

# Health Screening and Selection: Evidence from Biennial Subsidies in South Korea\*

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Public health screening programs are widely used, but their impact is often limited by low participation among high-risk individuals who stand to benefit most from early diagnosis. I study selection into health screenings and their causal effects using quasi-random variation from South Korea's National Health Screening Program, which subsidizes 90–100% of screening costs every other year at even-numbered ages. Using nationally representative survey data, I find that subsidy eligibility increases screening completion by 16–19 percentage points (183–295%). Compliers with the subsidies are predominantly from lower socioeconomic backgrounds and are in poorer health than those who always participate regardless of subsidies. Using national health insurance claims data, I find that subsidy eligibility increases both in-situ and invasive cancer diagnoses by 17–19%, as well as treatments for conditions including cancer precursors, hypertension, diabetes, high cholesterol, and osteoporosis. These results demonstrate that subsidies can effectively target high-risk individuals and strengthen the impact of public health screening programs.

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# 1 Introduction

Health screenings are widely considered an essential form of preventive care that reduces premature deaths from heart disease and cancer, the two leading causes of death in developed countries (Cutler, 2008). Based on clinical trials demonstrating their effectiveness, many governments have introduced public screening programs (CanScreen<sup>5</sup>, 2022). Yet the population-level impact of these programs is often limited by poor targeting (Woolhandler and Himmelstein, 1988; Zonderman et al., 2014). Screening participation is typically lower among individuals with lower socioeconomic status (SES), who face higher cancer and all-cause mortality, raising concerns about programs' effectiveness.<sup>1</sup> Underuse among high-risk individuals reduces opportunities for early detection, while overuse among low-risk individuals raises risks of false positives, overdiagnosis, and overtreatment (Welch et al., 2016; Autier et al., 2017; Kowalski, 2023). Therefore, improved targeting could enhance early diagnosis, reduce unintended harms, and help narrow health disparities across socioeconomic groups.

Prior evidence on selection into screenings and their causal effects mostly comes from local variation, such as age 40 cutoffs for mammography or income cutoffs for subsidies (Kadiyala and Strumpf, 2016; Kim and Lee, 2017; Einav et al., 2020; Kowalski, 2023). While these designs credibly identify selection and effects around these cutoffs, they reveal little about participation across the full population, including individuals with higher or lower health risks who are excluded from the local-cutoff analyses. This limitation matters in practice, since most of the world's largest screening programs, such as the Affordable Care Act's preventive care mandate in the U.S. and Europe's organized programs, offer universal access, not targeted cutoffs. Extrapolating from local-cutoff studies risks misrepresenting who benefits and the true value of these programs, leaving policymakers uncertain about whether large-scale screening efforts succeed in reaching high-risk individuals.<sup>2</sup>

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<sup>1</sup>Pill et al. (1988), Waller et al. (1990), Bender et al. (2015) and Jones et al. (2019) provide evidence that screening participants are more likely to have higher SES, show better health behaviors, and have lower mortality compared to nonparticipants.

<sup>2</sup>When large-scale programs have been studied, researchers often rely on difference-in-differences designs (Bitler and Carpenter, 2016; Van Ourti et al., 2020; Guthmuller et al., 2023). However, these

This study analyzes South Korea's National Health Screening Program (NHSP), a nationwide policy that provides blanket subsidies for all the citizens repeatedly throughout their lifetime. I investigate two research questions: First, who is most responsive to the subsidies? Are they high-risk or low-risk individuals? Second, what are the causal effects of screening for these compliers on disease diagnosis and preventive care use?

South Korea's NHSP subsidizes 90–100% of the costs for stomach, breast, and cervical cancer screenings. It also covers a comprehensive cardiovascular and general health examination, commonly referred to as a “general screening”. Subsidies begin at age 30 for cervical screening and at age 40 for other screenings, and are provided during the calendar year when an individual reaches an even-numbered age.<sup>3</sup> I show that this rule generates quasi-random variation in subsidy eligibility, conditional on a flexible function of age,<sup>4</sup> and creates large variation in screening take-up between treatment (even-aged individuals eligible for subsidies) and control (odd-aged individuals ineligible for subsidies) groups. The identifying assumption is that, conditional on age, there are no systematic differences between treatment and control groups other than subsidy eligibility. As I show later, my identifying variation is a short-term temporal variation in subsidy eligibility and captures both net increases in screening participation and intertemporal substitution, as individuals may shift screenings from unsubsidized to subsidized ages.

This study uses two complementary datasets: (i) Korean Health Panel Survey and (ii) National Health Insurance Service claims data. The survey provides nationally representative information on 107,200 individual-year pairs from 2008 to 2018, including detailed measures of socioeconomic status, health care utilization, and health behaviors. It also records the exact date, type, and result of each screening, which I use to analyze the impact of subsidies on take-up and selection. The administrative claims data cover more than 7,450,000 individual-year observations from 2002 to 2021 and include comprehensive

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approaches typically do not analyze selection into treatment.

<sup>3</sup>The policy rule verbatim states that individuals born in an even (odd) year are eligible for subsidies in even (odd) calendar years. This is equivalent to eligibility in the calendar year when one turns an even-numbered age.

<sup>4</sup>Age controls adjust for mechanical imbalance in an analytical sample starting at age 40 (an even number), which makes even-aged individuals younger on average than odd-aged individuals.

information on health care utilization. Importantly, they record all cancer diagnoses, regardless of whether detected through screening or not, which I use to analyze the impact of screenings. However, they have incomplete records of screening participation and are therefore not used to measure take-up.

I find that subsidy eligibility increases screening take-up by 16–19 percentage points (183–295%) for general, stomach, breast, and cervical screenings, corresponding to arc elasticities of  $-0.48$  to  $-0.72$ .<sup>5</sup> In addition, I document sizable spillovers across screening types. Subsidy eligibility increases take-up of annually subsidized liver and colorectal screenings by 2.7–3.3 percentage points (94–124%) and unsubsidized lung and prostate screenings by 0.6–0.7 percentage points (67–81%). These cross-screening spillovers are larger in magnitude than the cross-vaccine spillovers of 8–34% documented by [Carpenter and Lawler \(2019\)](#) and 1.5% by [Humlum et al. \(2024\)](#). These spillovers suggest that subsidies may generate broader health gains than intended, increasing the overall value of the policy.

To examine who responds to subsidies, I compare compliers, who screen if and only if subsidized, with always-takers, who always screen regardless of subsidies, and never-takers, who never screen. Compliers fall between always- and never-takers: they have lower socioeconomic status and worse health than always-takers, but show healthier behaviors than never-takers. Compared to always-takers, compliers have 12% lower household income, are 40% less likely to be college-educated, and are 13% less likely to be employed. Consistent with negative selection on SES, they are also 68% more likely to be diagnosed with a stomach disease. Compared to never-takers, compliers engage in healthier behaviors: they are 30% less likely to smoke and 18% more likely to engage in vigorous exercise. These findings suggest that subsidizing screening disproportionately attracts higher-risk individuals who stand to benefit more from earlier diagnosis. This selection pattern contrasts with prior work by [Einav et al. \(2020\)](#), who find that compliers

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<sup>5</sup>These effects are substantially larger than those found in prior studies on preventive care, including the RAND Health Insurance Experiment (arc elasticities  $-0.17$  to  $-0.43$ ), the Oregon Medicaid Experiment (41–63% increase), and a related study on income-based screening subsidies (arc elasticity  $-0.47$ ), with results reported in [Newhouse \(1993\)](#), [Finkelstein et al. \(2012\)](#), and [Kim and Lee \(2017\)](#). I later show that intertemporal substitution partly explains this difference.

with mammography guidelines are healthier than always-takers.

Next, using National Health Insurance Service claims data, I estimate the intent-to-treat (ITT) effects of subsidy eligibility on cancer diagnoses. Subsidy eligibility increases the one-year overall cancer detection rate by 0.16 percentage points (18%), with significant gains in six of the seven cancer types examined. Notably, stomach cancer diagnoses rise by 0.08 percentage points (41%), and breast cancer diagnoses by 0.05 percentage points (11%). The effects extend beyond cancers directly targeted by biennial subsidies. For example, colorectal cancer diagnoses increase by 0.02 percentage points (10%). While the main analysis focuses on ITT effects, local average treatment effects (LATE) of one screening can be calculated by dividing ITT estimates by increases in take-up rates estimated from survey data. The resulting LATEs are 0.0041 percentage points (213%) for stomach cancer, 0.0026 percentage points (56%) for breast cancer, and 0.0055 percentage points (287%) for colorectal cancer. These LATE estimates represent the cancer detection rates among compliers and are substantially large, consistent with the finding that compliers are disproportionately high-risk individuals. For context, [Guthmuller et al. \(2023\)](#) estimate a LATE of 0.0010 percentage points for breast cancer in Europe's organized screening programs, less than half of my estimate of 0.0026 percentage points. Similarly, [Kim and Lee \(2017\)](#) find a LATE of 0.0025 percentage points for stomach cancer from income-based subsidies in South Korea, smaller than my estimate of 0.0041 percentage points.

Medical studies emphasize the role of early detection of cancers in decreasing morbidity and mortality.<sup>6</sup> I break down aggregate diagnoses by cancer stage and show that screening increases detection of both early stage in-situ (stage 0) and invasive (stage 1–3) cancers. For instance, subsidy eligibility increases invasive breast cancer diagnoses by 0.03 percentage points (8%) and in-situ diagnoses by 0.016 percentage points (24%). Consistent with earlier detection, breast cancer patients diagnosed in the treatment group exhibit 1.7 percentage points (1.8%) higher five-year survival rate than those diagnosed

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<sup>6</sup>For a summary of the evidence, see the U.S. Preventive Services Task Force (USPSTF) evidence reviews ([Nicholson et al., 2024](#); [Davidson et al., 2021](#); [Krist et al., 2021](#); [Grossman et al., 2018](#); [Curry et al., 2018a](#)) and the Korean stomach cancer screening guideline ([Park et al., 2015](#)).

in the control group. These findings imply that screenings effectively detect cancers at early stages, where treatment can improve outcomes.

Finally, screenings increase the use of preventive care for precancerous conditions and cardiovascular risk factors. Beyond the detection of cancers, screenings help prevent cancer development by identifying precursors such as Helicobacter pylori infection and colon polyps, which are strongly linked to stomach and colorectal cancers. Treatments for these conditions increase by 0.003–0.720 percentage points (15–62%). Screenings also lead to higher medication use for high blood pressure, diabetes, and high cholesterol by 0.02–0.42 percentage points (0.17–3.01%). Among other conditions covered by screenings, prescriptions increase for osteoporosis by 0.11 percentage points (4%). These findings indicate that screenings not only detect cancers but also promote broader preventive care, potentially improving overall population health.<sup>7</sup>

A unique feature of the research design is the alternating treatment structure: individuals switch between treatment and control status each year. This generates identifying variation in screening take-up through two channels: (i) increasing overall participation over the lifetime and (ii) shifting screening timing from control to treatment ages. To shed light on the role of intertemporal substitution, I analyze two sharp changes in subsidy eligibility: the age 40 eligibility cutoff and January/December cutoff in subsidized years. The results show that biennial subsidies generate both a net increase and substitution, with screenings advanced more often than delayed. Given these findings, I interpret the treatment effect on cancer diagnoses as a short-run effect of screenings, consisting of both a lifetime increase in detection and earlier diagnosis.

This study advances our understanding of selection into health screenings. Previous research has mostly relied on local variation, such as income thresholds for subsidies or age 40 cutoffs for mammography recommendations (Kim and Lee, 2017; Einav et al., 2020; Kowalski, 2023). By design, such local variation restricts compliers to relatively homogeneous groups near the cutoff and leaves open the question of whether large-scale, universal screening initiatives, like the Affordable Care Act's preventive care mandate in

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<sup>7</sup>Impact on one-year mortality was also examined. Although estimates are negative for all-cause and cause-specific mortality, they are statistically insignificant, likely due to short follow-up period.

the U.S., effectively reach high-risk individuals.<sup>8</sup> In contrast, I exploit even- and odd-age eligibility rules in a national-scale program, generating broad, population-wide variation. I document that compliers are predominantly from lower socioeconomic backgrounds with poorer health, demonstrating that universal screening programs with subsidized access can successfully target the individuals most likely to benefit.

Beyond informing selection, the population-wide variation also provides a more complete picture of the causal effects of screenings. Prior studies, which focus narrowly on impacts around specific age or income cutoffs (Decker, 2005; Kadiyala and Strumpf, 2012, 2016; Kim and Lee, 2017; Myerson et al., 2020; Sun et al., 2022), do not reveal how effects differ across the broader population. Yet cancer incidence rises sharply with age and risk is concentrated among disadvantaged groups, making these distributional effects essential for assessing policy impact. By estimating effects at both the age 40 cutoff and at older ages, I show that screening subsidies have substantially larger effects on cancer detection among older individuals, and that impacts are also greater for low-income groups than for the high-income population. Without capturing this heterogeneity, we risk understating the benefits of universal screening programs and their role in reducing health disparities.

Finally, this study broadens both the types of screenings and the scope of outcomes analyzed. Prior research has largely focused on a single screening, most often mammography (Decker, 2005; Bitler and Carpenter, 2016, 2017; Van Ourti et al., 2020; Sun et al., 2022; Guthmuller et al., 2023). By jointly examining eight screening types, I reveal heterogeneous effects across screenings and positive spillovers in participation and cancer detection. Without this comprehensive perspective, we risk understating the overall health gains from subsidies and overlooking their cost-effectiveness. In addition, I extend outcomes beyond aggregate diagnoses by distinguishing early-stage in-situ cancers and precancerous conditions. This disaggregation demonstrates that screenings fulfill the goal of early detection and treatment, the key mechanism for improving survival.

The rest of the paper is organized as follows. Section 2 describes the disease burden

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<sup>8</sup>Kowalski (2023) examine selection in an influential Canadian clinical trial on mammography, but restrict their analytical sample to women aged 40–49, as their research question focuses on overdiagnosis among women in their 40s.

in South Korea and provides an overview of the National Health Screening Program. Section 3 outlines the identification strategy using even/odd age-based subsidies. Section 4 discusses the survey and administrative datasets used for this study. Section 5 presents the results on take-up, selection into screenings, and their causal effects. Section 6 concludes.

## 2 Institutional background

### 2.1 Disease burden and screenings in South Korea

Cancer and cardiovascular diseases are leading public health challenges in South Korea due to their high prevalence and contribution to mortality. In 2023, cancer accounted for approximately 24% of all deaths. Cardiovascular diseases, including heart disease, stroke, and hypertension, together with related risk factors like diabetes, were responsible for an additional 22%. Many of these conditions are preventable or manageable if identified early, making regular screening a critical component of population health policy.

To address this burden, the National Health Screening Program (NHSP) offers two types of publicly financed screenings. The first is a comprehensive set of tests commonly referred to as a “general health screening” that targets cardiovascular and metabolic risk factors, including high blood pressure, blood glucose, and cholesterol. It also includes urine tests to assess kidney and liver function, and chest X-rays to screen for tuberculosis.<sup>9</sup> Additional age-specific tests include osteoporosis screening, mental health assessments, and evaluations of functional capacity in older adults. Individuals identified as high-risk for hypertension, diabetes, or tuberculosis are referred for follow-up testing and consultation.

Second, five types of cancer screenings are subsidized under the NHSP: stomach, breast, cervical, liver, and colorectal. These screenings target cancers that are relatively common in South Korea and for which early detection is known to improve health out-

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<sup>9</sup>South Korea has the highest incidence and mortality rates of tuberculosis (TB) among OECD countries (Cho, 2018). TB screening is mandatory for students entering middle and high school, and adults receive biennial TB screening as part of the general health screening.

comes. Stomach cancer is particularly prevalent in Korea, and East Asia more broadly, and it has consistently ranked among the top five cancers in both incidence and mortality for both men and women.<sup>10</sup> It is screened using upper endoscopy. Breast cancer is the most frequently diagnosed cancer among Korean women and is screened using bilateral mammography. Cervical cancer incidence has declined steadily over the past two decades, and is screened with Pap smears. Liver cancer, while a major contributor to cancer mortality, is screened only for high-risk individuals, such as those with chronic liver disease or hepatitis, and is not universally subsidized. Screening is performed using liver ultrasound. Colorectal cancer, the second most common cancer in 2022, is initially screened using a fecal occult blood test (FOBT), with colonoscopy subsidized only for individuals who test positive in FOBT. However, due to relatively low costs and low perceived risk, many individuals opt to undergo colonoscopy directly without prior FOBT ([Baik and Lee, 2023](#)).

Not all high-burden cancers are covered by the NHSP. This study additionally considers two unsubsidized screenings: lung and prostate cancer. Lung cancer is the leading cause of cancer-related death in South Korea, but past screening technologies, chest radiography, have limited effectiveness in detecting early-stage disease in the general population ([American Lung Association, 2023](#)). As a result, lung cancer screening was not subsidized during the study period and was only added to the public program in 2019, right after our study period, for high-risk individuals with the new technology: low-dose computed tomography ([Aberle et al., 2013](#)). Prostate cancer, while highly common among older men, is also excluded from the subsidized program due to its slow progression. Even without screening, it is likely to remain asymptomatic throughout life, and routine screening could lead to overdiagnosis and overtreatment, raising concerns about potential harms outweighing benefits ([Neal and Donovan, 2000](#)). Both lung and prostate screenings are therefore typically paid for entirely out-of-pocket.

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<sup>10</sup>The age-standardized incidence rates per 100,000 were 27 for South Korea and 27.6 for Japan in 2022, compared to 4.1 in the United States and 6.4 in Germany ([World Cancer Research Fund, 2024](#)).

## 2.2 Subsidy schedule and program design

The National Health Screening Program (NHSP) provides subsidies to encourage participation in preventive screenings. The subsidy schedule follows clinical guidelines on starting age and screening frequency. For instance, screenings recommended biennially from age 40 are subsidized every two years beginning at that age. This section outlines the NHSP subsidy policy by screening type and highlights the quasi-random variation in subsidy timing that informs our identification strategy.

Screenings can be grouped into three categories based on their subsidy frequency: biennial, annual, and unsubsidized. Table 1 summarizes the rules. Biennial screenings, including general, stomach, breast, and cervical, are subsidized every other year. A key feature of the program is that subsidies are provided throughout the calendar years when one's age is even-numbered. Here, "age" refers to calendar age, calculated as the difference between the current year and birth year. For example, someone born in 1970 would be eligible for subsidized screenings throughout 2020, since her age is 50 ( $= 2020 - 1970$ ), an even number. Next year in 2021, however, her age would turn 51, an odd number, so she would need to pay the full costs for the same screenings if she wants to receive them.<sup>11</sup> Annual screenings include liver and colorectal cancer screenings, which are subsidized every year.<sup>12</sup> Lung and prostate cancer screenings are not subsidized under the NHSP during the study period.

For each subsidized screening, whether biennial or annual, there is a cutoff age where subsidies begin. For general, stomach, breast and liver screenings, subsidies start at age 40. There are no subsidies before age 40, even if one's age is even. Hence, general, stomach, and breast screenings that follow the biennial schedule are subsidized at ages 40, 42, 44 and so on.<sup>13</sup> Liver screening that follows the annual schedule is subsidized at ages

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<sup>11</sup>The policy rule verbatim states that those born in even (odd) years are eligible for subsidies at even (odd) years. This is mathematically equivalent to the rule where one is eligible for subsidies at even ages, defined as the difference between the current year and birth year. This is because the difference between two even numbers or two odd numbers is always even.

<sup>12</sup>Colorectal screening was biennially subsidized until 2011 and annually thereafter. Because it was annual for the majority of the study period, it is treated as such in the main analysis.

<sup>13</sup>Two minor exceptions exist in the general health screening program: (i) some employee- and self-employed-insured individuals may receive biennial subsidies before age 40, and (ii) non-office workers are eligible for annual rather than biennial screenings. These exceptions apply only to general screening,

40, 41, 42, and onward. The age cutoff for cervical screening is 30, while for colorectal screening, it is 50.<sup>14</sup> Since cervical screening follows a biennial schedule, subsidies are available at ages 30, 32, 34 and onward. Colorectal screening following annual schedule is subsidized at ages 50, 51, 52 and onward. No age thresholds apply to lung and prostate cancer screenings, as they were not subsidized.

The amount of subsidies is full coverage for the general health screening and 90% subsidy for 5 types of cancer screenings.<sup>15</sup> As a result, individuals pay nothing for general screening and only 10% of the cost for cancer screenings.<sup>16</sup> For lower-income individuals, even the 10% copay is waived, effectively making all screenings free.<sup>17</sup> Excluding liver screening, which is subsidized only for high-risk individuals, the total amount of available screening subsidies one can receive at even ages is approximately \$115 for men and \$165 for women. This corresponds to 4.5-6.5 percent of annual health care expenditure per capita ([World Bank, 2018](#)).

The program was designed to maximize participation by ensuring easy access to subsidized screenings. Screenings can be obtained at public health clinics or designated private hospitals and clinics approved by the NHSP.<sup>18</sup> While appointments are generally not required for general screening, they are typically necessary for more specialized procedures such as upper endoscopy for stomach screening or colonoscopy for colorectal screening. Eligible individuals receive paper mail or mobile reminders, which specify the available subsidized screenings and list nearby participating providers.

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not any cancer screening, and survey evidence suggests limited adherence to these exceptions. Since our analysis begins at age 40, they do not affect identification. Hence, we abstract from them.

<sup>14</sup>In 2016, the age cutoff for cervical screening was lowered to 20. Since age 30 was the cutoff for most of the study period, this analysis adopts age 30 threshold.

<sup>15</sup>Under the Occupational Safety and Health Act, employers are required to ensure their employees undergo general health screening. When employees do not participate, employers face financial penalty of approximately \$40. This mandate applies only to general screening, not to cancer screenings.

<sup>16</sup>Subsidy generosity increased over time. For example, cervical screening was fully subsidized during the study period, and colorectal screening became fully subsidized shortly afterward.

<sup>17</sup>The copay waiver applies to individuals whose health insurance premium falls below the median. Because the premium is income- and asset-based, the waiver primarily benefits lower-income individuals. See [Kim and Lee \(2017\)](#) for an analysis of take-up and detection effects at the premium threshold.

<sup>18</sup>As of December 2023, approximately 5,800 private facilities were designated for NHSP screening, equivalent to one center for every 900 adults aged 40 and older ([National Health Insurance Service, 2023](#)).

### 3 Identification

Figure 1a provides intuition for the subsidy-induced variation in screening take-up. The figure plots the take-up of general screening, which is subsidized biennially from age 40. Before age 40, take-up rates are similar for even and odd ages. However, after age 40, take-up at even ages jumps to around 27 percent, while take-up at odd ages remains steady around 10 percent. The take-up gap between the even and odd ages after 40 captures the effect of biennial subsidies on screening take-up.

While the distinction between even and odd ages seems plausibly exogenous, the even-age group is mechanically younger than the odd-age group in an analytical sample starting from age 40, an even number.<sup>19</sup> I argue that age is the only difference between the two groups. Hence, my identifying assumption is that, conditional on age, there are no systematic differences between the treatment group (even-aged individuals eligible for subsidies) and the control group (odd-aged individuals ineligible for subsidies) other than subsidy eligibility. To assess the validity of this assumption, I test for balance in observable characteristics using the following econometric specification.

$$y_{it} = \beta_0 + \beta_1 \cdot treat_{it} + f(age_{it}) + \epsilon_{it} \quad (1)$$

The treatment group indicator,  $treat_{it}$ , equals 1 if individual  $i$  has an even age in year  $t$ . Note that the treatment group is not individual-specific, but varies with years. Age control variable,  $f(age_{it})$ , is a function of age flexible enough to remove the age effect between the treatment and the control group. The main specification uses linear splines with 5-years intervals, and robustness checks with alternative age controls are reported in the Appendix Section B. Standard errors are clustered at the individual level.<sup>20</sup>

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<sup>19</sup>The imbalance arises from the choice of starting age rather than the ending age. In a sample beginning at age 40, the even-age group is always younger than the odd-age group, since the age distribution declines with age starting from 40. This generates imbalances in age and in other covariates correlated with age. By contrast, a sample starting at age 39 produces imbalances in the opposite direction. Appendix Section C exploits this reversal to provide nonparametric estimates of the main results.

<sup>20</sup>Despite using panel data, the main specification does not include any panel method to make it clear that the identification strategy does not require panel structure. Robustness checks using individual fixed effects are provided in Appendix Section B.

Table 2 presents conditional balance between treatment and control groups using survey data for individuals aged 40 to 89. First, note that the two groups are almost equally sized, suggesting that the even/odd age rule evenly divides the sample into treatment and control groups. Column 3 shows that, after controlling for age, differences in covariates are small and statistically insignificant. Hence, we can attribute the difference in take-up between the treatment and the control group as the causal effect of screening subsidies. Similar balance checks using two administrative health insurance datasets are presented in Table A2 and A3, confirming that treatment and control groups are well balanced conditional on age.

A unique feature of my research design is its alternating treatment structure: individuals switch between treatment and control status in successive years. This structure generates identifying variation in screening take-up through two channels: (i) an increase in net participation and (ii) an intertemporal shift in screening timing from control to treatment ages. Section 5.1.2 provides supporting evidence that biennial subsidies both raise overall participation and shift screening timing, with screenings more often advanced than delayed. Therefore, I interpret treatment as a combination of increased lifetime screening participation and earlier take-up at a subsidized age relative to the subsequent unsubsidized age

## 4 Data

This study draws on two complementary data sources: nationally representative survey data on health care utilization and administrative national health insurance claims data. The survey data provide detailed information on health screening take-up, but are not big enough to study rare outcomes, such as cancer incidence or mortality. The administrative claims data include a sufficient number of cancer incidences and deaths for statistical analysis, but have incomplete records of screening participation. Although the two datasets cannot be linked at the individual level, I use both to provide insight on the effect of subsidies on screening and subsequent behaviors.

