that this result provides additional support to our main findings.

	DID	TD	Using Treatment Only and Control Only Patients	PSM
	Model 1 β (std. err.)	Model 2 β (std. err.)	Model 3 β (std. err.)	Model 4 β (std. err.)
Month Dummies	Yes	Yes	Yes	Yes
Disease Dummies	Yes	Yes	Yes	Yes
Patient Fixed Effect	Yes	Yes	Yes	Yes
Post	0.0235 *** (0.003)	0.0235 *** (0.003)	0.0238 *** (0.004)	0.0218 *** (0.004)
Treat	−0.3722 (0.277)	−0.3738 (0.276)	–	−0.3819 (0.277)
Post × Treat	− 0.0299 *** (0.004)	−0.0267 *** (0.004)	− 0.0270 *** (0.004)	−0.0149 † (0.009)
Post × Treat × Male	–	−0.0025 * (0.001)	−0.0026 * (0.001)	−0.0089 (0.008)
Post × Treat × Low-Income	–	0.0037 * (0.002)	0.0035 * (0.002)	0.0027 (0.009)
Post × Treat × High-Income	–	−0.0005 (0.001)	−0.0005 (0.001)	0.0040 (0.011)
Post × Treat × Seoul Metro. Area	–	−0.0052 *** (0.001)	−0.0052 *** (0.001)	−0.0109 (0.176)
R-square	0.006	0.006	0.006	0.010
Number of observations	714,801	714,801	699,867	37,626
F-statistics	84.22 ***	77.49 ***	77.57 ***	4.59 ***

Table 2. Results of Linear Probability Models on Patient Healthcare Choice

The full estimation results are available in online supplementary material. *: p<0.05 / **: p<0.01 / ***: p<0.001 / †: p<0.1

Extended Sample Period

The policy change may take some time until the actual effect shows up or the effect can be short-lived, disappearing soon after the implementation. To further investigate this aspect of the policy, we extended our sample period from 2011-2012 to 2010-2013. We estimated the TD LPM (Eq. 2). Model 5 in **table 3** provides the estimation results. The coefficient of *"Post×Treat"* is highly significant and negative (-0.0218), indicating that the policy decreased the probability of choosing secondary/tertiary healthcare instead of primary healthcare among patients in the reference group (i.e. female, middle income, non-Seoul metropolitan area). Overall results using the extended sample period echoed our earlier findings from the other models.

We concluded that the impact of the policy did not show substantial changes in significance and magnitude when we extended the sample period up to December 2013 (27 months after the policy implementation).

	Extended Sample Period (2010-13)	Split Sample Using Visit Count	
		# of Visits > 5	# of Visit ≤ 5
Model	Model 5 β (std. err)	Model 6 β (std. err)	Model 7 β (std. err)
Month Dummies	Yes	Yes	Yes
Disease Dummies	Yes	Yes	Yes
Patient Fixed Effect	Yes	Yes	Yes
Post	0.0199 *** (0.003)	0.0262 *** (0.007)	0.0230 ** (0.004)
Treat	− 0.3628 * (0.153)	−0.4919 (0.356)	− 0.0759 ** (0.021)
Post × Treat	− 0.0218 *** (0.003)	− 0.0335 *** (0.008)	− 0.0252 *** (0.004)
Post × Treat × Male	− 0.0025 ** (0.001)	−0.0013 (0.003)	− 0.0027 * (0.001)
Post × Treat × Low-Income	0.0028 * (0.001)	0.0118 ** (0.004)	0.0016 (0.002)
Post × Treat × High-Income	−0.0001 (0.001)	0.0001 (0.003)	−0.0007 (0.001)
Post × Treat × Seoul Metro. Area	− 0.0066 *** (0.001)	− 0.0076 ** (0.003)	− 0.0044 *** (0.001)
R-square	0.006	0.006	0.006
Number of observations	1,077,928	155,418	559,383

F-statistics	$_{1}16.90^{***}$	$_{3}1.21^{***}$	$_{5}4.32^{***}$

Table 3. Results of Additional Models to Check Robustness

*: p<0.05 / **: p<0.01 / ***: p<0.001 / †: p<0.1. The full estimation results are available online in Online supplementary material

Policy Effect and Visit Frequency

Patients who visit healthcare facilities more frequently than others may have more information regarding the changes in healthcare policies than others. If so, the impact of policy can be more pronounced among these people. To further examine this in our empirical context, we decomposed our sample into two using the number of healthcare visits to treat mild diseases during the sample period: i) 5 times or less, ii) above 5 times. Model 6 and Model 7 in **table 3** show the estimation results using frequent visitors and the others, respectively. In the frequent visitor sample (Model 6), the coefficient of "*Post×Treat*" (-0.0335) is much larger than any other models. In contrast, from the less frequent visitor sample (Model 7), we found that the coefficient of "*Post×Treat*" (-0.0252) is smaller in magnitude than those of Model 1, Model 2, and Model 3. These results imply that the impact of differential copayment policy was stronger among frequent healthcare visitors. We also found that the coefficient of *"Post×Treat×LowInc"* was statistically significant in Model 6 (0.0118) while it was insignificant in Model 7. This finding indicates that the substantially weaker policy impact among low income patients than others was mainly driven by low-income frequent visitors. Both models had significant and negative coefficients of *"Post×Treat×Metro"*, which is consistent with all the other models.

DISCUSSION

Our study design leveraged the unique structure of the policy to examine the effects of the policy on individual patients' choices. Selecting similar diseases from the list of diseases in the initial 2011 implementation and the 2018 extension, we constructed a quasi-experimental setting. Furthermore, by focusing on mild diseases only, we avoided the omitted-variable problem caused by unobserved disease severity. We found that the South Korean government's 2011 differential copayment policy was successful in decreasing patients' unnecessary choice of secondary/tertiary healthcare over primary healthcare for mild diseases.

The impact of the policy differed across demographic groups. Specifically, the policy's effect was weaker among low income patients compared to richer patient groups. This result is distinct from those in several previous empirical studies in which many researchers have found that people with low income are more sensitive to cost sharing changes and that policies based on cost sharing

can exacerbate medical inequality.[45–49] The distinctiveness of our results can be explained by the Korean differential copayment policy focusing only on mild diseases. Before the policy, people in middle- and high-income groups visited secondary/tertiary healthcare more frequently than people in the low-income brackets. Since most of the visits by middle- and high-income people to secondary/tertiary healthcare institutions could have been handled just as well by primary healthcare institutions, their adjustment in healthcare choices after the policy implementation could be more pronounced.

The weaker policy impact on low-income people might be derived from the difference in the level of health information each group has regarding the policy. People with lower incomes tend to have poorer healthcare information compared to people with higher incomes.[50] Since they are poorly-informed regarding the policy and the increase in the cost sharing payment, their adjustment in healthcare choices after the policy could be weak. We also found that the weaker impact of the policy among low income people was not limited to short period after the policy implementation but lasted for extended period of time (27 months afterward). Moreover, our analyses indicated that this varying impact along with the income was mainly driven by the patients with frequent healthcare visits. This finding implies that the government can fulfill its policy goal more effectively by enhancing information sharing, especially focusing on low-income frequent healthcare visitors.

Another interesting finding from our study was that the policy impact was greater among the people living in the Seoul metropolitan area than among people living in other areas. As noted, healthcare resources are concentrated in the Seoul metropolitan area. Because there are more healthcare facilities overall in the Seoul metropolitan area, people there may find suitable primary healthcare institutions to substitute for secondary/tertiary healthcare institutions more easily than the people in other areas whose choices may be more limited. This might explain the pronounced policy effect in the Seoul metropolitan area. This finding points out that it is important to make primary healthcare outside the Seoul metropolitan area more accessible and attractive to patients.

We also found that the policy impact was greater among frequent healthcare visitors than others. All coefficients related our findings regarding the policy impact had larger values in magnitude in the data from frequent visitors than in those from the others. Again, this finding can be explained by the difference in the amount of health information patents have. Frequent healthcare users may have more information on the changes in copayment policy and update their behaviors accordingly while infrequent visitors are less informed and thus less sensitive to this change.

A few limitations of this study should be noted. As is the case with most studies using observed data, it is difficult to estimate the causal effect of the policy in a non-experimental setting. Since experimentation in our context had several challenges, including ethical issues, an experimental study was not feasible. Instead, we tried to control the effects of confounding variables by using control observations. We also performed a series of robustness tests to check the validity of our findings. However, further investigation is needed. As a related issue, we limited the subject of this study to similar pairs of mild diseases to construct an ideal control–treatment group setting. Although the Korean differential copayment policy in 2011 included 52 diseases and added 48 new diseases in 2018, our criteria to construct quasi-experimental setting using similar diseases yielded limited cases (26 diseases). More investigations using other diseases are required to further generalize our findings. We have monitored the effect of the policy up to 27 months from the implementation. It is also possible that the dissemination of information on policy change requires longer time. As such, follow-up study using longer sample period is needed.

CONCLUSION

We investigated the effect of the mild disease differential copayment policy introduced in South Korea in 2011 using the Sample Research Data Base provided by the KNHIS, conducting a difference-in-difference analysis with a quasi-experimental design. We found that the policy significantly decreased the proportion of patients choosing secondary/tertiary healthcare facilities over primary healthcare facilities by 2.99 percent point. The effect was stronger among people with middle/high incomes, those living in the Seoul metropolitan area, and those who frequently visited healthcare facilities to treat mild diseases. We performed a series of robustness checks and found all our results to be highly consistent.

Contributors We confirm that all the authors have made substantive intellectual contributions to the paper; SP contributed to the design and the interpretation of the study after reviewing the result of the study. SKJ conceptualized the study and analyzed the data. DBJ contributed the data acquisition and provided statistical analysis support. All authors supplied critical revisions to the manuscript.

Funding This work was supported by the Ministry of Science and ICT, Republic of Korea (NRF-2019R1F1A1055160) and SNU Business School, Seoul National University.

Competing interests None declared.

Ethics approval We obtained approval from the institutional review board of Korea Advanced Institute of Science and Technology (KH2018-94).

Provenance and peer review Not commissioned; externally peer reviewed.

Data sharing statement No additional data are available.

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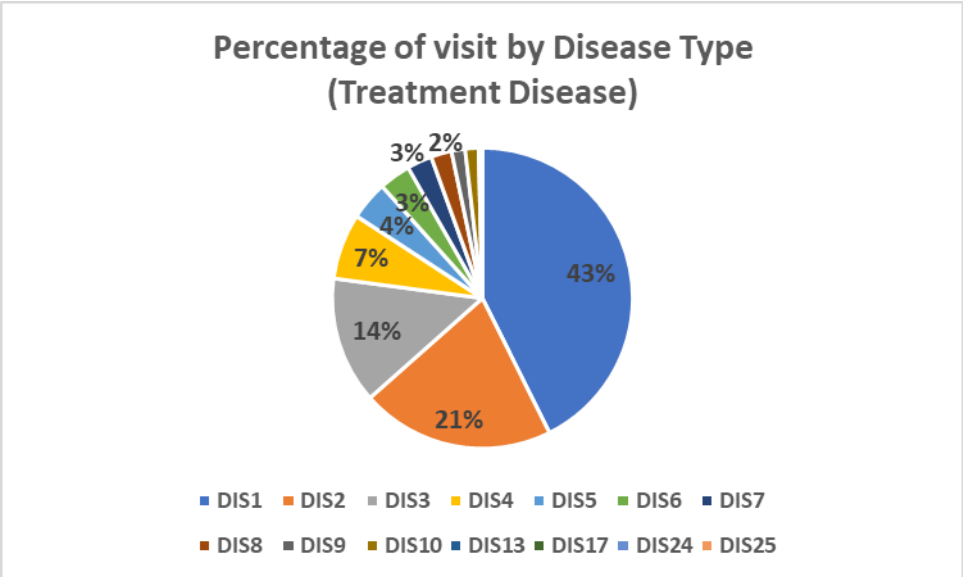
Online Supplementary material

Supplementary material A: Control Diseases and Treatment Diseases

Below we show the percentage of patients' visits by disease type.



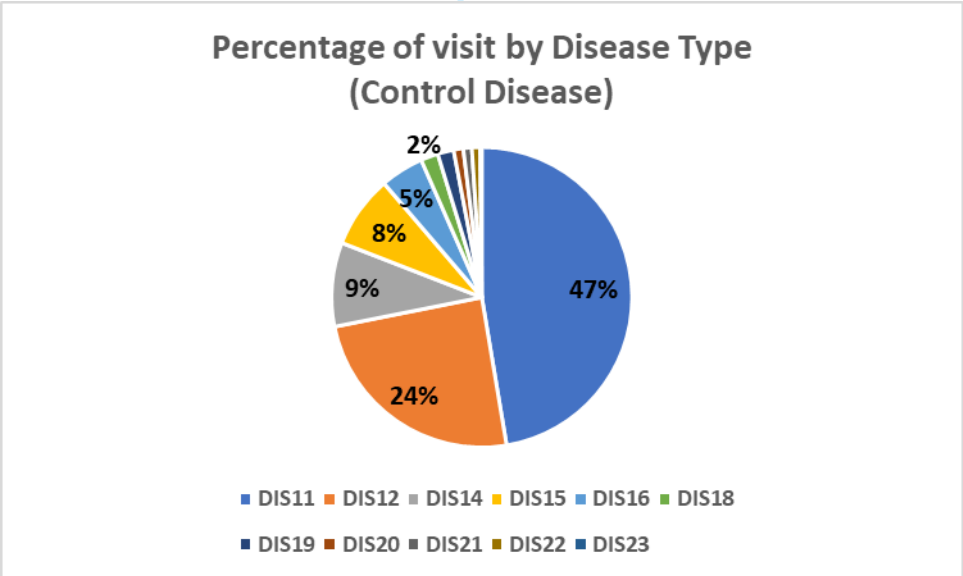


figure A: Percentage of patients' visit by disease type

DIS1 [J20.9]	DIS2 [J30.0-4]	DIS3 [L23.8,9]	DIS4 [K52.2, 3, 8, 9]	DIS5 [B35.2-6, 8, 9]
DIS6 [L20.8, 9]	DIS7 [H60.1, 3, 5, 8, 9]	DIS8 [S93.5, 6]	DIS9 [S63.6, 7]	DIS10 [J31.1, 2]
DIS11 [B35.0, 1]	DIS12 [J31.0]	DIS13 [M50.9]	DIS14 [L23.0-7]	DIS15 [J20.0-2]
DIS16 [S63.5]	DIS17 [M79.1, 4, 6, 8, 9]	DIS18 [M77.2, 3, 5]	DIS19 [H60.0]	DIS20 [M54.0, 1-6]
DIS21 [K52.1]	DIS22 [M50.3]	DIS23 [L20.0]	DIS24 [M54.8, 9]	DIS25 [M77.8, 9]

Supplementary material B: Observations from Summary Statistics

There were 12,565 control and 702,236 treatment observations. Secondary/tertiary healthcare visits accounted for 5.0 % of all treatment observations before the policy, and this number decreased to 4.0% after the policy. In the control observations, in contrast, secondary/tertiary healthcare visits accounted for 3.0% before the policy, and this number increased to 5.0% after the policy. These observations from summary statistics imply that the differential copayment policy was effective in nudging patients to select primary healthcare instead of secondary/tertiary healthcare in treating their mild diseases.

We observed that the proportions of visits to secondary/tertiary healthcare to treat the control diseases increased (**figure B**). Next, we checked whether this trend was common for other mild diseases during the sample period. we computed the proportion of secondary/tertiary visits for all 48 of the mild diseases that had not been included in the policy in 2011 but were added in the 2018 policy extension. We found that the proportion of secondary/tertiary visits overall increased from 4.3% in the before the policy (January 2011–September 2011) to 5.7% after the policy (October 2011–December 2012), showing a pattern similar to that of our selected control diseases. These observations from summary statistics also revealed an increasing demand for secondary/tertiary healthcare even for mild diseases, justifying the implementation of the differential copayment policy that was the focus of our study.

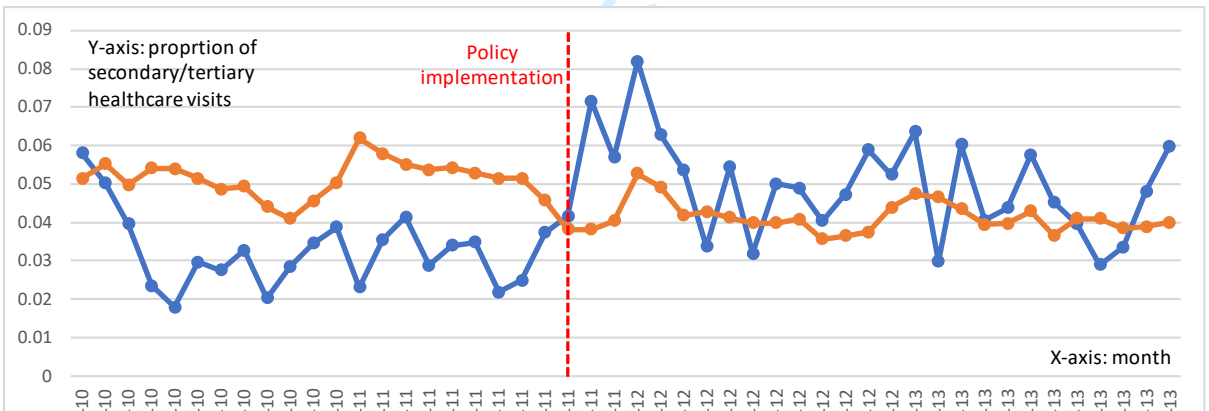


Figure B. Proportion of visiting secondary/tertiary healthcare facilities (2010-2013)

Proportion of visiting secondary/tertiary healthcare from 2010 to 2013. The dotted vertical line represents the implementation of differential coefficient policy in Oct. 2011.

Supplementary material C: Common Trend Test

We check whether the control observations and treatments observations share the same trend before the policy implementation. To this end, we estimate the following fixed effect model (within estimator):

$$Y_{it} = c_i + \beta_T(Treat_{it}) + \beta_{m1}(Month1_{it}) + \cdots + \beta_{m20}(Month20_{it}) + \beta_{int1}(Month1 \times Treat)_{it} + \cdots + \beta_{int20}(Month20 \times Treat)_{it} + u_{it}$$

where i denotes patient, t denotes t-th healthcare visit, and c_i is the patient fixed effect. Y_{it} is equal to 1 if secondary/tertiary healthcare is selected and zero if primary healthcare is selected in the observation. $Month1_{it}, \dots, Month20_{it}$ are monthly dummy variables. $treat_{it}$ is equal to one if the observation is a treatment observation and zero if the observation is a control observation. Month 21 (September 2011) is the month just before the policy implementation and we use this as the baseline (omitted category). **table C** reports the estimation results. Note that the estimated coefficients of interactions of "Treat" and month dummies capture the differences between the treatment and the control observations each month before the policy implementation. **figure C** graphically shows the estimates. All the estimates are insignificant, indicating that the treatment and the control observations have a common trend. This result indicates that our control observations provide good counterfactuals to estimate the impact of the differential copayment policy.

Variable	Year/Month	Estimates	Standard Error	P-value
Treat	-	0.0133	0.023	0.559
Month1 X Treat	Jan-10	-0.0251	0.031	0.415
Month2 X Treat	Feb-10	-0.0214	0.027	0.432
Month3 X Treat	Mar-10	-0.0304	0.03	0.310
Month4 X Treat	Apr-10	0.0421	0.026	0.107
Month5 X Treat	May-10	0.0187	0.024	0.430
Month6 X Treat	Jun-10	-0.0009	0.029	0.976
Month7 X Treat	Jul-10	-0.0071	0.027	0.789
Month8 X Treat	Aug-10	-0.0169	0.027	0.525
Month9 X Treat	Sep-10	-0.0177	0.027	0.514
Month10 X Treat	Oct-10	0.0016	0.026	0.952
Month11 X Treat	Nov-10	-0.0123	0.027	0.648
Month12 X Treat	Dec-10	-0.0147	0.028	0.597
Month13 X Treat	Jan-11	-0.0243	0.037	0.514
Month14 X Treat	Feb-11	0.0146	0.035	0.676
Month15 X Treat	Mar-11	0.0028	0.031	0.929
Month16 X Treat	Apr-11	0.0055	0.026	0.835
Month17 X Treat	May-11	0.0203	0.029	0.483

Month18 X Treat	Jun-11	0.0135	0.026	0.605
Month19 X Treat	Jul-11	-0.0295	0.028	0.296
Month20 X Treat	Aug-11	0.0072	0.025	0.772
Month1	Jan-10	0.0294	0.031	0.337
Month2	Feb-10	0.0314	0.027	0.245
Month3	Mar-10	0.0367	0.03	0.218
Month4	Apr-10	-0.0297	0.026	0.252
Month5	May-10	-0.0037	0.023	0.875
Month6	Jun-10	0.0119	0.029	0.677
Month7	Jul-10	0.0069	0.027	0.794
Month8	Aug-10	0.0210	0.026	0.424
Month9	Sep-10	0.0125	0.027	0.642
Month10	Oct-10	-0.0052	0.026	0.84
Month11	Nov-10	0.0085	0.027	0.753
Month12	Dec-10	0.0213	0.028	0.443
Month13	Jan-11	0.0401	0.037	0.28
Month14	Feb-11	0.0036	0.035	0.917
Month15	Mar-11	0.0064	0.031	0.836
Month16	Apr-11	0.0045	0.026	0.862
Month17	May-11	-0.0041	0.029	0.886
Month18	Jun-11	-0.0076	0.026	0.769
Month19	Jul-11	0.0318	0.028	0.256
Month20	Aug-11	-0.0064	0.025	0.795

table C. Result of Common Trend Test

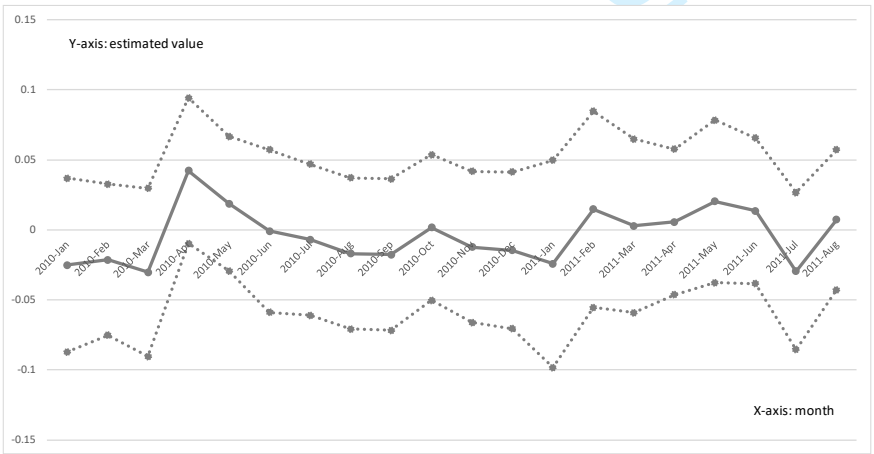


figure C: Common Trend Test Results

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

Supplementary material D: Full Estimation Results

We provide the full estimation results of Models 1 to 4 in table D.1 and those of Models 5 to 7 in table D.2.

	Main DID	Main with TD	Excluding Both T/C visitor	PSM
	Model 1 β (std. err)	Model 2 β (std. err)	Model 3 β (std. err)	Model 4 β (std. err)
Post	0.0235^{***} (0.003)	0.0235^{***} (0.003)	0.0238^{***} (0.004)	0.0218^{***} (0.004)
Treat	-0.3722 (0.277)	-0.3738 (0.276)	–	-0.3819 (0.277)
Post× Treat	-0.0299^{***} (0.004)	-0.0267^{***} (0.004)	-0.0270^{***} (0.004)	$-0.0149^{\dagger}$ (0.009)
Post× Treat× Male	–	-0.0025^{*} (0.001)	-0.0026^{*} (0.001)	-0.0089 (0.008)
Post× Treat×Low-Income	–	0.0037^{*} (0.002)	0.0035^{*} (0.002)	0.0027 (0.009)
Post× Treat×High-Income	–	-0.0005 (0.001)	-0.0005 (0.001)	0.0040 (0.011)
Post× Treat×Seoul Metro. Area	–	-0.0052^{***} (0.001)	-0.0052^{***} (0.001)	-0.0109 (0.176)
Month1 (Jan)	0.0121^{***} (0.001)	0.0122^{***} (0.001)	0.0121^{***} (0.001)	0.0172^{**} (0.007)
Month2 (Feb)	0.0127^{***} (0.001)	0.0127^{***} (0.001)	0.0128^{***} (0.002)	0.0136 (0.010)
Month3 (Mar)	0.0080^{***} (0.001)	0.0080^{***} (0.001)	0.0081^{***} (0.001)	0.008 (0.006)
Month4 (APR)	0.0102^{***} (0.001)	0.0103^{***} (0.001)	0.0103^{***} (0.001)	0.0062 (0.007)
Month5 (May)	0.0111^{***} (0.001)	0.0112^{***} (0.001)	0.0113^{***} (0.001)	-0.0055 (0.009)
Month6 (Jun)	0.0092^{***} (0.001)	0.0092^{***} (0.001)	0.0091^{***} (0.001)	$0.0125^{\dagger}$ (0.007)
Month7 (Jul)	0.0055^{***} (0.001)	0.0055^{***} (0.001)	0.0055^{***} (0.001)	-0.0004 (0.007)
Month8 (Aug)	0.0051^{***} (0.001)	0.0051^{***} (0.001)	0.0049^{***} (0.001)	-7.37×10^{-5} (0.012)
Month10 (Oct)	-0.0025^{*} (0.001)	-0.0025^{*} (0.001)	$-0.0023^{\dagger}$ (0.001)	0.0121 (0.016)
Month11 (Nov)	0.0003 (0.001)	0.0003 (0.001)	0.0004 (0.001)	0.0099 (0.008)
Month12 (Dec)	0.0052^{***} (0.001)	0.0052^{***} (0.001)	0.0053^{***} (0.001)	0.0008 (0.008)
Disease2 [J30.0-4]	-0.0009 (0.001)	-0.0009 (0.001)	-0.001 (0.001)	0.003 (0.004)
Disease3 [L23.8,9]	-0.0234^{***} (0.001)	-0.0234^{***} (0.001)	-0.0235^{***} (0.001)	-0.0169^{*} (0.008)
Disease4 [K52.2, 3, 8, 9]	0.0337^{***} (0.001)	0.0336^{***} (0.001)	0.0338^{***} (0.001)	0.0376^{***} (0.011)
Disease5 [B35.2-6, 8, 9]	-0.0109^{***} (0.001)	-0.0109^{***} (0.001)	-0.0108^{***} (0.001)	0.0217 (0.035)
Disease6 [L20.8, 9]	0.0110^{***} (0.002)	0.0109^{***} (0.002)	0.0108^{***} (0.002)	$0.0210^{\dagger}$ (0.013)