The survey data come from the Korean Health Panel Survey, spanning the years 2008-2018.<sup>21</sup> It is a longitudinal household survey with around 107,200 individual-year pairs. Data were collected annually through face-to-face interviews using computer-assisted personal interviewing (CAPI). All household members were surveyed each year, and all variables are self-reported.

The survey datasets include annual data on demographic and socioeconomic characteristics, health care utilization, and health behaviors. Socioeconomic status variables include yearly income and educational attainment. Health care utilization data are recorded at the visit level for outpatient, inpatient, and emergency services.<sup>22</sup> For each hospital visit, the dataset records the date, total expenditures on hospital and drug services, and diagnosis codes.<sup>23</sup> Hospital visits for health screenings include additional information on the type of screening (general or cancer-specific), medical tests performed, results, and any diagnosis made. The dataset also includes information on health behaviors such as smoking, drinking, and physical activity. For each behavior, the dataset indicates participation and frequency. For smoking and drinking, I consider both current use and daily use to capture behavioral intensity.<sup>24</sup> Physical activity is classified by intensity into vigorous exercise, moderate exercise, and walking.<sup>25</sup>

To complement the survey data, this study utilizes national health insurance claims data, specifically the Standard Cohort Database (2002-2019) and the Customized Cohort Database (2002-2021). While a slightly different set of variables are available in each dataset,<sup>26</sup> both are randomly drawn cohorts with similar average demographic and

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<sup>21</sup>It is version 1.7.1 made jointly by Korean Institute for Health and Social Affairs and National Health Insurance Services.

<sup>22</sup>Although data collection interviews were conducted annually, the unit of observations in the utilization datasets is at the visit level. This was possible because survey participants were asked to keep a specifically designed health diary and save all receipts from medical visits and pharmacy purchases. Survey enumerators collected these records during annual visits, and cross-checked diary entries against receipts. Each interview began by documenting medical visits since the date of the previous interview, ensuring no missing period.

<sup>23</sup>Diagnoses were coded using the Korean Classification of Diseases, a Korean version of ICD-10.

<sup>24</sup>Behavioral status is based on activity in the month preceding the interview.

<sup>25</sup>Specific examples for such activities were provided at the survey. They are measured based on activity during the week prior to the interview.

<sup>26</sup>While both provide demographic information and health care utilization records, the Standard Cohort Database provides data on prescription and cause-specific mortality, whereas the Customized Cohort Database includes cancer incidence data.

health insurance characteristics. Therefore, they are collectively referred to as health insurance claims data in this study. The number of observations in the analytical sample is around 8,670,000 individual-year pairs in the Standard Cohort Database and 7,450,000 individual-year pairs in the Customized Cohort Database.

The claims data contain cancer diagnosis records regardless of whether the diagnosis was made through screening or not. Cancer diagnoses are inferred using information from the Coinsurance Reduction Program for Rare and Severe Diseases (CRP), administered by the National Health Insurance Service (NHIS) from year 2004. Under this program, coinsurance rates for hospital visits, which normally range from 20-50%, are reduced to 0-10% for patients with rare and severe diseases, including cancers, stroke, tuberculosis, and other rare or incurable conditions that incur large health care expenditures. For newly diagnosed cancer patients, registering with the CRP is typically one of the first steps, making it a reliable source for identifying true cancer diagnoses, not including false positives.<sup>27</sup> The CRP is implemented independently from the health screening programs, so it captures cancer diagnoses regardless of how the cancer was detected, whether through screening or not. I use this program to infer new cancer diagnoses and investigate whether subsidies at even ages lead to more cancer diagnoses at even ages.

A limitation of the insurance claims data is the incomplete record of screening participation. This is because most screenings conducted at odd ages are paid entirely out-of-pocket and therefore do not generate any insurance claim. As a result, screenings at odd ages are not captured in the insurance claims data. In contrast, survey data do not suffer from this limitation, since it captures all the self-reported screening take-up. For this reason, this study relies on survey data for analyzing screening take-up and uses claims data primarily to investigate subsequent cancer diagnoses and preventive care use.

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<sup>27</sup>Even individuals who do not formally register can be identified if they visit a hospital for cancer treatment at least once. For CRP-registered cancer patients, the coinsurance rate is reduced to 5%. For unregistered patients, it drops to 10%. These reductions apply only to visits related to the registered condition. Unrelated visits are not covered by the program.

## 5 Results

### 5.1 Effect on screening take-up

#### 5.1.1 Effect on biennial screenings

Subsidy eligibility leads to large increases in screening participation. Figures 1a, 1b, 1c, and 1d show that across general, stomach, breast, and cervical screenings, participation is consistently higher in the treatment group (even ages) than in the control group (odd ages). Using Equation (1), I estimate that subsidy eligibility raises one-year participation by 18.7 percentage points (183%) for general screening, 19.0 percentage points (229%) for stomach screening, 19.1 percentage points (283%) for breast screening, and 16.4 percentage points (295%) for cervical screening. These effects correspond to arc elasticities of  $-0.48$ ,  $-0.65$ ,  $-0.72$ , and  $-0.60$ , respectively. The magnitudes are substantially larger than those documented in seminal studies of preventive care. The RAND Health Insurance Experiment, for example, reported arc elasticities of  $-0.17$  to  $-0.43$  for preventive services (Newhouse, 1993). The Oregon Medicaid Experiment found 41–63% increases in mammography and Pap tests (Finkelstein et al., 2012). A related study of screening subsidies in Korea estimated an arc elasticity of  $-0.47$  for stomach and breast screenings (Kim and Lee, 2017). The larger responses in my setting are partially due to shift in screening timing, which I examine in the next section.

Additional evidence from the subsidy cutoff age is reported in Table A27 in Appendix Section J. Participation rises sharply at the age threshold where subsidies begin, consistent with a causal effect of eligibility. Robustness checks using alternative specifications, additional covariates, and different analytical samples, reported in Appendix Section B and C, confirm that these findings are not driven by functional form assumptions, age controls, or sample composition.

#### 5.1.2 Intertemporal substitution

One mechanism through which subsidies affect participation is intertemporal substitution. The knowledge of subsidy schedule allows forward-looking individuals to temporally

reallocate screenings to subsidized ages. Such timing shifts can widen the take-up gap between subsidized and unsubsidized ages without any net increase in participation. While estimating the magnitude of intertemporal substitution is inherently challenging,<sup>28</sup> this study provides suggestive evidence that biennial subsidies generate both net increases in participation and shifts in screening timing, with advancing screenings more common than delaying them. Appendix Section D provides detailed analyses supporting these findings. Here, I summarize the key findings.

First, evidence for a net increase comes from age cutoffs for subsidy eligibility, such as age 40 for stomach and breast screenings. If individuals were substituting from unsubsidized (odd) to subsidized (even) ages, one would expect a sharp drop in participation at unsubsidized ages compared to pre-cutoff level, immediately following the cutoff as biennial subsidies become available. I do not find such decline, indicating limited substitution. A possible concern is compositional changes in screening participants at the cutoff. If more people start to participate at the cutoff regardless of subsidy eligibility, this could potentially mask drop in take-up at unsubsidized ages. To address this, I restrict the sample to individuals who participated in screenings before age cutoff and track their behavior. Among this group, participation at subsidized ages rises significantly, while participation at odd ages remains stable relative to the pre-cutoff period. This pattern indicates a genuine net increase in take-up for this particular group.

Second, evidence for shifts in timing comes from the distribution of screening dates within subsidized years. The monthly distribution of screening take-up reveals significant bunching in December of subsidized ages, consistent with individuals advancing screenings before subsidies expire. In contrast, there is no similar bunching in January, which would be expected if individuals delay screenings from unsubsidized ages and receive them immediately after subsidies become available. This asymmetry suggests that advancing screening is more common than delaying them. Based on these results, I interpret the

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<sup>28</sup>Estimating intertemporal substitution is challenging for two reasons. First, universal eligibility under the biennial subsidy program leaves no cross-sectional variation. Second, the alternating design creates a discontinuity at every age. Standard approaches to estimating substitution in health care utilization under nonlinear pricing (Einav et al., 2015, 2017; Abaluck et al., 2018; Alpert, 2016) rely on points away from a cutoff to predict counterfactual mass, which is infeasible here. Moreover, substitution may occur in both forward and backward directions, further complicating estimation.

treatment in this study as reflecting a combination of an increase in overall screening take-up and shifting screening forward to a subsidized year.

### 5.1.3 Cross spillover

This study documents spillovers across different screening types. Screenings not subject to the biennial subsidy, such as annually subsidized liver and colorectal screenings or unsubsidized prostate and lung screenings, have no reason to display systematic differences in take-up between even and odd ages. However, Figure 1e, 1f, 1g, and 1h show that they nonetheless exhibit higher take-up at even ages. Table 4 estimates the differences in take-up using the specification from Equation (1): take-up increases by 2.7 percentage points (94%) for liver, 3.3 percentage points (124%) for colorectal, 0.7 percentage points (81%) for prostate, and 0.6 percentage points (67%) for lung screening. There is also spillovers arising due to different age thresholds, such as discontinuous increase in cervical and colorectal screening take-ups at age 40, despite their subsidies starting from age 30 and 50, respectively. Table A28 in Appendix Section J provides estimates of these spillovers.

A likely mechanism of cross spillover is bundling of multiple screenings during a single hospital visit, reflecting shared fixed costs (e.g., time, travel, or fasting requirements for medical tests). Appendix Table A10 shows that 96% of individuals receiving both general and stomach screenings in the same year do so on the same day. Additionally, screenings not performed on the same day tend to occur after the general screening, consistent with doctors recommending further tests. Between doctors and patients, patients are more likely to be a driver of this bundling behavior according to selection patterns presented in A12 in Appendix Section E. Individuals who, after getting a biennial screening, further participate in annual or unsubsidized screenings are more likely to be in better health and from higher socioeconomic backgrounds than those who only get a biennial screening. Similar cross-vaccine spillovers have been documented, but their magnitudes, 8-34 percent for Carpenter and Lawler (2019) and 1.47 percent for Humlum et al. (2024), are much smaller than our estimates in percentage terms.

## 5.2 Selection into screening

### 5.2.1 Cross-sectional analysis

A common criticism against health screening programs is that they tend to attract relatively healthy individuals, while those at higher risk, such as people with lower income or education levels, are less likely to participate (Pill et al., 1988; Waller et al., 1990; Bender et al., 2015; Jones et al., 2019).<sup>29</sup> Underuse among high-risk individuals undermines the effectiveness of screenings for early diagnosis, whereas overuse among low-risk individuals increases the likelihood of false positives, overdiagnosis, and overtreatment (Kowalski, 2023; Welch et al., 2016; Autier et al., 2017). As a result, improving participation among higher-risk individuals is crucial for maximizing the benefits and minimizing the harms of screening programs.

To understand whether subsidized screenings successfully reach high-risk individuals, I analyze the types of people whose behavior changes in response to the policy. These individuals, who would not have been screened without subsidies but opt in when screenings are subsidized, are referred to as compliers, following the terminology of Imbens and Angrist (1994) and Angrist et al. (1996). Understanding who these compliers are is essential, as they represent the population whose behavior is most responsive to the policy and thus critical for assessing its effectiveness. I compare compliers with two other groups: always-takers, who always participate even in the absence of subsidies, and never-takers, who never participate even in the presence of subsidies.<sup>30</sup> Since the goal of screening is to detect diseases at an early stage, the policy should ideally target unhealthy individuals who are at higher risk of having undiagnosed conditions.

One approach to characterizing compliers relative to always-takers is to restrict the

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<sup>29</sup>The pattern of preventive care take-up by healthier individuals extends beyond screenings. Similar selection pattern is observed in the use of vaccinations, contraception, workplace wellness programs, and vitamin supplements (Thomas et al., 2021; Dalton et al., 2020; Jones et al., 2019; Oster, 2020). In developing countries, selection in response to the price of preventive health products has been shown for antimalarial medications, bednets, and water purification kits (Cohen and Dupas, 2010; Cohen et al., 2015; Dupas, 2014; Tarozzi et al., 2014; Ashraf et al., 2010).

<sup>30</sup>I impose monotonicity assumption, meaning that subsidies weakly increase the probability of screening. This assumption rules out defiers, who would get screened only in the absence of a subsidy. I relax this assumption in the longitudinal analysis in the next section.

analytical sample to screening participants and compare treatment and control groups.<sup>31</sup> Under this restriction, the participants in the treatment group are either always-takers or compliers, while the participants in the control group must be always-takers. The differences between participants in the treatment and the control come from group composition, specifically, the presence of compliers in the treatment group. Since all individuals in this restricted sample are screening participants, any observed differences cannot reflect treatment effects but instead reflect pre-existing characteristics. Similarly, restricting the sample to nonparticipants and comparing treatment and control allows for comparisons between untreated compliers and never-takers.<sup>32</sup>

Figure 2a, 2b, and 2c show that, compared to always-takers, compliers are more likely to be diagnosed with a stomach disease, have lower household income, and are less likely to be college graduates. Figure 2a plots the share of stomach screening participants who report being diagnosed with a disease.<sup>33</sup> Diagnosis rates are consistently higher among participants in treatment than those in control, which suggests that compliers have worse underlying health conditions prior to screening than always-takers. Similarly, Figure 2b and 2c show that screening participants in the treatment group have lower household income and are less likely to have a college degree. Together, these patterns imply that compliers are systematically of lower socioeconomic status relative to always-takers.

While the figures illustrate the selection patterns intuitively, they are limited to qualitative comparisons. To formally test and quantify the selection patterns observed in the descriptive figures, I estimate the average characteristics of compliers, always-takers, and never-takers in three steps. First, I estimate the characteristics of always-takers using

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<sup>31</sup>Complier characterization is straightforward in settings with one-sided noncompliance. For example, in a setting with only never-takers but no always-takers, treatment group can be used to distinguish between never-takers and compliers. Nonparticipants in the treatment group are never-takers, while the participants are compliers. Similar comparison can be made in the control group when there are only always-takers but no never-takers.

<sup>32</sup>In this analysis, I use the term “always-takers” or “never-takers” in the cross-sectional sense. In a given year, participants in the control group are “always-takers” in the sense that if they were in treatment group this year, they must have also participated in screening. I do not use panel structure in this analysis, that is, whether an individual actually participates all the years during the study period. I use the panel structure for selection analysis in the next section on longitudinal inference.

<sup>33</sup>The survey asked screening participants whether they had been diagnosed with any condition through screening. For stomach screening, this includes cancer as well as less severe conditions, such as inflammation or ulcers. See Appendix Section H for common diagnoses.

screening participants in the control group. Due to the exogeneity of the even/odd subsidy rule, always-takers in the treatment group are comparable to the always-takers in the control group conditional on age. Similarly, I estimate the characteristics of never-takers using nonparticipants in the treatment group. Second, I infer complier characteristics from the pool of screening participants in the treatment group, which is a convex combination of complier and always-taker characteristics, where the relative weights are derived from first-stage estimates of subsidy effects on take-up. Combining the first-stage estimates from Table 3 with the characteristics of always-takers from the first step, I back out the characteristics of compliers. Appendix Section G provides an estimating equation, which includes a saturated model of screening take-up and treatment group indicator variable, and detailed steps for estimation. This approach allows me to statistically estimate and compare the characteristics of all three compliance groups. It also distinguishes between compliers in the treatment group, treated compliers, and compliers in the control group, untreated compliers. Finally, I test whether the ratios of treated compliers to always-takers and untreated compliers to never-takers are equal to one. By comparing always-takers to treated compliers, and never-takers to untreated compliers, I remove any causal effect of screening and isolate the selection effect. This method was also used by Kim and Lee (2017), Einav et al. (2020), and Kowalski (2023) to characterize compliers in the health screening context.

Table 5 reports the estimated characteristics of always-takers, never-takers, treated compliers, and untreated compliers. It also presents the ratio of treated compliers to always-takers and of untreated compliers to never-takers for comparison where the null hypothesis is that the ratio is equal to one. Figure 3a plots the ratio of treated compliers to always-takers for each outcome, along with 95 percent confidence intervals. Regarding screening diagnoses, compliers are 68 percent more likely to be diagnosed with a stomach disease than always-takers, suggesting that compliers face higher underlying health risks.<sup>34</sup> In contrast, differences in diagnosis rates for breast, cervical, and colorectal

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<sup>34</sup>This pattern is not driven by differences in the quality or quantity of testing. Both subsidized and unsubsidized screenings are available at the same hospitals and are typically administered by the same medical staff, suggesting minimal variation in test quality. Moreover, as shown in Table A14 in Appendix Section G, always-takers actually receive more medical tests than compliers, including

screenings are not statistically significant.<sup>35</sup>

The socioeconomic characteristics of compliers further underscore their relative disadvantage. Compliers have 40 percent lower individual income and 12 percent lower household income compared to always-takers. Furthermore, compliers are 13 percent less likely to be employed and 40 percent less likely to have a college degree. These patterns indicate that the subsidy program induces participation of individuals from lower socioeconomic backgrounds who have previously been financially constrained from accessing screening. When combined with the elevated diagnosis rates, these findings suggest that compliers are not only more economically disadvantaged but also at higher health risk. This pattern is consistent with findings in [Bitler and Carpenter \(2016\)](#), which show that prohibiting mammogram deductibles led to increased screening take-up, particularly among low-income women and high school dropouts.

In terms of health behaviors, compliers are neither more nor less likely to smoke, drink, or exercise compared to always-takers. To account for substantial gender differences in these behaviors, I report results separately for male and female samples.<sup>36</sup> Table 5 and Figures 3a and 3b present the characteristics of male compliers, while Appendix Section G shows similar results for female compliers. Although the levels differ across genders, the comparison patterns between compliers and always-takers are consistent. Both among men and women, I find no statistically significant differences in health behaviors between compliers and always-takers.<sup>37</sup>

Figure 3b provides the relative characteristics of untreated compliers compared to never-takers. While the selection based on socioeconomic status is not pronounced, compliers demonstrate clear positive selection in health behaviors.<sup>38</sup> Specifically, compliers

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unsubsidized procedures such as sonograms and CT scans. Therefore, the higher disease detection rate among compliers is not attributable to more extensive or higher-quality testing.

<sup>35</sup>There were too few disease diagnoses from liver, lung, and prostate screenings, which renders comparison infeasible.

<sup>36</sup>For example, smoking is prevalent primarily among men: 39 percent of men are smokers compared to just 3 percent of women. Similarly, 13 percent of men report daily alcohol consumption, while only 1 percent of women do.

<sup>37</sup>Any causal effect of screening on health behaviors would cancel out in this comparison, as both treated compliers and always-takers receive screenings.

<sup>38</sup>Note that it is not possible to infer the health outcomes of never-takers through screening results, since they do not get screened by definition.

are less likely to smoke and more likely to exercise. Compared to always-takers, male compliers shown in Table 5 are 29 percent less likely to smoke and 18 percent more likely to do vigorous exercise, while female compliers shown in Table A13 are 65 percent less likely to smoke and 29 percent more likely to do vigorous exercise. This pattern of positive selection relative to never-takers is also observed from prior studies on screenings ([Einav et al., 2020](#); [Kowalski, 2023](#); [Jones et al., 2019](#)). These findings align with broader evidence on the correlation of health behaviors, in which individuals who engage in one form of preventive behavior are more likely to engage in others ([Oster, 2020](#); [Cutler and Lleras-Muney, 2010](#)).

A related study by [Einav et al. \(2020\)](#) examines breast cancer screening around age 40 and finds that compliers with clinical guidelines are less likely to have true positive breast cancer than always-takers, but observes no systematic socioeconomic differences between the two groups. My findings complement this evidence by highlighting a different pattern when the treatment is financial incentives, rather than medical recommendations. In my setting, compliers exhibit a slightly higher risk of stomach disease, and they come from lower socioeconomic backgrounds than always-takers. Taken together, these studies suggest that patterns of selection may vary across both health risk and socioeconomic dimensions, depending on the nature of the intervention and the population under study.<sup>39</sup>

My findings are more consistent with those of [Kim and Lee \(2017\)](#), who also study Korea's National Health Screening Program. While my study leverages biennial subsidies at even ages covering 90 percent of cancer screening copays, [Kim and Lee \(2017\)](#) examine the remaining copay waiver at even ages for those below a health insurance premium cutoff. They find that compliers have higher cancer and all-cause mortality than always-takers, consistent with negative selection into screening based on health risk. Together, these studies suggest that lowering the price of screening can effectively attract higher-risk participants from lower socioeconomic backgrounds who are more likely to benefit from early diagnoses.

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<sup>39</sup>[Lawler \(2020\)](#) examines national meningococcal vaccination recommendation for high school-aged adolescents and find that the adolescents from households with high education and high income were more responsive to the recommendation. This finding, along with [Einav et al. \(2020\)](#), suggests that recommendation alone is less likely to reach the high-risk group.

Appendix Section J and G report robustness checks. First, I use the age 40 eligibility cutoff to compare individuals who start screening prior to age 40 (always-takers) with those who start screening from age 40 at even ages (compliers). Consistent with the main findings, compliers have lower socioeconomic status and a higher incidence of stomach diseases than always-takers. Second, I examine selection pattern among the unemployed. Another source of free health screening, other than National Health Screening Program, is workplace wellness programs provided by large firms.<sup>40</sup> Employees in such firms often get annual health screenings provided by firms, which may make them always-takers. Although this alternative source of free screening does not change the selection results, it may affect the interpretation that individuals respond to lower prices. Table A15 shows that even among the non-working population, significant differences in diagnoses and household income remain, suggesting that workplace wellness programs do not fully account for the observed selection.<sup>41</sup>

### 5.2.2 Longitudinal analysis

Health screenings are not one-time event, but require repeated take-ups over the life course. The cross-sectional analysis captures behavior at a point in time, but it does not address how consistently individuals participate in screening over time. To capture this dimension, I use the recurring structure of the biennial subsidies to classify individuals by compliance behavior over a ten-year period (2009-2018).

The unique alternating subsidy schedule creates a repeated experiment every year. Over 10 years, each person faces five subsidized (even) and five unsubsidized (odd) ages. I interpret this setting as the one where treatment and control states are observed within the same individual over time and use experiments at subsidized ages as empirical counterfactuals for the experiments at unsubsidized ages, and vice versa.<sup>42</sup> Using 10 years

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<sup>40</sup>While private supplemental health insurance is prevalent in South Korea, it generally does not provide additional preventive screenings. This is because health screenings recommended by clinical guidelines are already fully provided by the National Health Screening Program, and the additional screenings are not deemed medically necessary.

<sup>41</sup>Jones et al. (2019) find that participants in workplace wellness programs are positively selected and have better health than nonparticipants. Therefore, workplace wellness program, if anything, underscores the importance of the public health screening programs that can attract high-risk participants.

<sup>42</sup>The concept of compliers in Angrist et al. (1996) and Imbens and Angrist (1994) is grounded in the

screening history, I classify individuals into four groups. Always-takers who screen at both subsidized and unsubsidized ages, compliers who screen at subsidized ages, but not at unsubsidized ages, defiers who screen at unsubsidized ages, but not at subsidized ages, and never-takers who screen at neither. This classification assumes persistence in compliance behavior over time. In reality, screening behavior may respond to transitory shocks, such as illness of oneself or of a family member (Fadlon and Nielsen, 2019; Hodor, 2021; Jeon and Pohl, 2017; Hoagland, 2025; García-Gómez et al., 2013), introducing noise and creating “almost-compliers” or “almost-always-takers”. To the extent that compliance behavior persists over time, this classification offers useful insight into selection into screening.

I restrict the analytical sample from the survey data to 5,701 individuals who are aged 40 or above and are observed in all 10 years.<sup>43</sup> I define the empirical screening probabilities at even ages as follow.

$$Pr(screen\_even)_i = \frac{1}{5} \sum_k \mathbb{1}\{screen_{ik} = 1\}, \quad k \text{ even} \quad (2)$$

The term  $screen_{ik}$  equals one if individual  $i$  of age  $k$  participated in a screening. The probability of screening at odd ages can be calculated in a similar way. Probabilities range from 0 to 1 in 0.2 increments. Figure 4 shows the joint distribution of even and odd screening probabilities. For exhaustive definition, I arbitrarily delineate the four groups and define those with ( $Pr(screen\_even) > 0.5$ ,  $Pr(screen\_odd) > 0.5$ ) as always-takers, those with ( $Pr(screen\_even) < 0.5$ ,  $Pr(screen\_odd) > 0.5$ ) as defiers, those with ( $Pr(screen\_even) < 0.5$ ,  $Pr(screen\_odd) < 0.5$ ) as never-takers, and those with ( $Pr(screen\_even) > 0.5$ ,  $Pr(screen\_odd) < 0.5$ ) as compliers. Based on these definitions,

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potential outcomes framework, where individuals are either in a treatment or control group. Compliers are defined by their unobserved behavior in a counterfactual state. For example, someone who takes up treatment when assigned to the treatment group but would not have done so if assigned to control is a complier. In this study, the alternating subsidy schedule creates a unique repeated experiment in which the same individual is observed across both treatment (even ages) and control (odd ages) conditions over time. This panel structure allows the use of observed screening behavior at odd ages as a proxy for the counterfactual to behavior at even ages, and vice versa.

<sup>43</sup>Although the dataset spans 11 years, I exclude the first year, 2008, because the data collection start date varies across individuals in that year.

around 29.4 percent falls in the category of compliers, 65.9 percent are never-takers, 2.5 percent are always-takers, and 2.2 percent are defiers.

Table 6 compares characteristics across compliance groups. The patterns are highly consistent with the ones from cross-sectional analysis. Column 5 provides comparison between compliers and always-takers. Panel A shows compliers are 41 percent more likely to find a stomach disease than always-takers. This is consistent with negative selection in income and education as shown in Panel B. I do not find significant differences in health behaviors, except that compliers are slightly less likely to exercise than always-takers.<sup>44</sup>

Comparison with never-takers also shows consistent pattern. Panel C shows compliers display better health behaviors than never-takers. They are less likely to smoke and more likely to exercise. Consistent with the cross-sectional analysis, the differences in socioeconomic status between compliers and never-takers, presented in Panel B, are ambiguous and do not indicate clear evidence of either positive or negative selection.

The panel approach identifies defiers, which the cross-sectional analysis cannot detect due to the monotonicity assumption. The estimated share of defiers is small, supporting the plausibility of the monotonicity assumption. The comparison between compliers and defiers provides insight into compliance behavior through the intertemporal substitution channel. On average, both groups undergo the same number of screenings, but the timing differs. Compliers concentrate participation at subsidized ages, while defiers do so at unsubsidized ages. Movement from defiers to compliers therefore largely reflects a shift in timing rather than an increase in overall screenings. Column 6 of Table 6 shows that the characteristics of compliers relative to defiers mirror those observed relative to always-takers. In both cases, compliers are more likely to be diagnosed with stomach disease and to come from lower socioeconomic backgrounds. This consistency suggests that the selection patterns among compliers are similar whether participation reflects net increases in screening or intertemporal substitution across years.

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<sup>44</sup>Following the cross-sectional analysis, I only use male sample for health behaviors to account for large differences in smoking and drinking behaviors between male and female.

### 5.3 Effect of health screenings

This section examines the causal effects of health screenings on three categories of outcomes. First, I analyze cancer diagnosis, focusing on whether screening increases cancer detection at an early stage. Second, I evaluate changes in preventive care use, specifically, treatment of precancerous conditions, management of cardiovascular risk factors, and diagnoses of osteoporosis and tuberculosis, which are two conditions examined in the general screening. Finally, I examine impact on mortality.

The analysis uses the Standard Cohort Database and the Customized Cohort Database from the National Health Insurance Service (NHIS), which include comprehensive records on cancer diagnoses, health care utilization, and mortality. Importantly, cancer diagnoses in these data are confirmed true positive cases, not including false positive results. As discussed in section 4, the insurance claims data have incomplete records on screening participation. As a result, I estimate intent-to-treat (ITT) effects using the reduced form specification presented in Equation (1). Local average treatment effects (LATE) can be derived manually by dividing ITT estimates by the increases in take-up rates reported in Table 3, which are estimated using survey data. The analytical sample is restricted to individuals aged 40 to 89.

Given that my identifying variation is short-term, that is, eligibility this year versus next year, all causal estimates should be interpreted as the effect of subsidy eligibility in the current year.<sup>45</sup> The mechanisms through which biennial subsidies affect screening participation is twofold: (i) a net increase in participation and (ii) shifting screening timing one year forward, as shown in Section 5.1.2. Therefore, the estimated effects on cancer diagnosis and preventive care use reflect both additional cases detected (or services used) and earlier timing by one year.

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<sup>45</sup>This is why I focus on short-term outcomes observed immediately after screening. Estimating long-term outcomes is challenging given the short-run nature of the variation.

### 5.3.1 Effect on cancer diagnoses

Cancer can be detected either through routine screening at an early, asymptomatic stage or after the onset of symptoms. When cancer is diagnosed due to noticeable discomfort, it is more likely to be at an advanced stage, making treatment more difficult, invasive, and costly. The primary goal of screening is early detection, which enables more effective, less invasive, and less costly treatment. Evidence from numerous large clinical trials and guideline reviews demonstrates that early detection through screening can reduce cancer-specific mortality across multiple cancer types (Tabár et al., 2011; Atkin et al., 2010; Bretthauer et al., 2022; Chen et al., 2003; Team, 2011; Andriole et al., 2009; Siu and Force, 2016; Curry et al., 2018a; Davidson et al., 2021; Krist et al., 2021; Grossman et al., 2018).

I find that subsidy eligibility leads to a significant increase in cancer diagnoses.<sup>46</sup> Figure 5b, 5c, and 5d plot detection rates for stomach, breast, and cervical cancer at each age, regardless of whether detected through preventive screening or not. Detection rates are consistently higher in the treatment group beginning at age 40, consistent with subsidy eligibility. Table 7 first rows for each cancer report ITT effects. Subsidy eligibility increases diagnoses by 0.077 percentage points (41%) for stomach cancer, 0.05 percentage points (11%) for breast cancer, and 0.02 percentage points (13%) for cervical cancer.

Subsidy eligibility also increases diagnoses for cancers not directly targeted by biennial subsidies. Figure 5e, 5f, 5g, and 5h show detection rates for liver and colorectal cancers, which were subsidized annually, and for lung and prostate cancers, which were not subsidized. In all four cases, detection rates are higher in the treatment group. Estimated effects indicate that subsidy eligibility increases diagnosis rates by 0.013 percentage points (16%) for liver cancer, 0.018 percentage points (10%) for colorectal cancer, 0.014 percentage points (15%) for lung cancer, and 0.01 percentage points (6%) for prostate cancer. Overall, subsidy eligibility raises the aggregate diagnosis rate by 0.16 percentage points (18%).<sup>47</sup> These patterns suggest that spillover in screening take-up, examined in

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<sup>46</sup>In this study, cancer refers to both invasive and in-situ cases unless otherwise specified. False positive results and benign tumors are excluded.

<sup>47</sup>In this study, overall or aggregate diagnosis refers to any of the seven cancers studied: stomach,

Section 5.1.3, leads to spillover in cancer diagnosis as well.

While the use of separate datasets to estimate participation and cancer diagnoses complicates the calculation of local average treatment effects (LATE), they can be manually calculated by dividing the ITT estimates by the first-stage increases in screening take-up from survey data.<sup>48</sup> The resulting LATEs are 0.0041 percentage points ( $= 0.00077/0.19$ ) for stomach cancer, 0.0026 percentage points ( $= 0.0005/0.191$ ) for breast cancer, and 0.0055 percentage points ( $= 0.00018/0.033$ ) for colorectal cancer, corresponding to 213%, 56%, and 287% of the respective control group means. These estimates are large relative to other quasi-experimental evidence.<sup>49</sup> For example, [Guthmuller et al. \(2023\)](#) report a LATE of 0.0010 percentage points for breast cancer in Europe's organized screening programs, less than half of my estimate. Similarly, in [Kim and Lee \(2017\)](#), LATEs for stomach and breast cancers were both around 0.0025 percentage points, while my estimate for stomach cancer is substantially higher. Since LATEs capture detection rates among compliers, these results align with my selection analysis indicating that compliers in this setting are disproportionately high-risk individuals.

The effects of subsidy eligibility on cancer diagnoses vary substantially by age, sex, income, and disability status. Age gradients are especially pronounced: relative to those aged 40–49, individuals aged 60–69 are 5.3 times, and those aged 70–79 are 9.4 times, more likely to be diagnosed. This suggests that research designs exploiting discontinuities at recommended starting ages primarily capture effects among younger, lower-risk groups ([Kadiyala and Strumpf, 2016](#); [Einav et al., 2020](#)), thereby understating the broader effectiveness of screening for older adults, who account for most cancer cases.<sup>50</sup> Men are 9 percent more likely than women to be diagnosed, though the gap is modest. Using insurance premiums as a proxy for income and wealth, I find that individuals below the

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breast, cervical, liver, colorectal, lung, or prostate.

<sup>48</sup>These estimates should be interpreted with caution, as differences in data sources and time periods may introduce bias.

<sup>49</sup>As emphasized by [Angrist and Hull \(2023\)](#), LATEs provide a more meaningful basis for comparison across studies than ITT estimates.

<sup>50</sup>Although early detection in younger adults may yield more life-years saved per case, the population-level impact of screening is concentrated among older individuals given their higher incidence and baseline risk. Thus, estimates at starting ages may not generalize across the age distribution.

median are 38 percent more likely to be diagnosed than those above it. Similarly, individuals with disabilities are 70 percent more likely to be diagnosed than those without. Overall, older adults, lower-income groups, and people with disabilities benefit disproportionately from the program, consistent with selection analyses showing that it benefits individuals with lower socioeconomic status more. Detailed heterogeneity analyses are reported in Appendix Section H.

Unpacking aggregate cancer diagnoses into early stage in-situ and invasive cases reveals that screenings increase both in-situ and invasive cancer diagnoses.<sup>51</sup> The second and third rows for each cancer in Table 7 report the ITT effects on invasive and in-situ diagnoses. Once the ITT estimates are normalized by the control group mean, the percentage increase, reported in column 3, is generally larger for in-situ cancers. Overall, screenings increase invasive cancer diagnoses by 17.0 percent and in-situ cancer diagnosis by 19.5 percent.

The pattern of early diagnosis is further supported by higher survival rates among patients diagnosed in the treatment group. I restrict the sample to individuals diagnosed with one of the seven major cancers listed in Table 8 and compare their survival rates by whether the diagnosis occurred in the treatment (even age) or control (odd age) group. Figure 6 plots survival trajectories from 1 to 10 years post-diagnosis, and Table 8 reports the commonly used 5-year survival rates. Among 52,962 cancer cases, the 5-year survival rate is 80.1 percent in the control group versus 81.3 percent in the treatment group, a 1.2 percentage point (1.47%) increase. This difference suggests earlier cancer detection in the treatment group, consistent with screening-induced lead time. However, this improvement in survival should not be confused with reduced mortality, since early diagnosis can improve survival rates without changing actual lifespan.<sup>52</sup> This survival advantage appears across most cancers, except for lung and prostate cancers. These exceptions are consistent with prior RCT evidence showing that screening for these two cancer types

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<sup>51</sup>In-situ cancers, also referred to as precancer or stage 0 cancer, are characterized by abnormal cells that have not yet invaded neighboring tissues. It can progress to invasive stages if left untreated. Although many cancers have an in-situ stage, it is particularly common in breast, cervical, and colorectal cancers.

<sup>52</sup>This is called “lead time bias” in health screening. Refer to Morrison (1982); Welch et al. (2000); Duffy et al. (2008); Gordis (2013) and Yang et al. (2021) for detail.

does not reliably detect earlier stage disease, explaining why these screenings are not subsidized by NHIS (Krist et al., 2021; Grossman et al., 2018).<sup>53</sup> Table 8 also shows a larger share of in-situ cancers among treatment group diagnoses, consistent with larger percentage increase in in-situ cancer diagnoses compared to invasive cancers observed in Table 7. Taken together, these results indicate that screenings effectively shift diagnoses toward earlier stages.

### 5.3.2 Effect on preventive care use

Beyond increasing cancer diagnoses, health screenings also lead to treatment of cancer precursors, thereby reducing long-term cancer risk. One key example is Helicobacter pylori (*H. pylori*) infection, a well established risk factor for stomach cancer (Polk and Peek Jr, 2010; Uemura et al., 2001; Butt and Epplein, 2019). During stomach screening, such as upper endoscopy, a biopsy sample can be collected to test for *H. pylori* infection. If an infection is detected, treatment typically involves a combination of antibiotics and a proton pump inhibitor, administered orally. I define *H. pylori* treatment as the concurrent prescription of two antibiotic classes and a proton pump inhibitor during a single hospital visit. Table 9 Panel A shows that subsidy eligibility increases treatment by 0.003 percentage points (15%), suggesting that stomach screening leads to diagnosis and treatment of the strong stomach cancer precursor.

Colorectal polyps, abnormal tissue growths in the colon or rectum, are another important cancer precursor. Since most cases of colorectal cancer originate from polyps, early removal via colonoscopy significantly reduces cancer risk and mortality (Song et al., 2020; Zauber et al., 2012; Shaukat et al., 2021). Table 9 Panel B shows that subsidy eligibility increases polypectomy rates by 0.72 percentage points (62%), suggesting that colonoscopy is not only diagnostic but also preventive through proactive polyp removal.

Health screenings also increase the diagnosis and treatment of cardiovascular risk factors, such as high blood pressure, diabetes, and high cholesterol, which are examined

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<sup>53</sup>NHIS began subsidizing lung cancer screening for high-risk individuals only after the study period. The subsidized test is a low-dose CT scan, whereas during the study period the dominant method for lung screening was chest X-ray.

during the general screening. Table 9 Panel B shows that subsidy eligibility increases medication use by 0.05 percentage points (0.19%) for high blood pressure, 0.02 percentage points (0.17%) for diabetes, and 0.42 percentage points (3.01%) for high cholesterol.<sup>54</sup> The magnitude is larger for high cholesterol drugs compared to the other two conditions, reflecting the fact that cholesterol levels are less likely to be measured outside of formal screenings, whereas blood pressure and blood sugar level can be more easily measured during routine visits to general practitioners. These results suggest that screenings increase the diagnosis of chronic conditions and prompt greater take-up of medication for disease management.

The general screening program also includes tuberculosis (TB) test, especially important in South Korea, which has among the highest TB incidence rates among the OECD countries (Cho, 2018). Chest X-rays are used for initial screening, followed by sputum tests and nucleic acid amplification if abnormalities are detected. TB treatment typically includes medications, including isoniazid, rifampin, pyrazinamide, and ethambutol. Table 9 Panel C shows that subsidy eligibility had no significant change in TB-related prescriptions.

Osteoporosis is another target of the general screening program, particularly among women after menopause. Screening includes bone density tests, which is only subsidized as a part of general screening at ages 54 and 66. First-line treatment comprise calcium/vitamin D supplementation and bisphosphonates medication to reduce fracture risk (Curry et al., 2018b). Table 9 Panel C shows that subsidy eligibility led to a 0.1 percentage points (4%) increase in osteoporosis medication prescription. Interestingly, although bone density screening is subsidized only for women, medication uptake increases for both men and women, reflecting spousal spillover in screening participation (Detailed analysis in Appendix Section I).

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<sup>54</sup>Appendix Section I provides a list of medications for high blood pressure, diabetes, and high cholesterol and reports the impact on each drug.

### 5.3.3 Effect on mortality

A central objective of health screening is to reduce mortality through early detection and treatment. Increases in survival rates following diagnosis do not necessarily indicate real improvements in health outcomes. This is because earlier diagnosis can mechanically lengthen the time from diagnosis to death without extending actual lifespan, a phenomenon known as lead-time bias. Survival rate analysis, which measures the proportion of patients alive a certain number of years after diagnosis, is susceptible to this bias. The most extreme form of lead-time bias is overdiagnosis, where screening detects cancers that would not have caused symptoms or death during an individual's lifetime. These cases inflate survival statistics, even though they may not reduce actual mortality. For this reason, survival rate analysis can be misleading.

To properly evaluate the health impact of screening, it is essential to conduct a mortality analysis, which compares death rates between the treatment and control groups, regardless of whether individuals were diagnosed with cancer. Unlike survival analysis, mortality analysis is not affected by lead-time bias or overdiagnosis and provides a more accurate estimate of the population-level benefits of screening. This section examines the short-run ITT effects of screening on one-year mortality. Due to the alternating structure of the subsidy program, this analysis is restricted to immediate effects within a one-year window and cannot capture potential longer-term benefits of screening.

Table 10 presents the results of the one-year mortality analysis. The baseline mortality rate in the control group is 0.91 percent. Treatment group exhibits 0.005 percentage points (0.53%) lower mortality, but the difference is not statistically significant. When decomposing deaths by cause, the estimates show lower mortality in the treatment group from overall cancer as well as from individual cancers such as stomach or breast cancer, but these differences are likewise not statistically significant. Given the low baseline mortality and the short follow-up period, these results are not unexpected. Most cancers develop and progress over a longer time horizon, and reductions in mortality from early detection typically take several years to materialize. As such, the lack of a short-term mortality effect does not imply that screening is ineffective. Rather, it reflects the diffi-

culty of detecting mortality improvements in the short term. Longer follow-up would be required to assess the full impact of screening on mortality outcomes.

## 6 Conclusion

This paper studies selection into health screenings and their causal effects using quasi-random variation from South Korea’s National Health Screening Program, which subsidizes 90–100% of screening costs every other year at even-numbered ages. Using survey and insurance claims data, this study has shown that compliers with the subsidies have lower socioeconomic status and poorer health conditions than those who always participate regardless of subsidies. Moreover, subsidy eligibility increases both in-situ and invasive cancer diagnoses and treatment for cancer precursors and risk factors for cardiovascular diseases. These results suggest that public health programs offering subsidized screenings can target high-risk individuals and promote cancer diagnoses and preventive care use.

A policy implication is that providing subsidized screenings not only increase participation, but also improve targeting of health screenings, particularly among low SES populations. This study suggests that blanket subsidies, as opposed to subsidies conditional on low income, are effective in improving access among people from low socioeconomic backgrounds. This study also showed that bundling screenings such that they can be received in a single hospital visit can help increase participation in screenings. It can also make subsidies more salient, as people perceive the total amount of subsidies available for various screenings.

An open question is the long-term effects of screenings. The current even/odd age-based design does not allow examining long-term effects due to alternating subsidies. However, to fully capture the benefits of screenings, it is important to examine long-term effects, as benefits of screenings may take time to materialize. Important long-run outcomes to examine are decrease in late-stage cancers, mortality, and health care expenditures. The short-term effects from this study have shown that screenings dispropor-

ately increase early-stage cancer diagnoses. If these early diagnoses represent prevention from progress to late-stage cancers, there should be decline in late-stage cancers in the long run. Failure to find decline in late-stage diagnoses would imply overdiagnosis, that is, detecting early-stage cancers that would not have caused harm during one's lifetime. Similarly, our ultimate goal would be whether screening actually leads to decline in mortality. Finally, screenings increase health care spending in the short-run, due to follow-up testing and treatment. We need to find out if this leads to long run drop in spending.

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## 7 Tables

**Table 1:** Screening subsidy schedule

	Biennial subsidy				Annual subsidy		Unsubsidized	
	General	Stomach	Breast	Cervical	Liver	Colorectal	Lung	Prostate
Frequency	2 years	2 years	2 years	2 years	0.5 year	1 year		
Subsidy starting age	40	40	40	30	40	50		
Subsidy amount	100%	90%	90%	100%	90%	90%	0%	0%
Full costs (\$)	50	65	40	15	90	10	100	20
Target		Female	Female	High risk group			Male	
Subsidized medical tests	Gastroscopy, biopsy	Mammogram	Pap smear	Ultrasound	Fecal occult blood test, colonoscopy, biopsy			

Notes: The table summarizes the National Health Screening Program (NHSP) subsidy schedule. Biennial screenings are subsidized in a calendar year with even-numbered ages (current year minus birth year), annual screenings every year, and unsubsidized screenings are not covered. Liver screening is offered up to twice per year for high-risk individuals. The minimum eligible age for cervical screening was lowered from 30 to 20 in 2016. Colorectal screening was biennial at even-numbered ages from age 50 until 2011, then annual thereafter. Colonoscopy is subsidized only after a positive fecal occult blood test (FOBT). Full costs reflect 2018 NHSP-set prices in USD.

**Table 2:** Balance table

	(1)	(2)	(3)
	Treatment (even age)	Control (odd age)	Conditional differences
Age	58.697 (12.532)	59.240 (12.353)	- -
Female	0.530 (0.499)	0.532 (0.499)	-0.002* (0.001)
Currently married	0.799 (0.401)	0.798 (0.402)	-0.0011 (0.0008)
Years of education	10.320 (4.510)	10.227 (4.538)	-0.003 (0.008)
Working status	0.610 (0.488)	0.608 (0.488)	-0.003* (0.001)
Individual income	1446.3 (2081.6)	1425.7 (2068.1)	2.762 (5.185)
Household income	4104.4 (3708.6)	4086.7 (3737.9)	3.221 (14.267)
Own a house	0.734 (0.442)	0.737 (0.441)	-0.0002 (0.0011)
Number of household members	3.067 (1.317)	3.051 (1.317)	-0.004 (0.003)
N	54274	52909	
Share	(0.51)	(0.49)	
F(8, 15939)			1.65
Prob > F			(0.10)

Notes: The table reports conditional balance between the treatment group (even-aged) and the control group (odd-aged) in the survey sample of individuals aged 40–89. Column 3 shows treatment–control differences conditional on 5-year linear age splines. Individual and household incomes are in 10,000 Korean Won. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table 3:** Effect of subsidies on take-up

	(1)	(2)	(3)	(4)
	General	Stomach	Breast	Cervical
treat	0.187*** (0.003)	0.190*** (0.003)	0.191*** (0.004)	0.164*** (0.003)
N	107183	107183	56923	56923
Adj $R^2$	0.061	0.069	0.080	0.074
F-statistic	4804	4830	2904	2520
Sample age range	[40, 89]	[40, 89]	[40, 89]	[30, 89]
Subsidy starting age	40	40	40	30
Age controls	Y	Y	Y	Y
Control group mean	0.102	0.083	0.067	0.056
Percentage increase	183	229	283	295

Notes: The table reports the effect of biennial subsidies on screening take-up, comparing treatment (even-aged) and control (odd-aged) groups in the survey data. The variable *treat* equals one if one has an even age and eligible for subsidized screenings. The sample includes ages 40–89 for general, stomach, and breast screenings, and 30–89 for cervical screening. Econometric specification is given in Equation (1) and control for 5-year linear age splines. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table 4:** Cross spillover across different screening types

	(1)	(2)	(3)	(4)
	Annually subsidized		Unsubsidized	
	Liver	Colorectal	Prostate	Lung
treat	0.027*** (0.001)	0.033*** (0.001)	0.007*** (0.001)	0.0062*** (0.0007)
N	107183	107183	50260	107183
Adj $R^2$	0.008	0.011	0.002	0.002
Sample age range	[40, 89]	[40, 89]	[40, 89]	[40, 89]
Subsidy starting age	40	50	Y	Y
Age controls	Y	Y	Y	Y
Control group mean	0.028	0.027	0.009	0.009
Percentage increase	94	124	81	67

Notes: The table reports cross-spillover effects for annually subsidized screenings (liver, colorectal) and unsubsidized screenings (prostate, lung) using the survey data. The variable *treat* equals one if one has an even age and eligible for subsidized screenings. The sample includes ages 40–89. Econometric specification is given in Equation (1). Estimates are conditional on 5-year linear age splines. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table 5:** Compliers with subsidies

	(1)	(2)	(3)	(4)	(5)	(6)
	Average values			Ratios		
	Always-takers	Treated compliers	Untreated compliers	Never-takers	$CP_1/AT$	$CP_0/NT$
<b>Panel A. Diagnoses</b>						
Stomach	0.174 (0.006)	0.293 (0.008)	-	-	1.684*** (0.084)	-
Breast	0.018 (0.003)	0.022 (0.003)	-	-	1.228 (0.354)	-
Cervical	0.067 (0.007)	0.061 (0.006)	-	-	0.906 (0.154)	-
Colorectal	0.212 (0.011)	0.252 (0.023)	-	-	1.190 (0.161)	-
<b>Panel B. SES</b>						
Individual income	1741 (48)	1037 (47)	1098 (45)	1341 (36)	0.596*** (0.029)	0.818*** (0.027)
Household income	4985 (84)	4379 (87)	4425 (108)	4209 (66)	0.878*** (0.018)	1.051** (0.021)
Years of education	10.393 (0.092)	10.081 (0.095)	10.080 (0.095)	9.795 (0.079)	0.970*** (0.009)	1.029*** (0.007)
College graduate	0.151 (0.009)	0.090 (0.008)	0.099 (0.008)	0.097 (0.007)	0.596*** (0.057)	1.021 (0.069)
Working status	0.713 (0.010)	0.619 (0.012)	0.640 (0.013)	0.670 (0.009)	0.868*** (0.016)	0.954*** (0.014)
<b>Panel C. Health behaviors</b>						
Current smoker	0.304 (0.017)	0.283 (0.020)	0.275 (0.021)	0.390 (0.015)	0.932 (0.067)	0.706*** (0.042)
Everyday smoker	0.287 (0.017)	0.273 (0.019)	0.254 (0.021)	0.376 (0.015)	0.950 (0.070)	0.677*** (0.045)
Current drinker	0.824 (0.014)	0.815 (0.016)	0.802 (0.018)	0.781 (0.012)	0.989 (0.020)	1.028 (0.020)
Everyday drinker	0.158 (0.012)	0.148 (0.014)	0.179 (0.016)	0.164 (0.010)	0.936 (0.095)	1.089 (0.091)
Vigorous exercise	0.368 (0.014)	0.335 (0.016)	0.354 (0.021)	0.299 (0.011)	0.910* (0.049)	1.183** (0.073)
Moderate exercise	0.540 (0.014)	0.505 (0.017)	0.524 (0.024)	0.444 (0.011)	0.935* (0.039)	1.180*** (0.056)
Walking	0.829 (0.011)	0.823 (0.014)	0.808 (0.019)	0.786 (0.009)	0.992 (0.021)	1.029 (0.025)

Notes: The table reports average values of screening diagnoses, socioeconomic status, and health behaviors among always-takers (AT), never-takers (NT), treated compliers ( $CP_1$ ), and untreated compliers ( $CP_0$ ) using the survey data. Treated compliers are compliers in the treatment group who participate in screening; untreated compliers are compliers in the control group who do not. Averages are calculated using Equation (5) in Appendix Section G. Diagnoses are not reported for untreated compliers and never-takers, as they do not receive screening. Health behaviors are calculated using the male sample to account for vast gender differences. Appendix Section G reports female values. Null hypotheses for ratios are  $H_0 : CP_1/AT = 1$  and  $H_0 : CP_0/NT = 1$ . All averages and ratios are measured at age 60. Standard errors are calculated using bootstrap with 500 replications, clustered at the individual level, and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table 6:** Compliers with subsidies using panel information