Disease7 [H60.1, 3, 5, 8, 9]	−0.0159*** (0.001)	−0.0159*** (0.001)	−0.0158*** (0.001)	−0.0048 (0.009)
Disease8 [S93.5, 6]	0.0066*** (0.002)	0.0066*** (0.002)	0.0065*** (0.002)	0.0163 (0.011)
Disease9 [S63.6, 7]	0.0090*** (0.002)	0.0090*** (0.002)	0.0085*** (0.002)	0.0209† (0.013)
Disease10 [J31.1, 2]	−0.0105*** (0.002)	−0.0106*** (0.002)	−0.0104*** (0.002)	−0.0115 (0.008)
Disease11 [B35.0, 1]	−0.3988 (0.277)	−0.4003 (0.276)	−0.0900*** (0.023)	−0.3972 (0.277)
Disease12 [J31.0]	−0.3665 (0.277)	−0.3681 (0.276)	−0.0548* (0.023)	−0.3688 (0.277)
Disease13 [M50.9]	0.0327*** (0.005)	0.0328*** (0.005)	0.0337*** (0.005)	0.0537 (0.044)
Disease14 [L23.0-7]	−0.4006 (0.277)	−0.4019 (0.276)	−0.0928*** (0.022)	−0.3996 (0.277)
Disease15 [J20.0-2]	−0.4016 (0.277)	−0.403 (0.276)	−0.1036*** (0.024)	−0.4021 (0.277)
Disease16 [S63.5]	−0.3550 (0.277)	−0.3564 (0.276)	−0.0267 (0.028)	−0.3567 (0.277)
Disease17 [M79.1, 4, 6, 8, 9]	0.0799*** (0.018)	0.0798*** (0.018)	0.0818*** (0.019)	−0.0057 (0.006)
Disease18 [M77.2, 3, 5]	−0.3868 (0.277)	−0.3882 (0.277)	−0.0741* (0.032)	−0.3819 (0.277)
Disease19 [H60.0]	−0.4047 (0.277)	−0.4062 (0.276)	−0.082** (0.027)	−0.4061 (0.277)
Disease20 [M54.0, 1-6]	−0.2534 (0.277)	−0.2548 (0.277)	0.0583** (0.023)	−0.2543 (0.277)
Disease21 [K52.1]	−0.3478 (0.278)	−0.3494 (0.277)	0.0044 (0.050)	−0.3481 (0.278)
Disease22 [M50.3]	−0.3922 (0.278)	−0.3937 (0.277)	−0.0897* (0.044)	−0.3916 (0.278)
Disease23 [L20.0]	−0.3947 (0.280)	−0.3965 (0.279)	0.0194 (0.120)	−0.3950 (0.280)
Disease24 [M54.8, 9]	0.1163 (0.104)	0.1160 (0.104)	0.1160 (0.104)	−
Disease25 [M77.8, 9]	0.3069* (0.131)	0.3069* (0.131)	0.3069* (0.131)	−
R-square	0.006	0.006	0.006	0.01
Number of observations	714,801	714,801	699,867	37,626
F-statistics	84.22***	77.49***	77.57***	4.59***

table D.1 Estimation Results of Fixed Effect Linear Probability Models on Healthcare Choices

Note) Standard errors are reported in parentheses. Baseline categories are "Female" in gender, "Middle-Income" in income, "The Other Areas" in residential area, and "Month 9" in month. †, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

	2010-13	Visit frequency segmentation	
		Visit > 5	Visit ≤ 5
Model	Model 5 β (std. err)	Model 6 β (std. err)	Model 7 β (std. err)
Post	0.0199^{***} (0.003)	0.0262^{***} (0.007)	0.0230^{**} (0.004)
Treat	-0.3628^{*} (0.153)	-0.4919 (0.356)	-0.0759^{**} (0.021)
Post×Treat	-0.0218^{***} (0.003)	-0.0335^{***} (0.008)	-0.0252^{***} (0.004)
Post× Treat×Male	-0.0025^{**} (0.001)	-0.0013 (0.003)	-0.0027^{*} (0.001)
Post× Treat ×Low-Income	0.0028^{*} (0.001)	0.0118^{**} (0.004)	0.0016 (0.002)
Post× Treat×High-Income	-0.0001 (0.001)	9.75×10^{-5} (0.003)	-0.0007 (0.001)
Post× Treat×Seoul Metro. Area	-0.0066^{***} (0.001)	-0.0076^{**} (0.003)	-0.0044^{***} (0.001)
Month1 (Jan)	0.0104^{***} (0.001)	0.0125^{***} (0.003)	0.0122^{***} (0.002)
Month2 (Feb)	0.0097^{***} (0.001)	0.0193^{***} (0.003)	0.0105^{***} (0.002)
Month3 (Mar)	0.0063^{***} (0.001)	0.0159^{***} (0.003)	0.0055^{***} (0.001)
Month4 (APR)	0.0075^{***} (0.001)	0.0159^{***} (0.003)	0.0085^{***} (0.001)
Month5 (May)	0.0078^{***} (0.001)	0.0211^{***} (0.003)	0.0077^{***} (0.001)
Month6 (Jun)	0.0072^{***} (0.001)	0.0179^{***} (0.003)	0.0060^{***} (0.002)
Month7 (Jul)	0.0039^{***} (0.001)	$0.0053^{\dagger}$ (0.003)	0.0057^{***} (0.002)
Month8 (Aug)	0.0040^{***} (0.001)	0.0040 (0.003)	0.0056^{***} (0.002)
Month10 (Oct)	-0.0029^{**} (0.001)	-0.0028 (0.003)	-0.0021 (0.001)
Month11 (Nov)	-0.0013 (0.001)	-0.0016 (0.003)	0.001 (0.001)
Month12 (Dec)	0.0029^{**} (0.001)	0.0125^{***} (0.003)	0.0029^{*} (0.001)
Disease2 [J30.0-4]	0.0012^{*} (0.001)	-0.0099^{***} (0.001)	0.0020^{**} (0.001)
Disease3 [L23.8,9]	-0.0214^{***} (0.001)	-0.0423^{***} (0.002)	-0.0175^{***} (0.001)
Disease4 [K52.2, 3, 8, 9]	0.0360^{***} (0.001)	0.0126^{***} (0.003)	0.0407^{***} (0.001)
Disease5 [B35.2-6, 8, 9]	-0.0104^{***} (0.001)	-0.0301^{***} (0.003)	-0.006^{***} (0.001)
Disease6 [L20.8, 9]	0.0150^{***} (0.001)	0.0023 (0.003)	0.0138^{***} (0.002)
Disease7 [H60.1, 3, 5, 8, 9]	-0.0145^{***} (0.001)	-0.0325^{***} (0.003)	-0.0106^{***} (0.002)
Disease8 [S93.5, 6]	0.0075^{***} (0.001)	-0.0148^{***} (0.004)	0.0127^{***} (0.002)
Disease9 [S63.6, 7]	0.0083^{***} (0.002)	-0.0145^{**} (0.005)	0.0157^{***} (0.003)
Disease10 [J31.1, 2]	-0.0096^{***} (0.002)	-0.0259^{***} (0.005)	-0.0057^{**} (0.002)

Disease11 [B35.0, 1]	−0.3839* (0.153)	−0.5313 (0.356)	−0.0996*** (0.021)
Disease12 [J31.0]	−0.3546* (0.153)	−0.4959 (0.356)	−0.0692*** (0.021)
Disease13 [M50.9]	0.0295*** (0.004)	0.0093 (0.014)	0.0383*** (0.006)
Disease14 [L23.0-7]	−0.3871* (0.153)	−0.5290 (0.357)	−0.1042*** (0.021)
Disease15 [J20.0-2]	−0.3921** (0.153)	−0.5263 (0.357)	−0.1071*** (0.022)
Disease16 [S63.5]	−0.3466* (0.153)	−0.4872 (0.357)	−0.0555* (0.024)
Disease17 [M79.1, 4, 6, 8, 9]	0.0794*** (0.014)	0.0523 (0.041)	0.0874*** (0.021)
Disease18 [M77.2, 3, 5]	−0.3874* (0.153)	−0.5372 (0.358)	−0.0798** (0.028)
Disease19 [H60.0]	−0.3961** (0.153)	−0.5483 (0.357)	−0.0972*** (0.024)
Disease20 [M54.0, 1-6]	−0.2380 (0.154)	−0.4470 (0.361)	0.0727*** (0.021)
Disease21 [K52.1]	−0.3761* (0.154)	−0.5537 (0.357)	−0.0108 (0.041)
Disease22 [M50.3]	−0.3320* (0.154)	−0.4862 (0.360)	−0.1124** (0.037)
Disease23 [L20.0]	−0.3967** (0.154)	−0.5342 (0.357)	−0.0932 (0.065)
Disease24 [M54.8, 9]	0.1349† (0.082)	−0.0792† (0.047)	0.1622 (0.125)
Disease25 [M77.8, 9]	0.3475** (0.116)	0.4716 (0.345)	0.2696* (0.136)
R-square	0.006	0.006	0.006
Number of observations	1,077,928	155,418	559,383
F-statistics	116.9***	31.21***	54.32***

table D.2 Estimation Results of Supplementary Models on Healthcare Choices

Note) Standard errors are reported in parentheses. Baseline categories are "Female" in gender, "Middle-Income" in income, "The Other Areas" in residential area, and "Month 9" in month. †, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Supplementary material E : Propensity Score Matching

Propensity score matching (PSM) is a popular method to tackle selection bias due to observables. If there is any confounding which affects control/treatment allocation, the estimation result can be biased. To prevent this, researchers model the probability of treatment allocation, or propensity score and balance the propensity score distribution of treatment group with that of control group by matching samples.

Since treatment/control dataset is imbalanced and demographic descriptive statistics are not the same between treatment group/control group, we performed a propensity score matching. Although main LPM model takes a healthcare visit as unit observation, matching was performed at individual level. That is, we extracted patient lists who visited healthcare to treat treatment disease/control disease respectively and patients in the treatment group list were matched with patients in the control group. Healthcare records related to selected patients were used in the estimation.

We calculated propensity scores using a logistic regression followed by nearest neighbor matching based on Euclidian distance metric; one-to-one nearest neighbor match. We also utilized average medical expenses per visit before the policy, visit counts before the policy as well as demographic variables including gender, age, income level, residential area in the model.

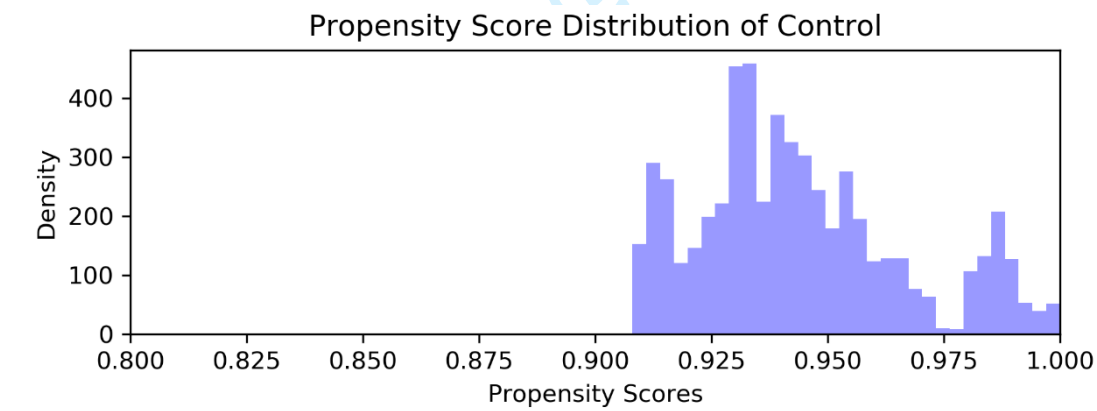


figure E.1 Propensity score distribution before matching

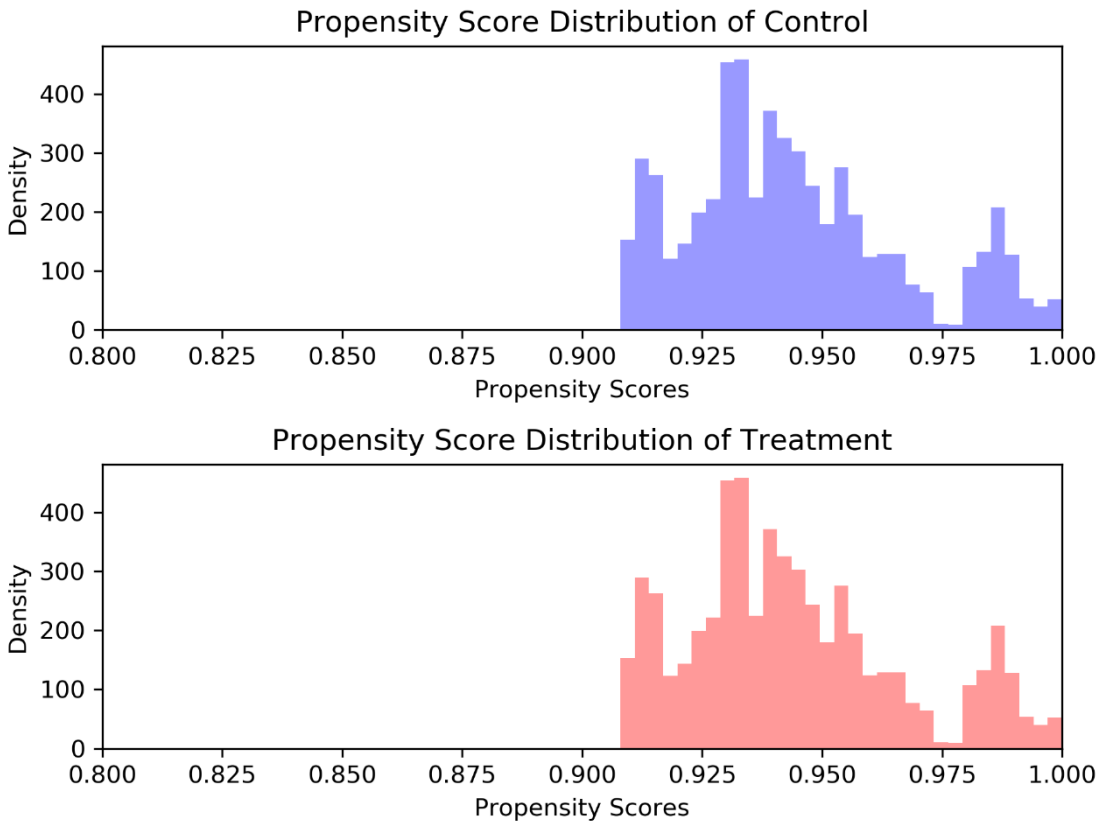


figure E.2 Propensity score distribution after matching

figure E.1 indicates that treatment/control group propensity score distribution shares the common propensity score region. This implies that the common support assumption is already supported. We conducted propensity score matching to make the supports of two groups more comparable. The estimation results using the matched datasets are provided in **table 2** (*Model 4*).

BMJ Open

Impact of Differential Copayment on Patient Healthcare Choice: Evidence from South Korean National Cohort Study

Journal:	*BMJ Open*
Manuscript ID	bmjopen-2020-044549.R1
Article Type:	Original research
Date Submitted by the Author:	15-Feb-2021
Complete List of Authors:	Jo, Sangkyun ; KAIST Jun, Duk Bin; KAIST Park, Sungho ; Seoul National University,
Primary Subject Heading:	Health policy
Secondary Subject Heading:	Health policy, Health economics, Public health
Keywords:	Health policy < HEALTH SERVICES ADMINISTRATION & MANAGEMENT, HEALTH ECONOMICS, Quality in health care < HEALTH SERVICES ADMINISTRATION & MANAGEMENT

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Impact of Differential Copayment on Patient Healthcare Choice: Evidence from South Korean National Cohort Study

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Competing Interests Statement
The authors have no affiliations with or involvement in any organization or entity with any financial interest, or non-financial interest in the subject matter or materials discussed in this manuscript.

Word counts: 5,341

Acknowledgements
This work was supported by the Ministry of Science and ICT, Republic of Korea (NRF-2019R1F1A1055160) and the Institute of Management Research at Seoul National University.

Impact of Differential Copayment Policy on Patient Healthcare Choice: Evidence from South Korean National Cohort Study

Abstract

Objective We evaluate the effectiveness of mild disease differential copayment policy aimed at reducing unnecessary patient visits to secondary/tertiary healthcare institutions in South Korea.

Design Retrospective study using difference-in-difference design

Setting Sample Research database provided by the Korean National Health Insurance Service, between 2010 and 2013.

Participants 206,947 patients who visited healthcare institutions to treat mild diseases during the sample period.

Methods A linear probability model with difference-in-difference approach was adopted to estimate the changes in patients' healthcare choices associated with the differential copayment policy. The dependent variable was a binary variable denoting whether a patient visited primary healthcare or secondary/tertiary healthcare to treat her/his mild disease. Patients' individual characteristics were controlled with a fixed effect.

Results We observed significant decrease in the proportion of patients choosing secondary/tertiary healthcare over primary healthcare by 2.99 percent point. The decrease associated with the policy was smaller by 14% in the low-income group compared to richer population, greater by 19% among the residents of Seoul metropolitan area than among people living elsewhere, and greater among frequent healthcare visitors by 33% than among people who less frequently visit healthcare.

Conclusion The mild disease differential copayment policy of South Korea was successful in discouraging unnecessary visits to secondary/tertiary healthcare institutions to treat mild diseases that can be treated well in primary healthcare.

Keywords: Differential copayment, Healthcare choice, Mild disease, Difference-in-difference, Primary healthcare

Strengths and limitations of this study

- The control group of this study provided a good counterfactual benchmark to precisely measure the change associated with the policy.
- Since the policy of this study pertains to mild diseases only, we could avoid the omitted variable problem due to unobserved disease severity.
- This study limited the subjects to similar pairs of mild diseases to construct a comparable control–treatment group setting.

INTRODUCTION

Excess demand for secondary and tertiary hospitals is a major healthcare challenge in many countries (e.g., China, Australia), resulting in overcrowding, long wait list, safety, and inefficiency issues in public health.[1–3] The South Korean government has also recognized it as a major problem and taken steps to address it.[4–6] In most countries, each tier of healthcare has its own role. In the case of South Korea (see Ministry of Health and Welfare Notification No. 2011-69), primary care should deal with outpatients for mild and common diseases, secondary care should deal with general hospitalizations and surgical care, and tertiary care should deal with treatments requiring high-level medical specialty. However, substantial proportion of mild disease patients visit secondary/tertiary hospitals. "Mild diseases" refer to the diseases with minor symptoms or illnesses designated by the Ministry of Health and Welfare and these diseases can mostly be treated well in primary healthcare.[7] In 2011, 4.7% of total patient visits to treat mild diseases were at secondary/tertiary healthcare facilities while the number of secondary/tertiary healthcare facilities was 319 (1.1%) and that of primary healthcare was 30,197 (98.9%). As patient visits to treat mild diseases increase, secondary/tertiary healthcare needs to allocate more resources to meet the demand, generating the inefficiency in attaining its main goal (i.e. to focus on severe or complicated cases).[8,9] Lee et al. [4] reported that among the outpatient usage of secondary/tertiary hospitals, approximately 85% can be sufficiently treated in primary healthcare.

A frequently used policy to tackle the excess demand problems in secondary and tertiary healthcare by governments is strengthening the gatekeeping role of the primary healthcare sector.[10,11] In many countries (e.g., the United Kingdom and the Scandinavian countries), patients cannot directly access secondary or tertiary healthcare without referral from primary healthcare.[12] Similarly, in South Korea, treatment at secondary or tertiary healthcare requires a referral letter from a primary care doctor. However, referral letters are frequently written at a patient's request and do not always reflect an actual need for care from higher-level hospitals.[13] Since the referral has no expiration date, the patient no longer needs a new referral when she/he visits to treat different diseases at the same department of the same hospital later. All in all, the South Korean referral system has failed in the gatekeeping role.

Another approach used to mitigate the excess demand problem is that of differential copayment.[14–16] In fact, the Korean government implemented a mild disease differential copayment policy in 2011. The policy imposed differential coinsurance rate on the prescribed medication when a patient visits healthcare due to mild disease. Before the policy, patients paid 30% of the prescribed medication cost regardless of the tier of healthcare he or she visited. After

the implementation of the policy, patients paid 40% (50%) of the cost when the prescription was issued at secondary (tertiary) healthcare. The coinsurance rate was maintained at 30% when the prescription is issued at primary healthcare. That is, the co-payment for medication increased by 33% or 67% when patient visited secondary or tertiary healthcare due to mild disease. Given the differential roles of secondary and tertiary healthcare, larger increase in the coinsurance rate was imposed on tertiary healthcare. The policy does not involve any cap on the cost of medication or the length of time the medication is required (In South Korea, prescriptions are usually valid for 3 days from the issued date. Medical institutions usually prescribes drugs for 14 days on average).[17] The rationale for the policy was that since the selected 52 diseases were mild ones that could be treated well in a primary healthcare, the extra cost would discourage patients from visiting secondary/tertiary healthcare institutions to treat these diseases.

The results from previous studies on the effects of differential copayment policies have been mixed.[15,18,19] Moreover, some study results should be interpreted with caution because many of them used aggregate measures (e.g., annual number of visits, total expenditures) without controlling for potential confounding effects. Huang and Tung [18] investigated if elderly Taiwanese patients' hospital tier choices have changed due to differential user charge using simple statistical tests (chi-square test, ANOVA, Scheffé test). They found that the impact was too small to be practically significant. Rosen et al. [20] investigated the effect of differential copayment on specialist visits in Israel using the difference-in-difference approach where they assigned medical beneficiaries who are exempted from the cost sharing as the control group. They found that the differential copayment policy failed to restrain visits to specialist physicians. As they noted, however, there were systematic differences between treatment group (non-medical beneficiaries) and control group (medical beneficiaries) and potential confounding was not ruled out. There have been a few empirical studies that investigated the effect of differential copayment policy of South Korea but they had the same limitations as the above cited papers – namely, no rigorous handling of the confounding effects.[21–23] Hone et al. [24] performed a systematic review to evaluate the impact of introducing differential user charges on healthcare service utilization. They found that the introduction of or increase in user charges for secondary care are associated with decreased secondary care utilization. However, they concluded that the impact of introducing differential user-charges on primary care utilization remains uncertain.

The main goal of this study is to examine the effectiveness of the differential copayment policy aimed at reducing unnecessary patient visits to secondary/tertiary healthcare institutions using detailed and representative individual-level data provided by the Korea National Health Insurance Services (KNHIS) and a difference-in-difference approach.

DATA

		Treatment disease	Control disease
		[B35.2-6, 8, 9] Dermatophytosis [H60.1, 3, 5, 8, 9] Otitis externa [J20.9] Acute bronchitis, unspecified [J30.0-4] Vasomotor and allergic rhinitis [J31.1, 2] Nasopharyngitis and pharyngitis [K52.2, 3, 8, 9] Other noninfective gastroenteritis and colitis [L20.8, 9] Atopic dermatitis [L23.8,9] Allergic contact dermatitis [M50.9] Cervical disc disorder, unspecified [M54.8, 9] Dorsalgia [M77.8, 9] Other enthesopathies [M79.1, 4, 6, 8, 9] Neuralgia and neuritis, unspecified [S63.6, 7] Dislocation, sprain and strain of joints and ligaments (hand) [S93.5, 6] Dislocation, sprain and strain of joints and ligaments (ankle/foot)	[B35.0, 1] Dermatophytosis [H60.0] Abscess of external ear [J20.0-2] Acute bronchitis [J31.0] Chronic rhinitis [K52.1] Toxic gastroenteritis and colitis [L20.0] Besnier's prurigo [L23.0-7] Allergic contact dermatitis [M50.3] Other cervical disc degeneration [M54.0, 1-6] Dorsalgia [M77.2, 3, 5] Other enthesopathies [M79.2] Neuralgia and neuritis, unspecified [S63.5] Sprain and strain of wrist
Variable	Categories of variable	Patients in treatment group (n=201,256)	Patients in control group (n=5,691)
Gender	Male	44.1%	42.6%
	Female	55.9%	57.4%
Age	Age_group1 (<20)	35.6%	14.4%
	Age_group2 (20's)	12.0%	8.8%
	Age_group3 (30's)	17.1%	17.8%
	Age_group4 (40's)	15.4%	23.8%
	Age_group5 (50's)	14.8%	26.2%
	Age_group6 (60's)	5.2%	9.1%
Income	Low (1–2 decile)	11.7%	12.9%
	Middle (3–8 decile)	55.4%	53.4%
	High (9–10 decile)	33.0%	33.8%
Residential area	Seoul-metro. area	55.9%	55.1%
	Other areas	44.1%	44.9%
Differential copayment policy (visit)	Pre-policy (1/1/2010 –9/30/2011)	Count: 394,316 (secondary/tertiary: 5.0%, primary: 95.0%)	Count: 6,452 (secondary/tertiary: 3.0%, primary: 97.0%)
	Post-policy (10/1/2011 –12/31/2012)	Count: 307,920 (secondary/tertiary: 4.0%, primary: 96.0%)	Count: 6,113 (secondary/tertiary: 5.0%, primary: 95.0%)

Table 1. Selected Diseases and Descriptive Statistics

Korean Standard Classification of Diseases-6 (KCD-6) code is shown in brackets. Detailed information on the selected disease is provided in Online Supplementary Material A.

This study used the Sample Research Data Base provided by the KNHIS, which provides mandatory social health insurance to all Koreans.[25,26] The dataset was designed and sampled to provide representative information regarding the healthcare usage of Koreans. Lee et al. [27] provided detailed explanation on the dataset. The 14-year cohort Sample Research Data Base

includes socioeconomic and demographic variables (e.g., gender, residential area, income level) and detailed information on medical treatments (e.g. medical diagnosis, type of medical facilities visited) for approximately 1 million people (2.2% of the total population) collected from 2002 to 2013. Recorded diagnoses follow the Korean Standard Classification of Diseases-6 (KCD-6) code, which is a slightly modified version of International Statistical Classification of Diseases and Related Health Problems, 10th revision (ICD-10).[28]

On October 1, 2011, the Korean government implemented a differential copayment policy with the most common 52 diseases. In 2018, the policy was extended to include additional 48 mild diseases to strengthen the effort to discourage unnecessary visits to secondary/tertiary healthcare to treat mild diseases by expanding the list.[29] We measured the impact of the policy by focusing on the initial implementation (sample period: January 2011–December 2012). Specifically, we constructed a set of treatment observations by selecting patient visits for the treatment of mild diseases selected from the set specified in 2011 (we refer to these as "treatment diseases") during the sample period. To construct a set of control observations, we selected patient visits whose purpose was to treat "control diseases" during the same sample period. The selected control diseases were similar to the treatment diseases (both belonged to the same middle-level categories in Korean Standard Classification of Diseases) and had been newly added in the 2018 extension (see **Table 1**). Consequently, our control observation provided a good counterfactual benchmark to precisely measure the change in patient behavior associated with the policy. We discuss key observations from summary statistics in **Online Supplementary Material B**.

We collected patient records of healthcare visits to treat the selected diseases between 2011 to 2012. Since we mainly examined the type of healthcare patients visited (i.e. primary vs. secondary/tertiary) and the change in this associated with the focal policy, we included initial visits to treat mild diseases in our sample but follow-up visits to treat the same disease in the same hospital were excluded. Moreover, we focused on primary diagnosis in the categorization of our observations. As a main empirical approach, we used a difference-in-difference method with patient fixed effect (we will provide more details in the next section). To this end, we included patients with two or more healthcare visits (follow-up visits are not counted) - specifically, at least one visit before the policy and one visit after the policy. Note that patients with only one visit are canceled out in the fixed effect estimation. Also, we only included patients younger than 65 years old since seniors (65+) are subject to a different cost sharing and insurance system. For the same reason, patients at the lowest income level (i.e., medical aid beneficiaries) were also excluded from the analysis. Key descriptive statistics of the selected samples are provided in **Table 1.**

ETHICS STATEMENT

The study protocol was approved by the Institutional Review Board of the Korea Advanced Institute of Science and Technology (KH2018-94). Informed consent was waived by the board.

METHODOLOGY

Our dataset has an unbalanced panel structure and the unit of analysis is a patient-visit. We adopted a linear regression model with patient-level fixed effect in our analysis. Accordingly, cluster standard errors were used in all inferences and the standard errors were clustered at individual patient level.[30] The dependent variable was whether the afflicted patients selected primary healthcare or secondary/tertiary healthcare in their visit to treat the focal diseases; thus, it is represented as a binary dummy variable (1 if secondary/tertiary healthcare was chosen and 0 if primary healthcare was chosen). This modeling approach is categorized as a linear probability model (LPM), where the estimated dependent variable can be interpreted as the probability of visiting secondary/tertiary healthcare rather than primary healthcare.[31–33]

We applied a difference-in-difference (DID) approach to measure the change in healthcare choice associated with the differential copayment policy. This method has been widely applied in previous studies to measure the impact of policies because it eliminates the effects of unobservable external factors by using control observations as counterfactuals.[18,34,35] Before applying the difference-in-difference approach, we checked the validity of our control observations by performing a parallel trend test to check whether the treatment and control observations followed the same pattern before the differential copayment policy and confirmed that they had the same trend (see **Online Supplementary Material C**).

Next, we defined the *"Treat"* dummy variable as 1 if a patient visit was to treat one of the treatment diseases and 0 if it was to treat one of the control diseases. We defined the *"Post"* dummy variable as 1 if the visit occurred after the policy implementation and 0 if it occurred before. We also included month dummy variables to capture time trends and/or seasonal variations in the dependent variable. To account for the differences in selected mild diseases, disease dummies are included. The change in healthcare choice associated with the differential copayment policy was measured using the coefficient of the interaction of *"Treat"* and *"Post."*

Furthermore, we added interaction terms to the base model to investigate how the changes in healthcare choices associated with the policy varied with key demographic variables. We refer to this model as Heterogenous DID Model (or Model 2). Specifically, we considered demographic

variables such as gender, income (low/middle/high), and residential area as categorical dummy variables. With respect to residential area, we looked at whether the patients lived in the Seoul metropolitan area or not. This was of interest because about 25 million people (50% of the country's population) live currently in the Seoul metropolitan area, where healthcare facilities and resources are highly concentrated.[36]

As stated, we define "*Treat*" variable based on whether the patient visit was to treat treatment disease or control disease. Accordingly, a patient can serve as treatment group in a visit but as control group in another visit. In contrast, in Model 3, we select patients who belong to only one group during the entire sample period and perform heterogenous DID analysis using them. Note that the assignment of treatment vs. control is at the individual patient level in this model. Next, we used Inverse Probability of Treatment Weighting (IPTW) to remove confounding from observable variables and then estimated the heterogeneous DID model (Model 4).

The specifications for the above-stated models are provided below.

DID with fixed effect (Model1)

$$Y_{it} = \alpha_i + \beta_T \cdot Treat_{it} + \beta_P \cdot Post_{it} + \beta_{DID} \cdot Treat_{it} \times Post_{it} + \tau_{Month} + \delta_{Disease} + u_{it} \quad \text{Eq. 1}$$

Heterogenous DID with fixed effect (Model2, Model3, Model4)

$$\begin{aligned} Y_{it} = \alpha_i + \beta_T \cdot Treat_{it} + \beta_P \cdot Post_{it} + \beta_{DID} \cdot Treat_{it} \times Post_{it} + \beta_{Male} \cdot Male_i \times Treat_{it} \times Post_{it} \\ + \beta_{LI} \cdot LowInc_i \times Treat_{it} \times Post_{it} + \beta_{HI} \cdot HighInc_i \times Treat_{it} \times Post_{it} \\ + \beta_{Metro} \cdot Metro_i \times Treat_{it} \times Post_{it} + \tau_{Month} + \delta_{Disease} + u_{it} \end{aligned} \quad \text{Eq. 2}$$

where i and t denote patient and healthcare visit, respectively, and Y_{it} is a binary indicator variable which takes the value of one if secondary/tertiary healthcare is visited by i at t and zero otherwise (i.e. primary healthcare visit). α_i is a patient-fixed effect which account for patient-specific characteristics in healthcare choice. τ_{Month} and $\delta_{Disease}$ are month- and disease-fixed effects to account for seasonality, time trend and disease-specific variations. $Male_i$, $LowInc_i$, $HighInc_i$, and $Metro_i$ are indicator variables denoting whether i is a male or not, i belongs to the low-income group or not, the high-income group or not, and i resides in the Seoul-metro area or not, respectively. u_{it} is an idiosyncratic error.

Patient and Public Involvement: No patient involved

RESULTS

Difference-in-Difference Analysis Using LPM

While the observations from descriptive statistics supported the effectiveness of the policy, we formally examined this after controlling for other effects such as unobserved patient-level characteristics, seasonal trend, and disease-specific characteristics using the proposed models. After confirming common trend between treatment and control group (see **Online Supplementary Material C**), the proposed fixed-effect LPM (Eq. 1) was estimated. Note that we used the within-estimator to handle the patient-level fixed effect.[37] The first column (Model 1) of **Table 2** presents the estimation result. Here, the coefficient of "*Treat*" indicates the estimated mean difference in the probability of selecting secondary/tertiary healthcare between the treatment and control observations. The coefficient of *"Post"* indicates the estimated change in the probability after policy implementation. We captured the effect of the policy through the interaction of *"Post"* and "*Treat*" represented as *"Post x Treat"*.

In Model 1, the coefficient of *"Treat"* (-0.3722, 95% CI -0.9149 to 0.1705) was not statistically significant, indicating that there is no significant difference between treatment and control observations in choosing secondary/tertiary healthcare over primary healthcare. In contrast, the coefficient of *"Post"* (0.0235, 95% CI 0.0167 to 0.0303) was positive and significant, indicating that the proportion of secondary/tertiary healthcare visits among the control diseases increased after policy implementation. More importantly, the DID term related to the policy effect (*"Post x Treat"*) was negative and significant (-0.0299, 95% CI -0.0368 to -0.0230). That is, the decrease associated with the policy was 2.99% point. From our data, we found that 4.93% of visits to treat mild diseases headed for secondary/tertiary healthcare before the policy. If we use this number as a baseline, the decrease amounts to -60%.

Heterogeneous Policy Effect

After verifying the effectiveness of the policy, we conducted additional analyses to examine the heterogeneity associated with the policy among different demographic groups. To this end, we added triple interaction terms between *"Post×Treat"* and dummies for gender, income, and residential area (Eq. 2). The estimation results are reported in **Table 2** (Model 2).

In Model 2, the coefficient of *"Post×Treat"* is highly significant and negative (-0.0267, 95% CI -0.0337 to -0.0197), indicating that the policy was associated with the decrease in the probability of choosing secondary/tertiary healthcare instead of primary healthcare. The coefficient of

"Post×Treat×LowInc" (0.0037, 95% CI 0.0007 to 0.0067) was statistically significant, while the coefficient of *"Post×Treat×HighInc"* (-0.0005, 95% CI -0.0026 to 0.0016) was not statistically significant. This indicates that the differential copayment policy was associated with a smaller decrease in the probability of choosing secondary/tertiary over primary healthcare among people in the low income group than among the others in the middle/high-income group. Specifically, *"Post×Treat"* is -0.0267 in Mid-Income group (baseline category) but the estimate becomes -0.0230 (=-0.0267+0.0037) in low-income group. We found that 4.23% of visits to treat mild diseases were at secondary/tertiary healthcare in low-income group before the policy. When we use this number as a baseline, the change associated with the policy amounts to -54%.

Similarly, the coefficient of *"Post×Treat×Metro"* (-0.0052, 95% CI -0.0072 to -0.0032) was significant and negative, revealing that the decrease in the probability of choosing secondary/tertiary over primary healthcare was larger by 19% among the residents of Seoul metropolitan area than among people living elsewhere. Specifically, *"Post×Treat"* is -0.0267 in the other areas (baseline category) but the estimate becomes -0.0319 (=-0.0267-0.0052) in low-income group. We found that 4.6% of visits to treat mild diseases were at secondary/tertiary healthcare in Seoul metropolitan area before the policy. When we use this number as a baseline, the change associated with the policy amounts to -69%.

Exclusion of Patients Who Have Both Treatment and Control Observations

We defined treatment and control observations based on the disease – whether the disease is influenced by the policy or not. Accordingly, a patient can have both treatment and control observations. In Model 3, in contrast, we selected the patients who visited healthcare due to treatment diseases only or control diseases only. As a result, our assignment of samples to treatment and control groups was not varying within a patient. The main purpose of this analysis is to tackle a potential problem of diagnosis code change to avoid increased cost due to the policy. If there were frequent and common code changes, many patients in treatment group would have moved to control group after the policy. Therefore, this exclusion of patients who have both treatment/control visits allows us to circumvent the issue of diagnosis code change. First, we note that we dropped only 14,934 observations (2% out of 714,802 observations) from this additional screening rule. This indicates that the diagnosis code change, if any, is not frequent. Model 3 in **Table 2** represents the estimation results from this model. Note that the "*Treat*" variable became time invariant in Model 3 and was absorbed into the fixed effect term. Overall, the main findings from Model 3 are highly consistent with those from Model 1 and Model 2. This adds robustness to our findings.

	DID	Heterogenous DID	Using Treatment Only and Control Only Patients	IPTW
	Model 1 β (std. err.)	Model 2 β (std. err.)	Model 3 β (std. err.)	Model 4 β (std. err.)
Month Dummies	Yes	Yes	Yes	Yes
Disease Dummies	Yes	Yes	Yes	Yes
Patient Fixed Effect	Yes	Yes	Yes	Yes
Post	0.0235 *** (0.003)	0.0235 *** (0.003)	0.0238 *** (0.004)	0.0236 *** (0.004)
Treat	−0.3722 (0.277)	−0.3738 (0.276)	–	−0.4257 (0.305)
Post × Treat	− 0.0299 *** (0.004)	−0.0267 *** (0.004)	− 0.0270 *** (0.004)	−0.0268 *** (0.004)
Post × Treat × Male	–	−0.0025 * (0.001)	−0.0026 * (0.001)	−0.0025 * (0.001)
Post × Treat × Low-Income	–	0.0037 * (0.002)	0.0035 * (0.002)	0.0039 * (0.002)
Post × Treat × High-Income	–	−0.0005 (0.001)	−0.0005 (0.001)	0.0005 (0.001)
Post × Treat × Seoul Metro. Area	–	−0.0052 *** (0.001)	−0.0052 *** (0.001)	−0.0052 *** (0.001)
R-square	0.006	0.006	0.006	0.006
Number of observations	714,801	714,801	699,867	714,801
F-statistics	84.22 ***	77.49 ***	77.57 ***	79.21 ***

Table 2. Results of Linear Probability Models on Patient Healthcare Choice

The full estimation results are available in **Online Supplementary Material D**. *: p<0.05 / **: p<0.01 / ***: p<0.001

Inverse Probability of Treatment Weighting

We used Inverse Probability of Treatment Weighting (IPTW) to remove confounding from observable variables and then estimated the heterogenous DID model (Eq.2).[38,39] Details on our IPTW procedure is provided in **Online Supplementary Material E**. Model 4 in Table 2 reports the estimation results. We found that a highly significant decrease in the probability of choosing secondary/tertiary over primary healthcare was associated with the implementation of differential copayment policy (-0.0268, 95% CI -0.0343 to -0.0193). Also, almost all estimated coefficients have

the same sign and similar magnitude as the estimates from the other models.

	Extended Sample Period (2010-13)	Split Sample Using Visit Count	
		# of Visits > 5	# of Visit ≤ 5
Model	Model 5 β (std. err)	Model 6 β (std. err)	Model 7 β (std. err)
Month Dummies	Yes	Yes	Yes
Disease Dummies	Yes	Yes	Yes
Patient Fixed Effect	Yes	Yes	Yes
Post	0.0199 *** (0.003)	0.0262 *** (0.007)	0.0230 ** (0.004)
Treat	− 0.3628 * (0.153)	−0.4919 (0.356)	− 0.0759 ** (0.021)
Post × Treat	− 0.0218 *** (0.003)	− 0.0335 *** (0.008)	− 0.0252 *** (0.004)
Post × Treat × Male	− 0.0025 ** (0.001)	−0.0013 (0.003)	− 0.0027 * (0.001)
Post × Treat × Low-Income	0.0028 * (0.001)	0.0118 ** (0.004)	0.0016 (0.002)
Post × Treat × High-Income	−0.0001 (0.001)	0.0001 (0.003)	−0.0007 (0.001)
Post × Treat × Seoul Metro. Area	− 0.0066 *** (0.001)	− 0.0076 ** (0.003)	− 0.0044 *** (0.001)
R-square	0.006	0.006	0.006
Number of observations	1,077,928	155,418	559,383
F-statistics	116.90 ***	31.21 ***	54.32 ***

Table 3. Results of Additional Models to Check Robustness

*: p<0.05 / **: p<0.01 / ***: p<0.001. The full estimation results are available online in **Online Supplementary Material D**.

Extended Sample Period

The policy change may take some time until the actual effect shows up or the effect can be short-lived, disappearing soon after the implementation. To further investigate this aspect of the policy, we extended our sample period from 2011-2012 to 2010-2013. We estimated the Heterogenous DID model (Eq. 2). Model 5 in **Table 3** provides the estimation results. As in other

model results, the coefficient of *"Post×Treat"* is highly significant and negative (-0.0218, 95% CI -0.0273 to -0.0163). Overall results using the extended sample period echoed our earlier findings from the other models. Next, we examined how patients' healthcare choices varied over time after the policy. To this end, we interacted dummies for the months after the policy with *"Treat."* We found that the change associated with the policy showed stable pattern rather than showing increasing or decreasing trends. We provide more detailed description on the model and results in **Online Supplementary Material G**.

Policy Effect and Visit Frequency

Patients who visit healthcare facilities more frequently than others would be subject to a greater financial burden if they do not change their behavior after the policy. In contrast, low-frequency patients might be more willing to pay the increased cost. If this is the case, the policy might be more effective among frequent visitors. To further examine this in our empirical context, we decomposed our sample into two using the number of healthcare visits to treat mild diseases during the sample period: i) 5 times or less, ii) above 5 times. Model 6 and Model 7 in **Table 3** show the estimation results using frequent visitors and the others, respectively. In the frequent visitor sample (Model 6), the coefficient of "*Post×Treat* " (-0.0335, 95% CI -0.0485 to -0.0185) is much larger than any other models. In contrast, from the less frequent visitor sample (Model 7), we found that the coefficient of "*Post×Treat* " (-0.0252, 95% CI -0.0330 to -0.0174) is smaller in magnitude than those of Model 1, Model 2, and Model 3. These results imply that the decrease in the visits to secondary/tertiary healthcare associated with the policy was stronger among frequent healthcare visitors. We also found that the coefficient of *"Post×Treat×LowInc"* was statistically significant in Model 6 (0.0118, 95% CI 0.0035 to 0.0201) while it was insignificant in Model 7. This finding indicates that the substantially smaller decrease in visits to secondary/tertiary over primary healthcare among low income patients was mainly driven by low-income frequent visitors. Both models had significant and negative coefficients of *"Post×Treat×Metro"*, which is consistent with all the other models.

Additional Robustness Checks

In our analysis, the control diseases are very similar to treatment diseases. This setting has some strengths but at the same time may suffer from some potential problems. For instance, doctors may change the diagnoses to ensure patients have low copayments. To mitigate this issue, we selected distinct set of mild diseases as control diseases and treatment diseases in a follow-up analysis (see **Online Supplementary Material F**) and obtained the high consistent results.

Next, we performed an analysis using seniors with the age of 65 and above in the sample. Note that these group of people are not subject to the policy. Since there is no change in the policy, we do not expect any significant change in their healthcare choices. Moreover, if the significant result of our main model comes from some other latent effects that change over time, we should also find significant DID effect in the analysis using these seniors. This analysis can be regarded as a placebo test or a pseudo shock test to add validity to our findings. We found that there is no significant change due to the policy in the senior group (see **Online Supplementary Material F**). All in all, we think that additional analyses have substantially improved the robustness of our findings.

DISCUSSION

Our study had several noteworthy strengths. First, we used a quasi-experimental setting with the difference-in-difference approach to precisely measure the policy's impact. Our control observation provided the ideal counterfactual benchmark to measure the effectiveness of the policy. Moreover, a series of robustness checks add validity to our findings. Second, the focal policy covered only mild diseases, allowing us to circumvent the omitted variable problem due to unobserved severity. Previous studies have looked at the impact of healthcare policies applied to wide variety of diseases for which patients' condition severity may also vary widely but remain unobserved by researchers.[40] In such cases, omitted disease severity becomes a critical challenge in measurement of a policy's effect. In contrast, our study examined a policy on mild diseases with only small variations in severity. Accordingly, we can circumvent the omitted variable problem due to unobserved severity.

We found that the South Korean government's 2011 differential copayment policy was significantly associated with the decrease in patients' unnecessary choice of secondary/tertiary healthcare over primary healthcare for mild diseases. This finding is consistent with the results from previous empirical studies. For example, researchers found that the introduction of or increase in user charges for secondary care are associated with decreased secondary care utilization.[41] The changes associated with the policy differed across demographic groups. Specifically, the decrease was smaller among low income patients compared to richer patient groups. This result is distinct from those in several previous empirical studies in which many researchers have found that people with low income are more sensitive to cost sharing changes and that policies based on cost sharing can exacerbate medical inequality.[42–46] For example, Powell-Jackson et al. [24,47] reported that user charge intervention increases primary healthcare utilization only in the lowest and middle income terciles. The distinctiveness of our results can be explained by the Korean differential

copayment policy focusing only on mild diseases. Before the policy, people in middle- and high-income groups visited secondary/tertiary healthcare more frequently than people in the low-income brackets. Since most of the visits by middle- and high-income people to secondary/tertiary healthcare institutions could have been handled just as well by primary healthcare institutions, their adjustment in healthcare choices after the policy implementation could be more pronounced. This finding is consistent with a stream of research that showed that carefully designed copayment policies can reduce disparity in healthcare access and usage (e.g. [48]).

The smaller changes associated with the policy among low-income people might be derived from the difference in the level of health information each group has regarding the policy. People with lower incomes tend to have poorer healthcare information compared to people with higher incomes.[49] Since they are poorly-informed regarding the policy and the increase in the cost sharing payment, their adjustment in healthcare choices after the policy could be weak. We also found that the smaller change associated with the policy among low income people was not limited to short period after the policy implementation but lasted for extended period of time (27 months afterward). Moreover, our analyses indicated that this heterogeneity along with the income was mainly driven by the patients with frequent healthcare visits. This finding implies that the government can fulfill its policy goal more effectively by enhancing information sharing, especially focusing on low-income frequent healthcare visitors.

Another interesting finding from our study was that the change in healthcare choices associated with the policy was greater among the people living in the Seoul metropolitan area than among people living in other areas. Healthcare resources are concentrated in the Seoul metropolitan area. For instance, according to Statistics Korea, the number of doctors per thousand was 3.5 in Seoul area but was only 2.2 in other areas in 2011. Because there are more healthcare facilities overall in the Seoul metropolitan area, people there may find suitable primary healthcare institutions to substitute for secondary/tertiary healthcare institutions more easily than the people in other areas whose choices may be more limited. This might explain the pronounced policy effect in the Seoul metropolitan area. This finding points out that it is important to make primary healthcare outside the Seoul metropolitan area more accessible and attractive to patients.

A few limitations of this study should be noted. As is the case with most studies using observed data, it is difficult to estimate the causal effect of the policy in a non-experimental setting. Since experimentation in our context had several challenges, including ethical issues, an experimental study was not feasible. Instead, we tried to control the effects of confounding variables by using control variables, fixed effects, and control observations. We also performed a series of

robustness tests to check the validity of our findings. In our study, we mainly investigated the changes in healthcare choices associated with a differential copayment policy. Another important variable is the number of consultations. We leave this as a future research agenda. Moreover, future study can investigate whether the patient visits differed between those who attended secondary and tertiary hospitals. A potential weakness of our sample is that doctors may change the diagnoses to ensure patients have low copayments. To mitigate this issue, we selected distinct set of mild diseases as control diseases and treatment diseases in a follow-up analysis (**Online Supplementary Material F**). Nevertheless, we acknowledge that the issue of disease code change cannot be fully ruled out. We also assume that there is no spillover effect due to changes in behaviors in our analysis.

CONCLUSION

We investigated the effect of the mild disease differential copayment policy introduced in South Korea in 2011 using the Sample Research Data Base provided by the KNHIS, conducting a difference-in-difference analysis with a quasi-experimental design. We found that a significant decrease in the proportion of patients choosing secondary/tertiary healthcare facilities over primary healthcare facilities was associated with the implementation of the policy. The change was pronounced among people with middle/high incomes, those living in the Seoul metropolitan area, and those who frequently visited healthcare facilities to treat mild diseases. We performed a series of robustness checks and found all our results to be highly consistent.

Contributors We confirm that all the authors have made substantive intellectual contributions to the paper; SP contributed to the design and the interpretation of the study after reviewing the result of the study. SKJ conceptualized the study and analyzed the data. DBJ contributed the data acquisition and provided statistical analysis support. All authors supplied critical revisions to the manuscript.

Funding This work was supported by the Ministry of Science and ICT, Republic of Korea (NRF-2019R1F1A1055160) and the Institute of Management Research at Seoul National University.

Competing interests None declared.

Ethics approval We obtained approval from the institutional review board of Korea Advanced Institute of Science and Technology (KH2018-94).

Provenance and peer review Not commissioned; externally peer reviewed.

Data sharing statement Data may be obtained from a third party and are not publicly available. We used the Sample Research Database from the Korea National Health Insurance Services (KNHIS). The same dataset can be obtained from KNHIS at nominal fee if the purpose of use is approved by KNHIS.

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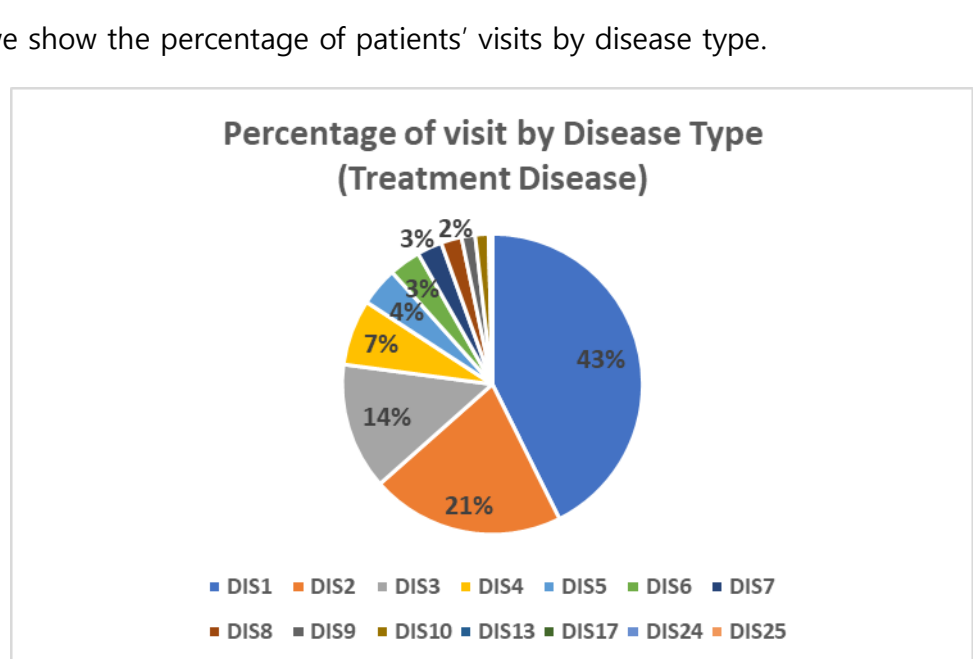
Online Supplementary Material

Supplementary Material A: Control Diseases and Treatment Diseases

Below we show the percentage of patients' visits by disease type.



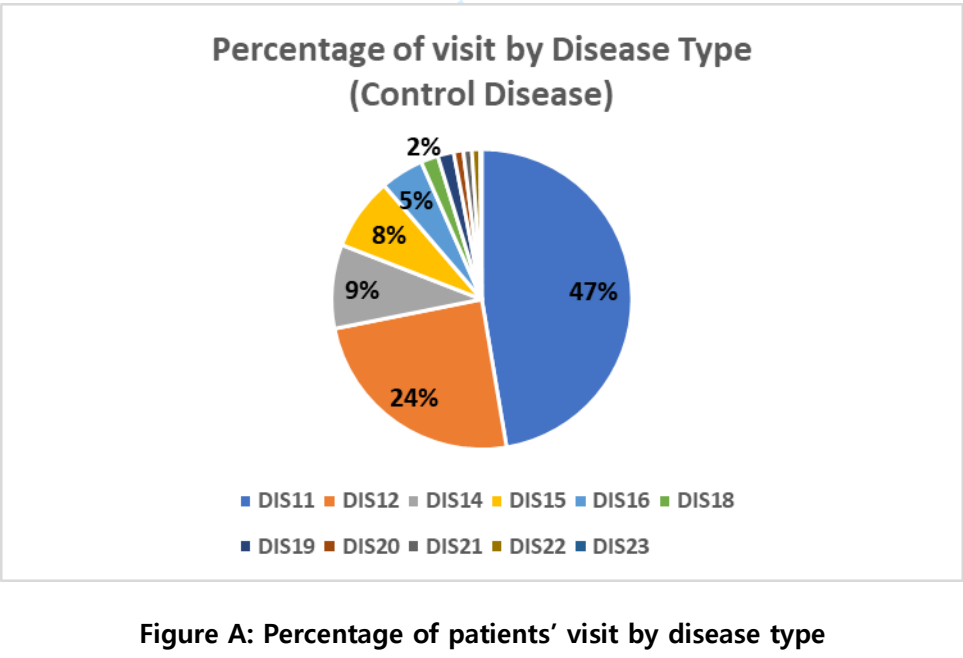


Figure A: Percentage of patients' visit by disease type

DIS1 [J20.9]	DIS2 [J30.0-4]	DIS3 [L23.8,9]	DIS4 [K52.2, 3, 8, 9]	DIS5 [B35.2-6, 8, 9]
DIS6 [L20.8, 9]	DIS7 [H60.1, 3, 5, 8, 9]	DIS8 [S93.5, 6]	DIS9 [S63.6, 7]	DIS10 [J31.1, 2]
DIS11 [B35.0, 1]	DIS12 [J31.0]	DIS13 [M50.9]	DIS14 [L23.0-7]	DIS15 [J20.0-2]
DIS16 [S63.5]	DIS17 [M79.1, 4, 6, 8, 9]	DIS18 [M77.2, 3, 5]	DIS19 [H60.0]	DIS20 [M54.0, 1-6]
DIS21 [K52.1]	DIS22 [M50.3]	DIS23 [L20.0]	DIS24 [M54.8, 9]	DIS25 [M77.8, 9]

Supplementary Material B: Observations from Summary Statistics

There were 12,565 control and 702,236 treatment observations. Secondary/tertiary healthcare visits accounted for 5.0 % of all treatment observations before the policy, and this number decreased to 4.0% after the policy. In the control observations, in contrast, secondary/tertiary healthcare visits accounted for 3.0% before the policy, and this number increased to 5.0% after the policy. These statistics provides two important implications. First, it reflects serious circumstances of the Korean medical delivery system. According to the "Major Health Insurance Statistics 2016" published by Health Insurance Review and Assessment Service, the number of secondary/tertiary healthcare facilities was less than 1%. Even though mild diseases can be sufficiently treated in primary healthcare, 4~5% of mild diseases visits are heading for secondary/tertiary healthcare. Consequently, secondary/tertiary healthcare have a difficulty in intensively treating severely diseases. Second, these observations from summary statistics imply that the differential copayment policy was effective in nudging patients to select primary healthcare instead of secondary/tertiary healthcare in treating their mild diseases. Under the circumstances that the concentration on secondary/tertiary healthcare is getting worse (increasing share of medical expenditure/number of visit days)[1], the policy seems to have alleviated the situation.

We observed that the proportions of visits to secondary/tertiary healthcare to treat the control diseases increased (**Figure B**). Next, we checked whether this trend was common for other mild diseases during the sample period. we computed the proportion of secondary/tertiary visits for all 48 of the mild diseases that had not been included in the policy in 2011 but were added in the 2018 policy extension. We found that the proportion of secondary/tertiary visits overall increased from 4.3% in the before the policy (January 2011–September 2011) to 5.7% after the policy (October 2011–December 2012), showing a pattern similar to that of our selected control diseases. These observations from summary statistics also revealed an increasing demand for secondary/tertiary healthcare even for mild diseases, justifying the implementation of the differential copayment policy that was the focus of our study.