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Average values			Ratios			
	Always-takers	Compliers	Defiers	Never-takers	CP/AT	CP/DF	CP/NT
<b>Panel A. Diagnoses</b>							
Stomach	0.181 (0.018)	0.256 (0.007)	0.189 (0.020)	-	1.413*** (0.143)	1.355** (0.146)	-
Breast	0.015 (0.007)	0.020 (0.003)	0.022 (0.011)	-	1.303 (0.653)	0.885 (0.452)	-
Cervical	0.061 (0.015)	0.058 (0.005)	0.058 (0.024)	-	0.950 (0.249)	0.997 (0.417)	-
Colorectal	0.183 (0.030)	0.260 (0.013)	0.212 (0.032)	-	1.423* (0.248)	1.226 (0.197)	-
<b>Panel B. SES</b>							
Individual income	2688 (247)	1043 (37)	2405 (222)	1288 (28)	0.388*** (0.038)	0.434*** (0.043)	0.810*** (0.034)
Household income	6093 (308)	3999 (67)	5587 (284)	3764 (46)	0.656*** (0.035)	0.716*** (0.038)	1.063*** (0.022)
Years of education	12.184 (0.312)	9.876 (0.098)	11.486 (0.349)	9.585 (0.075)	0.811*** (0.022)	0.860*** (0.027)	1.030** (0.013)
College graduate	0.270 (0.039)	0.123 (0.008)	0.291 (0.038)	0.142 (0.006)	0.456*** (0.072)	0.423*** (0.062)	0.869** (0.066)
Working status	0.753 (0.030)	0.560 (0.010)	0.733 (0.032)	0.603 (0.007)	0.744*** (0.033)	0.764*** (0.036)	0.930*** (0.020)
<b>Panel C. Health behaviors</b>							
Current smoker	0.272 (0.043)	0.268 (0.016)	0.381 (0.047)	0.399 (0.010)	0.984 (0.165)	0.702*** (0.096)	0.670*** (0.043)
Everyday smoker	0.260 (0.042)	0.257 (0.015)	0.350 (0.046)	0.386 (0.010)	0.991 (0.170)	0.735** (0.106)	0.667*** (0.044)
Current drinker	0.828 (0.042)	0.775 (0.014)	0.856 (0.031)	0.763 (0.008)	0.935 (0.050)	0.905*** (0.036)	1.016 (0.021)
Everyday drinker	0.111 (0.027)	0.141 (0.010)	0.175 (0.033)	0.151 (0.006)	1.274 (0.329)	0.807 (0.161)	0.933 (0.078)
Vigorous exercise	0.374 (0.032)	0.300 (0.010)	0.340 (0.028)	0.278 (0.006)	0.802*** (0.074)	0.883 (0.079)	1.080* (0.043)
Moderate exercise	0.526 (0.031)	0.474 (0.010)	0.525 (0.031)	0.419 (0.006)	0.902* (0.056)	0.903* (0.056)	1.133*** (0.029)
Walking	0.830 (0.023)	0.812 (0.007)	0.818 (0.019)	0.780 (0.005)	0.979 (0.029)	0.993 (0.025)	1.042*** (0.011)
<b>Panel D. Married subsample</b>							
Pr(even/odd or odd/even)	0.510	0.483	0.590	0.500			
Share	0.022	0.294	0.025	0.659			

Notes: The table reports average values of screening diagnoses, socioeconomic status, and health behaviors among always-takers (AT), never-takers (NT), compliers (CP), and defiers (DF) defined using 10-year health screening history in the survey data. The sample includes 5,701 individuals aged 40 or above in 2009 who participated in the survey all years from 2009–2018. Compliance groups are defined using the rule shown in Equation (2): compliers participate more than half of the time at subsidized ages and less than half at unsubsidized ages; always-takers participate more than half at subsidized and unsubsidized ages; never-takers participate less than half at both subsidized and unsubsidized ages; defiers participate more than half at unsubsidized ages and less than half at subsidized ages. Null hypotheses for ratios are  $H_0 : CP/AT = 1$ ,  $H_0 : CP/NT = 1$ , and  $H_0 : CP/DF = 1$ . Health behaviors are calculated using the male sample to account for vast gender differences. Appendix Section G reports female values. Pr(even/odd or odd/even) refers to the probability that one's age and the spouse's age are even-odd or odd-even; this is computed for the 4,742 married individuals. Share reports the proportion of each compliance group in the full sample. Standard errors are clustered at the individual level. They are reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table 7:** Effect on cancer diagnosis

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
Any cancer	0.0091	0.0016*** (0.0001)	17.545	7,449,256
invasive	0.0084	0.0014*** (0.0001)	17.024	7,449,256
in-situ	0.001003	0.00020*** (0.00003)	19.490	7,449,256
Stomach cancer	0.0019	0.00077*** (0.00006)	41.263	7,449,256
invasive	0.0018	0.00075*** (0.00005)	40.752	7,449,256
in-situ	0.000048	0.000027*** (0.000007)	55.232	7,449,256
Breast cancer	0.0047	0.0005*** (0.0001)	10.918	3,503,656
invasive	0.0043	0.0003*** (0.0001)	7.705	3,503,656
in-situ	0.000700	0.00016*** (0.00004)	23.548	3,503,656
Cervical cancer	0.0015	0.00020*** (0.00007)	13.073	3,503,656
invasive	0.0007	0.00008 (0.00005)	10.655	3,503,656
in-situ	0.000816	0.00014*** (0.00004)	16.718	3,503,656
Liver cancer	0.0009	0.00013*** (0.00004)	15.576	7,449,256
invasive	0.0009	0.00013*** (0.00004)	15.283	7,449,256
in-situ	0.000005	0.000002 (0.000002)	43.236	7,449,256
Colorectal cancer	0.0019	0.00018*** (0.00005)	9.675	7,449,256
invasive	0.0017	0.00017*** (0.00005)	9.949	7,449,256
in-situ	0.000227	0.00003* (0.00001)	11.944	7,449,256
Lung cancer	0.0009	0.00014*** (0.00004)	14.569	7,449,256
invasive	0.0009	0.00014*** (0.00004)	14.613	7,449,256
in-situ	0.000007	-0.000001 (0.000002)	-20.220	7,449,256
Prostate cancer	0.0016	0.00010 (0.00007)	6.341	3,945,600
invasive	0.0015	0.00010 (0.00007)	6.419	3,945,600
in-situ	0.000011	-0.0000009 (0.0000044)	-8.605	3,945,600

Notes: The table reports the effect of biennial subsidy eligibility on cancer diagnoses, using the Customized Cohort Database of national health insurance claims. For each cancer, the first row reports the overall diagnosis (invasive and in-situ), and the second and third rows report invasive and in-situ diagnoses separately. “Any cancer” refers to diagnosis of any of the seven cancers listed. The sample includes individuals aged 40–89. Column 1 shows the control (odd-age) group mean. Column 2 reports ITT estimates of the subsidy effect, comparing treatment and control groups controlling for age. Column 3 shows the ITT effect as a percentage of the control group mean. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table 8:** Cancer survival rates and share of in-situ cancers

	(1)	(2)	(3)	(4)
	Control group cancer diagnoses	Treatment - Control	Percent relative to control group diagnoses	N
Any cancer				
5 year survival rate	0.801	0.012*** (0.004)	1.472	52,962
Share of in-situ cancers	0.106	0.004 (0.003)	4.116	72,744
Stomach cancer				
5 year survival rate	0.843	0.020** (0.008)	2.321	12,697
Share of in-situ cancers	0.024	0.003 (0.003)	13.070	16,500
Breast cancer				
5 year survival rate	0.901	0.017** (0.008)	1.843	12,183
Share of in-situ cancers	0.142	0.020** (0.008)	13.888	17,037
Cervical cancer				
5 year survival rate	0.927	0.019* (0.010)	2.033	4,475
Share of in-situ cancers	0.536	0.024 (0.020)	4.443	5,608
Liver cancer				
5 year survival rate	0.537	0.030 (0.019)	5.659	4,991
Share of in-situ cancers	0.005	0.002 (0.003)	38.979	6,816
Colorectal cancer				
5 year survival rate	0.826	0.009 (0.010)	1.146	10,563
Share of in-situ cancers	0.117	0.002 (0.007)	1.872	14,332
Lung cancer				
5 year survival rate	0.496	-0.026 (0.019)	-5.315	4,872
Share of in-situ cancers	0.005	-0.002 (0.002)	-36.405	7,318
Prostate cancer				
5 year survival rate	0.820	-0.007 (0.016)	-0.804	3,935
Share of in-situ cancers	0.006	-0.001 (0.003)	-20.173	6,207

Notes: The table compares characteristics of cancers diagnosed at subsidized ages (even) with those diagnosed at unsubsidized ages (odd), using the Customized Cohort Database of national health insurance claims. The sample includes new cancer diagnoses for individuals aged 40–89. For survival rates, the last five years of the panel are excluded from the 5-year survival calculation. Column 1 shows the mean for cancers diagnosed at unsubsidized ages (odd). Column 2 reports differences in characteristics for cancers diagnosed at subsidized ages (even) relative to unsubsidized ages (odd), controlling for age with 5-year linear splines. Column 3 shows the relative difference (Column 2) as a percentage of the odd-age mean. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table 9:** Effect on preventive care use

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
<b>Panel A. Treatment for cancer precursors</b>				
Helicobacter pylori	0.00017	0.00003*** (0.00001)	15.194	8,673,954
Polypectomy	0.01174	0.00724*** (0.00009)	61.617	8,673,954
<b>Panel B. Medication use for chronic conditions</b>				
High blood pressure	0.23884	0.00046*** (0.00013)	0.193	8,673,954
Diabetes	0.08918	0.00015** (0.00006)	0.171	8,673,954
High cholesterol	0.13901	0.00419*** (0.00011)	3.014	8,673,954
<b>Panel C. Medication use for other diseases</b>				
Tuberculosis	0.00195	0.00001 (0.00002)	0.460	8,673,954
Osteoporosis	0.02613	0.00106*** (0.00007)	4.049	8,673,954

Notes: The table reports the treatment effect of biennial subsidies on preventive care use, using the Standard Cohort Database of national health insurance claims. The sample includes individuals aged 40–89. Column 1 shows the control (odd-age) group mean. Column 2 reports ITT estimates of the subsidy effect, comparing treatment and control groups controlling for age. Column 3 shows the ITT effect as a percentage of the control group mean. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table 10:** Effect on one-year mortality

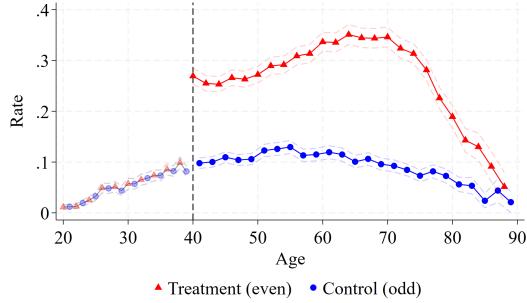
	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
Total death	0.009084	-0.000048 (0.000072)	-0.530	6,740,415
Cancer death	0.002805	-0.000015 (0.000041)	-0.548	6,740,415
Stomach cancer death	0.000334	-0.000002 (0.000014)	-0.515	6,740,415
Breast cancer death	0.000153	-0.000011 (0.000013)	-7.057	3,480,294
Cervical cancer death	0.000059	0.000008 (0.000008)	12.735	3,480,294
Liver cancer death	0.000434	-0.000015 (0.000016)	-3.511	6,740,415
Colorectal cancer death	0.000287	0.000016 (0.000013)	5.715	6,740,415
Lung cancer death	0.000646	-0.000003 (0.000020)	-0.503	6,740,415
Prostate cancer death	0.000116	0.000003 (0.000012)	2.358	3,260,121

Notes: The table reports the treatment effect of biennial subsidies on one-year mortality, using the Standard Cohort Database of national health insurance claims. The sample includes individuals aged 40–89. Column 1 shows the control (odd-aged) group mean. Column 2 reports ITT estimates of the subsidy effect, comparing treatment and control groups controlling for age. Column 3 shows the ITT effect as a percentage of the control group mean. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

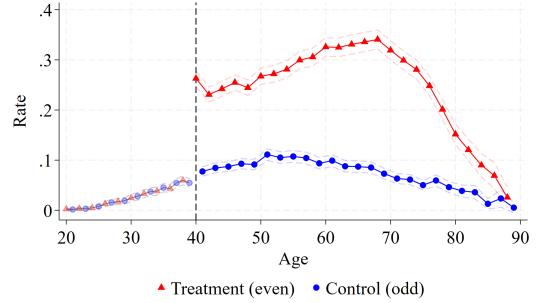
## 8 Figures

**Figure 1:** Screening rates

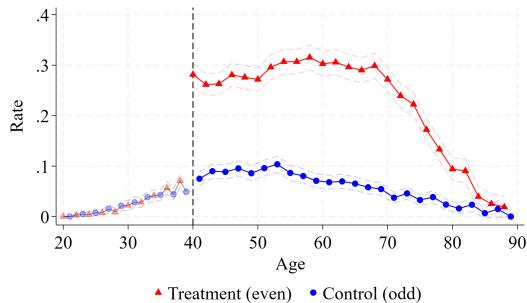
(a) General screening (biennially subsidized)



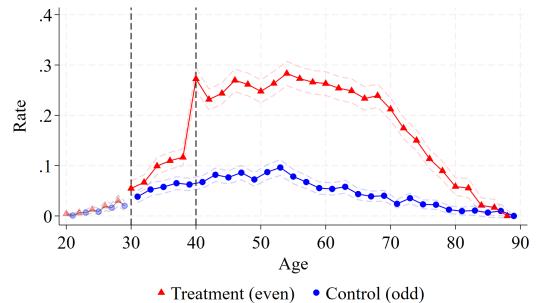
(b) Stomach screening (biennially subsidized)



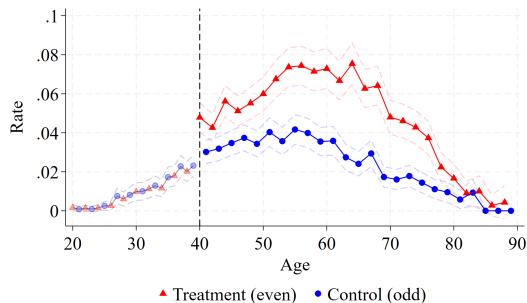
(c) Breast screening (biennially subsidized)



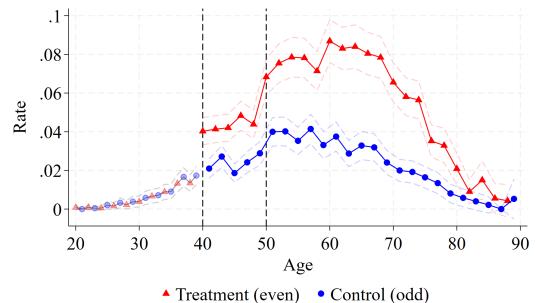
(d) Cervical screening (biennially subsidized)



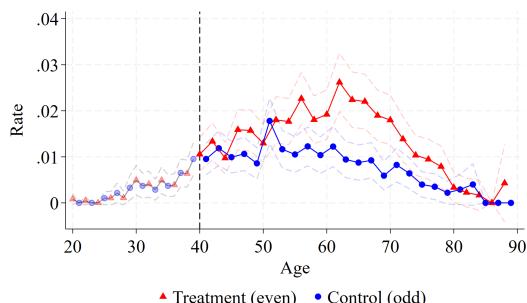
(e) Liver screening (annually subsidized)



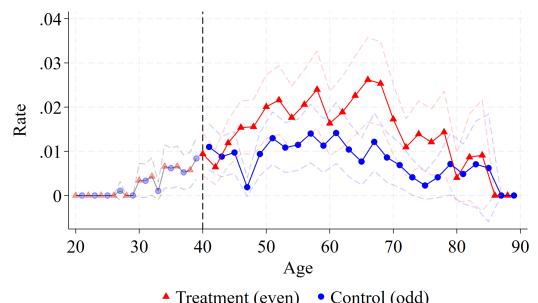
(f) Colorectal screening (annually subsidized)



(g) Lung screening (unsubsidized)



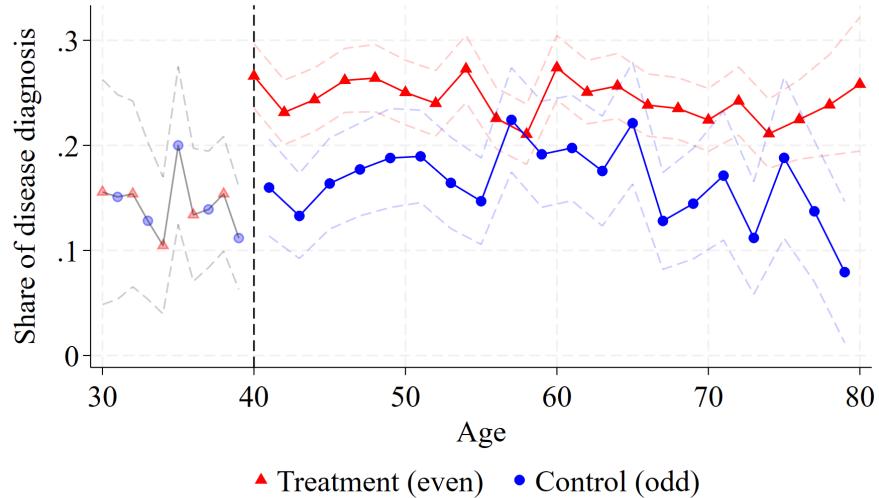
(h) Prostate screening (unsubsidized)



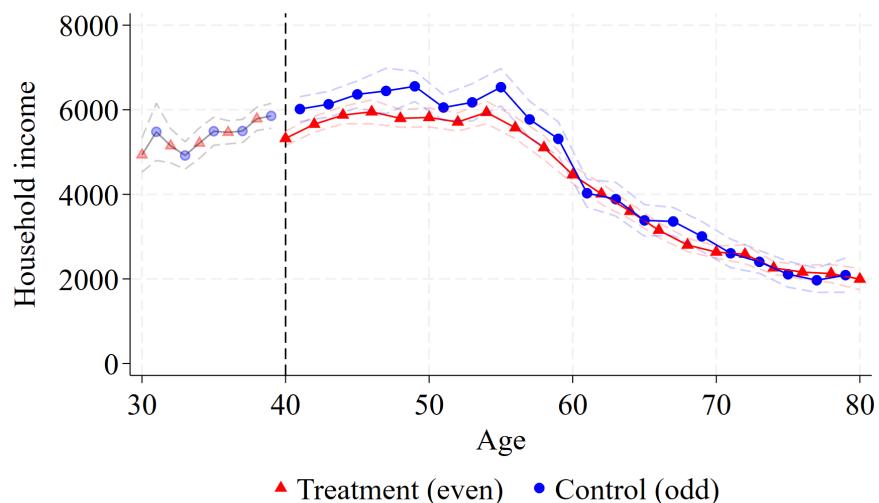
Notes: The figures plot take-up rates for 4 biennially subsidized screenings (general, stomach, breast, cervical), 2 annually subsidized screenings (liver, colorectal), and 2 unsubsidized screenings (lung, prostate) using survey data. Even ages are shown in red, odd ages in blue. Dashed vertical lines mark the subsidy starting age and age 40. Subsidies for cervical and colorectal screenings begin at ages 30 and 50, respectively. Dashed lines show 95% confidence intervals.

**Figure 2:** Screening participants characteristics

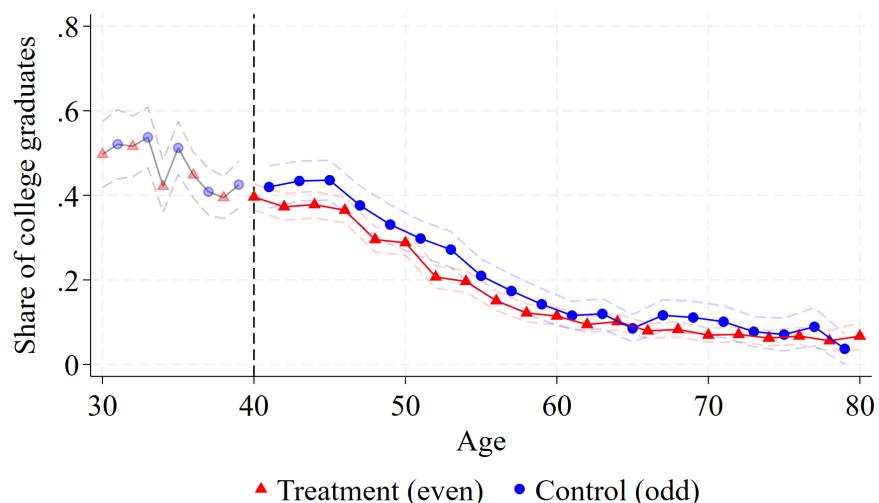
(a) Share of stomach disease diagnosis



(b) Household income



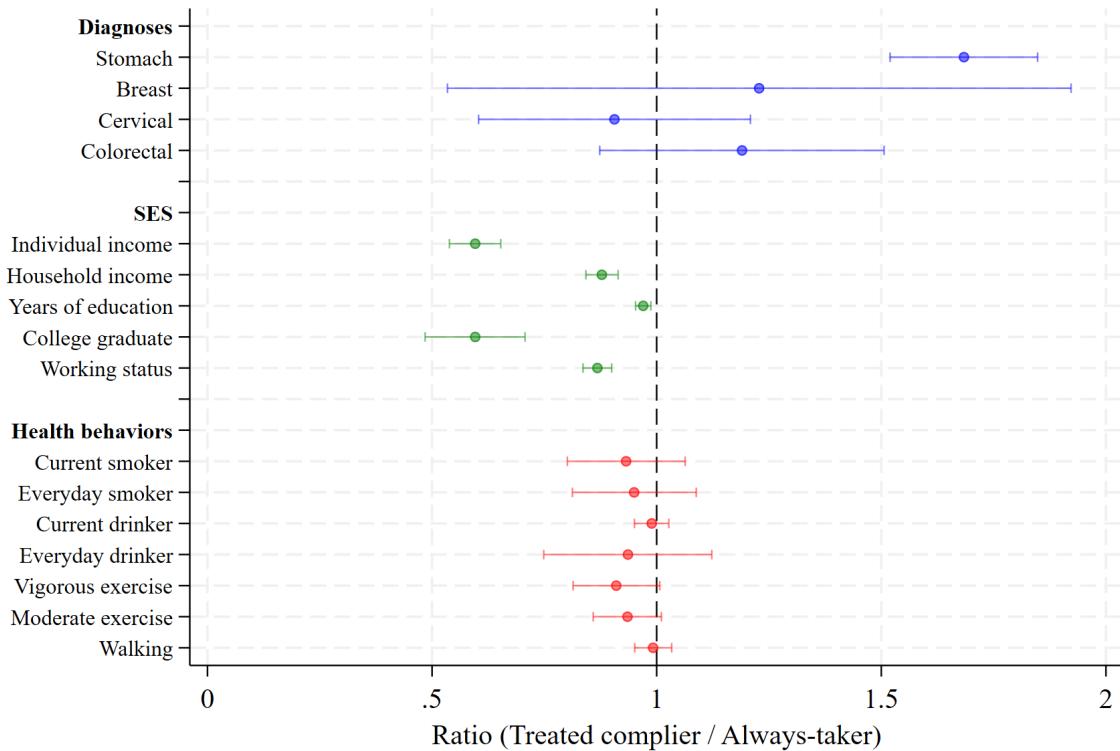
(c) Share of college graduates



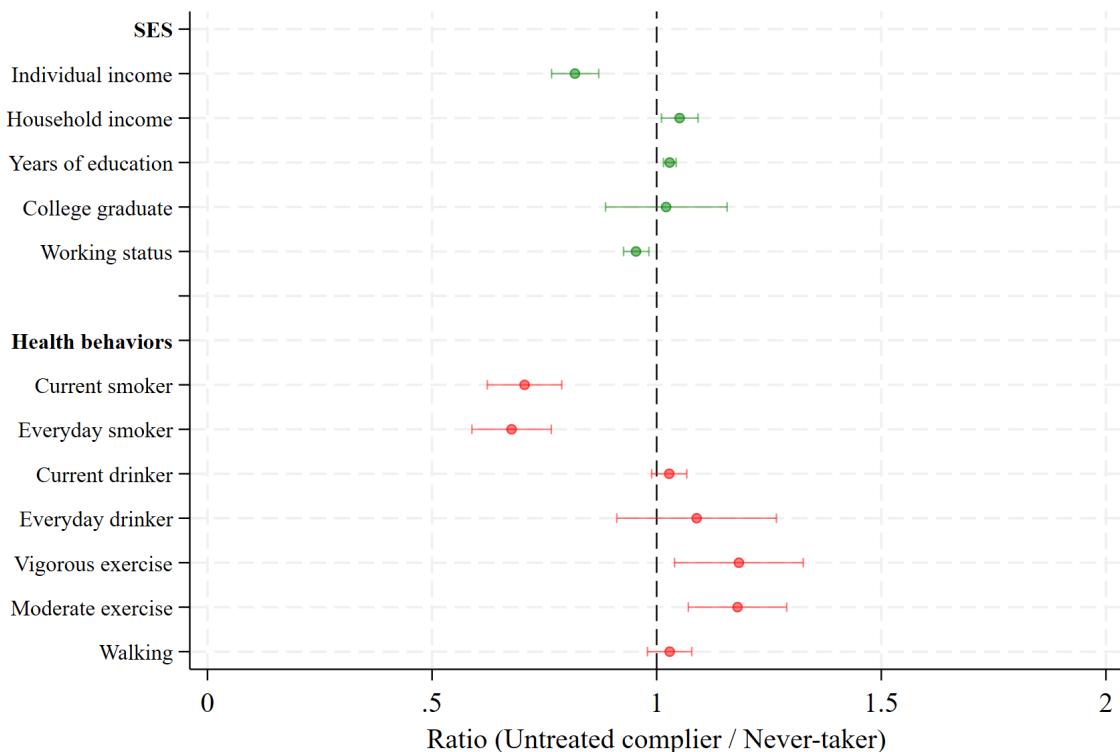
Notes: The first figure plots the share of stomach screenings in which participants were diagnosed with a disease. The sample is restricted to stomach screening participants. Diagnoses are coded using ICD-10, with examples provided in Appendix Section H. The second and third figures plot average household income (unit: 10,000 Korean Won) and the share of college graduates among participants in any screening. All figures use survey data. Even ages are shown in red, odd ages in blue. The dashed vertical line marks the subsidy starting age at 40. Dashed lines also show 95% confidence intervals.