[1] Health Insurance Major Statistics 2016,
Lee, et al. " Analysis of the current status of medical usage in tertiary hospitals and measures to normalize the role." (2019) – Health Insurance Review and Assessment report.

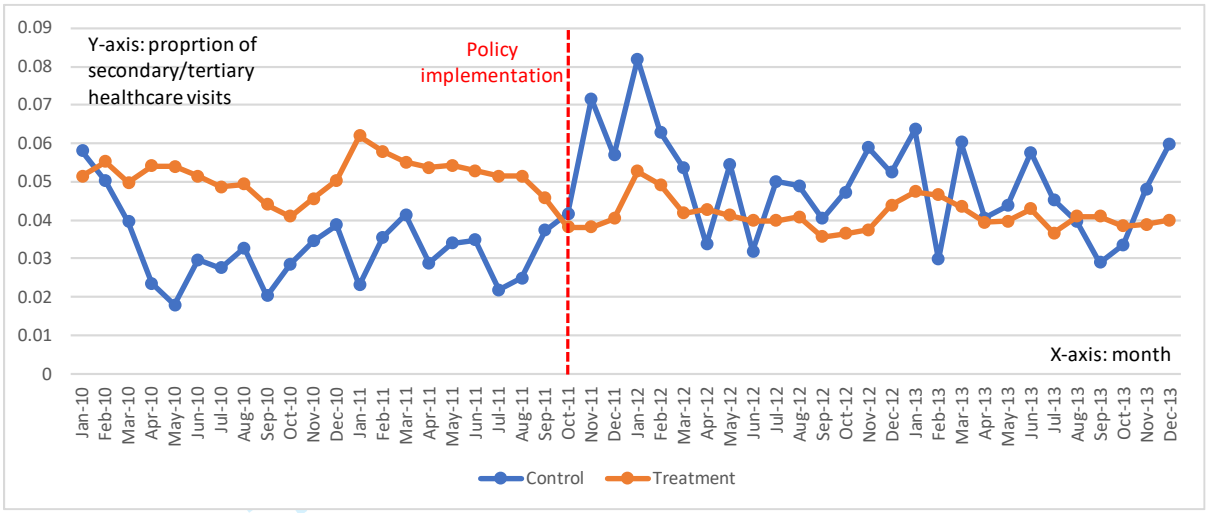


Figure B. Proportion of visiting secondary/tertiary healthcare facilities (2010-2013)

Proportion of visiting secondary/tertiary healthcare from 2010 to 2013. The dotted vertical line represents the implementation of differential coefficient policy in Oct. 2011.

Supplementary Material C: Common Trend Test

We check whether the control observations and treatments observations share the same trend before the policy implementation. To this end, we estimate the following fixed effect model (within estimator):

$$Y_{it} = c_i + \beta_T(Treat_{it}) + \beta_{m1}(Month1_{it}) + \cdots + \beta_{m20}(Month20_{it}) + \beta_{int1}(Month1 \times Treat)_{it} + \cdots + \beta_{int20}(Month20 \times Treat)_{it} + u_{it}$$

where i denotes patient, t denotes t-th healthcare visit, and c_i is the patient fixed effect. Y_{it} is equal to 1 if secondary/tertiary healthcare is selected and zero if primary healthcare is selected in the observation. $Month1_{it}, \ldots, Month20_{it}$ are monthly dummy variables. $treat_{it}$ is equal to one if the observation is a treatment observation and zero if the observation is a control observation. Month 21 (September 2011) is the month just before the policy implementation and we use this as the baseline (omitted category). **Table C** reports the estimation results. Note that the estimated coefficients of interactions of "Treat" and month dummies capture the differences between the treatment and the control observations each month before the policy implementation. **Figure C** graphically shows the estimates. All the estimates are insignificant, indicating that the treatment and the control observations have a common trend. This result indicates that our control observations provide good counterfactuals to estimate the impact of the differential copayment policy.

Variable	Year/Month	Estimates	Standard Error	P-value
Treat	-	0.0133	0.023	0.559
Month1 X Treat	Jan-10	-0.0251	0.031	0.415
Month2 X Treat	Feb-10	-0.0214	0.027	0.432
Month3 X Treat	Mar-10	-0.0304	0.03	0.310
Month4 X Treat	Apr-10	0.0421	0.026	0.107
Month5 X Treat	May-10	0.0187	0.024	0.430
Month6 X Treat	Jun-10	-0.0009	0.029	0.976
Month7 X Treat	Jul-10	-0.0071	0.027	0.789
Month8 X Treat	Aug-10	-0.0169	0.027	0.525
Month9 X Treat	Sep-10	-0.0177	0.027	0.514
Month10 X Treat	Oct-10	0.0016	0.026	0.952
Month11 X Treat	Nov-10	-0.0123	0.027	0.648
Month12 X Treat	Dec-10	-0.0147	0.028	0.597
Month13 X Treat	Jan-11	-0.0243	0.037	0.514
Month14 X Treat	Feb-11	0.0146	0.035	0.676
Month15 X Treat	Mar-11	0.0028	0.031	0.929
Month16 X Treat	Apr-11	0.0055	0.026	0.835
Month17 X Treat	May-11	0.0203	0.029	0.483

Month18 X Treat	Jun-11	0.0135	0.026	0.605
Month19 X Treat	Jul-11	-0.0295	0.028	0.296
Month20 X Treat	Aug-11	0.0072	0.025	0.772
Month1	Jan-10	0.0294	0.031	0.337
Month2	Feb-10	0.0314	0.027	0.245
Month3	Mar-10	0.0367	0.03	0.218
Month4	Apr-10	-0.0297	0.026	0.252
Month5	May-10	-0.0037	0.023	0.875
Month6	Jun-10	0.0119	0.029	0.677
Month7	Jul-10	0.0069	0.027	0.794
Month8	Aug-10	0.0210	0.026	0.424
Month9	Sep-10	0.0125	0.027	0.642
Month10	Oct-10	-0.0052	0.026	0.84
Month11	Nov-10	0.0085	0.027	0.753
Month12	Dec-10	0.0213	0.028	0.443
Month13	Jan-11	0.0401	0.037	0.28
Month14	Feb-11	0.0036	0.035	0.917
Month15	Mar-11	0.0064	0.031	0.836
Month16	Apr-11	0.0045	0.026	0.862
Month17	May-11	-0.0041	0.029	0.886
Month18	Jun-11	-0.0076	0.026	0.769
Month19	Jul-11	0.0318	0.028	0.256
Month20	Aug-11	-0.0064	0.025	0.795

Table C. Result of Common Trend Test

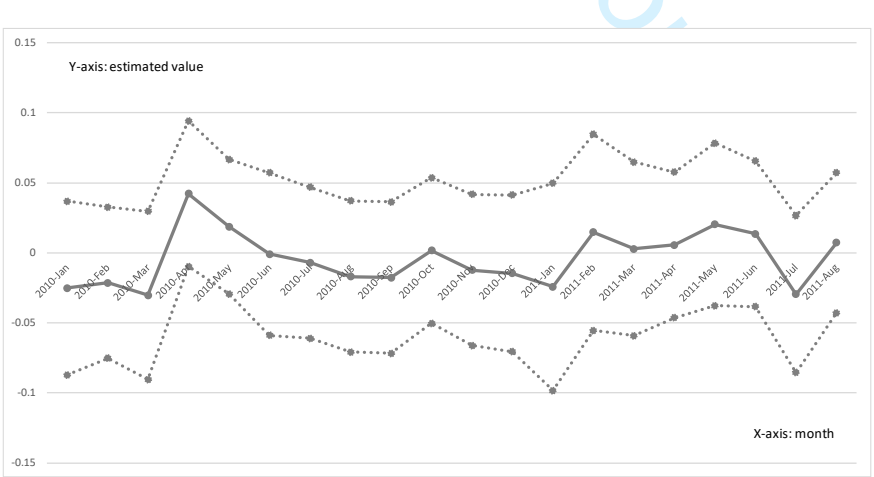


Figure C: Common Trend Test Results

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

Supplementary Material D: Full Estimation Results

We provide the full estimation results of Models 1 to 4 in Table D.1 and those of Models 5 to 7 in Table D.2.

	Main DID	Heterogeneity DID	Excluding Both T/C visitor	IPTW
	Model 1 β (std. err)	Model 2 β (std. err)	Model 3 β (std. err)	Model 4 β (std. err)
Post	0.0235^{***} (0.003)	0.0235^{***} (0.003)	0.0238^{***} (0.004)	0.0236^{***} (0.004)
Treat	-0.3722 (0.277)	-0.3738 (0.276)	–	-0.4257 (0.305)
Post× Treat	-0.0299^{***} (0.004)	-0.0267^{***} (0.004)	-0.0270^{***} (0.004)	-0.0268^{***} (0.004)
Post× Treat× Male	–	-0.0025^{*} (0.001)	-0.0026^{*} (0.001)	-0.0025^{*} (0.001)
Post× Treat×Low-Income	–	0.0037^{*} (0.002)	0.0035^{*} (0.002)	0.0039^{*} (0.002)
Post× Treat×High-Income	–	-0.0005 (0.001)	-0.0005 (0.001)	-0.0005 (0.001)
Post× Treat×Seoul Metro. Area	–	-0.0052^{***} (0.001)	-0.0052^{***} (0.001)	-0.0052^{***} (0.001)
Month1 (Jan)	0.0121^{***} (0.001)	0.0122^{***} (0.001)	0.0121^{***} (0.001)	0.0123^{***} (0.001)
Month2 (Feb)	0.0127^{***} (0.001)	0.0127^{***} (0.001)	0.0128^{***} (0.002)	0.0126^{***} (0.001)
Month3 (Mar)	0.0080^{***} (0.001)	0.0080^{***} (0.001)	0.0081^{***} (0.001)	0.0080^{***} (0.001)
Month4 (APR)	0.0102^{***} (0.001)	0.0103^{***} (0.001)	0.0103^{***} (0.001)	0.0102^{***} (0.001)
Month5 (May)	0.0111^{***} (0.001)	0.0112^{***} (0.001)	0.0113^{***} (0.001)	0.0110^{***} (0.001)
Month6 (Jun)	0.0092^{***} (0.001)	0.0092^{***} (0.001)	0.0091^{***} (0.001)	0.0092^{***} (0.001)
Month7 (Jul)	0.0055^{***} (0.001)	0.0055^{***} (0.001)	0.0055^{***} (0.001)	0.0055^{***} (0.001)
Month8 (Aug)	0.0051^{***} (0.001)	0.0051^{***} (0.001)	0.0049^{***} (0.001)	0.0053^{***} (0.001)
Month10 (Oct)	-0.0025^{*} (0.001)	-0.0025^{*} (0.001)	-0.0023 (0.001)	-0.0026^{*} (0.001)
Month11 (Nov)	0.0003 (0.001)	0.0003 (0.001)	0.0004 (0.001)	0.0002 (0.001)
Month12 (Dec)	0.0052^{***} (0.001)	0.0052^{***} (0.001)	0.0053^{***} (0.001)	-0.0051^{***} (0.001)
Disease2 [J30.0-4]	-0.0009 (0.001)	-0.0009 (0.001)	-0.001 (0.001)	-0.0008 (0.001)
Disease3 [L23.8,9]	-0.0234^{***} (0.001)	-0.0234^{***} (0.001)	-0.0235^{***} (0.001)	-0.0233^{***} (0.001)
Disease4 [K52.2, 3, 8, 9]	0.0337^{***} (0.001)	0.0336^{***} (0.001)	0.0338^{***} (0.001)	0.0334^{***} (0.001)
Disease5 [B35.2-6, 8, 9]	-0.0109^{***} (0.001)	-0.0109^{***} (0.001)	-0.0108^{***} (0.001)	-0.0110^{***} (0.001)
Disease6 [L20.8, 9]	0.0110^{***} (0.002)	0.0109^{***} (0.002)	0.0108^{***} (0.002)	0.0110^{***} (0.002)

Disease7 [H60.1, 3, 5, 8, 9]	-0.0159^{***} (0.001)	-0.0159^{***} (0.001)	-0.0158^{***} (0.001)	-0.0159^{***} (0.001)
Disease8 [S93.5, 6]	0.0066^{***} (0.002)	0.0066^{***} (0.002)	0.0065^{***} (0.002)	0.0067^{***} (0.002)
Disease9 [S63.6, 7]	0.0090^{***} (0.002)	0.0090^{***} (0.002)	0.0085^{***} (0.002)	0.0095^{***} (0.002)
Disease10 [J31.1, 2]	-0.0105^{***} (0.002)	-0.0106^{***} (0.002)	-0.0104^{***} (0.002)	-0.0107^{***} (0.002)
Disease11 [B35.0, 1]	-0.3988 (0.277)	-0.4003 (0.276)	-0.0900^{***} (0.023)	-0.4518 (0.305)
Disease12 [J31.0]	-0.3665 (0.277)	-0.3681 (0.276)	-0.0548^{*} (0.023)	-0.4204 (0.305)
Disease13 [M50.9]	0.0327^{***} (0.005)	0.0328^{***} (0.005)	0.0337^{***} (0.005)	0.0319^{***} (0.005)
Disease14 [L23.0-7]	-0.4006 (0.277)	-0.4019 (0.276)	-0.0928^{***} (0.022)	-0.4530 (0.305)
Disease15 [J20.0-2]	-0.4016 (0.277)	-0.403 (0.276)	-0.1036^{***} (0.024)	-0.4518 (0.305)
Disease16 [S63.5]	-0.3550 (0.277)	-0.3564 (0.276)	-0.0267 (0.028)	-0.4137 (0305)
Disease17 [M79.1, 4, 6, 8, 9]	0.0799^{***} (0.018)	0.0798^{***} (0.018)	0.0818^{***} (0.019)	-0.0776^{***} (0.018)
Disease18 [M77.2, 3, 5]	-0.3868 (0.277)	-0.3882 (0.277)	-0.0741^{*} (0.032)	-0.4408 (0.306)
Disease19 [H60.0]	-0.4047 (0.277)	-0.4062 (0.276)	-0.082^{**} (0.027)	-0.4617 (0.305)
Disease20 [M54.0, 1-6]	-0.2534 (0.277)	-0.2548 (0.277)	0.0583^{**} (0.023)	-0.3077 (0.306)
Disease21 [K52.1]	-0.3478 (0.278)	-0.3494 (0.277)	0.0044 (0.050)	-0.4128 (0.306)
Disease22 [M50.3]	-0.3922 (0.278)	-0.3937 (0.277)	-0.0897^{*} (0.044)	-0.4430 (0.306)
Disease23 [L20.0]	-0.3947 (0.280)	-0.3965 (0.279)	0.0194 (0.120)	-0.4662 (0.307)
Disease24 [M54.8, 9]	0.1163 (0.104)	0.1160 (0.104)	0.1160 (0.104)	–
Disease25 [M77.8, 9]	0.3069^{*} (0.131)	0.3069^{*} (0.131)	0.3069^{*} (0.131)	–
R-square	0.006	0.006	0.006	0.006
Number of observations	714,801	714,801	699,867	714,801
F-statistics	84.22^{***}	77.49^{***}	77.57^{***}	79.21^{***}

Table D.1 Estimation Results of Fixed Effect Linear Probability Models on Healthcare Choices

Note) Standard errors are reported in parentheses. Baseline categories are "Female" in gender, "Middle-Income" in income, "The Other Areas" in residential area, and "Month 9" in month. *, **, and *** denote statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	2010-13	Visit frequency segmentation	
		Visit > 5	Visit ≤ 5
Model	Model 5 β (std. err)	Model 6 β (std. err)	Model 7 β (std. err)
Post	0.0199^{***} (0.003)	0.0262^{***} (0.007)	0.0230^{**} (0.004)
Treat	-0.3628^{*} (0.153)	-0.4919 (0.356)	-0.0759^{**} (0.021)
Post×Treat	-0.0218^{***} (0.003)	-0.0335^{***} (0.008)	-0.0252^{***} (0.004)
Post× Treat×Male	-0.0025^{**} (0.001)	-0.0013 (0.003)	-0.0027^{*} (0.001)
Post× Treat ×Low-Income	0.0028^{*} (0.001)	0.0118^{**} (0.004)	0.0016 (0.002)
Post× Treat×High-Income	-0.0001 (0.001)	9.75×10^{-5} (0.003)	-0.0007 (0.001)
Post× Treat×Seoul Metro. Area	-0.0066^{***} (0.001)	-0.0076^{**} (0.003)	-0.0044^{***} (0.001)
Month1 (Jan)	0.0104^{***} (0.001)	0.0125^{***} (0.003)	0.0122^{***} (0.002)
Month2 (Feb)	0.0097^{***} (0.001)	0.0193^{***} (0.003)	0.0105^{***} (0.002)
Month3 (Mar)	0.0063^{***} (0.001)	0.0159^{***} (0.003)	0.0055^{***} (0.001)
Month4 (APR)	0.0075^{***} (0.001)	0.0159^{***} (0.003)	0.0085^{***} (0.001)
Month5 (May)	0.0078^{***} (0.001)	0.0211^{***} (0.003)	0.0077^{***} (0.001)
Month6 (Jun)	0.0072^{***} (0.001)	0.0179^{***} (0.003)	0.0060^{***} (0.002)
Month7 (Jul)	0.0039^{***} (0.001)	0.0053 (0.003)	0.0057^{***} (0.002)
Month8 (Aug)	0.0040^{***} (0.001)	0.0040 (0.003)	0.0056^{***} (0.002)
Month10 (Oct)	-0.0029^{**} (0.001)	-0.0028 (0.003)	-0.0021 (0.001)
Month11 (Nov)	-0.0013 (0.001)	-0.0016 (0.003)	0.001 (0.001)
Month12 (Dec)	0.0029^{**} (0.001)	0.0125^{***} (0.003)	0.0029^{*} (0.001)
Disease2 [J30.0-4]	0.0012^{*} (0.001)	-0.0099^{***} (0.001)	0.0020^{**} (0.001)
Disease3 [L23.8,9]	-0.0214^{***} (0.001)	-0.0423^{***} (0.002)	-0.0175^{***} (0.001)
Disease4 [K52.2, 3, 8, 9]	0.0360^{***} (0.001)	0.0126^{***} (0.003)	0.0407^{***} (0.001)
Disease5 [B35.2-6, 8, 9]	-0.0104^{***} (0.001)	-0.0301^{***} (0.003)	-0.006^{***} (0.001)
Disease6 [L20.8, 9]	0.0150^{***} (0.001)	0.0023 (0.003)	0.0138^{***} (0.002)
Disease7 [H60.1, 3, 5, 8, 9]	-0.0145^{***} (0.001)	-0.0325^{***} (0.003)	-0.0106^{***} (0.002)
Disease8 [S93.5, 6]	0.0075^{***} (0.001)	-0.0148^{***} (0.004)	0.0127^{***} (0.002)
Disease9 [S63.6, 7]	0.0083^{***} (0.002)	-0.0145^{**} (0.005)	0.0157^{***} (0.003)
Disease10 [J31.1, 2]	-0.0096^{***} (0.002)	-0.0259^{***} (0.005)	-0.0057^{**} (0.002)

Disease11 [B35.0, 1]	−0.3839* (0.153)	−0.5313 (0.356)	−0.0996*** (0.021)
Disease12 [J31.0]	−0.3546* (0.153)	−0.4959 (0.356)	−0.0692*** (0.021)
Disease13 [M50.9]	0.0295*** (0.004)	0.0093 (0.014)	0.0383*** (0.006)
Disease14 [L23.0-7]	−0.3871* (0.153)	−0.5290 (0.357)	−0.1042*** (0.021)
Disease15 [J20.0-2]	−0.3921** (0.153)	−0.5263 (0.357)	−0.1071*** (0.022)
Disease16 [S63.5]	−0.3466* (0.153)	−0.4872 (0.357)	−0.0555* (0.024)
Disease17 [M79.1, 4, 6, 8, 9]	0.0794*** (0.014)	0.0523 (0.041)	0.0874*** (0.021)
Disease18 [M77.2, 3, 5]	−0.3874* (0.153)	−0.5372 (0.358)	−0.0798** (0.028)
Disease19 [H60.0]	−0.3961** (0.153)	−0.5483 (0.357)	−0.0972*** (0.024)
Disease20 [M54.0, 1-6]	−0.2380 (0.154)	−0.4470 (0.361)	0.0727*** (0.021)
Disease21 [K52.1]	−0.3761* (0.154)	−0.5537 (0.357)	−0.0108 (0.041)
Disease22 [M50.3]	−0.3320* (0.154)	−0.4862 (0.360)	−0.1124** (0.037)
Disease23 [L20.0]	−0.3967** (0.154)	−0.5342 (0.357)	−0.0932 (0.065)
Disease24 [M54.8, 9]	0.1349 (0.082)	−0.0792 (0.047)	0.1622 (0.125)
Disease25 [M77.8, 9]	0.3475** (0.116)	0.4716 (0.345)	0.2696* (0.136)
R-square	0.006	0.006	0.006
Number of observations	1,077,928	155,418	559,383
F-statistics	116.9***	31.21***	54.32***

Table D.2 Estimation Results of Supplementary Models on Healthcare Choices

Note) Standard errors are reported in parentheses. Baseline categories are "Female" in gender, "Middle-Income" in income, "The Other Areas" in residential area, and "Month 9" in month. *, **, and *** denote statistical significance at the 5%, 1%, and 0.1% levels, respectively.

Supplementary Material E: Inverse Probability of Treatment Weighting

Inverse Probability of Treatment Weighting (IPTW) is a method to tackle selection bias due to observables. If there is any confounding which affects control/treatment allocation, the estimation result can be biased. To prevent this, researchers model the probability of treatment allocation, or propensity score and modify the weighting of observations.

Since treatment/control dataset is imbalanced and demographic descriptive statistics are not the same between treatment group/control group, we performed IPTW. We calculated propensity scores using a logistic regression.

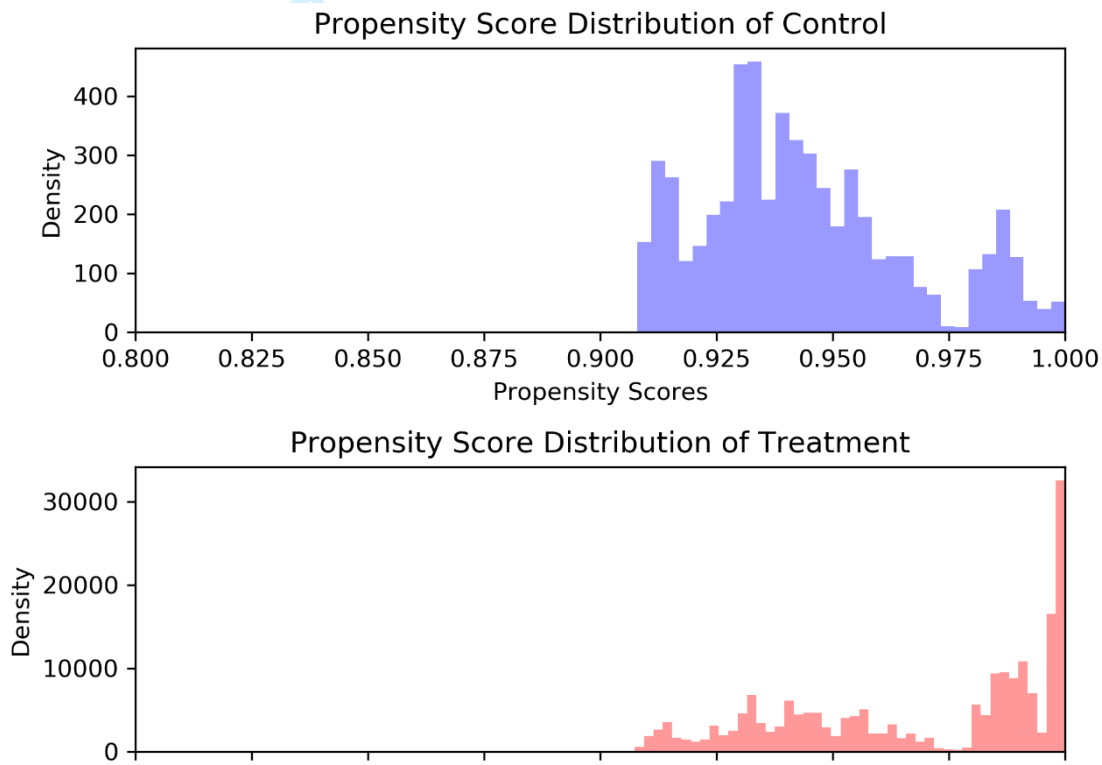


Figure E. Propensity score distribution before matching

Figure E indicates that treatment/control group propensity score distribution shares the common propensity score region. This implies that the common support assumption is already supported. We conducted IPTW to make the supports of two groups more comparable. The estimation results using the IPTW are provided in **Table 2** (*Model 4*).

Supplementary Material F: Additional Robustness Checks

1) Dissimilar treatment/control analysis

The control diseases of are very similar to treatment diseases in our main analysis. This setting has some strengths (i.e. similar and thus serves as a good counterfactual) but at the same time may suffer from some potential problems (e.g. changing disease codes to circumvent the increase in copayment). To mitigate this issue, we selected distinct set of mild diseases as control diseases and treatment diseases in a new additional analysis. To be more specific, among the mild diseases subject to the focal policy of 2011, the diseases excluded from the main analysis were selected as treatment diseases in the new analysis. Similarly, among the diseases subject to the policy of 2018, the diseases excluded from the main analysis were selected as control diseases. Figure F.1 below explains our selection of diseases in the new analysis. Since the treatment diseases are distinct or dissimilar from the control diseases, the new analysis circumvents the problem of disease code change by doctors. From this analysis, we obtained high consistent result compared to that from the main analysis; the DID term that indicates the association between the policy and patients' choice of healthcare was negative and statistically significant.

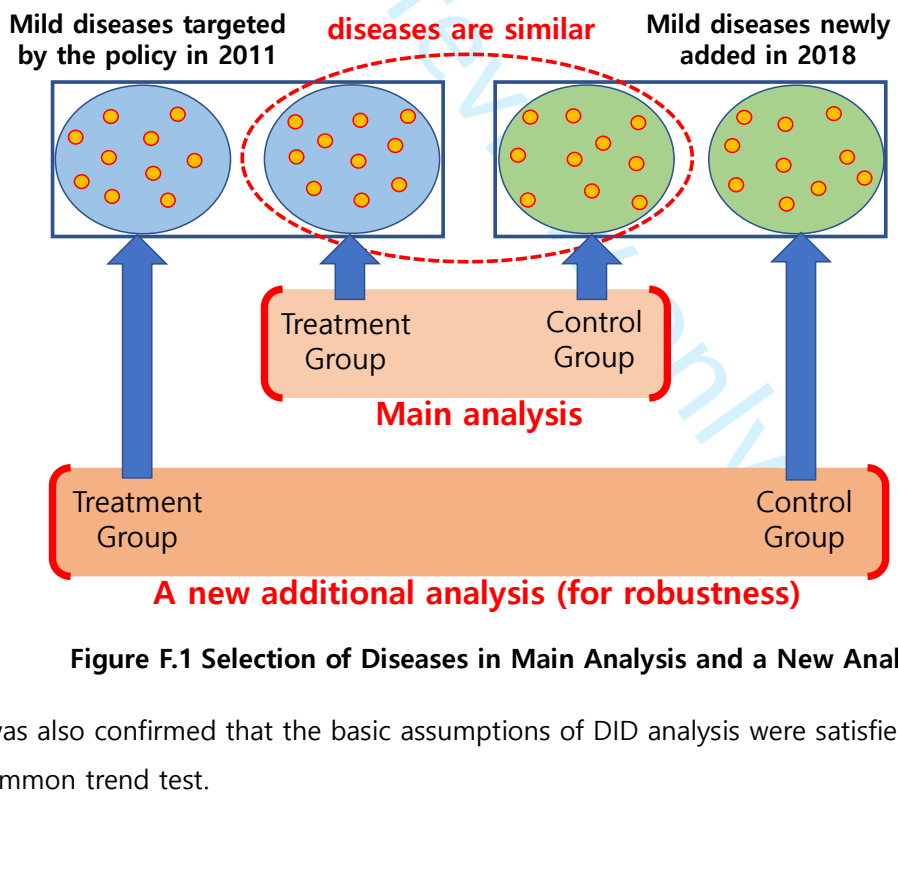


Figure F.1 Selection of Diseases in Main Analysis and a New Analysis

It was also confirmed that the basic assumptions of DID analysis were satisfied in this data with common trend test.