**Figure 3:** Compliers characterization

(a) Comparing compliers with always-takers

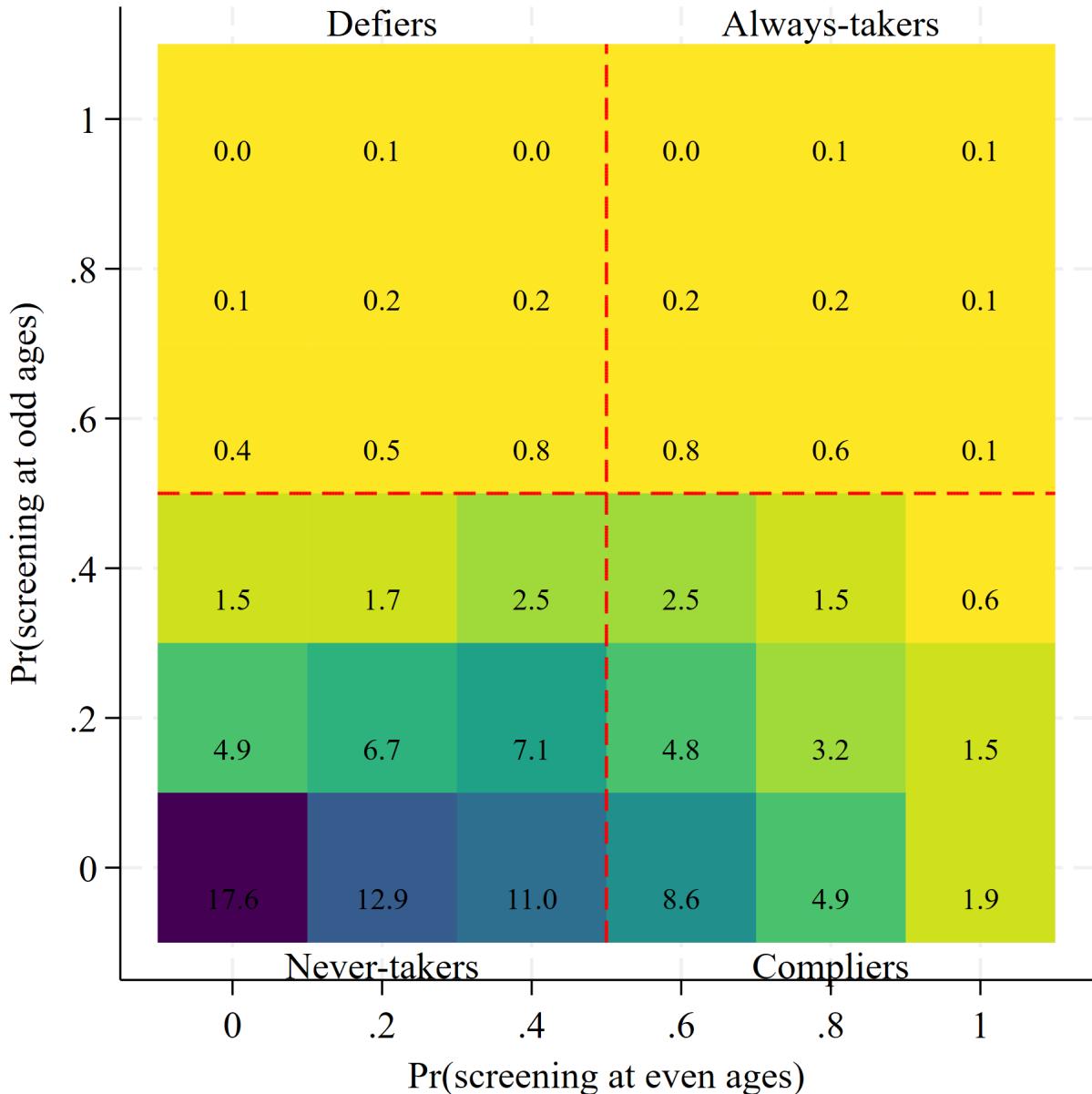


(b) Comparing compliers with never-takers



Notes: Figures plot the relative characteristics of compliers to always-takers or never-takers with 95% confidence intervals, using survey data. Treated compliers are compliers in the treatment group who participate in screenings, while untreated compliers are compliers in the control group who do not participate in screenings. Average values and ratios are obtained from the estimation of Equation (5) and reported in Table 5. Standard errors are calculated using bootstrap with 500 replications clustered at the individual level. Diagnoses indicate whether a screening participant was diagnosed with a disease. Health behaviors are coded as dummy variables for engaging in such activities among the male sample. Results for female health behaviors are presented in Appendix Section G.

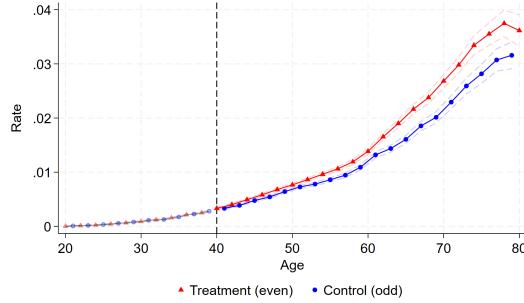
**Figure 4:** Joint distribution of the screening probability at even and odd ages



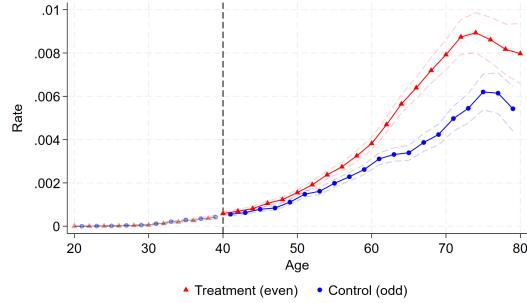
Notes: The figure plots the empirical joint distribution of screening probabilities at even and odd ages, based on 10-year health screening history in the survey data. The sample includes 5,701 individuals aged 40 or above in 2009 who participated in the survey all years from 2009–2018. Compliance groups are defined using the rule shown in Equation (2): compliers participate more than half of the time at subsidized ages and less than half at unsubsidized ages; always-takers participate more than half at subsidized and unsubsidized ages; never-takers participate less than half at both subsidized and unsubsidized ages; defiers participate more than half at unsubsidized ages and less than half at subsidized ages.

**Figure 5:** Cancer detection rates

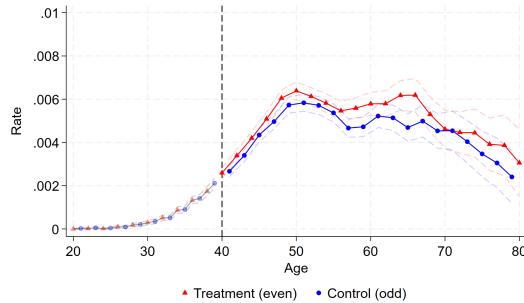
(a) Any cancer



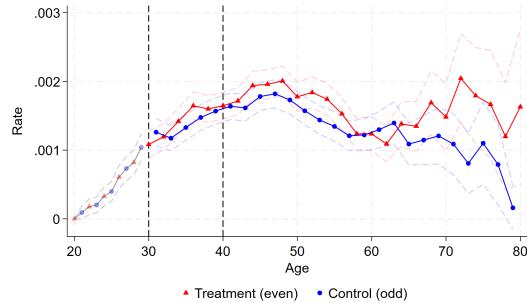
(b) Stomach cancer (biennially subsidized)



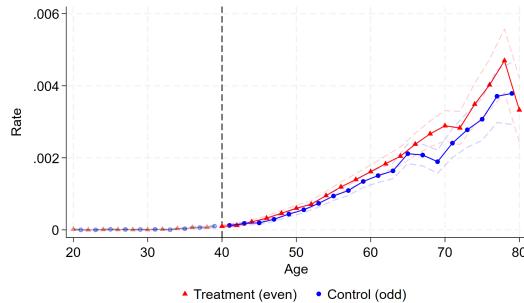
(c) Breast cancer (biennially subsidized)



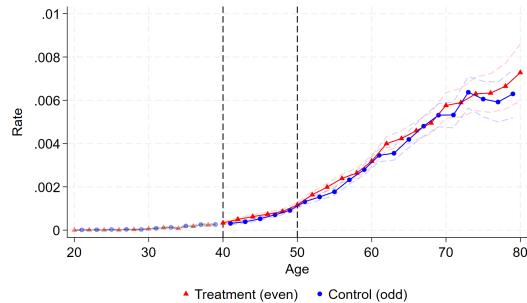
(d) Cervical cancer (biennially subsidized)



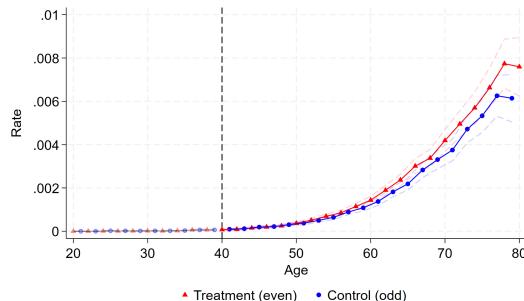
(e) Liver cancer (annually subsidized)



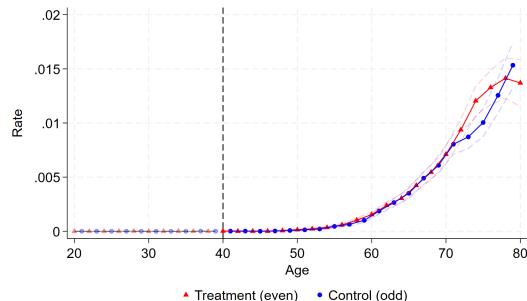
(f) Colorectal cancer (annually subsidized)



(g) Lung cancer (unsubsidized)

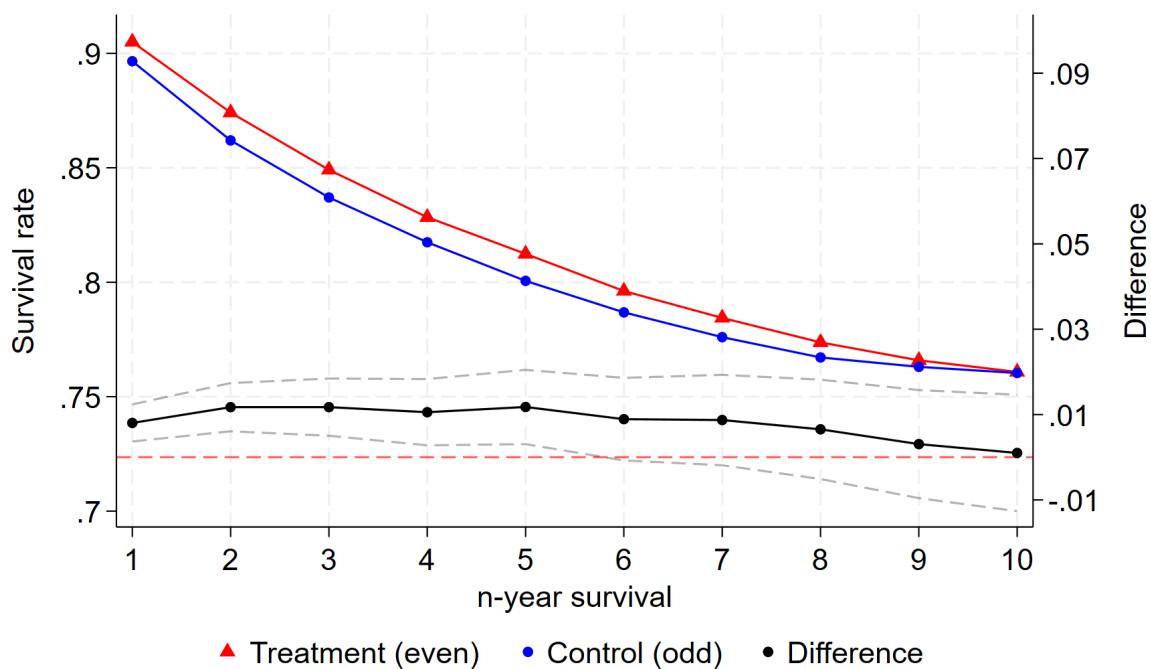


(h) Prostate cancer (unsubsidized)



Notes: Figures plot the true positive cancer diagnosis rate by age separately for the treatment (even) and control (odd) groups, using the Customized Cohort Database of national health insurance claims. Subsidy frequency for corresponding screenings is shown in the parentheses. Any cancer refers to the seven cancers listed. Cancer cases include both invasive and in situ types. Diagnoses are identified from the Coinsurance Reduction Program for Rare and Severe Diseases and include those detected with and without screenings.

**Figure 6:** Cancer survival rates by window



Notes: The figure plots survival rates for any cancer patients diagnosed at subsidized (even) and unsubsidized (odd) ages, with survival windows ranging from 1 to 10 years, using the Customized Cohort Database of national health insurance claims. Survival rates are shown on the left y-axis. Black dots plot the difference in survival rates controlling for age, with 95 percent confidence intervals, and should be read from the right y-axis. Cancer refers to any of the seven cancers listed in Figure 5.

## Appendix A Balance tests using insurance claims data

This section presents balance checks that were not presented in the main paper. I first present the balance table, estimated using the survey data, but with additional unadjusted differences between the treatment and control group. Next, I present two balance tables, estimated using the Customized Cohort Database and Standard Cohort Database from the National Health Insurance Service (NHIS) claims data.

Table [A1](#) presents the same balance table as in [2](#), estimated using the survey data, with the additional unconditional differences between the treatment and control groups, presented in column 3. As discussed in Section [3](#), the imbalances arise by design due to the use of analytical sample starting from age 40, an even number. The conditional differences, presented in column 4, show that age adjustment makes the point estimates smaller in absolute value and also reduces the standard errors.

Table [A2](#) and [A3](#) present the balance tables using the Customized Cohort Database and Standard Cohort Database from the National Health Insurance Service claims data. Comparing unconditional and conditional differences reveals that age adjustment makes the treatment and control group comparable. While some age-adjusted differences are significant, they are small in magnitudes, supporting balance between even- and odd-aged individuals.

**Table A1:** Balance test with unconditional and conditional differences

	(1)	(2)	(3)	(4)
	Treatment (even)	Control (odd)	Unconditional differences	Conditional differences
Age	58.697 (12.532)	59.240 (12.353)	-0.543*** (0.026)	- -
Female	0.530 (0.499)	0.532 (0.499)	-0.002** (0.001)	-0.002* (0.001)
Currently married	0.799 (0.401)	0.798 (0.402)	0.0009 (0.0009)	-0.0011 (0.0008)
Years of education	10.320 (4.510)	10.227 (4.538)	0.093*** (0.009)	-0.003 (0.008)
Working status	0.610 (0.488)	0.608 (0.488)	0.001 (0.002)	-0.003* (0.001)
Individual income	1446.3 (2081.6)	1425.7 (2068.1)	20.607*** (5.508)	2.762 (5.185)
Household income	4104.4 (3708.6)	4086.7 (3737.9)	17.735 (14.555)	3.221 (14.267)
Own a house	0.734 (0.442)	0.737 (0.441)	-0.002* (0.001)	-0.0002 (0.0011)
Number of household members	3.067 (1.317)	3.051 (1.317)	0.016*** (0.003)	-0.004 (0.003)
N	54274	52909		
Share	(0.51)	(0.49)		
F(8, 15939)				1.65 (0.10)

Notes: This table reports the unconditional and conditional balance check between the treatment group (even age group) and the control group (odd age group). The sample consists of those with age in [40, 89]. Column 3 reports the unconditional difference between the treatment and the control group. Column 4 reports the difference conditional on linear splines of age with 5 years interval. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A2:** Balance table with NHIS Customized Cohort Database

	(1)	(2)	(3)	(4)
	Treatment (even)	Control (odd)	Unconditional difference	Conditional difference
Age	52.158 (9.445)	52.748 (9.272)	-0.590*** (0.001)	- -
Female	0.470 (0.499)	0.467 (0.499)	0.003*** (0.000)	0.0002*** (0.0000)
Insurance premium (KRW)	100,276 (106,844)	100,848 (107,646)	-572*** (31)	65** (31)
Self-employed insurance: head	0.206 (0.404)	0.208 (0.406)	-0.002*** (0.000)	-0.0003*** (0.0001)
Self-employed insurance: dependent	0.147 (0.354)	0.146 (0.354)	0.0008*** (0.0001)	0.00009 (0.00010)
Employee insurance: head	0.385 (0.487)	0.382 (0.486)	0.003*** (0.000)	0.0007*** (0.0001)
Employee insurance: dependent	0.238 (0.426)	0.239 (0.427)	-0.001*** (0.000)	-0.0005*** (0.0001)
Medical aid insurance: head	0.017 (0.128)	0.017 (0.130)	-0.0005*** (0.0000)	0.00002 (0.00003)
Living in a metropolitan city	0.451 (0.498)	0.451 (0.498)	-0.0003*** (0.0001)	0.0003*** (0.0001)
Population/1,000	419.984 (261.563)	419.369 (261.887)	0.615*** (0.045)	-0.046 (0.043)
Working	0.606 (0.489)	0.605 (0.489)	0.001*** (0.000)	0.0003* (0.0001)
Agriculture, forestry and fishery	0.004 (0.065)	0.004 (0.065)	-0.00002 (0.00003)	0.00003 (0.00003)
Manufacturing	0.363 (0.481)	0.361 (0.480)	0.002*** (0.000)	-0.0003* (0.0002)
Have disability	0.058 (0.234)	0.060 (0.238)	-0.002*** (0.000)	-0.000003 (0.000031)
Disability grade	6.055 (4.922)	6.067 (4.935)	-0.012*** (0.003)	0.003 (0.003)
External physical disability	0.900 (0.300)	0.901 (0.299)	-0.0006*** (0.0002)	-0.0001 (0.0002)
Internal physical disability	0.060 (0.237)	0.060 (0.237)	-0.0001 (0.0002)	-0.000001 (0.000178)
Developmental disability	0.018 (0.134)	0.018 (0.132)	0.0007*** (0.0001)	0.0003*** (0.0001)
N	4,045,234	3,868,634		
Share	(0.51)	(0.49)		
F-statistic				F(17, 567574) = 4.83
Prob > F				0.000

Notes: This table reports the balance check between the treatment group (even-aged individuals) and the control group (odd-aged individuals) using the Customized Cohort Database from the Korean National Health Insurance Service. The sample consists of those with age in [40, 89]. Column 3 reports the unconditional difference between treatment and control group. Column 4 reports the differences between treatment and control group conditional on linear splines of age with 5 years interval. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A3:** Balance table with NHIS Standard Cohort Database

	(1)	(2)	(3)	(4)
	Treatment (even)	Control (odd)	Unconditional difference	Conditional difference
Age	55.559 (11.669)	56.195 (11.508)	-0.636*** (0.002)	- (0.000)
Female	0.517 (0.500)	0.518 (0.500)	-0.001*** (0.000)	0.0001*** (0.0000)
Insurance premium decile	6.230 (2.987)	6.225 (2.996)	0.004*** (0.001)	0.002* (0.001)
Self-employed insurance: head	0.228 (0.419)	0.228 (0.420)	-0.0006*** (0.0001)	0.00003 (0.00011)
Self-employed insurance: dependent	0.142 (0.349)	0.141 (0.348)	0.001*** (0.000)	-0.0001 (0.0001)
Employee insurance: head	0.276 (0.447)	0.270 (0.444)	0.006*** (0.000)	0.0005*** (0.0001)
Employee insurance: dependent	0.315 (0.464)	0.320 (0.467)	-0.005*** (0.000)	-0.0004*** (0.0001)
Medical aid insurance: head	0.032 (0.176)	0.033 (0.178)	-0.0008*** (0.0000)	0.000008 (0.000034)
Living in a metropolitan city	0.452 (0.498)	0.452 (0.498)	0.0004*** (0.0001)	0.0001 (0.0001)
Population/1,000	394.315 (255.342)	393.104 (255.390)	1.211*** (0.040)	0.021 (0.038)
Have disability	0.075 (0.263)	0.076 (0.266)	-0.002*** (0.000)	-0.00005 (0.00004)
Disability grade	1.630 (0.483)	1.632 (0.482)	-0.002*** (0.000)	-0.0001 (0.0003)
N	4,413,578	4,260,376		
Share	(0.51)	(0.49)		
F-statistic			F(11, 650368) = 4.87	
Prob > F				0.000

Notes: This table reports the balance check between the treatment group (even-aged individuals) and the control group (odd-aged individuals) using the Standard Cohort Database from the Korean National Health Insurance Service. The sample consists of those with age in [40, 89]. Column 3 reports the unconditional difference between treatment and control group. Column 4 reports the differences between treatment and control group conditional on linear splines of age with 5 years interval. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\* indicates significance at the 10/5/1% levels.

## Appendix B Robustness checks for age controls

This section presents robustness checks for the age control variables. As discussed in Section 3, I use linear splines with 5 years interval to adjust for age differences between the treatment and control groups. This section provides robustness checks by using linear splines with different intervals or by adding additional control variables.

Table A4 provides robustness check for balance table, presented in Table 2 in the main text. In addition to linear splines with 5 years interval, I provide age adjusted differences using linear splines with 3 and 7 years interval. The adjusted differences are small and largely not statistically significant.

Table A5 provides robustness check for the effect of subsidies on take-up, presented in Table 3 in the main text. In addition to linear splines with 5 years interval, I provide first stage effects, estimated using 3 and 7 years interval in Panel A. I also add additional control variables in Panel B. In addition to age controls using linear splines with 5 years interval, I include full set of control variables, presented in the balance table, Table 2. Alternatively, I also add individual fixed effects instead of full set of controls. The resulting estimates are highly consistent, supporting the robustness of the main specification.

Table A6 provides robustness check for the cross spillover, presented in Table 4 in the main text. Similar to the previous robustness check, I change the interval length of linear splines age controls or add full set of control variables or individual fixed effects. The resulting estimates are consistent.

**Table A4:** Robustness check for balance table

	(1)	(2)	(3)
	3 years	5 years	7 years
Female	-0.002* (0.001)	-0.002* (0.001)	-0.002* (0.001)
Currently married	-0.0014* (0.0009)	-0.0011 (0.0008)	-0.0012 (0.0008)
Years of education	-0.003 (0.008)	-0.003 (0.008)	-0.003 (0.008)
Working status	-0.003** (0.002)	-0.003* (0.001)	-0.003* (0.001)
Individual income	1.1 (5.3)	2.8 (5.2)	1.2 (5.2)
Household income	0.6 (15.4)	3.2 (14.3)	-4.5 (14.1)
Own a house	-0.0002 (0.0011)	-0.0002 (0.0011)	0.00002 (0.00109)
Number of household members	-0.004 (0.003)	-0.004 (0.003)	-0.004* (0.003)

Notes: This table reports the adjusted difference between the treatment (even-aged individuals) and the control (odd-aged individuals) group conditional on linear splines of age with 3, 5 and 7 years interval. The sample consists of those with age in [40, 89]. Standard errors are clustered at individual level and reported in parentheses. A \*/\*\*/\*\* indicates significance at the 10/5/1% levels.

**Table A5:** Effect of subsidies on take-up with different age controls

	(1)	(2)	(3)	(4)
	General	Stomach	Breast	Cervical
<b>Panel A. Linear splines of age</b>				
Interval 3	0.187*** (0.003)	0.190*** (0.003)	0.191*** (0.004)	0.144*** (0.003)
Interval 5	0.187*** (0.003)	0.190*** (0.003)	0.191*** (0.004)	0.145*** (0.003)
Interval 7	0.187*** (0.003)	0.190*** (0.003)	0.191*** (0.004)	0.144*** (0.003)
<b>Panel B. Linear splines with 5 years interval plus additional covariates</b>				
Full controls	0.187*** (0.003)	0.190*** (0.003)	0.191*** (0.004)	0.145*** (0.003)
Individual FE	0.189*** (0.003)	0.191*** (0.003)	0.192*** (0.004)	0.146*** (0.003)

Notes: This table reports the effect of biennial subsidies on 4 types of screening take-up with different control variables. Screenings reported in column 1 to 4 are subject to biennial subsidies when ages are even-numbered. Panel A uses linear splines of age with 3, 5, and 7 years intervals as controls. Panel B uses linear splines of age with 5 years interval plus additional covariates. Full controls specification includes all the variables reported in balance table (Table 2) as controls. Individual FE specification includes individual fixed effects as controls. Standard errors are clustered at individual level and reported in parentheses. A \*/\*\*/\*\* indicates significance at the 10/5/1% levels.

**Table A6:** Cross spillover with different controls

	(1)	(2)	(3)	(4)
	Liver	Colorectal	Prostate	Lung
<b>Panel A. Linear splines of age</b>				
Interval 3	0.027*** (0.001)	0.033*** (0.001)	0.007*** (0.001)	0.0062*** (0.0007)
Interval 5	0.027*** (0.001)	0.033*** (0.001)	0.007*** (0.001)	0.0062*** (0.0007)
Interval 7	0.027*** (0.001)	0.033*** (0.001)	0.007*** (0.001)	0.0062*** (0.0007)
<b>Panel B. Linear splines with 5 years interval plus additional covariates</b>				
Full controls	0.027*** (0.001)	0.033*** (0.001)	0.007*** (0.001)	0.0062*** (0.0007)
Individual FE	0.028*** (0.001)	0.033*** (0.001)	0.007*** (0.001)	0.0063*** (0.0007)

Notes: This table reports the effect of biennial subsidies on 4 types of screening take-up with different control variables. Liver and colorectal screenings are subject to annual subsidies, while prostate and lung screenings are not subsidized. Panel A uses linear splines of age with 3, 5, and 7 years intervals as controls. Panel B uses linear splines of age with 5 years interval plus additional covariates. Full controls specification includes all the variables reported in balance table (Table 2) as controls. Individual FE specification includes individual fixed effects as controls. Standard errors are clustered at individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

## Appendix C Robustness checks with different analytical samples

This section presents robustness checks for the balance between treatment and control groups by using analytical samples of different starting ages. As discussed in Section 3, the imbalance between treatment and control groups stems from the choice of analytical sample starting from age 40, an even number. This section alternatively uses analytical sample starting from 39 or 41 to show that the sample starting age is creating mechanical imbalance between the treatment and the control groups. I provide robustness checks for main results, running all the regressions without the age controls but with three different analytical samples. The imbalances running in opposite directions depending on the starting age provide nonparametric bounds for the estimates. This obviates the need to specify any functional form for age control variables.

Table A7 presents unadjusted differences between treatment and control groups with samples starting from age 39, 40, and 41. It shows that depending on the choice of first age, the differences run in opposite direction. This confirms that the mechanical imbalance between the two groups are indeed driven by the choice of an analytical sample.

Table A8 presents the effects of biennial subsidies on take-up without adjusting for age difference between treatment and control groups. The effects are estimated in three analytical samples with different starting ages. Since the imbalance runs in opposite direction, depending on the choice of first age, the resulting estimates provide nonparametric bounds for the effects of subsidies. The resulting estimates are highly consistent, supporting the robustness of my main specification.

Similarly, Table A9 presents cross spillover with three different analytical samples with different starting ages. The resulting estimates are consistent with our main estimates, reported in Table 4 in the main text.

**Table A7:** Balance test with different starting ages

	(1)	(2)	(3)
	Age ∈ [39, 89]	Age ∈ [40, 89]	Age ∈ [41, 89]
Age	0.521*** (0.026)	-0.543*** (0.026)	0.562*** (0.025)
Female	-0.001 (0.001)	-0.002** (0.001)	-0.001 (0.001)
Currently married	-0.0009 (0.0009)	0.0009 (0.0009)	-0.0018** (0.0009)
Years of education	-0.094*** (0.009)	0.093*** (0.009)	-0.107*** (0.010)
Working status	-0.006*** (0.001)	0.001 (0.002)	-0.007*** (0.002)
Individual income	-16.235*** (5.470)	20.607*** (5.508)	-24.789*** (5.618)
Household income	-23.153 (14.766)	17.735 (14.555)	-31.182** (14.995)
Own a house	0.003*** (0.001)	-0.002* (0.001)	0.003** (0.001)
Number of household members	-0.027*** (0.003)	0.016*** (0.003)	-0.034*** (0.003)
N	110121	107183	104153

Notes: This table reports the balance check between the treatment (even-aged individuals) and the control (odd-aged individuals) group using samples with different starting ages (39, 40, 41). The ending ages are at 89 for the three samples. All regressions do not include any control variable. The coefficients report the average difference between the treatment and the control group. Standard errors are clustered at individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A8:** Bounding estimates for the effect of subsidies on take-up

	(1)	(2)	(3)
	Age ∈ [39, 89]	Age ∈ [40, 89]	Age ∈ [41, 89]
General	0.187*** (0.003)	0.186*** (0.003)	0.188*** (0.003)
Stomach	0.191*** (0.003)	0.189*** (0.003)	0.190*** (0.003)
Breast	0.192*** (0.004)	0.191*** (0.004)	0.190*** (0.004)
Cervical	0.165*** (0.003)	0.165*** (0.003)	0.162*** (0.003)
N	110121	107183	104153

Notes: This table reports the effect of biennial subsidies on 4 types of screening take-up, using samples with different starting age (39, 40, 41). It does not include any control variable. These four screenings are subject to biennial subsidies at even-numbered ages. Standard errors are clustered at individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A9:** Bounding estimates for cross spillover

	(1)	(2)	(3)
	Age ∈ [39, 89]	Age ∈ [40, 89]	Age ∈ [41, 89]
Liver	0.027*** (0.001)	0.026*** (0.001)	0.027*** (0.001)
Colorectal	0.033*** (0.001)	0.033*** (0.001)	0.034*** (0.001)
Lung	0.0061*** (0.0007)	0.0061*** (0.0007)	0.0064*** (0.0007)
Prostate	0.007*** (0.001)	0.007*** (0.001)	0.008*** (0.001)
N	110121	107183	104153

Notes: This table reports the effect of biennial subsidies on the take-up of liver and colorectal screenings (annually subsidized) and lung and prostate screenings (unsubsidized), using samples with different starting age (39, 40, 41). It does not include any control variable. Standard errors are clustered at individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

## Appendix D Intertemporal substitution

This section presents detailed analyses on intertemporal substitution. The biennial subsidies at even ages create an incentive to shift screening timing from odd to even ages to receive subsidized screenings. This section provides evidence that biennial subsidies lead to both net increase in participation and shift in screening timing, with advancing screenings more common than delaying them.

First, the evidence for net increase in take-up comes from age cutoff. I focus on cohorts around age 40 to examine how screening take-up changes at the subsidy eligibility cutoff. If individuals were merely substituting from subsidized (even) to unsubsidized (odd) ages, one would expect a sharp drop in participation at unsubsidized ages immediately following the cutoff. However, as shown in the figures in the main text (Figure 1), I find no such decline, which shows there is no clear sign of substitution around the cutoff age.

One concern with this argument is compositional changes at the age cutoff. Since age cutoff coincides with the recommended starting age for screenings, if new people start to participate from age 40, regardless of even or odd, this opposing force could mask the drop in screening rate at unsubsidized ages after age 40. Therefore, I fix the sample by focusing on those who were already participating before age 40 to examine if they exhibit any drop at unsubsidized ages after age 40. I track 4 age cohorts at ages 36 to 43.

Panel A and B in Figure A2 present stomach and breast screening take-up pattern for participants at age 36, 37, 38 and 39 separately. By definition, age 36 participants show take-up rate of one at age 36.<sup>55</sup> Comparing before and after age 40, one can see that take-up at unsubsidized ages after 40 is not any lower than pre-40 take-up level, but the take-up at subsidized ages are clearly much higher. This suggests that as one passes age 40, take-up at subsidized ages rise due to subsidies and it does not come at the cost of drop in take-up at unsubsidized ages.

Next, the evidence for intertemporal substitution comes from the monthly distribution

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<sup>55</sup>One reason I do not examine those who participated at least once before age 40 is due to age 39 participants. By definition, they show average take-up of one at age 39. Therefore, the take-up pattern for those who participated at least once before 40 shows abnormally large take-up at age 39 and makes it hard to compare before and after 40.

of screening take-up. I examine stomach and breast screening monthly take-up distribution from age 40 to 89. If there were intertemporal substitution, then it would be most pronounced in January or December of the year when there are sharp changes in subsidy eligibility. One can get screening a couple of weeks early and receive it in December of subsidized (even) age instead of January of unsubsidized (odd) age. Similarly, one can delay it a couple of weeks and receive it in January of subsidized age instead of December of unsubsidized age.

Figure A2a and A2e present the monthly distribution for stomach and breast screening take-up from age 40 to 89, separately for subsidized (even) and unsubsidized (odd) ages. In each figure, there is a large bunching in December of the treatment group, suggesting that individuals advance their screenings and participate before subsidies expire. In contrast, there is no comparable bunching in January of the treatment group, which would be expected if delays into subsidized years were common. This asymmetry suggests that advancement is more prevalent than delay.

To make it more rigorous, I employ difference-in-differences design and compare the monthly change in take-up before and after age 40 to take into account seasonal take-up pattern in the absence of subsidies. Using the exact day of screening information, I transform the individual-year data into individual-month-year data and run the following econometric specification.

$$\begin{aligned}
screen_{imt} = & \theta_0 + \theta_1 \cdot after40_{imt} + \theta_2 \cdot age\_even_{imt} + \sum_{m=2}^{12} month_m \\
& + \theta_3 \cdot after40_{imt} \cdot age\_even_{imt} + \sum_{m=2}^{12} month_m \cdot after40_{imt} \\
& + \sum_{m=2}^{12} month_m \cdot age\_even_{imt} + \sum_{m=2}^{12} month_m \cdot after40_{imt} \cdot age\_even_{imt} + \varepsilon_{imt}
\end{aligned} \tag{3}$$

It is a fully saturated model of the following variables: (i)  $after40_{imt}$ , a dummy variable that equals one if the age of individual  $i$  is 40 or above in month  $m$  and year  $t$ , (ii)  $age\_even_{imt}$ , a dummy variable that equals one if the age of individual  $i$  is even in month  $m$  and year  $t$ , and (iii)  $\{month_m\}_{m=2}^{12}$ , dummy variables for months using January as the reference month. The outcome variable,  $screen_{imt}$ , is a dummy variable that equals

one if individual  $i$  received the stomach or breast screening in month  $m$ , year  $t$ . The standard errors are clustered at the individual level. The analytical sample consists of individuals with ages 20 to 89. Our first coefficient of interest is the coefficient of the terms  $age\_even_{imt} + \sum_{m=2}^{12} month_m \cdot age\_even_{imt}$  that provides comparison in monthly take-up between even- and odd-aged individuals before age 40. There should be no systematic difference, since it is the period before subsidies apply. The second coefficient of interest is the coefficient of the terms  $\sum_{m=2}^{12} month_m \cdot after40_{imt}$  that provides comparison in monthly take-up of odd-aged individuals before and after 40. Finally, the third coefficient of interest is the coefficient of the terms  $\sum_{m=2}^{12} month_m \cdot after40_{imt} + \sum_{m=2}^{12} month_m \cdot after40_{imt} \cdot age\_even_{imt}$  that provides comparison in monthly take-up of even-aged individuals before and after 40.

The second set of figures in each panel, Figure A2b and A2f provide comparison of monthly take-up between treatment group before age 40 and control group before age 40. These are the coefficients of the term  $age\_even_{imt} + \sum_{m=2}^{12} month_m \cdot age\_even_{imt}$ . This is the period before biennial subsidies kick in. Therefore, we do not find any systematic difference in monthly take-up between the treatment and the control group.

The third set of figures in each panel, Figure A2c and A2g present the comparison of monthly take-up between control group before age 40 and control group after age 40. These are coefficients of the term  $\sum_{m=2}^{12} month_m \cdot after40_{imt}$ . The figures show no systematic monthly variation in take-up, suggesting that there was no differential change in take-up across months in the control group before and after age 40 cutoff.

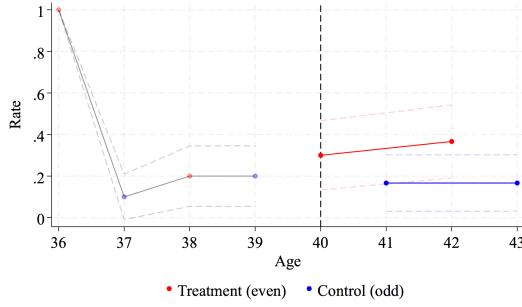
The fourth set of figures in each panel, Figure A2d and A2h present the comparison of monthly take-up between treatment before age 40 and treatment after age 40. These are coefficients of the term  $\sum_{m=2}^{12} month_m \cdot after40_{imt} + \sum_{m=2}^{12} month_m \cdot after40_{imt} \cdot age\_even_{imt}$ . For both stomach and breast screenings, there is a noticeable increase in March and December, compared to January. The large spike in December suggests advancing screenings, as individuals rush to complete their screenings before subsidies expire. However, there is no comparable increase in January, suggesting that delaying into subsidized years are uncommon, even after accounting for seasonal fluctuation in

screening take-up. The increase in March is likely driven by reminder mails from regional offices of National Health Insurance Service, typically sent in March and April to inform people of the screenings they should receive. These reminders are sent every year, since odd-aged individuals could also be eligible for certain screenings, like general, liver or colorectal screenings.

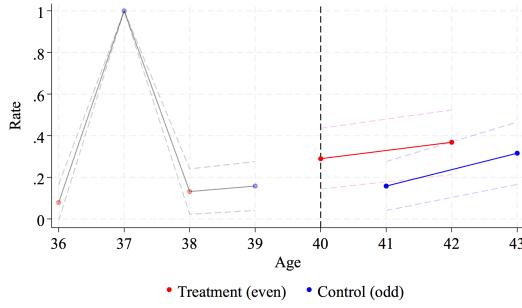
**Figure A1:** Screening take-up for participants before 40

**Panel A.** Stomach screening participants

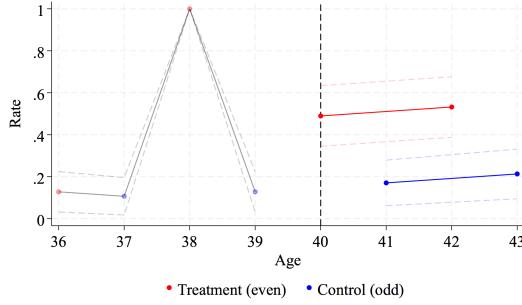
(a) Age 36 participants



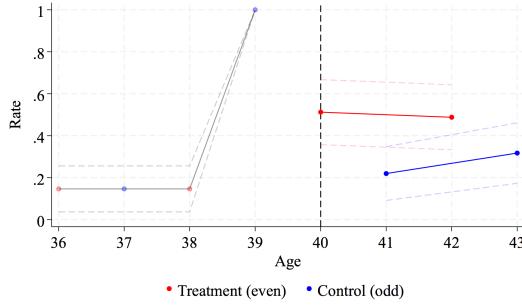
(b) Age 37 participants



(c) Age 38 participants

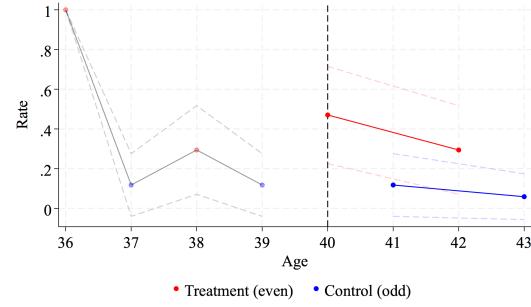


(d) Age 39 participants

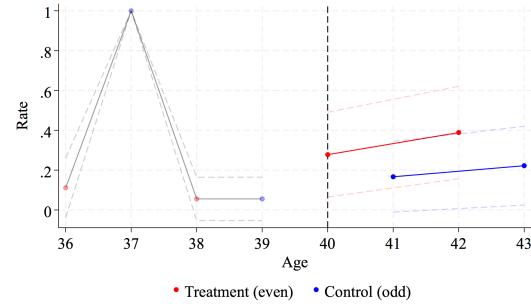


**Panel B.** Breast screening participants

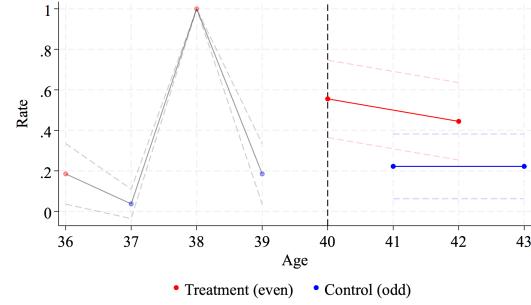
(e) Age 36 participants



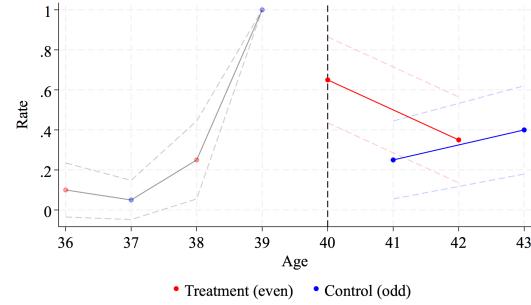
(f) Age 37 participants



(g) Age 38 participants



(h) Age 39 participants

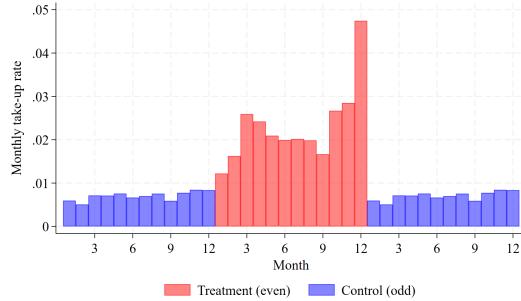


Notes: Figures plot the stomach and breast screening take-up for those who already participate in screening before age 40. The sample is restricted to four age cohorts around age 40. Each figure plots the take-up among either the stomach or breast cancer participants at age 36, 37, 38 or 39. Even ages are colored in red and odd ages are colored in blue. 95 percent confidence intervals are shown in dashed line.

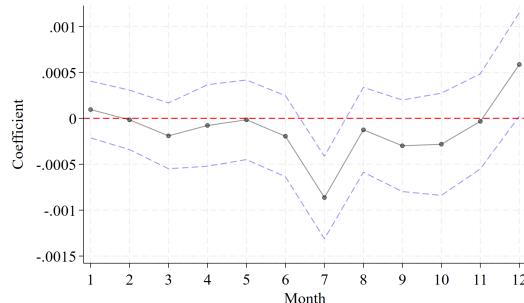
**Figure A2:** Monthly screening take-up

**Panel A.** Stomach screening

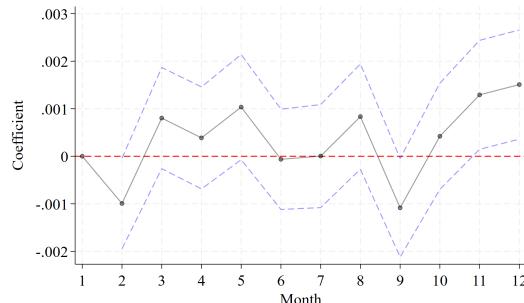
(a) Average monthly take-up



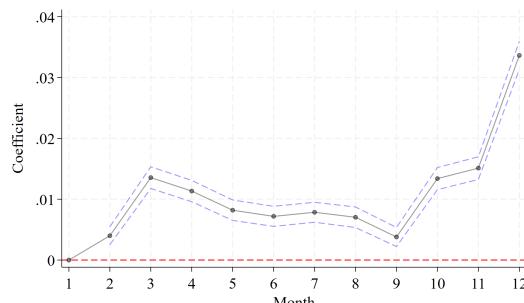
(b) Treatment before 40 vs Control before 40



(c) Control before 40 vs Control after 40

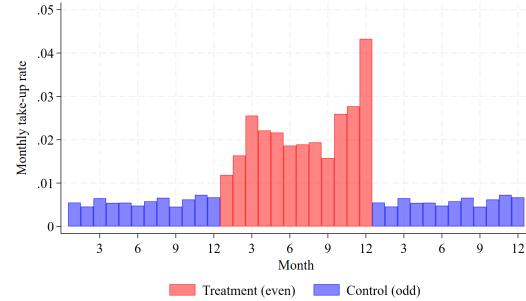


(d) Treatment before 40 vs Treatment after 40

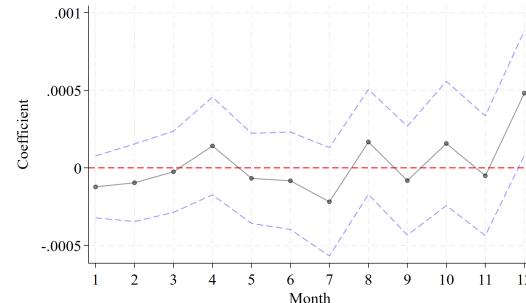


**Panel B.** Breast screening

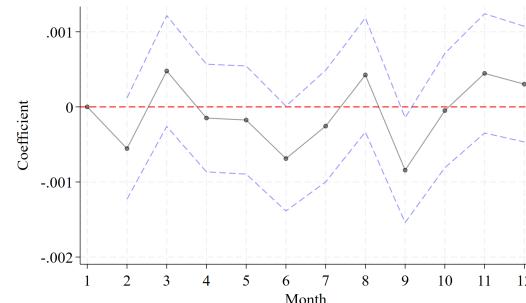
(e) Average monthly take-up



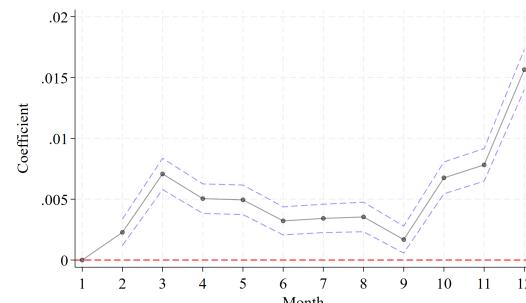
(f) Treatment before 40 vs Control before 40



(g) Control before 40 vs Control after 40



(h) Treatment before 40 vs Treatment after 40



Notes: Figures plot the analysis results using the individual-month-year level data. First figures in each panel plots the average monthly take-up of stomach and breast screenings. Control group is repeated after treatment group to provide an easy comparison. Second figures plot the monthly differences in take-up between treatment and control group before age 40. Third figures plot the monthly differences in take-up in the control group between before and after age 40 using January as the reference month. Fourth figures plot the monthly differences in take-up in the treatment group between before and after age 40 using January as the reference month.

## Appendix E Additional analyses on cross spillover

This section presents additional analyses on the cross spillover, discussed in Section 5.1.3 in the main text. Table A10 presents the share of screenings that happen on the same day with general screening. For instance, among stomach screening participants, 88 percent also receive general screening in the same year. Among participants in general and stomach screenings, 96 percent receive the two screenings on the same day. If they are not received on the same day, 2.7 percent receive general screening first followed by stomach screening within 30 days. On the other hand, only 0.8 percent receive stomach screening first followed by general screening.

Table A11 presents the heterogeneity in cross spillover effects between male and female. The goal is to examine which of the 4 biennial screenings is generating spillover effects. Among 4 biennial screenings, general and stomach screenings are subsidized for everyone, but breast and cervical screenings are provided only for women. If the two female screenings are generating spillover, there should be larger spillover effect for women compared to men. The heterogeneous treatment effect by gender shown in Table A11 does not support this hypothesis. If anything, they seem to be slightly smaller for women in the case of colorectal screening. This implies that general and stomach screenings are the ones that generate spillover effects, not breast and cervical screenings. This could be due to the fact general and stomach screenings are the most commonly received types of screenings.

I also analyze the selection in cross spillover, that is, characteristics of individuals who, in addition to receiving biennial screenings at even ages, further receive annually subsidized or unsubsidized screenings. Our data indicate that participants in annual and unsubsidized screenings are a subset of biennial screening participants. Among annual or unsubsidized screening participants at even ages, more than 97% also receive biennial screenings in the same year. This implies that individuals typically first receive biennial screenings and some of them opt for additional annual or unsubsidized screenings. This one-sided noncompliance simplifies the selection analysis. One only needs to examine who participate in annual or unsubsidized screening among biennial screening participants. I

run the following regression to estimate the characteristics of compliers in spillover.

$$y_{it} = \delta_0 + \delta_1 \cdot screen_{it} + \varepsilon_{it} \quad (4)$$

The sample is restricted to biennial screening participants at even ages.<sup>56</sup> The explanatory variable,  $screen_{it}$ , is an indicator variable for participating in any of the annual or unsubsidized screenings. Standard errors are clustered at the individual level.

Table A12 presents the characteristics of compliers in cross spillover. Compared to individuals who participate only in biennial screenings, those who additionally undergo annually subsidized or unsubsidized screenings are less likely to be diagnosed with stomach, breast and cervical disease, suggesting better overall health. This pattern can be attributed to their higher socioeconomic status. Specifically, compliers have higher individual and household income, higher educational attainments and are more likely to be working.

The positive selection observed in socioeconomic status and health conditions suggests the cross spillover is primarily driven by patients rather than providers. If physicians were the main drivers, additional screenings would likely be recommended based on medical necessity, leading to negative selection on health conditions, i.e., greater uptake among individuals with poorer health. However, the strong positive selection on health, income, and education indicates that cross spillover is largely driven by patients who are more health-conscious and financially capable of affording additional, unsubsidized screenings.

**Table A10:** Share of screenings taken on the same day

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Stomach	Breast	Cervical	Liver	Colorectal	Prostate	Lung
Pr(general = 1   screen = 1)	0.878	0.861	0.834	0.844	0.799	0.786	0.699
Pr(same day   screen = 1, general = 1)	0.964	0.947	0.899	0.948	0.856	0.960	0.937
Pr(general first   screen = 1, general = 1)	0.027	0.037	0.053	0.036	0.110	0.024	0.045
Pr(general later   screen = 1, general = 1)	0.008	0.015	0.031	0.008	0.179	0.004	0.003

Notes: This table examines if people receive screenings on the same day with the general health screening. The sample is those with age 40 to 89.  $screen = 1$  refers to the take-up of given screening in each column. General first (later) means the screening concerned is received after (before) the general screening within 30 days.

<sup>56</sup>When the outcome variable is a diagnosis of a stomach disease, the sample is restricted to stomach screening participants, with similar restrictions applied to breast and cervical screenings. For the other outcome variables, the sample is restricted to participants in any of the four biennial screenings.