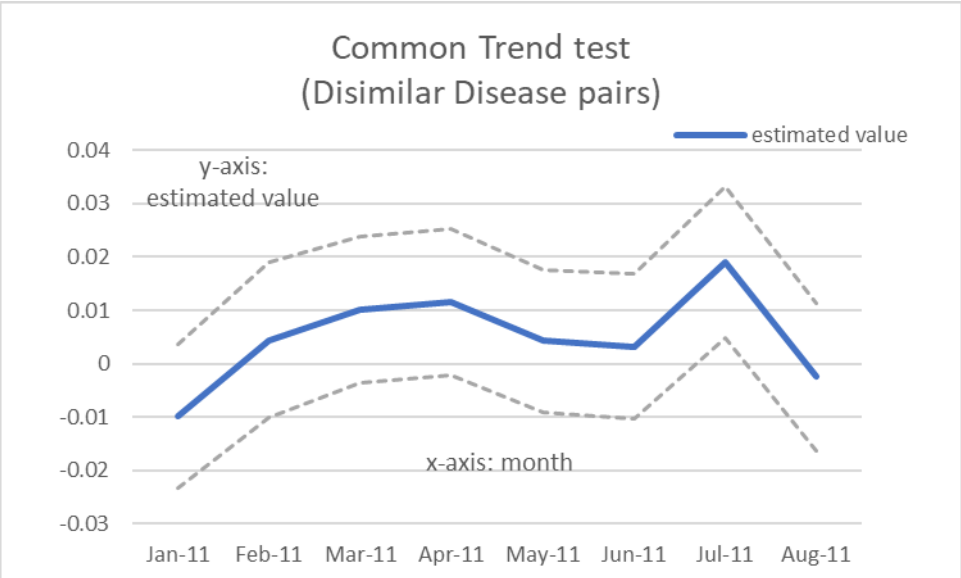


Figure F.2 Common Trend Test (Dissimilar Disease pairs)

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

Model F.1 of Table F indicates the result. In this analysis, in which disease code change cannot occur, the coefficient of policy influence (-0.0126, 95% CI -0.0158 to -0.0095) shows statistical significance.

2) Age over 65

Additionally, we performed a similar analysis using seniors with the age of 65 and above in our sample. To obtain sufficient observations in the analysis, we extracted the records of elderly people from 2010 to 12. Note that these group of people are not subject to the policy. Since there is no change in the policy, we do not expect any significant change in their healthcare choices. Moreover, if the significant result of our main model comes from some other latent effects that change over time, we should also find significant DID effect in the analysis using these seniors. This analysis can be regarded as a placebo test or a pseudo shock test to add validity to our main analysis. Model F.2 in Table F indicates the result of LPM model that describes the healthcare utilization pattern of age over 65. As we expected, we found that there is no significant change due to the policy in the senior group (the estimated coefficient of DID term is insignificant (-0.0158, 95% CI -0.0469 to 0.0154).

As in the main analysis, it was confirmed that the basic assumptions of DID analysis were satisfied through the common trend test. Figure F.3 visualizes the result of common trend test.

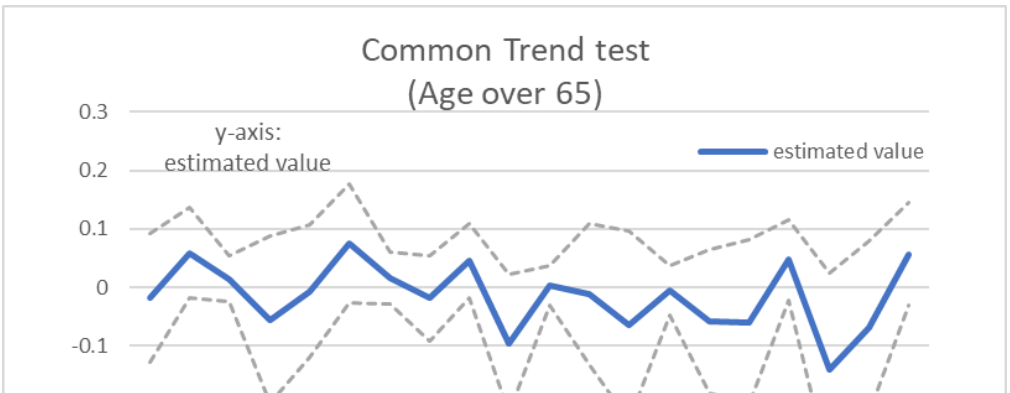


Figure F.3 Common Trend Test (Age over 65)

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

	Dissimilar Treatment/Control disease pairs	Age over 65 (Policy not applied)
Model	Model F.1 β (std. err)	Model F.2 β (std. err)
Month Dummies	Yes	Yes
Disease Dummies	Yes	Yes
Patient Fixed Effect	Yes	Yes
Post	0.0101^{***} (0.002)	0.0187 (0.016)
Treat	0.0129 (0.009)	−0.0301 (0.037)
Post×Treat	-0.0126^{***} (0.002)	−0.0158 (0.016)
R-square	0.033	0.005
Number of observations	1,294,828	30,027
F-statistics	16.52^{***}	3.36^{***}

Table F. Results of analyses to check spillover effect

*: p<0.05 / **: p<0.01 / ***: p<0.001

Supplementary material G: The choice of healthcare after the policy implementation over time (monthly)

We split the DID term by month to see if the influence of the policy changed over time. To this end, we estimate the following fixed effect model (within estimator):

$$Y_{it} = c_i + \beta_T(Treat_{it}) + \beta_{m1}(Month1_{it}) + \cdots + \beta_{m20}(Month20_{it}) + \beta_{int1}(Month1 \times Treat)_{it} + \cdots + \delta_{Disease} + u_{it}$$

where i denotes patient, t denotes t-th healthcare visit, and c_i is the patient fixed effect. Y_{it} is equal to 1 if secondary/tertiary healthcare is selected and zero if primary healthcare is selected in the observation. $Month1_{it}, \ldots, Month20_{it}$ are monthly dummy variables. $treat_{it}$ is equal to one if the observation is a treatment observation and zero if the observation is a control observation. Month 1 (October 2011) is the month just after the policy implementation. **Table G** reports the estimation results. Note that the estimated coefficients of interactions of "Treat" and month dummies capture the differences between the treatment and the control observations each month after the policy implementation. **Figure G** graphically shows the estimates.

Variable	Year/Month	Estimates	lower	upper	p-value
Treat	-	-0.3635	-0.6629	-0.0640	0.0174
Disease2 [J30.0-4]	-	0.0010	0.0000	0.0020	0.0395
Disease3 [L23.8,9]	-	-0.0214	-0.0224	-0.0204	0.0000
Disease4 [K52.2, 3, 8, 9]	-	0.0363	0.0342	0.0383	0.0000
Disease5 [B35.2-6, 8, 9]	-	-0.0105	-0.0123	-0.0087	0.0000
Disease6 [L20.8, 9]	-	0.0155	0.0126	0.0183	0.0000
Disease7 [H60.1, 3, 5, 8, 9]	-	-0.0149	-0.0172	-0.0127	0.0000
Disease8 [S93.5, 6]	-	0.0076	0.0047	0.0104	0.0000
Disease9 [S63.6, 7]	-	0.0082	0.0047	0.0117	0.0000
Disease10 [J31.1, 2]	-	-0.0095	-0.0126	-0.0064	0.0000
Disease11 [B35.0, 1]	-	-0.3848	-0.6843	-0.0854	0.0118
Disease12 [J31.0]	-	-0.3549	-0.6544	-0.0555	0.0202
Disease13 [M50.9]	-	0.0295	0.0215	0.0376	0.0000
Disease14 [L23.0-7]	-	-0.3876	-0.6871	-0.0880	0.0112
Disease15 [J20.0-2]	-	-0.3928	-0.6924	-0.0933	0.0102
Disease16 [S63.5]	-	-0.3476	-0.6475	-0.0478	0.0231
Disease17 [M79.1, 4, 6, 8, 9]	-	0.0797	0.0518	0.1077	0.0000
Disease18 [M77.2, 3, 5]	-	-0.3885	-0.6886	-0.0883	0.0112
Disease19 [H60.0]	-	-0.3976	-0.6972	-0.0979	0.0093
Disease20 [M54.0, 1-6]	-	-0.2398	-0.5425	0.0630	0.1206
Disease21 [K52.1]	-	-0.3758	-0.6768	-0.0747	0.0144
Disease22 [M50.3]	-	-0.3327	-0.6349	-0.0305	0.0309

Disease23 [L20.0]	-	-0.3955	-0.6980	-0.0930	0.0104
Disease24 [M54.8, 9]	-	0.1357	-0.0256	0.2970	0.0992
Disease25 [M77.8, 9]	-	0.3462	0.1194	0.5730	0.0028
Month1 X Treat	Oct-11	-0.0324	-0.0779	0.0131	0.1623
Month2 X Treat	Nov-11	-0.0384	-0.0610	-0.0158	0.0009
Month3 X Treat	Dec-11	-0.0234	-0.0437	-0.0031	0.0238
Month4 X Treat	Jan-12	-0.0305	-0.0610	0.0000	0.0498
Month5 X Treat	Feb-12	-0.0314	-0.0577	-0.0052	0.0188
Month6 X Treat	Mar-12	-0.0160	-0.0408	0.0088	0.2054
Month7 X Treat	Apr-12	-0.0136	-0.0346	0.0075	0.2074
Month8 X Treat	May-12	-0.0253	-0.0484	-0.0022	0.0318
Month9 X Treat	Jun-12	-0.0163	-0.0347	0.0022	0.0840
Month10 X Treat	Jul-12	-0.0248	-0.0447	-0.0049	0.0147
Month11 X Treat	Aug-12	-0.0380	-0.0590	-0.0170	0.0004
Month12 X Treat	Sep-12	-0.0186	-0.0402	0.0029	0.0902
Month13 X Treat	Oct-12	-0.0301	-0.0505	-0.0098	0.0037
Month14 X Treat	Nov-12	-0.0542	-0.0819	-0.0265	0.0001
Month15 X Treat	Dec-12	-0.0312	-0.0538	-0.0086	0.0068
Month16 X Treat	Jan-13	-0.0326	-0.0662	0.0010	0.0575
Month17 X Treat	Feb-13	0.0069	-0.0182	0.0319	0.5917
Month18 X Treat	Mar-13	-0.0275	-0.0576	0.0026	0.0730
Month19 X Treat	Apr-13	-0.0186	-0.0427	0.0054	0.1288
Month20 X Treat	May-13	-0.0216	-0.0435	0.0003	0.0532
Month21 X Treat	Jun-13	-0.0332	-0.0585	-0.0080	0.0100
Month22 X Treat	Jul-13	-0.0304	-0.0539	-0.0070	0.0110
Month23 X Treat	Aug-13	-0.0178	-0.0400	0.0045	0.1174
Month24 X Treat	Sep-13	-0.0111	-0.0310	0.0088	0.2759
Month25 X Treat	Oct-13	-0.0138	-0.0343	0.0066	0.1836
Month26 X Treat	Nov-13	-0.0271	-0.0502	-0.0040	0.0213
Month27 X Treat	Dec-13	-0.0283	-0.0510	-0.0057	0.0143
Month1	Oct-11	0.0161	-0.0293	0.0615	0.4877
Month2	Nov-11	0.0261	0.0036	0.0485	0.0228
Month3	Dec-11	0.0139	-0.0063	0.0340	0.1775
Month4	Jan-12	0.0330	0.0027	0.0632	0.0326
Month5	Feb-12	0.0324	0.0064	0.0584	0.0146
Month6	Mar-12	0.0118	-0.0128	0.0363	0.3470
Month7	Apr-12	0.0096	-0.0112	0.0305	0.3644
Month8	May-12	0.0208	-0.0022	0.0437	0.0759
Month9	Jun-12	0.0098	-0.0084	0.0279	0.2909
Month10	Jul-12	0.0177	-0.0020	0.0373	0.0775

Month11	Aug-12	0.0307	0.0100	0.0514	0.0036
Month12	Sep-12	0.0063	-0.0150	0.0277	0.5615
Month13	Oct-12	0.0175	-0.0027	0.0376	0.0889
Month14	Nov-12	0.0417	0.0142	0.0692	0.0030
Month15	Dec-12	0.0239	0.0016	0.0463	0.0359
Month16	Jan-13	0.0328	-0.0006	0.0662	0.0544
Month17	Feb-13	-0.0081	-0.0329	0.0167	0.5219
Month18	Mar-13	0.0251	-0.0048	0.0550	0.0994
Month19	Apr-13	0.0138	-0.0100	0.0376	0.2562
Month20	May-13	0.0164	-0.0053	0.0380	0.1377
Month21	Jun-13	0.0309	0.0059	0.0560	0.0154
Month22	Jul-13	0.0234	0.0003	0.0466	0.0473
Month23	Aug-13	0.0137	-0.0083	0.0357	0.2221
Month24	Sep-13	0.0051	-0.0145	0.0248	0.6083
Month25	Oct-13	0.0065	-0.0137	0.0267	0.5271
Month26	Nov-13	0.0166	-0.0063	0.0395	0.1549
Month27	Dec-13	0.0189	-0.0035	0.0413	0.0984

Table G. Result of the choice of healthcare after the policy implementation over time

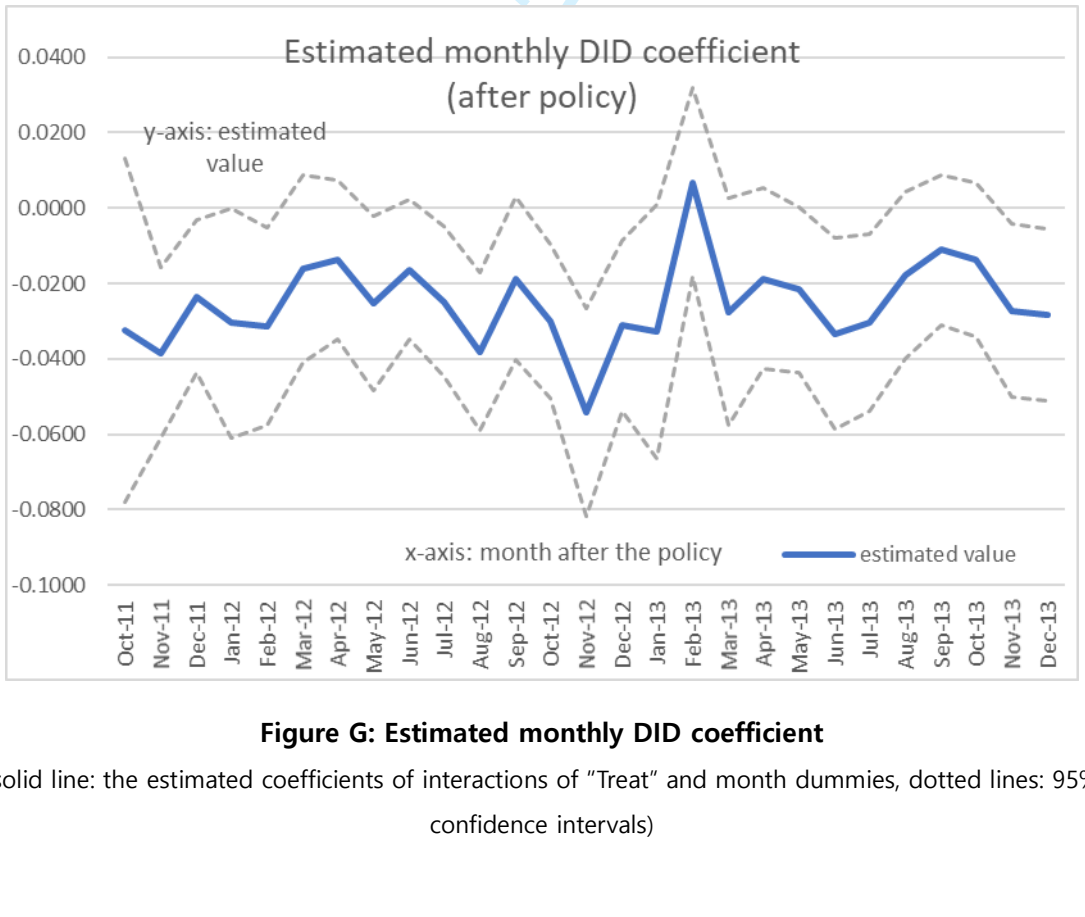


Figure G: Estimated monthly DID coefficient

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

STROBE 2007 (v4) Statement—Checklist of items that should be included in reports of *cohort studies*

Section/Topic	Item #	Recommendation	Reported on page #
Title and abstract	1	(*a*) Indicate the study's design with a commonly used term in the title or the abstract	Page 1
		(*b*) Provide in the abstract an informative and balanced summary of what was done and what was found	Page 2
Introduction			
Background/rationale	2	Explain the scientific background and rationale for the investigation being reported	Page 3
Objectives	3	State specific objectives, including any prespecified hypotheses	Page 4
Methods			
Study design	4	Present key elements of study design early in the paper	Page 6
Setting	5	Describe the setting, locations, and relevant dates, including periods of recruitment, exposure, follow-up, and data collection	Page 5~7
Participants	6	(*a*) Give the eligibility criteria, and the sources and methods of selection of participants. Describe methods of follow-up	Page 6~7
		(*b*) For matched studies, give matching criteria and number of exposed and unexposed	Page 6, 11
Variables	7	Clearly define all outcomes, exposures, predictors, potential confounders, and effect modifiers. Give diagnostic criteria, if applicable	Page 7~8
Data sources/ measurement	8*	For each variable of interest, give sources of data and details of methods of assessment (measurement). Describe comparability of assessment methods if there is more than one group	Page 6~7
Bias	9	Describe any efforts to address potential sources of bias	Page 11, 13
Study size	10	Explain how the study size was arrived at	Page 6
Quantitative variables	11	Explain how quantitative variables were handled in the analyses. If applicable, describe which groupings were chosen and why	Page 6~7
Statistical methods	12	(*a*) Describe all statistical methods, including those used to control for confounding	Page 8, 11
		(*b*) Describe any methods used to examine subgroups and interactions	Page 9
		(*c*) Explain how missing data were addressed	Page 6
		(*d*) If applicable, explain how loss to follow-up was addressed	- (No missing value)
		(*e*) Describe any sensitivity analyses	Page 10~13
Results			

Participants	13*	(a) Report numbers of individuals at each stage of study—eg numbers potentially eligible, examined for eligibility, confirmed eligible, included in the study, completing follow-up, and analysed	Page 11~12
		(b) Give reasons for non-participation at each stage	Page 10, 13~14
		(c) Consider use of a flow diagram	Supplementary Material F
Descriptive data	14*	(a) Give characteristics of study participants (eg demographic, clinical, social) and information on exposures and potential confounders	Page 5
		(b) Indicate number of participants with missing data for each variable of interest	-
		(c) Summarise follow-up time (eg, average and total amount)	Page 5
Outcome data	15*	Report numbers of outcome events or summary measures over time	Page 5
Main results	16	(*a*) Give unadjusted estimates and, if applicable, confounder-adjusted estimates and their precision (eg, 95% confidence interval). Make clear which confounders were adjusted for and why they were included	Page 9~14
		(*b*) Report category boundaries when continuous variables were categorized	Page 5
		(*c*) If relevant, consider translating estimates of relative risk into absolute risk for a meaningful time period	-
Other analyses	17	Report other analyses done—eg analyses of subgroups and interactions, and sensitivity analyses	Page 10~14
Discussion			
Key results	18	Summarise key results with reference to study objectives	Page 14~15
Limitations			
Interpretation	20	Give a cautious overall interpretation of results considering objectives, limitations, multiplicity of analyses, results from similar studies, and other relevant evidence	Page 14~15
Generalisability	21	Discuss the generalisability (external validity) of the study results	Page 15~16
Other information			
Funding	22	Give the source of funding and the role of the funders for the present study and, if applicable, for the original study on which the present article is based	Page 16

*Give information separately for cases and controls in case-control studies and, if applicable, for exposed and unexposed groups in cohort and cross-sectional studies.

Note: An Explanation and Elaboration article discusses each checklist item and gives methodological background and published examples of transparent reporting. The STROBE checklist is best used in conjunction with this article (freely available on the Web sites of PLoS Medicine at http://www.plosmedicine.org/, Annals of Internal Medicine at http://www.annals.org/, and Epidemiology at http://www.epidem.com/). Information on the STROBE Initiative is available at www.strobe-statement.org.

BMJ Open

Impact of Differential Copayment on Patient Healthcare Choice: Evidence from South Korean National Cohort Study

Journal:	*BMJ Open*
Manuscript ID	bmjopen-2020-044549.R2
Article Type:	Original research
Date Submitted by the Author:	17-May-2021
Complete List of Authors:	Jo, Sangkyun ; KAIST Jun, Duk Bin; KAIST Park, Sungho ; Seoul National University,
Primary Subject Heading:	Health policy
Secondary Subject Heading:	Health policy, Health economics, Public health
Keywords:	Health policy < HEALTH SERVICES ADMINISTRATION & MANAGEMENT, HEALTH ECONOMICS, Quality in health care < HEALTH SERVICES ADMINISTRATION & MANAGEMENT

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Impact of Differential Copayment on Patient Healthcare Choice: Evidence from South Korean National Cohort Study

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Competing Interests Statement
The authors have no affiliations with or involvement in any organization or entity with any financial interest, or non-financial interest in the subject matter or materials discussed in this manuscript.

Word counts: 5,341

Acknowledgements
This work was supported by the Ministry of Science and ICT, Republic of Korea (NRF-2019R1F1A1055160) and the Institute of Management Research at Seoul National University.

Impact of Differential Copayment Policy on Patient Healthcare Choice: Evidence from South Korean National Cohort Study

Abstract

Objective We evaluate the effectiveness of mild disease differential copayment policy aimed at reducing unnecessary patient visits to secondary/tertiary healthcare institutions in South Korea.

Design Retrospective study using difference-in-difference design

Setting Sample Research database provided by the Korean National Health Insurance Service, between 2010 and 2013.

Participants 206,947 patients who visited healthcare institutions to treat mild diseases during the sample period.

Methods A linear probability model with difference-in-difference approach was adopted to estimate the changes in patients' healthcare choices associated with the differential copayment policy. The dependent variable was a binary variable denoting whether a patient visited primary healthcare or secondary/tertiary healthcare to treat her/his mild disease. Patients' individual characteristics were controlled with a fixed effect.

Results We observed significant decrease in the proportion of patients choosing secondary/tertiary healthcare over primary healthcare by 2.99 percent point. The decrease associated with the policy was smaller by 14% in the low-income group compared to richer population, greater by 19% among the residents of Seoul metropolitan area than among people living elsewhere, and greater among frequent healthcare visitors by 33% than among people who less frequently visit healthcare.

Conclusion The mild disease differential copayment policy of South Korea was successful in discouraging unnecessary visits to secondary/tertiary healthcare institutions to treat mild diseases that can be treated well in primary healthcare.

Keywords: Differential copayment, Healthcare choice, Mild disease, Difference-in-difference, Primary healthcare

Strengths and limitations of this study

- The control group of this study provided a good counterfactual benchmark to precisely measure the change associated with the policy.
- Since the policy of this study pertains to mild diseases only, we could avoid the omitted variable problem due to unobserved disease severity.
- This study limited the subjects to similar pairs of mild diseases to construct a comparable control–treatment group setting.

INTRODUCTION

Excess demand for secondary and tertiary hospitals is a major healthcare challenge in many countries (e.g., China, Australia), resulting in overcrowding, long wait list, safety, and inefficiency issues in public health.[1–3] The South Korean government has also recognized it as a major problem and taken steps to address it.[4–6] In most countries, each tier of healthcare has its own role. In the case of South Korea (see Ministry of Health and Welfare Notification No. 2011-69), primary care should deal with outpatients for mild and common diseases, secondary care should deal with general hospitalizations and surgical care, and tertiary care should deal with treatments requiring high-level medical specialty. However, substantial proportion of mild disease patients visit secondary/tertiary hospitals. "Mild diseases" refer to the diseases with minor symptoms or illnesses designated by the Ministry of Health and Welfare and these diseases can mostly be treated well in primary healthcare.[7] In 2011, 4.7% of total patient visits to treat mild diseases were at secondary/tertiary healthcare facilities while the number of secondary/tertiary healthcare facilities was 319 (1.1%) and that of primary healthcare was 30,197 (98.9%). As patient visits to treat mild diseases increase, secondary/tertiary healthcare needs to allocate more resources to meet the demand, generating the inefficiency in attaining its main goal (i.e. to focus on severe or complicated cases).[8,9] Lee et al. [4] reported that among the outpatient usage of secondary/tertiary hospitals, approximately 85% can be sufficiently treated in primary healthcare.

A frequently used policy to tackle the excess demand problems in secondary and tertiary healthcare by governments is strengthening the gatekeeping role of the primary healthcare sector.[10,11] In many countries (e.g., the United Kingdom and the Scandinavian countries), patients cannot directly access secondary or tertiary healthcare without referral from primary healthcare.[12] Similarly, in South Korea, treatment at secondary or tertiary healthcare requires a referral letter from a primary care doctor. However, referral letters are frequently written at a patient's request and do not always reflect an actual need for care from higher-level hospitals.[13] Since the referral has no expiration date, the patient no longer needs a new referral when she/he visits to treat different diseases at the same department of the same hospital later. All in all, the South Korean referral system has failed in the gatekeeping role.

Another approach used to mitigate the excess demand problem is that of differential copayment.[14–16] In fact, the Korean government implemented a mild disease differential copayment policy in 2011. The policy imposed differential coinsurance rate on the prescribed medication when a patient visits healthcare due to mild disease. Before the policy, patients paid 30% of the prescribed medication cost regardless of the tier of healthcare he or she visited. After

the implementation of the policy, patients paid 40% (50%) of the cost when the prescription was issued at secondary (tertiary) healthcare. The coinsurance rate was maintained at 30% when the prescription is issued at primary healthcare. That is, the co-payment for medication increased by 33% or 67% when patient visited secondary or tertiary healthcare due to mild disease. Given the differential roles of secondary and tertiary healthcare, larger increase in the coinsurance rate was imposed on tertiary healthcare. The policy does not involve any cap on the cost of medication or the length of time the medication is required (In South Korea, prescriptions are usually valid for 3 days from the issued date. Medical institutions usually prescribes drugs for 14 days on average).[17] The rationale for the policy was that since the selected 52 diseases were mild ones that could be treated well in a primary healthcare, the extra cost would discourage patients from visiting secondary/tertiary healthcare institutions to treat these diseases.

The results from previous studies on the effects of differential copayment policies have been mixed.[15,18,19] Moreover, some study results should be interpreted with caution because many of them used aggregate measures (e.g., annual number of visits, total expenditures) without controlling for potential confounding effects. Huang and Tung [18] investigated if elderly Taiwanese patients' hospital tier choices have changed due to differential user charge using simple statistical tests (chi-square test, ANOVA, Scheffé test). They found that the impact was too small to be practically significant. Rosen et al. [20] investigated the effect of differential copayment on specialist visits in Israel using the difference-in-difference approach where they assigned medical beneficiaries who are exempted from the cost sharing as the control group. They found that the differential copayment policy failed to restrain visits to specialist physicians. As they noted, however, there were systematic differences between treatment group (non-medical beneficiaries) and control group (medical beneficiaries) and potential confounding was not ruled out. There have been a few empirical studies that investigated the effect of differential copayment policy of South Korea but they had the same limitations as the above cited papers – namely, no rigorous handling of the confounding effects.[21–23] Hone et al. [24] performed a systematic review to evaluate the impact of introducing differential user charges on healthcare service utilization. They found that the introduction of or increase in user charges for secondary care are associated with decreased secondary care utilization. However, they concluded that the impact of introducing differential user-charges on primary care utilization remains uncertain.

The main goal of this study is to examine the effectiveness of the differential copayment policy aimed at reducing unnecessary patient visits to secondary/tertiary healthcare institutions using detailed and representative individual-level data provided by the Korea National Health Insurance Services (KNHIS) and a difference-in-difference approach.

DATA

		Treatment disease	Control disease
		[B35.2-6, 8, 9] Dermatophytosis [H60.1, 3, 5, 8, 9] Otitis externa [J20.9] Acute bronchitis, unspecified [J30.0-4] Vasomotor and allergic rhinitis [J31.1, 2] Nasopharyngitis and pharyngitis [K52.2, 3, 8, 9] Other noninfective gastroenteritis and colitis [L20.8, 9] Atopic dermatitis [L23.8,9] Allergic contact dermatitis [M50.9] Cervical disc disorder, unspecified [M54.8, 9] Dorsalgia [M77.8, 9] Other enthesopathies [M79.1, 4, 6, 8, 9] Neuralgia and neuritis, unspecified [S63.6, 7] Dislocation, sprain and strain of joints and ligaments (hand) [S93.5, 6] Dislocation, sprain and strain of joints and ligaments (ankle/foot)	[B35.0, 1] Dermatophytosis [H60.0] Abscess of external ear [J20.0-2] Acute bronchitis [J31.0] Chronic rhinitis [K52.1] Toxic gastroenteritis and colitis [L20.0] Besnier's prurigo [L23.0-7] Allergic contact dermatitis [M50.3] Other cervical disc degeneration [M54.0, 1-6] Dorsalgia [M77.2, 3, 5] Other enthesopathies [M79.2] Neuralgia and neuritis, unspecified [S63.5] Sprain and strain of wrist
Variable	Categories of variable	Patients in treatment group (n=201,256)	Patients in control group (n=5,691)
Gender	Male	44.1%	42.6%
	Female	55.9%	57.4%
Age	Age_group1 (<20)	35.6%	14.4%
	Age_group2 (20's)	12.0%	8.8%
	Age_group3 (30's)	17.1%	17.8%
	Age_group4 (40's)	15.4%	23.8%
	Age_group5 (50's)	14.8%	26.2%
	Age_group6 (60's)	5.2%	9.1%
Income	Low (1–2 decile)	11.7%	12.9%
	Middle (3–8 decile)	55.4%	53.4%
	High (9–10 decile)	33.0%	33.8%
Residential area	Seoul-metro. area	55.9%	55.1%
	Other areas	44.1%	44.9%
Differential copayment policy (visit)	Pre-policy (1/1/2010 –9/30/2011)	Count: 394,316 (secondary/tertiary: 5.0%, primary: 95.0%)	Count: 6,452 (secondary/tertiary: 3.0%, primary: 97.0%)
	Post-policy (10/1/2011 –12/31/2012)	Count: 307,920 (secondary/tertiary: 4.0%, primary: 96.0%)	Count: 6,113 (secondary/tertiary: 5.0%, primary: 95.0%)

Table 1. Selected Diseases and Descriptive Statistics

Korean Standard Classification of Diseases-6 (KCD-6) code is shown in brackets. Detailed information on the selected disease is provided in Online Supplementary Material A.

This study used the Sample Research Data Base provided by the KNHIS, which provides mandatory social health insurance to all Koreans.[25,26] The dataset was designed and sampled to provide representative information regarding the healthcare usage of Koreans. Lee et al. [27] provided detailed explanation on the dataset. The 14-year cohort Sample Research Data Base

includes socioeconomic and demographic variables (e.g., gender, residential area, income level) and detailed information on medical treatments (e.g. medical diagnosis, type of medical facilities visited) for approximately 1 million people (2.2% of the total population) collected from 2002 to 2013. Recorded diagnoses follow the Korean Standard Classification of Diseases-6 (KCD-6) code, which is a slightly modified version of International Statistical Classification of Diseases and Related Health Problems, 10th revision (ICD-10).[28]

On October 1, 2011, the Korean government implemented a differential copayment policy with the most common 52 diseases. In 2018, the policy was extended to include additional 48 mild diseases to strengthen the effort to discourage unnecessary visits to secondary/tertiary healthcare to treat mild diseases by expanding the list.[29] We measured the impact of the policy by focusing on the initial implementation (sample period: January 2011–December 2012). Specifically, we constructed a set of treatment observations by selecting patient visits for the treatment of mild diseases selected from the set specified in 2011 (we refer to these as "treatment diseases") during the sample period. To construct a set of control observations, we selected patient visits whose purpose was to treat "control diseases" during the same sample period. The selected control diseases were similar to the treatment diseases (both belonged to the same middle-level categories in Korean Standard Classification of Diseases) and had been newly added in the 2018 extension (see **Table 1**). Consequently, our control observation provided a good counterfactual benchmark to precisely measure the change in patient behavior associated with the policy. We discuss key observations from summary statistics in **Online Supplementary Material B**.

We collected patient records of healthcare visits to treat the selected diseases between 2011 to 2012. Since we mainly examined the type of healthcare patients visited (i.e. primary vs. secondary/tertiary) and the change in this associated with the focal policy, we included initial visits to treat mild diseases in our sample but follow-up visits to treat the same disease in the same hospital were excluded. Moreover, we focused on primary diagnosis in the categorization of our observations. As a main empirical approach, we used a difference-in-difference method with patient fixed effect (we will provide more details in the next section). To this end, we included patients with two or more healthcare visits (follow-up visits are not counted) - specifically, at least one visit before the policy and one visit after the policy. Note that patients with only one visit are canceled out in the fixed effect estimation. Also, we only included patients younger than 65 years old since seniors (65+) are subject to a different cost sharing and insurance system. For the same reason, patients at the lowest income level (i.e., medical aid beneficiaries) were also excluded from the analysis. Key descriptive statistics of the selected samples are provided in **Table 1.**

ETHICS STATEMENT

The study protocol was approved by the Institutional Review Board of the Korea Advanced Institute of Science and Technology (KH2018-94). Informed consent was waived by the board.

METHODOLOGY

Our dataset has an unbalanced panel structure and the unit of analysis is a patient-visit. We adopted a linear regression model with patient-level fixed effect in our analysis. Accordingly, cluster standard errors were used in all inferences and the standard errors were clustered at individual patient level.[30] The dependent variable was whether the afflicted patients selected primary healthcare or secondary/tertiary healthcare in their visit to treat the focal diseases; thus, it is represented as a binary dummy variable (1 if secondary/tertiary healthcare was chosen and 0 if primary healthcare was chosen). This modeling approach is categorized as a linear probability model (LPM), where the estimated dependent variable can be interpreted as the probability of visiting secondary/tertiary healthcare rather than primary healthcare.[31–33]

We applied a difference-in-difference (DID) approach to measure the change in healthcare choice associated with the differential copayment policy. This method has been widely applied in previous studies to measure the impact of policies because it eliminates the effects of unobservable external factors by using control observations as counterfactuals.[18,34,35] Before applying the difference-in-difference approach, we checked the validity of our control observations by performing a parallel trend test to check whether the treatment and control observations followed the same pattern before the differential copayment policy and confirmed that they had the same trend (see **Online Supplementary Material C**).

Next, we defined the *"Treat"* dummy variable as 1 if a patient visit was to treat one of the treatment diseases and 0 if it was to treat one of the control diseases. We defined the *"Post"* dummy variable as 1 if the visit occurred after the policy implementation and 0 if it occurred before. We also included month dummy variables to capture time trends and/or seasonal variations in the dependent variable. To account for the differences in selected mild diseases, disease dummies are included. The change in healthcare choice associated with the differential copayment policy was measured using the coefficient of the interaction of *"Treat"* and *"Post."*

Furthermore, we added interaction terms to the base model to investigate how the changes in healthcare choices associated with the policy varied with key demographic variables. We refer to this model as Heterogenous DID Model (or Model 2). Specifically, we considered demographic

variables such as gender, income (low/middle/high), and residential area as categorical dummy variables. With respect to residential area, we looked at whether the patients lived in the Seoul metropolitan area or not. This was of interest because about 25 million people (50% of the country's population) live currently in the Seoul metropolitan area, where healthcare facilities and resources are highly concentrated.[36]

As stated, we define "*Treat*" variable based on whether the patient visit was to treat treatment disease or control disease. Accordingly, a patient can serve as treatment group in a visit but as control group in another visit. In contrast, in Model 3, we select patients who belong to only one group during the entire sample period and perform heterogenous DID analysis using them. Note that the assignment of treatment vs. control is at the individual patient level in this model. Next, we used Inverse Probability of Treatment Weighting (IPTW) to remove confounding from observable variables and then estimated the heterogeneous DID model (Model 4).

The specifications for the above-stated models are provided below.

DID with fixed effect (Model1)

$$Y_{it} = \alpha_i + \beta_T \cdot Treat_{it} + \beta_P \cdot Post_{it} + \beta_{DID} \cdot Treat_{it} \times Post_{it} + \tau_{Month} + \delta_{Disease} + u_{it} \quad \text{Eq. 1}$$

Heterogenous DID with fixed effect (Model2, Model3, Model4)

$$\begin{aligned} Y_{it} = \alpha_i + \beta_T \cdot Treat_{it} + \beta_P \cdot Post_{it} + \beta_{DID} \cdot Treat_{it} \times Post_{it} + \beta_{Male} \cdot Male_i \times Treat_{it} \times Post_{it} \\ + \beta_{LI} \cdot LowInc_i \times Treat_{it} \times Post_{it} + \beta_{HI} \cdot HighInc_i \times Treat_{it} \times Post_{it} \\ + \beta_{Metro} \cdot Metro_i \times Treat_{it} \times Post_{it} + \tau_{Month} + \delta_{Disease} + u_{it} \end{aligned} \quad \text{Eq. 2}$$

where i and t denote patient and healthcare visit, respectively, and Y_{it} is a binary indicator variable which takes the value of one if secondary/tertiary healthcare is visited by i at t and zero otherwise (i.e. primary healthcare visit). α_i is a patient-fixed effect which account for patient-specific characteristics in healthcare choice. τ_{Month} and $\delta_{Disease}$ are month- and disease-fixed effects to account for seasonality, time trend and disease-specific variations. $Male_i$, $LowInc_i$, $HighInc_i$, and $Metro_i$ are indicator variables denoting whether i is a male or not, i belongs to the low-income group or not, the high-income group or not, and i resides in the Seoul-metro area or not, respectively. u_{it} is an idiosyncratic error.

Patient and Public Involvement: No patient involved

RESULTS

Difference-in-Difference Analysis Using LPM

While the observations from descriptive statistics supported the effectiveness of the policy, we formally examined this after controlling for other effects such as unobserved patient-level characteristics, seasonal trend, and disease-specific characteristics using the proposed models. After confirming common trend between treatment and control group (see **Online Supplementary Material C**), the proposed fixed-effect LPM (Eq. 1) was estimated. Note that we used the within-estimator to handle the patient-level fixed effect.[37] The first column (Model 1) of **Table 2** presents the estimation result. Here, the coefficient of "*Treat*" indicates the estimated mean difference in the probability of selecting secondary/tertiary healthcare between the treatment and control observations. The coefficient of *"Post"* indicates the estimated change in the probability after policy implementation. We captured the effect of the policy through the interaction of *"Post"* and "*Treat*" represented as *"Post x Treat"*.

In Model 1, the coefficient of *"Treat"* (-0.3722, 95% CI -0.9149 to 0.1705) was not statistically significant, indicating that there is no significant difference between treatment and control observations in choosing secondary/tertiary healthcare over primary healthcare. In contrast, the coefficient of *"Post"* (0.0235, 95% CI 0.0167 to 0.0303) was positive and significant, indicating that the proportion of secondary/tertiary healthcare visits among the control diseases increased after policy implementation. More importantly, the DID term related to the policy effect (*"Post x Treat"*) was negative and significant (-0.0299, 95% CI -0.0368 to -0.0230). That is, the decrease associated with the policy was 2.99% point. From our data, we found that 4.93% of visits to treat mild diseases headed for secondary/tertiary healthcare before the policy. If we use this number as a baseline, the decrease amounts to -60%.

Heterogeneous Policy Effect

After verifying the effectiveness of the policy, we conducted additional analyses to examine the heterogeneity associated with the policy among different demographic groups. To this end, we added triple interaction terms between *"Post×Treat"* and dummies for gender, income, and residential area (Eq. 2). The estimation results are reported in **Table 2** (Model 2).

In Model 2, the coefficient of *"Post×Treat"* is highly significant and negative (-0.0267, 95% CI -0.0337 to -0.0197), indicating that the policy was associated with the decrease in the probability of choosing secondary/tertiary healthcare instead of primary healthcare. The coefficient of

"Post×Treat×LowInc" (0.0037, 95% CI 0.0007 to 0.0067) was statistically significant, while the coefficient of *"Post×Treat×HighInc"* (-0.0005, 95% CI -0.0026 to 0.0016) was not statistically significant. This indicates that the differential copayment policy was associated with a smaller decrease in the probability of choosing secondary/tertiary over primary healthcare among people in the low income group than among the others in the middle/high-income group. Specifically, *"Post×Treat"* is -0.0267 in Mid-Income group (baseline category) but the estimate becomes -0.0230 (=-0.0267+0.0037) in low-income group. We found that 4.23% of visits to treat mild diseases were at secondary/tertiary healthcare in low-income group before the policy. When we use this number as a baseline, the change associated with the policy amounts to -54%.

Similarly, the coefficient of *"Post×Treat×Metro"* (-0.0052, 95% CI -0.0072 to -0.0032) was significant and negative, revealing that the decrease in the probability of choosing secondary/tertiary over primary healthcare was larger by 19% among the residents of Seoul metropolitan area than among people living elsewhere. Specifically, *"Post×Treat"* is -0.0267 in the other areas (baseline category) but the estimate becomes -0.0319 (=-0.0267-0.0052) in low-income group. We found that 4.6% of visits to treat mild diseases were at secondary/tertiary healthcare in Seoul metropolitan area before the policy. When we use this number as a baseline, the change associated with the policy amounts to -69%.

Exclusion of Patients Who Have Both Treatment and Control Observations

We defined treatment and control observations based on the disease – whether the disease is influenced by the policy or not. Accordingly, a patient can have both treatment and control observations. In Model 3, in contrast, we selected the patients who visited healthcare due to treatment diseases only or control diseases only. As a result, our assignment of samples to treatment and control groups was not varying within a patient. The main purpose of this analysis is to tackle a potential problem of diagnosis code change to avoid increased cost due to the policy. If there were frequent and common code changes, many patients in treatment group would have moved to control group after the policy. Therefore, this exclusion of patients who have both treatment/control visits allows us to circumvent the issue of diagnosis code change. First, we note that we dropped only 14,934 observations (2% out of 714,802 observations) from this additional screening rule. This indicates that the diagnosis code change, if any, is not frequent. Model 3 in **Table 2** represents the estimation results from this model. Note that the "*Treat*" variable became time invariant in Model 3 and was absorbed into the fixed effect term. Overall, the main findings from Model 3 are highly consistent with those from Model 1 and Model 2. This adds robustness to our findings.

	DID	Heterogenous DID	Using Treatment Only and Control Only Patients	IPTW
	Model 1 β (std. err.)	Model 2 β (std. err.)	Model 3 β (std. err.)	Model 4 β (std. err.)
Month Dummies	Yes	Yes	Yes	Yes
Disease Dummies	Yes	Yes	Yes	Yes
Patient Fixed Effect	Yes	Yes	Yes	Yes
Post	0.0235 *** (0.003)	0.0235 *** (0.003)	0.0238 *** (0.004)	0.0236 *** (0.004)
Treat	−0.3722 (0.277)	−0.3738 (0.276)	–	−0.4257 (0.305)
Post × Treat	− 0.0299 *** (0.004)	−0.0267 *** (0.004)	− 0.0270 *** (0.004)	−0.0268 *** (0.004)
Post × Treat × Male	–	−0.0025 * (0.001)	−0.0026 * (0.001)	−0.0025 * (0.001)
Post × Treat × Low-Income	–	0.0037 * (0.002)	0.0035 * (0.002)	0.0039 * (0.002)
Post × Treat × High-Income	–	−0.0005 (0.001)	−0.0005 (0.001)	0.0005 (0.001)
Post × Treat × Seoul Metro. Area	–	−0.0052 *** (0.001)	−0.0052 *** (0.001)	−0.0052 *** (0.001)
R-square	0.006	0.006	0.006	0.006
Number of observations	714,801	714,801	699,867	714,801
F-statistics	84.22 ***	77.49 ***	77.57 ***	79.21 ***

Table 2. Results of Linear Probability Models on Patient Healthcare Choice

The full estimation results are available in **Online Supplementary Material D**. *: p<0.05 / **: p<0.01 / ***: p<0.001

Inverse Probability of Treatment Weighting

We used Inverse Probability of Treatment Weighting (IPTW) to remove confounding from observable variables and then estimated the heterogenous DID model (Eq.2).[38,39] Details on our IPTW procedure is provided in **Online Supplementary Material E**. Model 4 in Table 2 reports the estimation results. We found that a highly significant decrease in the probability of choosing secondary/tertiary over primary healthcare was associated with the implementation of differential copayment policy (-0.0268, 95% CI -0.0343 to -0.0193). Also, almost all estimated coefficients have

the same sign and similar magnitude as the estimates from the other models.

	Extended Sample Period (2010-13)	Split Sample Using Visit Count	
		# of Visits > 5	# of Visit ≤ 5
Model	Model 5 β (std. err)	Model 6 β (std. err)	Model 7 β (std. err)
Month Dummies	Yes	Yes	Yes
Disease Dummies	Yes	Yes	Yes
Patient Fixed Effect	Yes	Yes	Yes
Post	0.0199 *** (0.003)	0.0262 *** (0.007)	0.0230 ** (0.004)
Treat	− 0.3628 * (0.153)	−0.4919 (0.356)	− 0.0759 ** (0.021)
Post × Treat	− 0.0218 *** (0.003)	− 0.0335 *** (0.008)	− 0.0252 *** (0.004)
Post × Treat × Male	− 0.0025 ** (0.001)	−0.0013 (0.003)	− 0.0027 * (0.001)
Post × Treat × Low-Income	0.0028 * (0.001)	0.0118 ** (0.004)	0.0016 (0.002)
Post × Treat × High-Income	−0.0001 (0.001)	0.0001 (0.003)	−0.0007 (0.001)
Post × Treat × Seoul Metro. Area	− 0.0066 *** (0.001)	− 0.0076 ** (0.003)	− 0.0044 *** (0.001)
R-square	0.006	0.006	0.006
Number of observations	1,077,928	155,418	559,383
F-statistics	116.90 ***	31.21 ***	54.32 ***

Table 3. Results of Additional Models to Check Robustness

*: p<0.05 / **: p<0.01 / ***: p<0.001. The full estimation results are available online in **Online Supplementary Material D**.

Extended Sample Period

The policy change may take some time until the actual effect shows up or the effect can be short-lived, disappearing soon after the implementation. To further investigate this aspect of the policy, we extended our sample period from 2011-2012 to 2010-2013. We estimated the Heterogenous DID model (Eq. 2). Model 5 in **Table 3** provides the estimation results. As in other

model results, the coefficient of *"Post×Treat"* is highly significant and negative (-0.0218, 95% CI -0.0273 to -0.0163). Overall results using the extended sample period echoed our earlier findings from the other models. Next, we examined how patients' healthcare choices varied over time after the policy. To this end, we interacted dummies for the months after the policy with *"Treat."* We found that the change associated with the policy showed stable pattern rather than showing increasing or decreasing trends. We provide more detailed description on the model and results in **Online Supplementary Material G**.

Policy Effect and Visit Frequency

Patients who visit healthcare facilities more frequently than others would be subject to a greater financial burden if they do not change their behavior after the policy. In contrast, low-frequency patients might be more willing to pay the increased cost. If this is the case, the policy might be more effective among frequent visitors. To further examine this in our empirical context, we decomposed our sample into two using the number of healthcare visits to treat mild diseases during the sample period: i) 5 times or less, ii) above 5 times. Model 6 and Model 7 in **Table 3** show the estimation results using frequent visitors and the others, respectively. In the frequent visitor sample (Model 6), the coefficient of "*Post×Treat* " (-0.0335, 95% CI -0.0485 to -0.0185) is much larger than any other models. In contrast, from the less frequent visitor sample (Model 7), we found that the coefficient of "*Post×Treat* " (-0.0252, 95% CI -0.0330 to -0.0174) is smaller in magnitude than those of Model 1, Model 2, and Model 3. These results imply that the decrease in the visits to secondary/tertiary healthcare associated with the policy was stronger among frequent healthcare visitors. We also found that the coefficient of *"Post×Treat×LowInc"* was statistically significant in Model 6 (0.0118, 95% CI 0.0035 to 0.0201) while it was insignificant in Model 7. This finding indicates that the substantially smaller decrease in visits to secondary/tertiary over primary healthcare among low income patients was mainly driven by low-income frequent visitors. Both models had significant and negative coefficients of *"Post×Treat×Metro"*, which is consistent with all the other models.

Additional Robustness Checks

In our analysis, the control diseases are very similar to treatment diseases. This setting has some strengths but at the same time may suffer from some potential problems. For instance, doctors may change the diagnoses to ensure patients have low copayments. To mitigate this issue, we selected distinct set of mild diseases as control diseases and treatment diseases in a follow-up analysis (see **Online Supplementary Material F**) and obtained the high consistent results.

Next, we performed an analysis using seniors with the age of 65 and above in the sample. Note that these group of people are not subject to the policy. Since there is no change in the policy, we do not expect any significant change in their healthcare choices. Moreover, if the significant result of our main model comes from some other latent effects that change over time, we should also find significant DID effect in the analysis using these seniors. This analysis can be regarded as a placebo test or a pseudo shock test to add validity to our findings. We found that there is no significant change due to the policy in the senior group (see **Online Supplementary Material F**). All in all, we think that additional analyses have substantially improved the robustness of our findings.

DISCUSSION

We found that the South Korean government's 2011 differential copayment policy was significantly associated with the decrease in patients' unnecessary choice of secondary/tertiary healthcare over primary healthcare for mild diseases. This finding is consistent with the results from previous empirical studies. For example, researchers found that the introduction of or increase in user charges for secondary care are associated with decreased secondary care utilization.[40] The changes associated with the policy differed across demographic groups. Specifically, the decrease was smaller among low income patients compared to richer patient groups. This result is distinct from those in several previous empirical studies in which many researchers have found that people with low income are more sensitive to cost sharing changes and that policies based on cost sharing can exacerbate medical inequality.[41–45] For example, Powell-Jackson et al. [24,46] reported that user charge intervention increases primary healthcare utilization only in the lowest and middle income terciles. The distinctiveness of our results can be explained by the Korean differential copayment policy focusing only on mild diseases. Before the policy, people in middle- and high-income groups visited secondary/tertiary healthcare more frequently than people in the low-income brackets. Since most of the visits by middle- and high-income people to secondary/tertiary healthcare institutions could have been handled just as well by primary healthcare institutions, their adjustment in healthcare choices after the policy implementation could be more pronounced. This finding is consistent with a stream of research that showed that carefully designed copayment policies can reduce disparity in healthcare access and usage (e.g. [47]).

The smaller changes associated with the policy among low-income people might be derived from the difference in the level of health information each group has regarding the policy. People with lower incomes tend to have poorer healthcare information compared to people with higher incomes.[48] Since they are poorly-informed regarding the policy and the increase in the

cost sharing payment, their adjustment in healthcare choices after the policy could be weak. We also found that the smaller change associated with the policy among low income people was not limited to short period after the policy implementation but lasted for extended period of time (27 months afterward). Moreover, our analyses indicated that this heterogeneity along with the income was mainly driven by the patients with frequent healthcare visits. This finding implies that the government can fulfill its policy goal more effectively by enhancing information sharing, especially focusing on low-income frequent healthcare visitors.

Another interesting finding from our study was that the change in healthcare choices associated with the policy was greater among the people living in the Seoul metropolitan area than among people living in other areas. Healthcare resources are concentrated in the Seoul metropolitan area. For instance, according to Statistics Korea, the number of doctors per thousand was 3.5 in Seoul area but was only 2.2 in other areas in 2011. Because there are more healthcare facilities overall in the Seoul metropolitan area, people there may find suitable primary healthcare institutions to substitute for secondary/tertiary healthcare institutions more easily than the people in other areas whose choices may be more limited. This might explain the pronounced policy effect in the Seoul metropolitan area. This finding points out that it is important to make primary healthcare outside the Seoul metropolitan area more accessible and attractive to patients.

Our study had several noteworthy strengths. First, we used a quasi-experimental setting with the difference-in-difference approach to precisely measure the policy's impact. Our control observation provided the ideal counterfactual benchmark to measure the effectiveness of the policy. Moreover, a series of robustness checks add validity to our findings. Second, the focal policy covered only mild diseases, allowing us to circumvent the omitted variable problem due to unobserved severity. Previous studies have looked at the impact of healthcare policies applied to wide variety of diseases for which patients' condition severity may also vary widely but remain unobserved by researchers.[49] In such cases, omitted disease severity becomes a critical challenge in measurement of a policy's effect. In contrast, our study examined a policy on mild diseases with only small variations in severity. Accordingly, we can circumvent the omitted variable problem due to unobserved severity.

A few limitations of this study should be noted. As is the case with most studies using observed data, it is difficult to estimate the causal effect of the policy in a non-experimental setting. Since experimentation in our context had several challenges, including ethical issues, an experimental study was not feasible. Instead, we tried to control the effects of confounding variables by using control variables, fixed effects, and control observations. We also performed a series of

robustness tests to check the validity of our findings. In our study, we mainly investigated the changes in healthcare choices associated with a differential copayment policy. Another important variable is the number of consultations. We leave this as a future research agenda. Moreover, future study can investigate whether the patient visits differed between those who attended secondary and tertiary hospitals. A potential weakness of our sample is that doctors may change the diagnoses to ensure patients have low copayments. To mitigate this issue, we selected distinct set of mild diseases as control diseases and treatment diseases in a follow-up analysis (**Online Supplementary Material F**). Nevertheless, we acknowledge that the issue of disease code change cannot be fully ruled out. We also assume that there is no spillover effect due to changes in behaviors in our analysis.

CONCLUSION

We investigated the effect of the mild disease differential copayment policy introduced in South Korea in 2011 using the Sample Research Data Base provided by the KNHIS, conducting a difference-in-difference analysis with a quasi-experimental design. We found that a significant decrease in the proportion of patients choosing secondary/tertiary healthcare facilities over primary healthcare facilities was associated with the implementation of the policy. The change was pronounced among people with middle/high incomes, those living in the Seoul metropolitan area, and those who frequently visited healthcare facilities to treat mild diseases. We performed a series of robustness checks and found all our results to be highly consistent.

Contributors We confirm that all the authors have made substantive intellectual contributions to the paper; SP contributed to the design and the interpretation of the study after reviewing the result of the study. SKJ conceptualized the study and analyzed the data. DBJ contributed the data acquisition and provided statistical analysis support. All authors supplied critical revisions to the manuscript.

Funding This work was supported by the Ministry of Science and ICT, Republic of Korea (NRF-2019R1F1A1055160) and the Institute of Management Research at Seoul National University.

Competing interests None declared.

Ethics approval We obtained approval from the institutional review board of Korea Advanced Institute of Science and Technology (KH2018-94).

Provenance and peer review Not commissioned; externally peer reviewed.

Data sharing statement Data may be obtained from a third party and are not publicly available. We used the Sample Research Database from the Korea National Health Insurance Services (KNHIS). The same dataset can be obtained from KNHIS at nominal fee if the purpose of use is approved by KNHIS.