**Table A11:** Heterogeneity in cross spillover effects across gender

	(1)	(2)	(3)
	Liver	Colorectal	Lung
treat	0.025*** (0.002)	0.036*** (0.002)	0.007*** (0.001)
treat × Female	0.003 (0.003)	-0.005* (0.003)	-0.002 (0.001)
Female	-0.017*** (0.002)	-0.012*** (0.002)	-0.0078*** (0.0009)
N	107183	107183	107183
Control group mean	0.028	0.027	0.009
Age range	[40, 89]	[40, 89]	[40, 89]

Notes: This table reports estimates of cross spillover for men and women. The sample consists of those with age from 40 to 89. The variable *treat* equals one if one has an even age and eligible for subsidized screenings. Standard errors are clustered at individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A12:** Compliers in cross spillover

	(1)	(2)	(3)
	Participants in		
	Annual screenings	Unsubsidized screenings	Sample mean
<b>Panel A. Diagnoses</b>			
Stomach	-0.028*** (0.006)	-0.087*** (0.010)	0.228
Breast	-0.007** (0.003)	-0.020*** (0.004)	0.022
Cervical	-0.024*** (0.006)	-0.034** (0.015)	0.067
<b>Panel B. SES</b>			
Individual income	874*** (49)	1499*** (110)	1592
Household income	1012*** (66)	1393*** (145)	4564
Years of education	0.975*** (0.073)	1.342*** (0.129)	10.769
College graduate	0.074*** (0.007)	0.131*** (0.014)	0.196
Working status	0.063*** (0.008)	0.141*** (0.012)	0.635

Notes: This table reports the relative characteristics of biennial screening participants who further participate in annual and unsubsidized screenings using the survey data. Table 1 shows the list of biennial, annual and unsubsidized screenings. The sample is restricted to even age participants in any of the 4 biennial screenings. When outcome variable is stomach or breast or cervical disease diagnosis, the sample is restricted to stomach or breast or cervical screening participants, respectively. Column 1 reports the difference for those who further receive annual screenings and column 2 reports the difference for those who further receive unsubsidized screenings. Column 3 reports the sample mean among the even age biennial screening participants. All the coefficients are from separate regressions. Standard errors are clustered at the individual level. They are reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

## Appendix F Disease diagnoses from the survey data

This section provides a list of ICD-10 disease diagnosis codes that were reported as a disease found through screenings in the survey data. The survey asked screening participants if they had found any disease through screening, and if so, the diagnoses were coded using the Korean Classification of Diseases diagnosis code (Korean version of the ICD-10). I list below the ICD-10 codes that were reported for each screening. The diagnoses are listed in the order of frequency, so this provides a list of common diagnoses made through screening.

- Stomach diseases
  - K29 Gastritis and duodenitis
  - K52 Other and unspecified noninfective gastroenteritis and colitis
  - K21 Gastro-esophageal reflux disease
  - K25 Gastric ulcer
  - B98 Helicobacter pylori
  - K31 Other diseases of stomach and duodenum
  - K20 Esophagitis
  - C16 Malignant neoplasm of stomach
  - K26 Duodenal ulcer
- Colorectal diseases
  - K63 Other diseases of intestine
  - D12 Benign neoplasm of colon, rectum, anus and anal canal
  - D13 Benign neoplasm of other and ill-defined parts of digestive system
  - R19 Other symptoms and signs involving the digestive system and abdomen
  - C18 Malignant neoplasm of colon
- Breast diseases

- N63 Unspecified lump in breast
  - N64 Other disorders of breast
  - D24 Benign neoplasm of breast
  - N60 Benign mammary dysplasia
  - C50 Malignant neoplasm of breast
- Female reproductive part diseases
    - N76 Other inflammation of vagina and vulva
    - N71 Inflammatory disease of uterus, except cervix
    - N85 Other noninflammatory disorders of uterus, except cervix
    - N83 Noninflammatory disorders of ovary, fallopian tube and broad ligament

## Appendix G Complier selection

### G.1 Methodology for characterizing compliers

To formally estimate the average group characteristics and make comparisons between always-takers, compliers and never-takers, I follow the approach used in [Kim and Lee \(2017\)](#); [Einav et al. \(2020\)](#) and [Kowalski \(2023\)](#).<sup>57</sup> The key idea is to infer always-takers' characteristics from screening participants in the control group and infer never-takers' characteristics from screening nonparticipants in the treatment group. The exogeneity of the assignment mechanism guarantees that the characteristics of always- and never-takers will be the same in both the treatment and the control group. The compliers' characteristics can be backed out from the equation where the characteristic of screening participants (nonparticipants) in the treatment (control) group is a weighted sum of always-takers' (never-takers') and compliers' characteristics, with the weights corresponding to the relative share of each group.

I present detailed steps to infer complier characteristics in the even/odd subsidy design.<sup>58</sup> I estimate the following equation to estimate group characteristics.

$$y_{it} = \lambda_1 age\_even_{it} + \lambda_2 screen_{it} + \lambda_3 age\_even_{it} \times screen_{it} + \boldsymbol{\lambda}'_4 \mathbf{f}(\mathbf{age}_{it}) + \varepsilon_{it} \quad (5)$$

Given a characteristic variable,  $y_{it}$ , the above equation can be used to estimate the average characteristic of always-takers by imposing the condition  $age\_even_{it} = 0$  and  $screen_{it} = 1$ . This is because always-takers are the only group that gets screened even in the absence of subsidies. The estimates are given by  $g_{AT}(y) = \hat{\lambda}_2 + \hat{\boldsymbol{\lambda}}'_4 \mathbf{f}(\mathbf{age}_{it})$ . Similarly, noting that never-takers are the ones who do not get screened despite the presence of subsidies, I impose  $age\_even_{it} = 1$  and  $screen_{it} = 0$ , and the resulting estimates are given by  $g_{NT}(y) = \hat{\lambda}_1 + \hat{\boldsymbol{\lambda}}'_4 \mathbf{f}(\mathbf{age}_{it})$ .

Compliers characteristics can be derived from either the screening participants in the

<sup>57</sup> [Marbach and Hangartner \(2020\)](#) gives a nice summary of the methodology.

<sup>58</sup> Appendix in [Einav et al. \(2020\)](#) present the detailed steps to characterize compliers at age 40 in the regression discontinuity setting.

treatment group as a weighted sum with always-takers or the screening nonparticipants in the control group as a weighted sum with never-takers. While random assignment mechanism implies their average characteristics will be the same in both the treatment and the control group, we differentiate them by denoting compliers in the treatment group as treated compliers and compliers in the control group as untreated compliers.

To calculate the characteristics of treated compliers, denote the average characteristic of treated compliers as  $g_C^1(y)$ , untreated compliers as  $g_C^0(y)$ , and the screening participants in the treatment group as  $g_T(y)$ . The screening participants in the treatment group are always-takers and treated compliers whose average is given by  $g_T(y) = \frac{\pi_{AT}}{\pi_{AT} + \pi_C} g_{AT}(y) + \frac{\pi_C}{\pi_{AT} + \pi_C} g_C^1(y)$ , where  $\pi_{AT}$  and  $\pi_C$  are share of always-takers and compliers, respectively. Imposing  $age\_even_{it} = 1$  and  $screen_{it} = 1$ , we get  $g_T(y) = \hat{\lambda}_1 + \hat{\lambda}_2 + \hat{\lambda}_3 + \hat{\lambda}_4' f(\text{age}_{it})$ . The share of always-takers and compliers can be calculated from the first stage regression given in (1) as  $\pi_C = \hat{\beta}_1$ ,  $\pi_{AT} = \hat{\beta}_0$  adjusting for age.<sup>59</sup> Inserting all the estimated terms to the equation  $g_C^1(y) = [(\pi_{AT} + \pi_C)g_T(y) - \pi_{AT}g_{AT}(y)]/\pi_C$ , the complier characteristic can be backed out. The average characteristics for untreated compliers can be calculated in a similar way.<sup>60</sup>

To characterize compliers in reference to always-takers and never-takers, I take ratios between treated compliers and always-takers,  $\frac{g_C^1(y)}{g_{AT}(y)}$ , and between untreated compliers and never-takers,  $\frac{g_C^0(y)}{g_{NT}(y)}$ .<sup>61</sup> Standard errors are calculated using bootstrap with 500 replications clustering at the individual level. The null hypothesis used for ratios is that the ratio is equal to one.

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<sup>59</sup>Under monotonicity, the share for never-takers is  $\pi_{NT} = 1 - \pi_{AT} - \pi_C = 1 - \hat{\beta}_0 - \hat{\beta}_1$

<sup>60</sup>I examine selection pattern at age 60 by imposing  $age_{it} = 60$ . The results are robust to the different choices of age.

<sup>61</sup>The reason I differentiate treated and untreated compliers is the possibility that health screening may affect health behaviors. While demographic variables are in general pre-specified and not likely to be affected by health screening, health behaviors such as smoking or drinking can be affected by health screening. Comparing treated compliers with never-takers may be contaminated since treated compliers have received screening while never-takers have not. Same applies to comparison between untreated compliers with always-takers. Therefore, I compare always-takers with treated compliers, both of whom participated in screening, and never-takers with untreated compliers, both of whom did not participate in screening.

## G.2 Health behaviors among female sample

Table 5 in the main text presented selection in health behaviors for men. Table A13 presents the selection results in health behaviors for women. Health behaviors exhibit large differences between men and women, and that is why their selection patterns are examined separately. For example, smoking is primarily a male activity. While both men and women drink, most of the everyday drinkers are men. By splitting the sample, I control for this large difference in their health behaviors. I find consistent pattern as in the male subsample that female compliers are less likely to smoke and more likely to exercise than never-takers.

**Table A13:** Health behaviors among female compliers with subsidies

	(1)	(2)	(3)	(4)	(5)	(6)
	Average value				Ratio	
	Always-takers	Treated compliers	Untreated compliers	Never-takers	$CP_1/AT$	$CP_0/NT$
<b>Panel A. Health behaviors</b>						
Current smoker	0.018 (0.005)	0.017 (0.005)	0.011 (0.005)	0.030 (0.005)	0.921 (0.327)	0.355*** (0.141)
Everyday smoker	0.015 (0.005)	0.017 (0.005)	0.009 (0.005)	0.028 (0.004)	1.086 (0.516)	0.342*** (0.147)
Current drinker	0.526 (0.016)	0.515 (0.017)	0.506 (0.017)	0.492 (0.013)	0.978 (0.029)	1.029 (0.024)
Everyday drinker	0.009 (0.003)	0.010 (0.003)	0.009 (0.003)	0.013 (0.002)	1.075 (0.474)	0.720 (0.269)
Vigorous exercise	0.167 (0.010)	0.147 (0.010)	0.164 (0.011)	0.127 (0.007)	0.879 (0.077)	1.290*** (0.087)
Moderate exercise	0.355 (0.013)	0.355 (0.014)	0.347 (0.016)	0.305 (0.010)	1.001 (0.045)	1.135*** (0.049)
Walking	0.841 (0.010)	0.835 (0.010)	0.826 (0.013)	0.812 (0.008)	0.993 (0.014)	1.017 (0.016)

Notes: This table reports the average values of health behaviors among always-takers, never-takers, treated compliers and untreated compliers, estimated using the survey data. The analytical sample is restricted to female sample. Treated compliers are compliers in the treatment group who participate in screening. Untreated compliers are compliers in the control group who do not participate. The average value is calculated using Equation (5). The null hypotheses used for ratios are  $H_0 : CP_1/AT = 1$  and  $H_0 : CP_0/NT = 1$  for comparison with always-takers and never-takers, respectively, where AT = Always-takers, NT = Never-takers,  $CP_1$  = Treated compliers and  $CP_0$  = Untreated compliers. All the average values and ratios are calculated at age 60. Standard errors are calculated using bootstrap with 500 replications and are clustered at individual level. They are reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

### G.3 Additional analyses on selection

This section provides additional analyses on selection into screenings. Table A14 provides selection into specific medical tests used for screenings. Table A15 provides selection pattern for non-working population.

Table A14 presents the probability of receiving specific medical tests for always-takers and treated compliers. Panel A presents tests covered by the National Health Insurance Service (NHIS). Blood/urine/X-ray tests are common tests in general health screening. Stool test is used in colorectal screening. Endoscopy is used for both stomach and colorectal screenings. Biopsy is used in various screenings, normally as diagnostic tests, for closer examination of tissues. For these covered tests, compliers are more likely or as likely to use them as always-takers.

Panel B presents tests not covered by NHIS. Since these tests are not covered, they are usually paid out-of-pocket by patients.<sup>62</sup> Compliers are less likely to use these tests than always-takers. This finding is consistent with negative selection of compliers in terms of socioeconomic status. It also suggests that compliers' worse health condition, as evidenced by higher probability of finding a disease through screening, is not because compliers receive more medical tests.

Table A15 presents the selection analysis for non-working population. Workplace wellness program that offers annual health screenings are another source of free screenings, and employees who have access to the programs may be categorized as always-takers. Although this does not alter the selection analysis results, it may affect the interpretation that individuals respond to lower prices of screenings. However, Table A15 shows that even among non-working population, compliers have significantly higher incidence of stomach diseases and lower household income, suggesting that our findings are not entirely driven by workplace wellness programs.

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<sup>62</sup>CT is Computed Tomography scan, MRI is Magnetic Resonance Imaging scan, PET is Positron Emission Tomography scan, EEG is Electroencephalogram, and EKG is Electrocardiogram. While bone density test is part of general screening, it is subsidized only for women at age 54 and 66. For male or women at different ages, the test should be paid out-of-pocket.

**Table A14:** Comparing medical tests received

	(1)	(2)	(3)
	Average value		Ratio
	Always-takers	Treated Compliers	$CP_1/AT$
<b>Panel A. Tests covered by NHIS</b>			
Blood/urine/stool/X-ray	0.795 (0.005)	0.885 (0.006)	1.114*** (0.012)
Endoscopy	0.812 (0.006)	0.834 (0.006)	1.027** (0.011)
Biopsy	0.025 (0.002)	0.030 (0.002)	1.181 (0.154)
<b>Panel B. Tests not covered by NHIS</b>			
Sonogram	0.319 (0.007)	0.274 (0.007)	0.857*** (0.031)
CT	0.042 (0.003)	0.016 (0.002)	0.370*** (0.070)
MRI	0.010 (0.001)	0.006 (0.001)	0.608** (0.183)
PET	0.002 (0.001)	-0.000 (0.000)	-0.213*** (0.264)
EEG	0.002 (0.001)	0.002 (0.001)	0.755 (0.586)
EKG	0.159 (0.005)	0.126 (0.005)	0.790*** (0.048)
Bone density	0.034 (0.003)	0.028 (0.003)	0.844 (0.123)

Notes: This table reports the receipt of medical tests among always-takers and treated compliers. Panel A presents tests that are covered by the National Health Insurance Service (NHIS). Blood/urine/stool/X-ray tests are main tests of general screening. Endoscopy is used in stomach and colorectal screenings. Biopsy is used in various screenings, normally as diagnostic tests, for closer examination of tissues. Panel B presents tests that are not covered by the NHIS. These are the tests where screening participants bear the full costs. The average value is calculated using Equation (5). It is not reported for never-takers and untreated compliers, since by definition, they do not receive screening. The null hypotheses used for ratios are  $H_0 : CP_1/AT = 1$  where AT = Always-takers and  $CP_1$  = Treated compliers. Standard errors are calculated using bootstrap with 500 replications and are clustered at individual level. They are reported in parentheses. A \*/\*\*/\*\* indicates significance at the 10/5/1% levels.

**Table A15:** Compliers with subsidies among non-working population

	(1)	(2)	(3)	(4)	(5)	(6)
	Average value				Ratio	
	Always-takers	Treated compliers	Untreated compliers	Never-takers	$CP_1/AT$	$CP_0/NT$
<b>Panel A. Diagnoses</b>						
Stomach	0.171 (0.011)	0.283 (0.012)	-	-	1.654*** (0.144)	-
Breast	0.018 (0.005)	0.027 (0.005)	-	-	1.552 (0.897)	-
Cervical	0.056 (0.009)	0.059 (0.009)	-	-	1.046 (0.294)	-
Colorectal	0.195 (0.019)	0.266 (0.035)	-	-	1.365 (0.287)	-
<b>Panel B. SES</b>						
Individual income	166 (18)	159 (19)	166 (20)	171 (17)	0.958 (0.112)	0.969 (0.099)
Household income	4381 (134)	4131 (131)	4186 (138)	3728 (107)	0.943* (0.031)	1.123*** (0.028)
Years of education	10.363 (0.147)	10.430 (0.154)	10.396 (0.151)	9.652 (0.126)	1.007 (0.015)	1.077*** (0.012)
College graduate	0.128 (0.013)	0.113 (0.013)	0.124 (0.013)	0.096 (0.010)	0.881 (0.109)	1.293*** (0.111)
<b>Panel C. Health behaviors</b>						
Current smoker	0.363 (0.038)	0.309 (0.036)	0.348 (0.040)	0.419 (0.033)	0.849** (0.077)	0.829** (0.069)
Everyday smoker	0.363 (0.037)	0.299 (0.036)	0.347 (0.040)	0.412 (0.032)	0.824** (0.076)	0.843** (0.070)
Current drinker	0.825 (0.035)	0.797 (0.033)	0.774 (0.040)	0.722 (0.028)	0.965 (0.040)	1.073 (0.049)
Everyday drinker	0.148 (0.025)	0.103 (0.024)	0.086 (0.031)	0.132 (0.019)	0.692* (0.166)	0.649* (0.206)
Vigorous exercise	0.287 (0.028)	0.323 (0.029)	0.366 (0.034)	0.241 (0.023)	1.124 (0.118)	1.517*** (0.140)
Moderate exercise	0.515 (0.033)	0.509 (0.031)	0.545 (0.040)	0.398 (0.025)	0.989 (0.072)	1.372*** (0.102)
Walking	0.862 (0.027)	0.866 (0.023)	0.855 (0.038)	0.777 (0.021)	1.004 (0.033)	1.101** (0.050)

Notes: This table reports the average values of screening diagnoses, socioeconomic status, and health behaviors among always-takers, never-takers, treated compliers and untreated compliers. The analytical sample consists of individuals aged 40 to 89 who were not working. Treated compliers are compliers in the treatment group who participate in screening. Untreated compliers are compliers in the control group who do not participate. The average value is calculated using Equation (5). Diagnoses are not reported for untreated compliers and never-takers, since by definition, they do not receive screening. Health behaviors are calculated using the male sample only to account for vast gender differences. The null hypotheses used for ratios are  $H_0 : CP_1/AT = 1$  and  $H_0 : CP_0/NT = 1$  for comparison with always-takers and never-takers, respectively, where AT = Always-takers, NT = Never-takers,  $CP_1$  = Treated compliers and  $CP_0$  = Untreated compliers. All the average values and ratios are calculated at age 60. Standard errors are calculated using bootstrap with 500 replications and are clustered at individual level. They are reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

## Appendix H Cancer diagnosis

This section provides additional analyses on cancer diagnoses. First, Table A16, A17, and A18 present heterogeneous impact of screening on cancer diagnoses across age, sex, and insurance premium amount. Next, I present detailed survival rates for cancer patients. Table A20 and A21 present 1, 3, 5, 7, and 10 year survival rates, separately for those diagnosed in the treatment (even age) and the control (odd age) group.

**Table A16:** Effect on cancer diagnosis by age

	(1)	(2)	(3)	(4)	(5)
	[40,49]	[50,59]	[60,69]	[70,79]	[80,89]
Any cancer	0.000621*** (0.000115)	0.001317*** (0.000179)	0.003306*** (0.000351)	0.005848*** (0.000774)	0.002742 (0.001820)
Control group mean	0.004764	0.008617	0.015742	0.026716	0.032823
% relative to control	13.027	15.280	20.998	21.890	8.354
Stomach cancer	0.000168*** (0.000048)	0.000523*** (0.000088)	0.002035*** (0.000182)	0.003033*** (0.000377)	0.002620*** (0.000754)
Control group mean	0.000783	0.001932	0.003473	0.005540	0.005168
% relative to control	21.422	27.064	58.588	54.750	50.709
Breast cancer	0.000447*** (0.000157)	0.000451** (0.000212)	0.000859*** (0.000315)	0.000417 (0.000510)	0.000516 (0.000678)
Control group mean	0.004202	0.005345	0.004985	0.003804	0.001363
% relative to control	10.644	8.439	17.227	10.966	37.819
Cervical cancer	0.000172* (0.000093)	0.000224** (0.000105)	0.000063 (0.000144)	0.000775*** (0.000275)	0.000467 (0.000467)
Control group mean	0.001717	0.001381	0.001248	0.000889	0.000568
% relative to control	9.997	16.236	5.012	87.182	82.294
Liver cancer	0.000042 (0.000026)	0.000131** (0.000058)	0.000322*** (0.000122)	0.000583** (0.000258)	-0.000817* (0.000493)
Control group mean	0.000245	0.000892	0.001793	0.002961	0.003289
% relative to control	17.239	14.691	17.943	19.682	-24.853
Colorectal cancer	0.000112*** (0.000039)	0.000197** (0.000080)	0.000180 (0.000170)	0.000280 (0.000346)	0.001275 (0.000846)
Control group mean	0.000570	0.001868	0.004068	0.005910	0.006577
% relative to control	19.724	10.572	4.425	4.734	19.383
Lung cancer	-0.000009 (0.000020)	0.000100** (0.000047)	0.000350*** (0.000123)	0.000887*** (0.000315)	0.000299 (0.000724)
Control group mean	0.000184	0.000660	0.002109	0.004907	0.006514
% relative to control	-4.653	15.184	16.585	18.080	4.597
Prostate cancer	-0.000001 (0.000009)	0.000076 (0.000055)	0.000036 (0.000210)	0.001131* (0.000637)	-0.000802 (0.001676)
Control group mean	0.000028	0.000451	0.003431	0.010078	0.015826
% relative to control	-3.715	16.820	1.035	11.224	-5.066

Notes: The table reports the heterogeneous effect of biennial subsidy eligibility on cancer diagnoses across age, using the Customized Cohort Database of national health insurance claims. Any cancer refers to diagnosis of any of the seven cancers listed. Analytical sample includes individuals aged 40–89. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A17:** Effect on cancer diagnosis by sex

	(1)	(2)
	Male	Female
Any cancer	0.001660*** (0.000158)	0.001527*** (0.000171)
Control group mean	0.008957	0.009278
Percent relative to control	18.534	16.460
Stomach cancer	0.001010*** (0.000089)	0.000497*** (0.000060)
Control group mean	0.002589	0.001042
Percent relative to control	39.033	47.670
Breast cancer		0.000509*** (0.000126)
Control group mean		0.004663
Percent relative to control		10.918
Cervical cancer		0.000196*** (0.000065)
Control group mean		0.001503
Percent relative to control		13.073
Liver cancer	0.000180*** (0.000065)	0.000084** (0.000033)
Control group mean	0.001321	0.000350
Percent relative to control	13.626	24.030
Colorectal cancer	0.000198** (0.000079)	0.000161*** (0.000059)
Control group mean	0.002448	0.001212
Percent relative to control	8.100	13.307
Lung cancer	0.000162*** (0.000054)	0.000107** (0.000043)
Control group mean	0.001216	0.000616
Percent relative to control	13.307	17.428
Prostate cancer	0.000099 (0.000071)	
Control group mean	0.001556	
Percent relative to control	6.341	

Notes: The table reports the heterogeneous effect of biennial subsidy eligibility on cancer diagnoses by sex, using the Customized Cohort Database of national health insurance claims. Any cancer refers to diagnosis of any of the seven cancers listed. Analytical sample includes individuals aged 40–89. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A18:** Effect on cancer diagnosis by insurance premium

	(1)	(2)
	Below median	Above median
Any cancer	0.001216*** (0.000200)	0.000884*** (0.000191)
Control group mean	0.007094	0.005932
Percent relative to control	17.140	14.905
Stomach cancer	0.000592*** (0.000096)	0.000505*** (0.000105)
Control group mean	0.001444	0.001575
Percent relative to control	41.017	32.048
Breast cancer	0.000429** (0.000191)	0.000801* (0.000422)
Control group mean	0.003536	0.005384
Percent relative to control	12.130	14.879
Cervical cancer	0.000288** (0.000117)	0.000296 (0.000199)
Control group mean	0.001333	0.001375
Percent relative to control	21.586	21.568
Liver cancer	0.000111* (0.000059)	0.000108 (0.000068)
Control group mean	0.000592	0.000618
Percent relative to control	18.812	17.397
Colorectal cancer	0.000004 (0.000084)	0.000147* (0.000086)
Control group mean	0.001437	0.001340
Percent relative to control	0.279	10.974
Lung cancer	0.000144*** (0.000051)	-0.000027 (0.000056)
Control group mean	0.000505	0.000578
Percent relative to control	28.566	-4.627
Prostate cancer	0.000001 (0.000136)	-0.000039 (0.000081)
Control group mean	0.001255	0.000819
Percent relative to control	0.068	-4.751

Notes: The table reports the heterogeneous effect of biennial subsidy eligibility on cancer diagnoses by insurance premium, using the Customized Cohort Database of national health insurance claims. Any cancer refers to diagnosis of any of the seven cancers listed. Analytical sample includes individuals aged 40–89. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\* indicates significance at the 10/5/1% levels.