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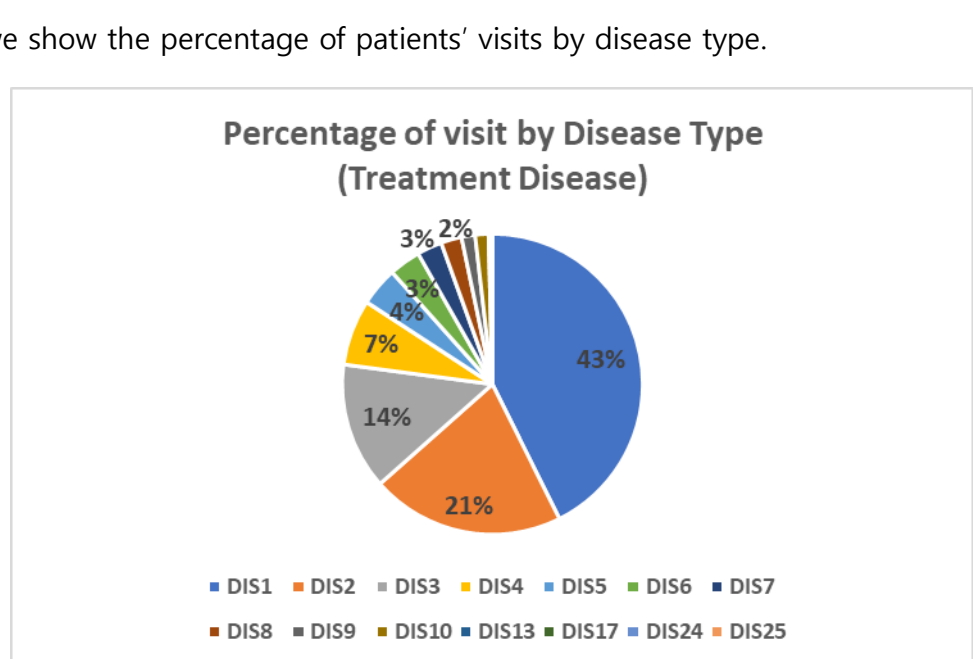
Online Supplementary Material

Supplementary Material A: Control Diseases and Treatment Diseases

Below we show the percentage of patients' visits by disease type.



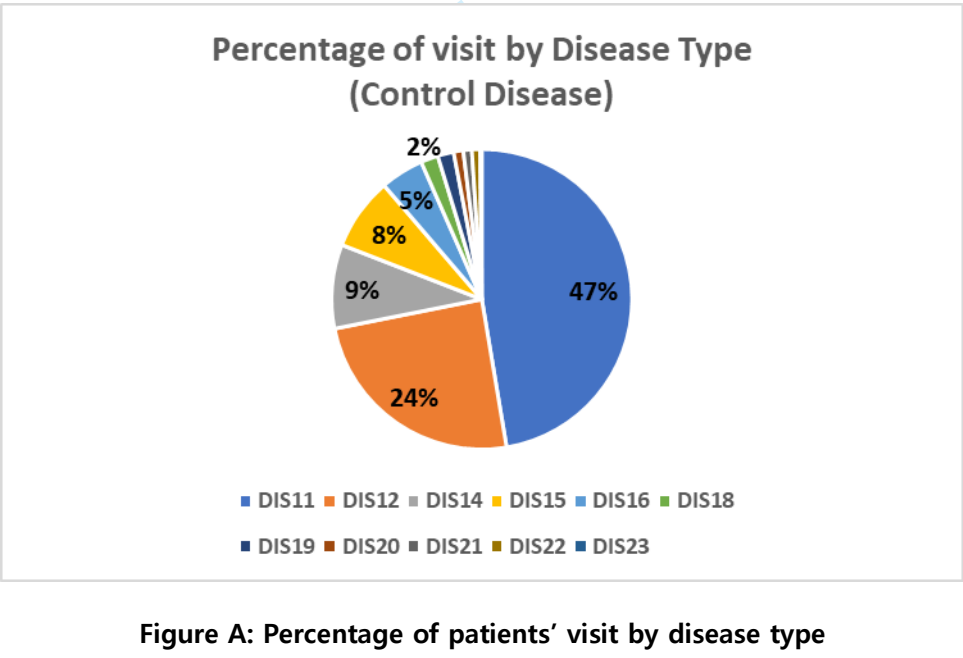


Figure A: Percentage of patients' visit by disease type

DIS1 [J20.9]	DIS2 [J30.0-4]	DIS3 [L23.8,9]	DIS4 [K52.2, 3, 8, 9]	DIS5 [B35.2-6, 8, 9]
DIS6 [L20.8, 9]	DIS7 [H60.1, 3, 5, 8, 9]	DIS8 [S93.5, 6]	DIS9 [S63.6, 7]	DIS10 [J31.1, 2]
DIS11 [B35.0, 1]	DIS12 [J31.0]	DIS13 [M50.9]	DIS14 [L23.0-7]	DIS15 [J20.0-2]
DIS16 [S63.5]	DIS17 [M79.1, 4, 6, 8, 9]	DIS18 [M77.2, 3, 5]	DIS19 [H60.0]	DIS20 [M54.0, 1-6]
DIS21 [K52.1]	DIS22 [M50.3]	DIS23 [L20.0]	DIS24 [M54.8, 9]	DIS25 [M77.8, 9]

Supplementary Material B: Observations from Summary Statistics

There were 12,565 control and 702,236 treatment observations. Secondary/tertiary healthcare visits accounted for 5.0 % of all treatment observations before the policy, and this number decreased to 4.0% after the policy. In the control observations, in contrast, secondary/tertiary healthcare visits accounted for 3.0% before the policy, and this number increased to 5.0% after the policy. These statistics provides two important implications. First, it reflects serious circumstances of the Korean medical delivery system. According to the "Major Health Insurance Statistics 2016" published by Health Insurance Review and Assessment Service, the number of secondary/tertiary healthcare facilities was less than 1%. Even though mild diseases can be sufficiently treated in primary healthcare, 4~5% of mild diseases visits are heading for secondary/tertiary healthcare. Consequently, secondary/tertiary healthcare have a difficulty in intensively treating severely diseases. Second, these observations from summary statistics imply that the differential copayment policy was effective in nudging patients to select primary healthcare instead of secondary/tertiary healthcare in treating their mild diseases. Under the circumstances that the concentration on secondary/tertiary healthcare is getting worse (increasing share of medical expenditure/number of visit days)[1], the policy seems to have alleviated the situation.

We observed that the proportions of visits to secondary/tertiary healthcare to treat the control diseases increased (**Figure B**). Next, we checked whether this trend was common for other mild diseases during the sample period. we computed the proportion of secondary/tertiary visits for all 48 of the mild diseases that had not been included in the policy in 2011 but were added in the 2018 policy extension. We found that the proportion of secondary/tertiary visits overall increased from 4.3% in the before the policy (January 2011–September 2011) to 5.7% after the policy (October 2011–December 2012), showing a pattern similar to that of our selected control diseases. These observations from summary statistics also revealed an increasing demand for secondary/tertiary healthcare even for mild diseases, justifying the implementation of the differential copayment policy that was the focus of our study.

[1] Health Insurance Major Statistics 2016,
Lee, et al. " Analysis of the current status of medical usage in tertiary hospitals and measures to normalize the role." (2019) – Health Insurance Review and Assessment report.

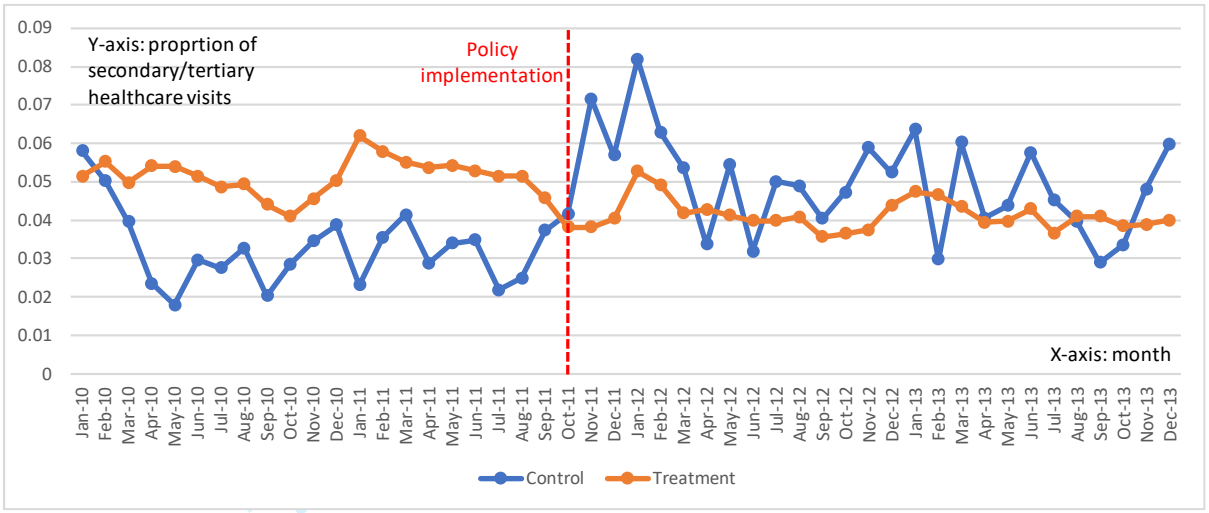


Figure B. Proportion of visiting secondary/tertiary healthcare facilities (2010-2013)

Proportion of visiting secondary/tertiary healthcare from 2010 to 2013. The dotted vertical line represents the implementation of differential coefficient policy in Oct. 2011.

Supplementary Material C: Common Trend Test

We check whether the control observations and treatments observations share the same trend before the policy implementation. To this end, we estimate the following fixed effect model (within estimator):

$$Y_{it} = c_i + \beta_T(Treat_{it}) + \beta_{m1}(Month1_{it}) + \cdots + \beta_{m20}(Month20_{it}) + \beta_{int1}(Month1 \times Treat)_{it} + \cdots + \beta_{int20}(Month20 \times Treat)_{it} + u_{it}$$

where i denotes patient, t denotes t-th healthcare visit, and c_i is the patient fixed effect. Y_{it} is equal to 1 if secondary/tertiary healthcare is selected and zero if primary healthcare is selected in the observation. $Month1_{it}, \ldots, Month20_{it}$ are monthly dummy variables. $treat_{it}$ is equal to one if the observation is a treatment observation and zero if the observation is a control observation. Month 21 (September 2011) is the month just before the policy implementation and we use this as the baseline (omitted category). **Table C** reports the estimation results. Note that the estimated coefficients of interactions of "Treat" and month dummies capture the differences between the treatment and the control observations each month before the policy implementation. **Figure C** graphically shows the estimates. All the estimates are insignificant, indicating that the treatment and the control observations have a common trend. This result indicates that our control observations provide good counterfactuals to estimate the impact of the differential copayment policy.

Variable	Year/Month	Estimates	Standard Error	P-value
Treat	-	0.0133	0.023	0.559
Month1 X Treat	Jan-10	-0.0251	0.031	0.415
Month2 X Treat	Feb-10	-0.0214	0.027	0.432
Month3 X Treat	Mar-10	-0.0304	0.03	0.310
Month4 X Treat	Apr-10	0.0421	0.026	0.107
Month5 X Treat	May-10	0.0187	0.024	0.430
Month6 X Treat	Jun-10	-0.0009	0.029	0.976
Month7 X Treat	Jul-10	-0.0071	0.027	0.789
Month8 X Treat	Aug-10	-0.0169	0.027	0.525
Month9 X Treat	Sep-10	-0.0177	0.027	0.514
Month10 X Treat	Oct-10	0.0016	0.026	0.952
Month11 X Treat	Nov-10	-0.0123	0.027	0.648
Month12 X Treat	Dec-10	-0.0147	0.028	0.597
Month13 X Treat	Jan-11	-0.0243	0.037	0.514
Month14 X Treat	Feb-11	0.0146	0.035	0.676
Month15 X Treat	Mar-11	0.0028	0.031	0.929
Month16 X Treat	Apr-11	0.0055	0.026	0.835
Month17 X Treat	May-11	0.0203	0.029	0.483

Month18 X Treat	Jun-11	0.0135	0.026	0.605
Month19 X Treat	Jul-11	-0.0295	0.028	0.296
Month20 X Treat	Aug-11	0.0072	0.025	0.772
Month1	Jan-10	0.0294	0.031	0.337
Month2	Feb-10	0.0314	0.027	0.245
Month3	Mar-10	0.0367	0.03	0.218
Month4	Apr-10	-0.0297	0.026	0.252
Month5	May-10	-0.0037	0.023	0.875
Month6	Jun-10	0.0119	0.029	0.677
Month7	Jul-10	0.0069	0.027	0.794
Month8	Aug-10	0.0210	0.026	0.424
Month9	Sep-10	0.0125	0.027	0.642
Month10	Oct-10	-0.0052	0.026	0.84
Month11	Nov-10	0.0085	0.027	0.753
Month12	Dec-10	0.0213	0.028	0.443
Month13	Jan-11	0.0401	0.037	0.28
Month14	Feb-11	0.0036	0.035	0.917
Month15	Mar-11	0.0064	0.031	0.836
Month16	Apr-11	0.0045	0.026	0.862
Month17	May-11	-0.0041	0.029	0.886
Month18	Jun-11	-0.0076	0.026	0.769
Month19	Jul-11	0.0318	0.028	0.256
Month20	Aug-11	-0.0064	0.025	0.795

Table C. Result of Common Trend Test

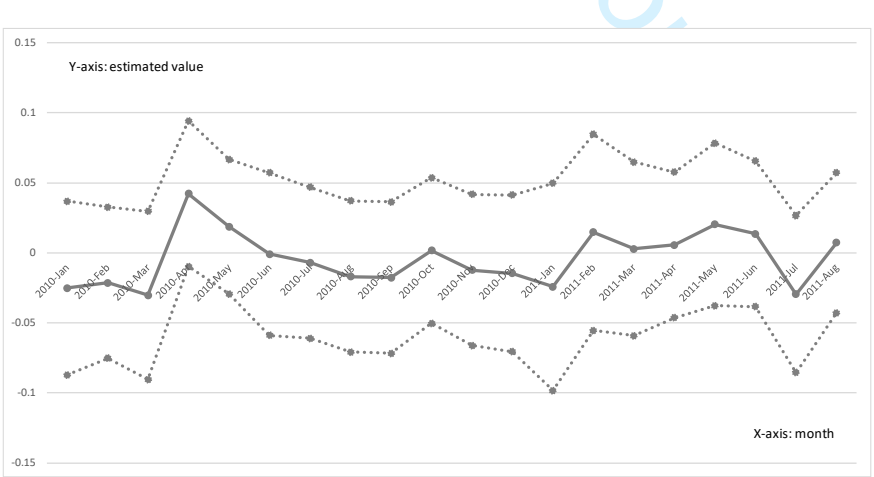


Figure C: Common Trend Test Results

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

Supplementary Material D: Full Estimation Results

We provide the full estimation results of Models 1 to 4 in Table D.1 and those of Models 5 to 7 in Table D.2.

	Main DID	Heterogeneity DID	Excluding Both T/C visitor	IPTW
	Model 1 β (std. err)	Model 2 β (std. err)	Model 3 β (std. err)	Model 4 β (std. err)
Post	0.0235^{***} (0.003)	0.0235^{***} (0.003)	0.0238^{***} (0.004)	0.0236^{***} (0.004)
Treat	-0.3722 (0.277)	-0.3738 (0.276)	–	-0.4257 (0.305)
Post× Treat	-0.0299^{***} (0.004)	-0.0267^{***} (0.004)	-0.0270^{***} (0.004)	-0.0268^{***} (0.004)
Post× Treat× Male	–	-0.0025^{*} (0.001)	-0.0026^{*} (0.001)	-0.0025^{*} (0.001)
Post× Treat×Low-Income	–	0.0037^{*} (0.002)	0.0035^{*} (0.002)	0.0039^{*} (0.002)
Post× Treat×High-Income	–	-0.0005 (0.001)	-0.0005 (0.001)	-0.0005 (0.001)
Post× Treat×Seoul Metro. Area	–	-0.0052^{***} (0.001)	-0.0052^{***} (0.001)	-0.0052^{***} (0.001)
Month1 (Jan)	0.0121^{***} (0.001)	0.0122^{***} (0.001)	0.0121^{***} (0.001)	0.0123^{***} (0.001)
Month2 (Feb)	0.0127^{***} (0.001)	0.0127^{***} (0.001)	0.0128^{***} (0.002)	0.0126^{***} (0.001)
Month3 (Mar)	0.0080^{***} (0.001)	0.0080^{***} (0.001)	0.0081^{***} (0.001)	0.0080^{***} (0.001)
Month4 (APR)	0.0102^{***} (0.001)	0.0103^{***} (0.001)	0.0103^{***} (0.001)	0.0102^{***} (0.001)
Month5 (May)	0.0111^{***} (0.001)	0.0112^{***} (0.001)	0.0113^{***} (0.001)	0.0110^{***} (0.001)
Month6 (Jun)	0.0092^{***} (0.001)	0.0092^{***} (0.001)	0.0091^{***} (0.001)	0.0092^{***} (0.001)
Month7 (Jul)	0.0055^{***} (0.001)	0.0055^{***} (0.001)	0.0055^{***} (0.001)	0.0055^{***} (0.001)
Month8 (Aug)	0.0051^{***} (0.001)	0.0051^{***} (0.001)	0.0049^{***} (0.001)	0.0053^{***} (0.001)
Month10 (Oct)	-0.0025^{*} (0.001)	-0.0025^{*} (0.001)	-0.0023 (0.001)	-0.0026^{*} (0.001)
Month11 (Nov)	0.0003 (0.001)	0.0003 (0.001)	0.0004 (0.001)	0.0002 (0.001)
Month12 (Dec)	0.0052^{***} (0.001)	0.0052^{***} (0.001)	0.0053^{***} (0.001)	-0.0051^{***} (0.001)
Disease2 [J30.0-4]	-0.0009 (0.001)	-0.0009 (0.001)	-0.001 (0.001)	-0.0008 (0.001)
Disease3 [L23.8,9]	-0.0234^{***} (0.001)	-0.0234^{***} (0.001)	-0.0235^{***} (0.001)	-0.0233^{***} (0.001)
Disease4 [K52.2, 3, 8, 9]	0.0337^{***} (0.001)	0.0336^{***} (0.001)	0.0338^{***} (0.001)	0.0334^{***} (0.001)
Disease5 [B35.2-6, 8, 9]	-0.0109^{***} (0.001)	-0.0109^{***} (0.001)	-0.0108^{***} (0.001)	-0.0110^{***} (0.001)
Disease6 [L20.8, 9]	0.0110^{***} (0.002)	0.0109^{***} (0.002)	0.0108^{***} (0.002)	0.0110^{***} (0.002)

Disease7 [H60.1, 3, 5, 8, 9]	-0.0159^{***} (0.001)	-0.0159^{***} (0.001)	-0.0158^{***} (0.001)	-0.0159^{***} (0.001)
Disease8 [S93.5, 6]	0.0066^{***} (0.002)	0.0066^{***} (0.002)	0.0065^{***} (0.002)	0.0067^{***} (0.002)
Disease9 [S63.6, 7]	0.0090^{***} (0.002)	0.0090^{***} (0.002)	0.0085^{***} (0.002)	0.0095^{***} (0.002)
Disease10 [J31.1, 2]	-0.0105^{***} (0.002)	-0.0106^{***} (0.002)	-0.0104^{***} (0.002)	-0.0107^{***} (0.002)
Disease11 [B35.0, 1]	-0.3988 (0.277)	-0.4003 (0.276)	-0.0900^{***} (0.023)	-0.4518 (0.305)
Disease12 [J31.0]	-0.3665 (0.277)	-0.3681 (0.276)	-0.0548^{*} (0.023)	-0.4204 (0.305)
Disease13 [M50.9]	0.0327^{***} (0.005)	0.0328^{***} (0.005)	0.0337^{***} (0.005)	0.0319^{***} (0.005)
Disease14 [L23.0-7]	-0.4006 (0.277)	-0.4019 (0.276)	-0.0928^{***} (0.022)	-0.4530 (0.305)
Disease15 [J20.0-2]	-0.4016 (0.277)	-0.403 (0.276)	-0.1036^{***} (0.024)	-0.4518 (0.305)
Disease16 [S63.5]	-0.3550 (0.277)	-0.3564 (0.276)	-0.0267 (0.028)	-0.4137 (0305)
Disease17 [M79.1, 4, 6, 8, 9]	0.0799^{***} (0.018)	0.0798^{***} (0.018)	0.0818^{***} (0.019)	-0.0776^{***} (0.018)
Disease18 [M77.2, 3, 5]	-0.3868 (0.277)	-0.3882 (0.277)	-0.0741^{*} (0.032)	-0.4408 (0.306)
Disease19 [H60.0]	-0.4047 (0.277)	-0.4062 (0.276)	-0.082^{**} (0.027)	-0.4617 (0.305)
Disease20 [M54.0, 1-6]	-0.2534 (0.277)	-0.2548 (0.277)	0.0583^{**} (0.023)	-0.3077 (0.306)
Disease21 [K52.1]	-0.3478 (0.278)	-0.3494 (0.277)	0.0044 (0.050)	-0.4128 (0.306)
Disease22 [M50.3]	-0.3922 (0.278)	-0.3937 (0.277)	-0.0897^{*} (0.044)	-0.4430 (0.306)
Disease23 [L20.0]	-0.3947 (0.280)	-0.3965 (0.279)	0.0194 (0.120)	-0.4662 (0.307)
Disease24 [M54.8, 9]	0.1163 (0.104)	0.1160 (0.104)	0.1160 (0.104)	–
Disease25 [M77.8, 9]	0.3069^{*} (0.131)	0.3069^{*} (0.131)	0.3069^{*} (0.131)	–
R-square	0.006	0.006	0.006	0.006
Number of observations	714,801	714,801	699,867	714,801
F-statistics	84.22^{***}	77.49^{***}	77.57^{***}	79.21^{***}

Table D.1 Estimation Results of Fixed Effect Linear Probability Models on Healthcare Choices

Note) Standard errors are reported in parentheses. Baseline categories are "Female" in gender, "Middle-Income" in income, "The Other Areas" in residential area, and "Month 9" in month. *, **, and *** denote statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	2010-13	Visit frequency segmentation	
		Visit > 5	Visit ≤ 5
Model	Model 5 β (std. err)	Model 6 β (std. err)	Model 7 β (std. err)
Post	0.0199^{***} (0.003)	0.0262^{***} (0.007)	0.0230^{**} (0.004)
Treat	-0.3628^{*} (0.153)	-0.4919 (0.356)	-0.0759^{**} (0.021)
Post×Treat	-0.0218^{***} (0.003)	-0.0335^{***} (0.008)	-0.0252^{***} (0.004)
Post× Treat×Male	-0.0025^{**} (0.001)	-0.0013 (0.003)	-0.0027^{*} (0.001)
Post× Treat ×Low-Income	0.0028^{*} (0.001)	0.0118^{**} (0.004)	0.0016 (0.002)
Post× Treat×High-Income	-0.0001 (0.001)	9.75×10^{-5} (0.003)	-0.0007 (0.001)
Post× Treat×Seoul Metro. Area	-0.0066^{***} (0.001)	-0.0076^{**} (0.003)	-0.0044^{***} (0.001)
Month1 (Jan)	0.0104^{***} (0.001)	0.0125^{***} (0.003)	0.0122^{***} (0.002)
Month2 (Feb)	0.0097^{***} (0.001)	0.0193^{***} (0.003)	0.0105^{***} (0.002)
Month3 (Mar)	0.0063^{***} (0.001)	0.0159^{***} (0.003)	0.0055^{***} (0.001)
Month4 (APR)	0.0075^{***} (0.001)	0.0159^{***} (0.003)	0.0085^{***} (0.001)
Month5 (May)	0.0078^{***} (0.001)	0.0211^{***} (0.003)	0.0077^{***} (0.001)
Month6 (Jun)	0.0072^{***} (0.001)	0.0179^{***} (0.003)	0.0060^{***} (0.002)
Month7 (Jul)	0.0039^{***} (0.001)	0.0053 (0.003)	0.0057^{***} (0.002)
Month8 (Aug)	0.0040^{***} (0.001)	0.0040 (0.003)	0.0056^{***} (0.002)
Month10 (Oct)	-0.0029^{**} (0.001)	-0.0028 (0.003)	-0.0021 (0.001)
Month11 (Nov)	-0.0013 (0.001)	-0.0016 (0.003)	0.001 (0.001)
Month12 (Dec)	0.0029^{**} (0.001)	0.0125^{***} (0.003)	0.0029^{*} (0.001)
Disease2 [J30.0-4]	0.0012^{*} (0.001)	-0.0099^{***} (0.001)	0.0020^{**} (0.001)
Disease3 [L23.8,9]	-0.0214^{***} (0.001)	-0.0423^{***} (0.002)	-0.0175^{***} (0.001)
Disease4 [K52.2, 3, 8, 9]	0.0360^{***} (0.001)	0.0126^{***} (0.003)	0.0407^{***} (0.001)
Disease5 [B35.2-6, 8, 9]	-0.0104^{***} (0.001)	-0.0301^{***} (0.003)	-0.006^{***} (0.001)
Disease6 [L20.8, 9]	0.0150^{***} (0.001)	0.0023 (0.003)	0.0138^{***} (0.002)
Disease7 [H60.1, 3, 5, 8, 9]	-0.0145^{***} (0.001)	-0.0325^{***} (0.003)	-0.0106^{***} (0.002)
Disease8 [S93.5, 6]	0.0075^{***} (0.001)	-0.0148^{***} (0.004)	0.0127^{***} (0.002)
Disease9 [S63.6, 7]	0.0083^{***} (0.002)	-0.0145^{**} (0.005)	0.0157^{***} (0.003)
Disease10 [J31.1, 2]	-0.0096^{***} (0.002)	-0.0259^{***} (0.005)	-0.0057^{**} (0.002)

Disease11 [B35.0, 1]	−0.3839* (0.153)	−0.5313 (0.356)	−0.0996*** (0.021)
Disease12 [J31.0]	−0.3546* (0.153)	−0.4959 (0.356)	−0.0692*** (0.021)
Disease13 [M50.9]	0.0295*** (0.004)	0.0093 (0.014)	0.0383*** (0.006)
Disease14 [L23.0-7]	−0.3871* (0.153)	−0.5290 (0.357)	−0.1042*** (0.021)
Disease15 [J20.0-2]	−0.3921** (0.153)	−0.5263 (0.357)	−0.1071*** (0.022)
Disease16 [S63.5]	−0.3466* (0.153)	−0.4872 (0.357)	−0.0555* (0.024)
Disease17 [M79.1, 4, 6, 8, 9]	0.0794*** (0.014)	0.0523 (0.041)	0.0874*** (0.021)
Disease18 [M77.2, 3, 5]	−0.3874* (0.153)	−0.5372 (0.358)	−0.0798** (0.028)
Disease19 [H60.0]	−0.3961** (0.153)	−0.5483 (0.357)	−0.0972*** (0.024)
Disease20 [M54.0, 1-6]	−0.2380 (0.154)	−0.4470 (0.361)	0.0727*** (0.021)
Disease21 [K52.1]	−0.3761* (0.154)	−0.5537 (0.357)	−0.0108 (0.041)
Disease22 [M50.3]	−0.3320* (0.154)	−0.4862 (0.360)	−0.1124** (0.037)
Disease23 [L20.0]	−0.3967** (0.154)	−0.5342 (0.357)	−0.0932 (0.065)
Disease24 [M54.8, 9]	0.1349 (0.082)	−0.0792 (0.047)	0.1622 (0.125)
Disease25 [M77.8, 9]	0.3475** (0.116)	0.4716 (0.345)	0.2696* (0.136)
R-square	0.006	0.006	0.006
Number of observations	1,077,928	155,418	559,383
F-statistics	116.9***	31.21***	54.32***

Table D.2 Estimation Results of Supplementary Models on Healthcare Choices

Note) Standard errors are reported in parentheses. Baseline categories are "Female" in gender, "Middle-Income" in income, "The Other Areas" in residential area, and "Month 9" in month. *, **, and *** denote statistical significance at the 5%, 1%, and 0.1% levels, respectively.