**Table A19:** Effect on cancer diagnosis by disability status

	(1)	(2)
var	Without disability	With disability
Any cancer	0.001533*** (0.000118)	0.002603*** (0.000571)
Control group mean	0.008754	0.014529
Percent relative to control	17.512	17.914
Stomach cancer	0.000752*** (0.000056)	0.001025*** (0.000269)
Control group mean	0.001788	0.003021
Percent relative to control	42.078	33.944
Breast cancer	0.000503*** (0.000128)	0.000657 (0.000619)
Control group mean	0.004685	0.004116
Percent relative to control	10.748	15.954
Cervical cancer	0.000182*** (0.000067)	0.000569* (0.000309)
Control group mean	0.001510	0.001304
Percent relative to control	12.063	43.637
Liver cancer	0.000122*** (0.000036)	0.000339 (0.000231)
Control group mean	0.000769	0.002353
Percent relative to control	15.827	14.396
Colorectal cancer	0.000163*** (0.000050)	0.000454 (0.000292)
Control group mean	0.001743	0.003796
Percent relative to control	9.362	11.948
Lung cancer	0.000111*** (0.000035)	0.000530*** (0.000203)
Control group mean	0.000864	0.002012
Percent relative to control	12.809	26.358
Prostate cancer	0.000121* (0.000071)	-0.000149 (0.000314)
Control group mean	0.001431	0.002947
Percent relative to control	8.434	-5.044

Notes: The table reports the heterogeneous effect of biennial subsidy eligibility on cancer diagnoses by disability status, using the Customized Cohort Database of national health insurance claims. Any cancer refers to diagnosis of any of the seven cancers listed. The analytical sample includes individuals aged 40–89. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A20:** Survival rate comparisons

	(1)	(2)	(3)	(4)
	Control group cancer diagnoses	Treatment - Control	Percent relative to control group diagnoses	N
<b>Panel A. Any cancer</b>				
1 year survival rate	0.897	0.008*** (0.002)	0.898	72,744
3 year survival rate	0.837	0.012*** (0.003)	1.404	65,667
5 year survival rate	0.801	0.012*** (0.004)	1.472	52,962
7 year survival rate	0.776	0.009 (0.005)	1.124	41,682
10 year survival rate	0.760	0.001 (0.007)	0.134	27,390
<b>Panel B. Stomach cancer</b>				
1 year survival rate	0.918	0.015*** (0.004)	1.583	16,500
3 year survival rate	0.873	0.017*** (0.006)	1.957	15,225
5 year survival rate	0.843	0.020** (0.008)	2.321	12,697
7 year survival rate	0.813	0.020** (0.010)	2.488	10,302
10 year survival rate	0.791	0.014 (0.013)	1.734	6,937
<b>Panel C. Breast cancer</b>				
1 year survival rate	0.964	0.005* (0.003)	0.486	17,037
3 year survival rate	0.929	0.012** (0.005)	1.261	15,284
5 year survival rate	0.901	0.017** (0.008)	1.843	12,183
7 year survival rate	0.877	0.017* (0.010)	1.960	9,523
10 year survival rate	0.852	0.015 (0.013)	1.726	6,440
<b>Panel D. Cervical cancer</b>				
1 year survival rate	0.969	0.002 (0.005)	0.173	5,608
3 year survival rate	0.944	0.012 (0.008)	1.262	5,186
5 year survival rate	0.927	0.019* (0.010)	2.033	4,475
7 year survival rate	0.918	0.021* (0.012)	2.330	3,800
10 year survival rate	0.905	0.024 (0.015)	2.638	2,871

Notes: This table compares the survival rates of cancers diagnosed at even ages with the ones diagnosed at odd ages. The sample consists of cancer diagnosis made at age in [40, 89]. Column 1 reports the survival rates of the cancers diagnosed at odd ages. Column 2 reports the difference in survival rates of the cancers diagnosed at even ages compared to the ones diagnosed at odd ages controlling for age. Column 3 reports the relative size of the difference (column 2) in percentage compared to the odd group mean (column 1). Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A21:** Survival rate comparisons

	(1)	(2)	(3)	(4)
	Control group cancer diagnoses	Treatment - Control	Percent relative to control group diagnoses	N
<b>Panel E. Liver cancer</b>				
1 year survival rate	0.729	0.028** (0.011)	3.789	6,816
3 year survival rate	0.611	0.029* (0.016)	4.813	6,184
5 year survival rate	0.537	0.030 (0.019)	5.659	4,991
7 year survival rate	0.498	0.013 (0.023)	2.669	3,912
10 year survival rate	0.497	-0.039 (0.029)	-7.769	2,405
<b>Panel F. Colorectal cancer</b>				
1 year survival rate	0.919	0.006 (0.004)	0.633	14,332
3 year survival rate	0.859	0.012 (0.007)	1.400	12,960
5 year survival rate	0.826	0.009 (0.010)	1.146	10,563
7 year survival rate	0.802	0.006 (0.012)	0.696	8,388
10 year survival rate	0.782	-0.007 (0.015)	-0.884	5,546
<b>Panel G. Lung cancer</b>				
1 year survival rate	0.699	-0.003 (0.011)	-0.449	7,318
3 year survival rate	0.566	-0.011 (0.016)	-1.894	6,378
5 year survival rate	0.496	-0.026 (0.019)	-5.315	4,872
7 year survival rate	0.459	-0.038* (0.023)	-8.261	3,544
10 year survival rate	0.412	-0.028 (0.030)	-6.771	2,015
<b>Panel H. Prostate cancer</b>				
1 year survival rate	0.942	-0.002 (0.006)	-0.183	6,207
3 year survival rate	0.879	-0.008 (0.011)	-0.861	5,403
5 year survival rate	0.820	-0.007 (0.016)	-0.804	3,935
7 year survival rate	0.770	-0.015 (0.022)	-1.926	2,796
10 year survival rate	0.703	-0.015 (0.031)	-2.198	1,527

Notes: This table compares the survival rates of cancers diagnosed at even ages with the ones diagnosed at odd ages. The sample consists of cancer diagnosis made at age in [40, 89]. Column 1 reports the survival rates of the cancers diagnosed at odd ages. Column 2 reports the difference in survival rates of the cancers diagnosed at even ages compared to the ones diagnosed at odd ages controlling for age. Column 3 reports the relative size of the difference (column 2) in percentage compared to the odd group mean (column 1). Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

## Appendix I Treatment of non-cancer diseases

**Table A22:** Effect on high blood pressure medicine prescription

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
High blood pressure	0.23884	0.00046*** (0.00013)	0.193	8,673,954
ARBs (Angiotensin II Receptor Blockers)	0.10179	0.00038*** (0.00009)	0.378	8,673,954
Calcium Channel Blockers (CCBs)	0.09779	0.00021** (0.00009)	0.218	8,673,954
Diuretics (Water Pills)	0.07136	-0.00003 (0.00010)	-0.046	8,673,954
Beta-Blockers	0.04289	0.00005 (0.00008)	0.121	8,673,954
ACE Inhibitors	0.01357	-0.00003 (0.00004)	-0.233	8,673,954
Aldosterone Antagonists	0.00970	-0.00007 (0.00004)	-0.705	8,673,954
Alpha-Blockers	0.00768	0.00001 (0.00004)	0.109	8,673,954

Notes: This table reports the treatment effect of biennial health screening subsidies on high blood pressure medicine prescription. The sample consists of individuals with age in [40, 89]. Column 1 reports the mean of the odd age (control) group. Column 2 ITT estimates report the effect of biennial subsidies on outcome variables by comparing even with odd age groups controlling for age. Column 3 reports the relative size of the ITT effect (column 2) in percentage compared to the control group mean (column 1). Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A23:** Effect on diabetes medicine prescription

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
Diabetes	0.08918	0.00015** (0.00006)	0.171	8,673,954
Biguanides	0.06832	0.00011* (0.00007)	0.165	8,673,954
Sulfonylureas	0.04318	-0.00006 (0.00006)	-0.139	8,673,954
DPP-4 Inhibitors	0.01836	0.00005 (0.00004)	0.298	8,673,954
Thiazolidinediones (TZDs)	0.00671	-0.00002 (0.00003)	-0.299	8,673,954
Alpha-Glucosidase Inhibitors	0.00432	0.00001 (0.00002)	0.195	8,673,954
SGLT2 Inhibitors	0.00202	-0.00000 (0.00002)	-0.204	8,673,954
Meglitinides	0.00174	-0.00001 (0.00002)	-0.303	8,673,954
Insulin (Various Types)	0.00165	0.00001 (0.00001)	0.615	8,673,954
GLP-1 Receptor Agonists	0.00014	0.00000 (0.00001)	3.127	8,673,954

Notes: This table reports the treatment effect of biennial health screening subsidies on diabetes medicine prescription. The sample consists of individuals with age in [40, 89]. Column 1 reports the mean of the odd age (control) group. Column 2 ITT estimates report the effect of biennial subsidies on outcome variables by comparing even with odd age groups controlling for age. Column 3 reports the relative size of the ITT effect (column 2) in percentage compared to the control group mean (column 1). Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A24:** Effect on high cholesterol medicine prescription

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
High cholesterol	0.13901	0.00419*** (0.00011)	3.014	8,673,954
Statins	0.13359	0.00405*** (0.00011)	3.033	8,673,954
Fibrates	0.00752	0.00016*** (0.00004)	2.076	8,673,954
Ezetimibe	0.00194	0.00006*** (0.00002)	3.131	8,673,954

Notes: This table reports the treatment effect of biennial health screening subsidies on high cholesterol medicine prescription. The sample consists of individuals with age in [40, 89]. Column 1 reports the mean of the odd age (control) group. Column 2 ITT estimates report the effect of biennial subsidies on outcome variables by comparing even with odd age groups controlling for age. Column 3 reports the relative size of the ITT effect (column 2) in percentage compared to the control group mean (column 1). Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A25:** Effect on medicine prescription by age group

	(1)	(2)	(3)	(4)	(5)
	[40, 49]	[50, 59]	[60, 69]	[70, 79]	[80, 89]
Helicobacter pylori	0.000037*** (0.000013)	0.000024 (0.000017)	0.000051** (0.000024)	-0.000002 (0.000029)	-0.000063 (0.000049)
Control group mean	0.000118	0.000174	0.000220	0.000227	0.000206
% relative to control	31.285	13.652	23.290	-0.688	-30.725
Polypectomy	0.003481*** (0.000100)	0.008355*** (0.000172)	0.012820*** (0.000247)	0.008191*** (0.000280)	0.001993*** (0.000296)
Control group mean	0.006066	0.014344	0.018002	0.014954	0.006117
% relative to control	57.392	58.250	71.212	54.778	32.577
High blood pressure	-0.000254 (0.000178)	0.000486* (0.000250)	0.001083*** (0.000361)	0.001527*** (0.000501)	-0.000001 (0.000913)
Control group mean	0.090684	0.216549	0.364081	0.486891	0.523951
% relative to control	-0.280	0.224	0.297	0.314	-0.000
Diabetes	-0.000012 (0.000079)	0.000283** (0.000124)	0.000387** (0.000191)	0.000360 (0.000270)	-0.000144 (0.000497)
Control group mean	0.032267	0.083299	0.143012	0.180062	0.159770
% relative to control	-0.037	0.340	0.271	0.200	-0.090
High cholesterol	0.001293*** (0.000138)	0.005234*** (0.000229)	0.007147*** (0.000322)	0.007085*** (0.000411)	0.000838 (0.000695)
Control group mean	0.048579	0.142428	0.226165	0.254385	0.228404
% relative to control	2.663	3.675	3.160	2.785	0.367
Tuberculosis	-0.000020 (0.000028)	-0.000052 (0.000036)	0.000052 (0.000053)	0.000082 (0.000083)	0.000041 (0.000168)
Control group mean	0.001272	0.001743	0.002294	0.003452	0.004206
% relative to control	-1.610	-2.962	2.269	2.363	0.963
Osteoporosis	-0.000012 (0.000030)	0.001069*** (0.000097)	0.003977*** (0.000216)	0.001180*** (0.000360)	0.000357 (0.000629)
Control group mean	0.001557	0.014120	0.047748	0.085153	0.081273
% relative to control	-0.790	7.573	8.328	1.385	0.439

Notes: This table reports the treatment effect of biennial health screening subsidies on medicine prescription by age group. The sample consists of individuals with age in [40, 89]. Column 2-6 reports the ITT estimates for each age range. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A26:** Effect on medicine prescription by sex

	(1)	(2)
	Male	Female
Helicobacter pylori	0.000030** (0.000013)	0.000022* (0.000012)
Control group mean	0.000165	0.026383
Percent relative to control	17.892	12.790
Polypectomy	0.009000*** (0.000142)	0.005596*** (0.000098)
Control group mean	0.016088	0.026383
Percent relative to control	55.941	72.646
High blood pressure	0.000622*** (0.000180)	0.000309 (0.000190)
Control group mean	0.224382	0.026383
Percent relative to control	0.277	0.122
Diabetes	-0.000099 (0.000100)	0.000394*** (0.000083)
Control group mean	0.098233	0.026383
Percent relative to control	-0.101	0.488
High cholesterol	0.003869*** (0.000155)	0.004485*** (0.000160)
Control group mean	0.125866	0.026383
Percent relative to control	3.074	2.965
Tuberculosis	0.000007 (0.000030)	0.000011 (0.000026)
Control group mean	0.002286	0.026383
Percent relative to control	0.294	0.687
Osteoporosis	0.000914*** (0.000044)	0.001182*** (0.000122)
Control group mean	0.004155	0.026383
Percent relative to control	22.005	2.538

Notes: This table reports the treatment effect of biennial health screening subsidies on medicine prescription by sex. The sample consists of individuals with age in [40, 89]. Column 2-3 reports the ITT estimates for male and female. Standard errors are clustered at the individual level and reported in parentheses. A \*/\*\*/\*\* indicates significance at the 10/5/1% levels.

## Appendix J Regression discontinuity analysis using age cutoffs

This section examines screening take-up, selection into screening, and causal effects of screenings around the subsidy eligibility age cutoffs using a regression discontinuity (RD) design. Subsidy eligibility begins at age 40 for most screenings, with exceptions at age 30 for cervical and 50 for colorectal screenings. Since these age thresholds coincide with clinical guidelines for screening initiation, the estimated effects capture the combined influence of subsidies and clinical recommendations.

Figure 1 in the main text presents screening rates for eight types of screenings across ages. Consistent with the introduction of subsidies at age 40, general, stomach, breast, and liver screenings show sharp increases in take-up at that threshold. While take-up rises substantially at even ages, rates at odd ages remain relatively stable. Cervical screening exhibits a modest increase at age 30, when subsidies first become available, but a much larger jump at age 40, particularly at even ages. Similarly, colorectal screening shows clear discontinuities at both ages 40 and 50 among even ages. By contrast, unsubsidized lung and prostate screenings display no visible discontinuities at age 40.

To formally estimate these discontinuities, I employ the following regression discontinuity specification.

$$\begin{aligned}
 y_{it} = & \alpha_0 + \alpha_1 k_{it} + \alpha_2 \mathbb{1}\{k_{it} \geq 0\} + \alpha_3 \text{age\_even}_{it} + \alpha_4 k_{it} \times \mathbb{1}\{k_{it} \geq 0\} + \alpha_5 k_{it} \times \text{age\_even}_{it} \\
 & + \alpha_6 \text{age\_even}_{it} \times \mathbb{1}\{k_{it} \geq 0\} + \alpha_7 k_{it} \times \text{age\_even}_{it} \times \mathbb{1}\{k_{it} \geq 0\} + \varepsilon_{it}
 \end{aligned} \tag{6}$$

The running variable is the centered age,  $k_{it} = (\text{age}_{it} - \text{cutoff})$ , where the cutoff equals 30 for cervical screening, 50 for colorectal screening, and 40 for all other screenings. The variable  $\text{age\_even}_{it}$  is a dummy equal to one if individual  $i$  turns an even-numbered age in year  $t$ . The specification is a fully saturated model of the centered age variable  $k_{it}$ , the even-age indicator  $\text{age\_even}_{it}$ , and the post-cutoff indicator  $\mathbb{1}\{k_{it} \geq 0\}$ .

This specification allows me to estimate four coefficients of interest: the baseline

take-up rates immediately before the cutoff for even- and odd-aged individuals, and the discontinuous increases in take-up at the cutoff for each group. Specifically, the pre-cutoff take-up is given by  $\alpha_0$  for odd ages and  $\alpha_0 + \alpha_3$  for even ages. The discontinuous increase at the cutoff is given by  $\alpha_2$  for odd ages and by  $\alpha_2 + \alpha_6$  for even ages.

Table A27 presents the estimation results. Panel A focuses on odd-aged individuals, who are not eligible for subsidies after the cutoff. Despite the lack of subsidies, screening take-up modestly increases at the cutoff for stomach, breast, and cervical screenings. A plausible explanation is the effect of clinical guidelines: recommendation to start screening at these ages can lead to overall increase in take-up at both even and odd ages. For annually subsidized liver and colorectal screenings, increase at odd ages is expected, since subsidies are provided every year. I find a significant increase at odd ages for colorectal screening, but not for liver screening. This could be because liver screening is subsidized only for a limited group of high-risk individuals.

Panel B examines the discontinuous change in take-up for even-aged individuals, eligible for subsidies after the cutoff. Consistent with subsidy eligibility, even-age take-up increases significantly at the cutoffs. Notably, stomach screening take-up increases by 18.5 percentage points (286%) and breast screening take-up by 18.9 percentage points (230%) at the age 40 cutoff. Overall, the results confirm substantial discontinuous increase at the subsidy eligibility thresholds.

As shown in Figure 1d and 1f, cervical and colorectal screenings further display large increase in take-up at age 40, despite their respective subsidies beginning at age 30 and 50. Table A28, using the same specification as Equation (6), quantifies these spillovers. Even-age take-up rises by 11.4 percentage points (82%) for cervical screenings and 2.3 percentage points (139%) for colorectal screenings at age 40. These findings suggest that participation in one screening influences participation in others, consistent with cross-screening spillover effects.

Next, I examine selection into screening at age 40 cutoff. Compliers at the cutoff are individuals who do not get screening before age 40 but start to participate at age 40. This captures the combined effects of subsidies and clinical recommendation. I employ

the same methodology explained in Appendix Section G, adapted here to an regression discontinuity framework.

$$y_{it} = \eta_0 + \eta_1 k_{it} + \eta_2 \mathbb{1}\{k_{it} \geq 0\} + \eta_3 screen_{it} + \eta_4 k_{it} \times \mathbb{1}\{k_{it} \geq 0\} + \eta_5 k_{it} \times screen_{it} \\ + \eta_6 screen_{it} \times \mathbb{1}\{k_{it} \geq 0\} + \eta_7 k_{it} \times screen_{it} \times \mathbb{1}\{k_{it} \geq 0\} + \varepsilon_{it} \quad (7)$$

It is a fully saturated model of three variables: (i) the centered age running variable  $k_{it} = (age_{it} - 40)$ , (ii) the post-cutoff indicator  $\mathbb{1}\{k_{it} \geq 0\}$ , and (iii) the screening take-up indicator  $screen_{it}$ . The analytical sample is restricted to individuals aged 30-39 and even-aged individuals aged 40-48. Odd-aged individuals above age 40 are excluded to focus on groups eligible for subsidies. Always-takers start to participate even before age 40, so their characteristics can be estimated by setting  $\mathbb{1}\{k_{it} = 1\}$  and  $screen_{it} = 1$ . Never-takers do not participate even after age 40, so their characteristics can be estimated by plugging in  $\mathbb{1}\{k_{it} = 1\}$  and  $screen_{it} = 0$ . Treated and untreated compliers' characteristics can be backed out by examining individuals who participate after age 40 and those who do not participate before age 40, respectively. Since we estimate everything at age 40 cutoff, we let  $k_{it} = 0$ .

Table A29 reports the results. Compliers are more likely to be diagnosed with a disease, indicating poorer health. They are also less likely to be employed, with correspondingly lower individual and household incomes. I do not find any statistically significant differences in health behaviors. While these patterns are consistent with the selection pattern derived from even/odd age-based variation,<sup>63</sup> many estimates here are imprecisely estimated, potentially due to the coarseness of the running variable. This underscores the value of even/odd age-based eligibility as an alternative and sharper source of exogenous variation.

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<sup>63</sup>Unlike the main findings, compliers are less likely to be diagnosed with diseases of the female genitalia. This likely reflects that cervical screening has been subsidized since age 30, so higher-risk women participate earlier, leaving relatively lower-risk women to begin at age 40.

**Table A27:** Effect of subsidies on take-up using age cutoff

	(1)	(2)	(3)	(4)	(5)	(6)
	Biennial			Annual		
	General	Stomach	Breast	Cervical	Liver	Colorectal
<b>Panel A. Odd age group</b>						
Pre-cutoff take-up	0.088*** (0.005)	0.061*** (0.004)	0.052*** (0.005)	0.023*** (0.004)	0.026*** (0.003)	0.027*** (0.003)
Increase at age cutoff	0.011 (0.007)	0.016*** (0.006)	0.028*** (0.008)	0.018*** (0.007)	0.004 (0.004)	0.014*** (0.004)
<b>Panel B. Even age group</b>						
Pre-cutoff take-up	0.108*** (0.006)	0.065*** (0.005)	0.082*** (0.007)	0.034*** (0.005)	0.023*** (0.003)	0.047*** (0.004)
Increase at age cutoff	0.154*** (0.009)	0.185*** (0.008)	0.189*** (0.011)	0.023*** (0.008)	0.023*** (0.004)	0.025*** (0.005)
N	54,642	54,642	27,714	22,381	54,642	57,655
Adj $R^2$	0.054	0.074	0.089	0.030	0.007	0.008
Even group % increase	143	286	230	67	103	53
Sample age range	[30, 49]	[30, 49]	[30, 49]	[20, 39]	[30, 49]	[40, 59]
Subsidy starting age	40	40	40	30	40	50

Notes: This table reports the effect of subsidies on the take-up of 6 types of subsidized screenings using regression discontinuity design. 10 years before and after the subsidy starting age are used as analytical sample. Econometric specification is given in Equation (6). Pre-40 group take-up measures the baseline take-up right before the threshold, given as  $\alpha_0$  for the odd-aged individuals and  $\alpha_0 + \alpha_3$  for the even-aged individuals in the specification. Increase at age 40 measures the discontinuous change in take-up at the age cutoff, given as  $\alpha_2$  for the odd-aged individuals and  $\alpha_2 + \alpha_6$  for the even-aged individuals in the specification. The percentage increase refers to the relative size of the discontinuous increase at the cutoff compared to the baseline level among even-aged individuals.. Standard errors are clustered at the individual and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A28:** Cross spillover due to different age cutoffs

	(1)	(2)
	Cervical	Colorectal
<b>Panel A. Odd age group</b>		
Pre-40 take-up	0.070*** (0.006)	0.019*** (0.002)
Increase at age 40	0.003 (0.008)	0.001 (0.003)
<b>Panel B. Even age group</b>		
Pre-40 take-up	0.138*** (0.009)	0.017*** (0.002)
Increase at age 40	0.114*** (0.012)	0.023*** (0.004)
N	27,714	54,642
Adj $R^2$	0.062	0.008
Even group % increase	82	139
Sample age range	[30, 49]	[30, 49]
Subsidy starting age	40	40

Notes: This table reports the effect of subsidies on the take-up of cervical and colorectal screenings using regression discontinuity design at age 40. 10 years before and after the subsidy starting age are used as analytical sample. Econometric specification is given in Equation (6). Pre-40 group take-up measures the baseline take-up right before the threshold, given as  $\alpha_0$  for the odd-aged individuals and  $\alpha_0 + \alpha_3$  for the even-aged individuals in the specification. Increase at age 40 measures the discontinuous change in take-up at the age cutoff, given as  $\alpha_2$  for the odd-aged individuals and  $\alpha_2 + \alpha_6$  for the even-aged individuals in the specification. The percentage increase refers to the relative size of the discontinuous increase at the cutoff compared to the baseline level among even-aged individuals.. Standard errors are clustered at the individual and reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.

**Table A29:** Compliers with subsidies using age 40 cutoff

	(1)	(2)	(3)	(4)	(5)	(6)
	Average value				Ratio	
	Always-takers	Treated compliers	Untreated compliers	Never-takers	$CP_1/AT$	$CP_0/NT$
<b>Panel A. Diagnoses</b>						
Stomach	0.132 (0.021)	0.288 (0.018)	-	-	2.190*** (0.462)	-
Breast	0.018 (0.012)	0.044 (0.010)	-	-	2.507 (750.215)	-
Cervical	0.167 (0.024)	0.067 (0.023)	-	-	0.398*** (0.179)	-
Colorectal	0.161 (0.039)	0.213 (0.058)	-	-	1.320 (0.695)	-
<b>Panel B. SES</b>						
Individual income	2624 (99)	1923 (116)	1667 (188)	2263 (45)	0.733*** (0.059)	0.737*** (0.090)
Household income	5884 (127)	5174 (139)	4018 (276)	4740 (57)	0.879*** (0.035)	0.848** (0.063)
Years of education	14.138 (0.102)	14.039 (0.116)	13.549 (0.218)	13.642 (0.051)	0.993 (0.012)	0.993 (0.018)
College graduate	0.388 (0.023)	0.413 (0.025)	0.281 (0.045)	0.323 (0.010)	1.064 (0.102)	0.870 (0.150)
Working status	0.765 (0.019)	0.691 (0.023)	0.615 (0.042)	0.766 (0.008)	0.902** (0.044)	0.803*** (0.058)
<b>Panel C. Health behaviors</b>						
Current smoker	0.467 (0.038)	0.442 (0.048)	0.714 (0.093)	0.573 (0.015)	0.947 (0.158)	1.247 (0.174)
Everyday smoker	0.446 (0.038)	0.410 (0.048)	0.681 (0.092)	0.545 (0.015)	0.920 (0.163)	1.249 (0.182)
Current drinker	0.892 (0.022)	0.888 (0.031)	0.833 (0.059)	0.903 (0.009)	0.996 (0.051)	0.923 (0.069)
Everyday drinker	0.074 (0.015)	0.084 (0.026)	0.106 (0.050)	0.075 (0.008)	1.144 (0.613)	1.407 (0.770)
Vigorous exercise	0.420 (0.034)	0.389 (0.047)	0.255 (0.088)	0.355 (0.013)	0.924 (0.164)	0.719 (0.265)
Moderate exercise	0.549 (0.034)	0.532 (0.046)	0.406 (0.095)	0.462 (0.013)	0.969 (0.133)	0.880 (0.220)
Walking	0.737 (0.029)	0.746 (0.042)	0.808 (0.087)	0.729 (0.012)	1.011 (0.087)	1.108 (0.131)

Notes: This table reports the average values of screening diagnoses, socioeconomic status, and health behaviors among always-takers, never-takers, treated compliers and untreated compliers. Compliance groups are defined using the age 40 cutoff. Analytical sample consists of individuals aged 30 to 39 and even-aged individuals aged 40 to 48. Treated compliers are compliers in the treatment group who participate in screening. Untreated compliers are compliers in the control group who do not participate. The average value is calculated using Equation (7), adapted to the regression discontinuity design specification. Diagnoses are not reported for untreated compliers and never-takers, since by definition, they do not receive screening. The null hypotheses used for ratios are  $H_0 : CP_1/AT = 1$  and  $H_0 : CP_0/NT = 1$  for comparison with always-takers and never-takers, respectively, where AT = Always-takers, NT = Never-takers,  $CP_1$  = Treated compliers and  $CP_0$  = Untreated compliers. Standard errors are calculated using bootstrap with replications and are clustered at individual level. They are reported in parentheses. A \*/\*\*/\*\*\* indicates significance at the 10/5/1% levels.