Supplementary Material E: Inverse Probability of Treatment Weighting

Inverse Probability of Treatment Weighting (IPTW) is a method to tackle selection bias due to observables. If there is any confounding which affects control/treatment allocation, the estimation result can be biased. To prevent this, researchers model the probability of treatment allocation, or propensity score and modify the weighting of observations.

Since treatment/control dataset is imbalanced and demographic descriptive statistics are not the same between treatment group/control group, we performed IPTW. We calculated propensity scores using a logistic regression.

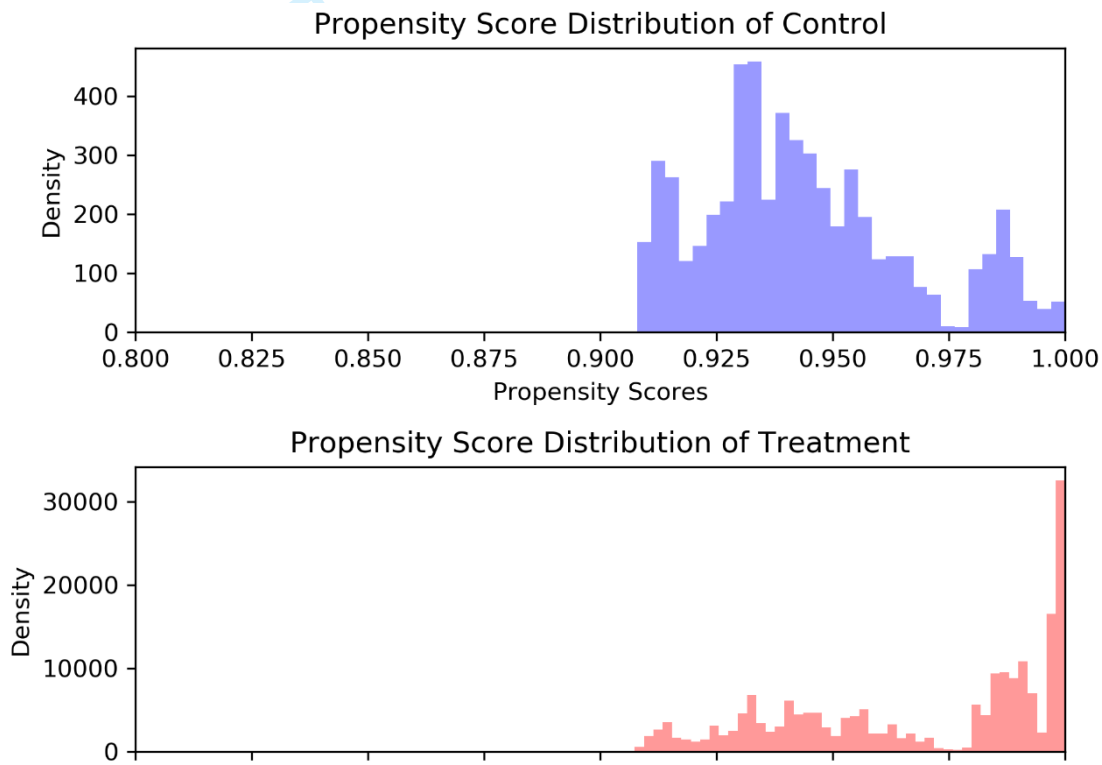


Figure E. Propensity score distribution before matching

Figure E indicates that treatment/control group propensity score distribution shares the common propensity score region. This implies that the common support assumption is already supported. We conducted IPTW to make the supports of two groups more comparable. The estimation results using the IPTW are provided in **Table 2** (*Model 4*).

Supplementary Material F: Additional Robustness Checks

1) Dissimilar treatment/control analysis

The control diseases of are very similar to treatment diseases in our main analysis. This setting has some strengths (i.e. similar and thus serves as a good counterfactual) but at the same time may suffer from some potential problems (e.g. changing disease codes to circumvent the increase in copayment). To mitigate this issue, we selected distinct set of mild diseases as control diseases and treatment diseases in a new additional analysis. To be more specific, among the mild diseases subject to the focal policy of 2011, the diseases excluded from the main analysis were selected as treatment diseases in the new analysis. Similarly, among the diseases subject to the policy of 2018, the diseases excluded from the main analysis were selected as control diseases. Figure F.1 below explains our selection of diseases in the new analysis. Since the treatment diseases are distinct or dissimilar from the control diseases, the new analysis circumvents the problem of disease code change by doctors. From this analysis, we obtained high consistent result compared to that from the main analysis; the DID term that indicates the association between the policy and patients' choice of healthcare was negative and statistically significant.

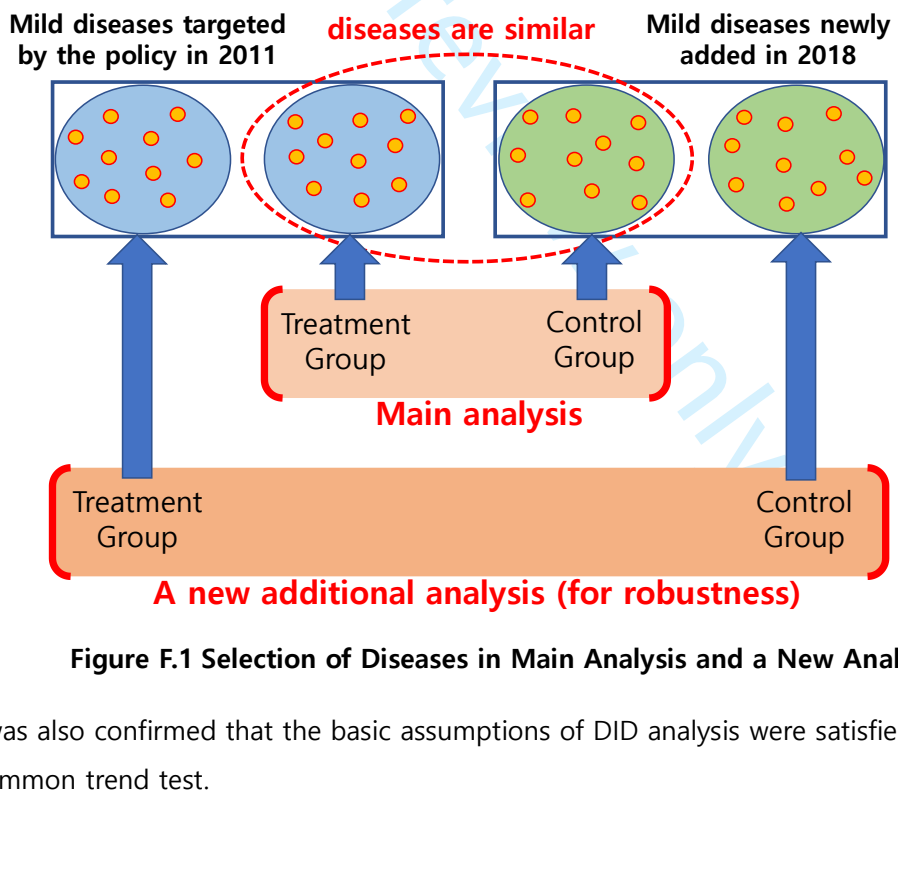


Figure F.1 Selection of Diseases in Main Analysis and a New Analysis

It was also confirmed that the basic assumptions of DID analysis were satisfied in this data with common trend test.

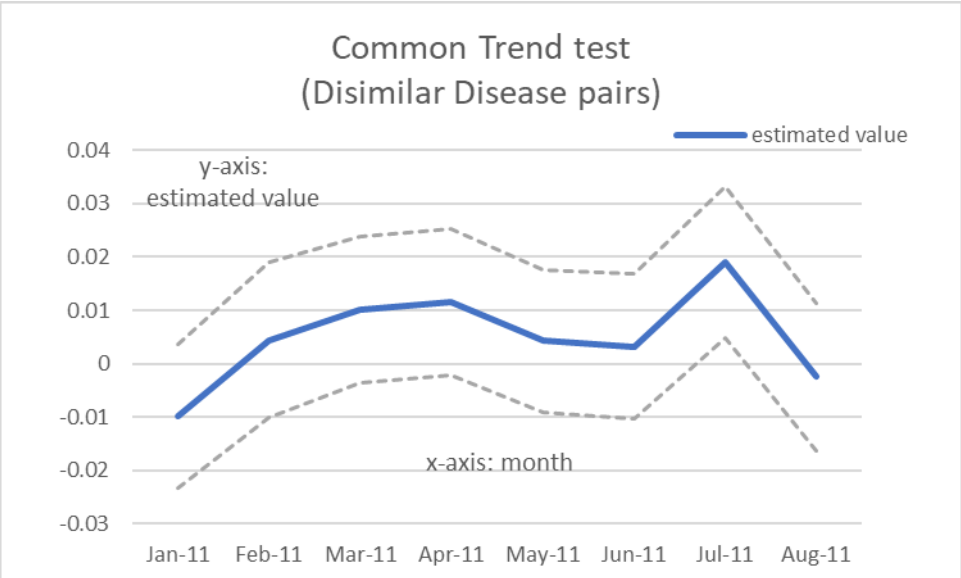


Figure F.2 Common Trend Test (Dissimilar Disease pairs)

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

Model F.1 of Table F indicates the result. In this analysis, in which disease code change cannot occur, the coefficient of policy influence (-0.0126, 95% CI -0.0158 to -0.0095) shows statistical significance.

2) Age over 65

Additionally, we performed a similar analysis using seniors with the age of 65 and above in our sample. To obtain sufficient observations in the analysis, we extracted the records of elderly people from 2010 to 12. Note that these group of people are not subject to the policy. Since there is no change in the policy, we do not expect any significant change in their healthcare choices. Moreover, if the significant result of our main model comes from some other latent effects that change over time, we should also find significant DID effect in the analysis using these seniors. This analysis can be regarded as a placebo test or a pseudo shock test to add validity to our main analysis. Model F.2 in Table F indicates the result of LPM model that describes the healthcare utilization pattern of age over 65. As we expected, we found that there is no significant change due to the policy in the senior group (the estimated coefficient of DID term is insignificant (-0.0158, 95% CI -0.0469 to 0.0154).

As in the main analysis, it was confirmed that the basic assumptions of DID analysis were satisfied through the common trend test. Figure F.3 visualizes the result of common trend test.

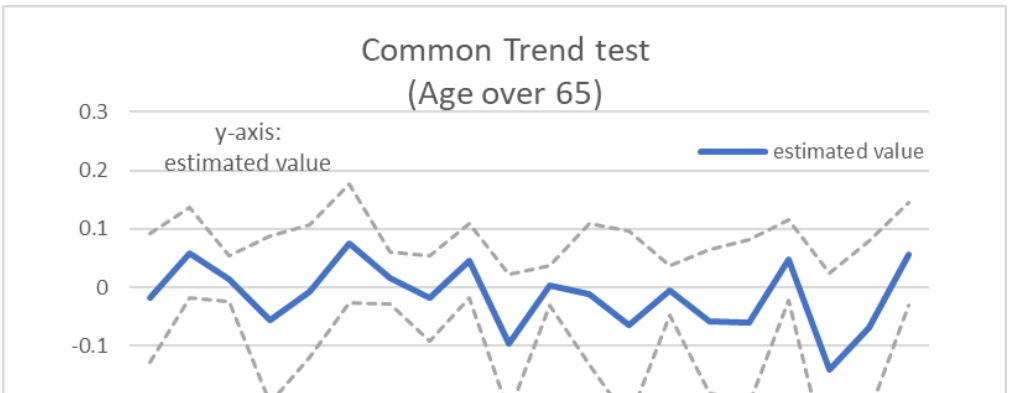


Figure F.3 Common Trend Test (Age over 65)

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

	Dissimilar Treatment/Control disease pairs	Age over 65 (Policy not applied)
Model	Model F.1 β (std. err)	Model F.2 β (std. err)
Month Dummies	Yes	Yes
Disease Dummies	Yes	Yes
Patient Fixed Effect	Yes	Yes
Post	0.0101^{***} (0.002)	0.0187 (0.016)
Treat	0.0129 (0.009)	−0.0301 (0.037)
Post×Treat	-0.0126^{***} (0.002)	−0.0158 (0.016)
R-square	0.033	0.005
Number of observations	1,294,828	30,027
F-statistics	16.52^{***}	3.36^{***}

Table F. Results of analyses to check spillover effect

*: p<0.05 / **: p<0.01 / ***: p<0.001

Supplementary material G: The choice of healthcare after the policy implementation over time (monthly)

We split the DID term by month to see if the influence of the policy changed over time. To this end, we estimate the following fixed effect model (within estimator):

$$Y_{it} = c_i + \beta_T(Treat_{it}) + \beta_{m1}(Month1_{it}) + \cdots + \beta_{m20}(Month20_{it}) + \beta_{int1}(Month1 \times Treat)_{it} + \cdots + \delta_{Disease} + u_{it}$$

where i denotes patient, t denotes t-th healthcare visit, and c_i is the patient fixed effect. Y_{it} is equal to 1 if secondary/tertiary healthcare is selected and zero if primary healthcare is selected in the observation. $Month1_{it}, \ldots, Month20_{it}$ are monthly dummy variables. $treat_{it}$ is equal to one if the observation is a treatment observation and zero if the observation is a control observation. Month 1 (October 2011) is the month just after the policy implementation. **Table G** reports the estimation results. Note that the estimated coefficients of interactions of "Treat" and month dummies capture the differences between the treatment and the control observations each month after the policy implementation. **Figure G** graphically shows the estimates.

Variable	Year/Month	Estimates	lower	upper	p-value
Treat	-	-0.3635	-0.6629	-0.0640	0.0174
Disease2 [J30.0-4]	-	0.0010	0.0000	0.0020	0.0395
Disease3 [L23.8,9]	-	-0.0214	-0.0224	-0.0204	0.0000
Disease4 [K52.2, 3, 8, 9]	-	0.0363	0.0342	0.0383	0.0000
Disease5 [B35.2-6, 8, 9]	-	-0.0105	-0.0123	-0.0087	0.0000
Disease6 [L20.8, 9]	-	0.0155	0.0126	0.0183	0.0000
Disease7 [H60.1, 3, 5, 8, 9]	-	-0.0149	-0.0172	-0.0127	0.0000
Disease8 [S93.5, 6]	-	0.0076	0.0047	0.0104	0.0000
Disease9 [S63.6, 7]	-	0.0082	0.0047	0.0117	0.0000
Disease10 [J31.1, 2]	-	-0.0095	-0.0126	-0.0064	0.0000
Disease11 [B35.0, 1]	-	-0.3848	-0.6843	-0.0854	0.0118
Disease12 [J31.0]	-	-0.3549	-0.6544	-0.0555	0.0202
Disease13 [M50.9]	-	0.0295	0.0215	0.0376	0.0000
Disease14 [L23.0-7]	-	-0.3876	-0.6871	-0.0880	0.0112
Disease15 [J20.0-2]	-	-0.3928	-0.6924	-0.0933	0.0102
Disease16 [S63.5]	-	-0.3476	-0.6475	-0.0478	0.0231
Disease17 [M79.1, 4, 6, 8, 9]	-	0.0797	0.0518	0.1077	0.0000
Disease18 [M77.2, 3, 5]	-	-0.3885	-0.6886	-0.0883	0.0112
Disease19 [H60.0]	-	-0.3976	-0.6972	-0.0979	0.0093
Disease20 [M54.0, 1-6]	-	-0.2398	-0.5425	0.0630	0.1206
Disease21 [K52.1]	-	-0.3758	-0.6768	-0.0747	0.0144
Disease22 [M50.3]	-	-0.3327	-0.6349	-0.0305	0.0309

Disease23 [L20.0]	-	-0.3955	-0.6980	-0.0930	0.0104
Disease24 [M54.8, 9]	-	0.1357	-0.0256	0.2970	0.0992
Disease25 [M77.8, 9]	-	0.3462	0.1194	0.5730	0.0028
Month1 X Treat	Oct-11	-0.0324	-0.0779	0.0131	0.1623
Month2 X Treat	Nov-11	-0.0384	-0.0610	-0.0158	0.0009
Month3 X Treat	Dec-11	-0.0234	-0.0437	-0.0031	0.0238
Month4 X Treat	Jan-12	-0.0305	-0.0610	0.0000	0.0498
Month5 X Treat	Feb-12	-0.0314	-0.0577	-0.0052	0.0188
Month6 X Treat	Mar-12	-0.0160	-0.0408	0.0088	0.2054
Month7 X Treat	Apr-12	-0.0136	-0.0346	0.0075	0.2074
Month8 X Treat	May-12	-0.0253	-0.0484	-0.0022	0.0318
Month9 X Treat	Jun-12	-0.0163	-0.0347	0.0022	0.0840
Month10 X Treat	Jul-12	-0.0248	-0.0447	-0.0049	0.0147
Month11 X Treat	Aug-12	-0.0380	-0.0590	-0.0170	0.0004
Month12 X Treat	Sep-12	-0.0186	-0.0402	0.0029	0.0902
Month13 X Treat	Oct-12	-0.0301	-0.0505	-0.0098	0.0037
Month14 X Treat	Nov-12	-0.0542	-0.0819	-0.0265	0.0001
Month15 X Treat	Dec-12	-0.0312	-0.0538	-0.0086	0.0068
Month16 X Treat	Jan-13	-0.0326	-0.0662	0.0010	0.0575
Month17 X Treat	Feb-13	0.0069	-0.0182	0.0319	0.5917
Month18 X Treat	Mar-13	-0.0275	-0.0576	0.0026	0.0730
Month19 X Treat	Apr-13	-0.0186	-0.0427	0.0054	0.1288
Month20 X Treat	May-13	-0.0216	-0.0435	0.0003	0.0532
Month21 X Treat	Jun-13	-0.0332	-0.0585	-0.0080	0.0100
Month22 X Treat	Jul-13	-0.0304	-0.0539	-0.0070	0.0110
Month23 X Treat	Aug-13	-0.0178	-0.0400	0.0045	0.1174
Month24 X Treat	Sep-13	-0.0111	-0.0310	0.0088	0.2759
Month25 X Treat	Oct-13	-0.0138	-0.0343	0.0066	0.1836
Month26 X Treat	Nov-13	-0.0271	-0.0502	-0.0040	0.0213
Month27 X Treat	Dec-13	-0.0283	-0.0510	-0.0057	0.0143
Month1	Oct-11	0.0161	-0.0293	0.0615	0.4877
Month2	Nov-11	0.0261	0.0036	0.0485	0.0228
Month3	Dec-11	0.0139	-0.0063	0.0340	0.1775
Month4	Jan-12	0.0330	0.0027	0.0632	0.0326
Month5	Feb-12	0.0324	0.0064	0.0584	0.0146
Month6	Mar-12	0.0118	-0.0128	0.0363	0.3470
Month7	Apr-12	0.0096	-0.0112	0.0305	0.3644
Month8	May-12	0.0208	-0.0022	0.0437	0.0759
Month9	Jun-12	0.0098	-0.0084	0.0279	0.2909
Month10	Jul-12	0.0177	-0.0020	0.0373	0.0775

Month11	Aug-12	0.0307	0.0100	0.0514	0.0036
Month12	Sep-12	0.0063	-0.0150	0.0277	0.5615
Month13	Oct-12	0.0175	-0.0027	0.0376	0.0889
Month14	Nov-12	0.0417	0.0142	0.0692	0.0030
Month15	Dec-12	0.0239	0.0016	0.0463	0.0359
Month16	Jan-13	0.0328	-0.0006	0.0662	0.0544
Month17	Feb-13	-0.0081	-0.0329	0.0167	0.5219
Month18	Mar-13	0.0251	-0.0048	0.0550	0.0994
Month19	Apr-13	0.0138	-0.0100	0.0376	0.2562
Month20	May-13	0.0164	-0.0053	0.0380	0.1377
Month21	Jun-13	0.0309	0.0059	0.0560	0.0154
Month22	Jul-13	0.0234	0.0003	0.0466	0.0473
Month23	Aug-13	0.0137	-0.0083	0.0357	0.2221
Month24	Sep-13	0.0051	-0.0145	0.0248	0.6083
Month25	Oct-13	0.0065	-0.0137	0.0267	0.5271
Month26	Nov-13	0.0166	-0.0063	0.0395	0.1549
Month27	Dec-13	0.0189	-0.0035	0.0413	0.0984

Table G. Result of the choice of healthcare after the policy implementation over time

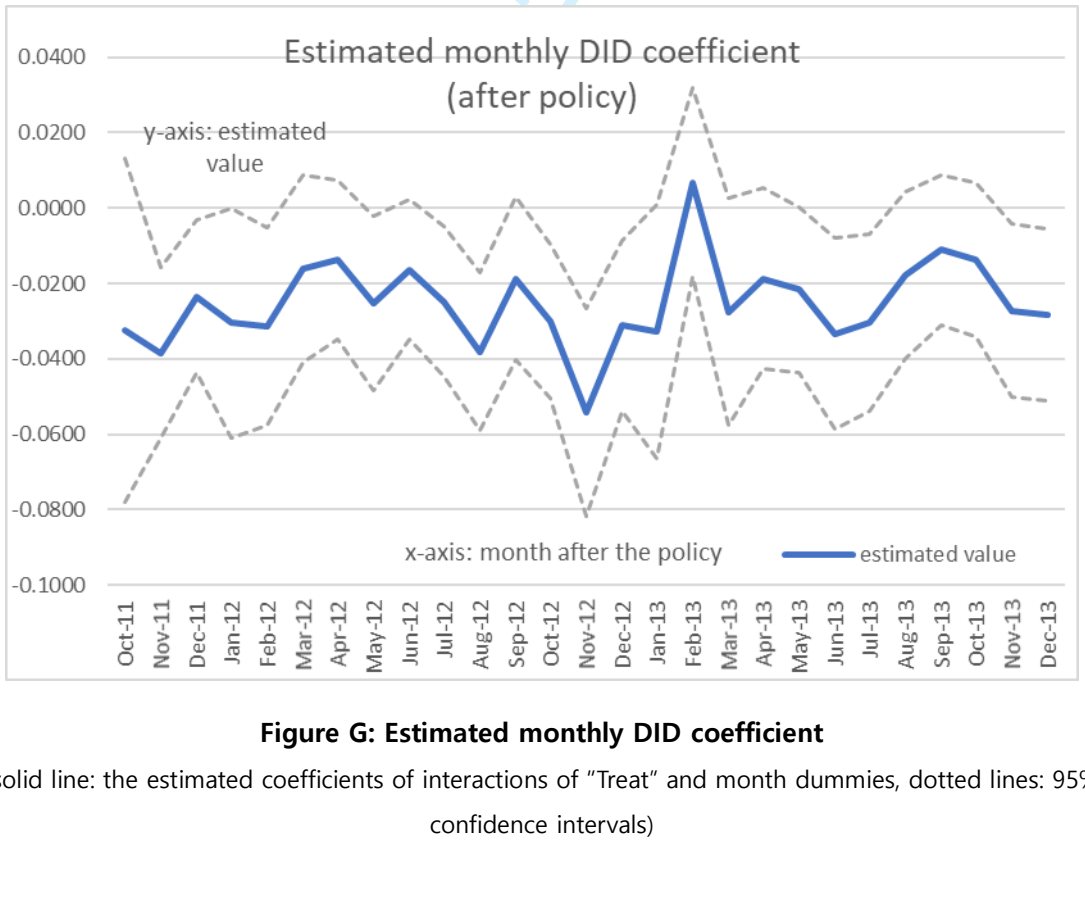


Figure G: Estimated monthly DID coefficient

(solid line: the estimated coefficients of interactions of "Treat" and month dummies, dotted lines: 95% confidence intervals)

STROBE 2007 (v4) Statement—Checklist of items that should be included in reports of *cohort studies*

Section/Topic	Item #	Recommendation	Reported on page #
Title and abstract	1	(*a*) Indicate the study's design with a commonly used term in the title or the abstract	Page 1
		(*b*) Provide in the abstract an informative and balanced summary of what was done and what was found	Page 2
Introduction			
Background/rationale	2	Explain the scientific background and rationale for the investigation being reported	Page 3
Objectives	3	State specific objectives, including any prespecified hypotheses	Page 4
Methods			
Study design	4	Present key elements of study design early in the paper	Page 6
Setting	5	Describe the setting, locations, and relevant dates, including periods of recruitment, exposure, follow-up, and data collection	Page 5~7
Participants	6	(*a*) Give the eligibility criteria, and the sources and methods of selection of participants. Describe methods of follow-up	Page 6~7
		(*b*) For matched studies, give matching criteria and number of exposed and unexposed	Page 6, 11
Variables	7	Clearly define all outcomes, exposures, predictors, potential confounders, and effect modifiers. Give diagnostic criteria, if applicable	Page 7~8
Data sources/ measurement	8*	For each variable of interest, give sources of data and details of methods of assessment (measurement). Describe comparability of assessment methods if there is more than one group	Page 6~7
Bias	9	Describe any efforts to address potential sources of bias	Page 11, 13
Study size	10	Explain how the study size was arrived at	Page 6
Quantitative variables	11	Explain how quantitative variables were handled in the analyses. If applicable, describe which groupings were chosen and why	Page 6~7
Statistical methods	12	(*a*) Describe all statistical methods, including those used to control for confounding	Page 8, 11
		(*b*) Describe any methods used to examine subgroups and interactions	Page 9
		(*c*) Explain how missing data were addressed	Page 6
		(*d*) If applicable, explain how loss to follow-up was addressed	- (No missing value)
		(*e*) Describe any sensitivity analyses	Page 10~13
Results			

Participants	13*	(a) Report numbers of individuals at each stage of study—eg numbers potentially eligible, examined for eligibility, confirmed eligible, included in the study, completing follow-up, and analysed	Page 11~12
		(b) Give reasons for non-participation at each stage	Page 10, 13~14
		(c) Consider use of a flow diagram	Supplementary Material F
Descriptive data	14*	(a) Give characteristics of study participants (eg demographic, clinical, social) and information on exposures and potential confounders	Page 5
		(b) Indicate number of participants with missing data for each variable of interest	-
		(c) Summarise follow-up time (eg, average and total amount)	Page 5
Outcome data	15*	Report numbers of outcome events or summary measures over time	Page 5
Main results	16	(*a*) Give unadjusted estimates and, if applicable, confounder-adjusted estimates and their precision (eg, 95% confidence interval). Make clear which confounders were adjusted for and why they were included	Page 9~14
		(*b*) Report category boundaries when continuous variables were categorized	Page 5
		(*c*) If relevant, consider translating estimates of relative risk into absolute risk for a meaningful time period	-
Other analyses	17	Report other analyses done—eg analyses of subgroups and interactions, and sensitivity analyses	Page 10~14
Discussion			
Key results	18	Summarise key results with reference to study objectives	Page 14~15
Limitations			
Interpretation	20	Give a cautious overall interpretation of results considering objectives, limitations, multiplicity of analyses, results from similar studies, and other relevant evidence	Page 14~15
Generalisability	21	Discuss the generalisability (external validity) of the study results	Page 15~16
Other information			
Funding	22	Give the source of funding and the role of the funders for the present study and, if applicable, for the original study on which the present article is based	Page 16

*Give information separately for cases and controls in case-control studies and, if applicable, for exposed and unexposed groups in cohort and cross-sectional studies.

Note: An Explanation and Elaboration article discusses each checklist item and gives methodological background and published examples of transparent reporting. The STROBE checklist is best used in conjunction with this article (freely available on the Web sites of PLoS Medicine at http://www.plosmedicine.org/, Annals of Internal Medicine at http://www.annals.org/, and Epidemiology at http://www.epidem.com/). Information on the STROBE Initiative is available at www.strobe-statement.org.