Health screening and selection: Evidence from biennial subsidies in South Korea*

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Individuals from lower socioeconomic backgrounds show larger disease burdens but lower health screening rates. This study examines the role of subsidies in attracting high risk participants from disadvantaged backgrounds using National Health Screening Program from South Korea that provides biennial subsidies for general and cancer screenings at even ages. Comparing even and odd age groups, I find subsidies increase participation in both subsidized and unsubsidized screenings, as well as the spouse's participation. Importantly, subsidies attract those with lower income and education levels who face higher disease risks. This pattern is observed not only among compliers with direct subsidy, but also among compliers in spillovers across screenings and between spouses. Finally, subsidies lead to more cancer diagnoses at even ages, with patients diagnosed at even ages having earlier stage cancers and higher survival rates.

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1 Introduction

Cancers and heart diseases are the two leading causes of death in many developed countries. These conditions often develop silently, without acute symptoms in their early stages, but become increasingly difficult to treat once advanced. The primary goal of health screenings is to detect these diseases at an early stage, when treatment is most effective, thereby reducing premature deaths and suffering. According to Cutler (2008), the early detection and treatment of cancers through screenings made the most important and cost-effective contribution in the war on cancer since 1990 in the United States.

Previous studies on health screenings focused on two key issues, both closely tied to the selection problem in screening. First, those most likely to benefit from screenings are often least likely to participate. Socioeconomic disparities in disease burden and screening take-up are well documented, with individuals of lower income and education levels bearing a disproportionate burden of heart diseases and cancers but participating in screenings at much lower rates (Pill et al., 1988; Waller et al., 1990; Strandberg et al., 1995; Bender et al., 2015; Jones et al., 2019; Einav et al., 2020). Second, health screenings can cause potential harms, including false positives, overdiagnosis and overtreatment.[1] These issues are particularly pronounced among low-risk healthy participants, who often have high screening take-up rates (Stewart-Brown and Farmer, 1997; Rubin, 2019; Kowalski, 2023). The uneven distribution of benefits, harms, and take-up rates underscores the importance of understanding selection into screening when designing screening recommendations.

This paper investigates selection into screening across various margins in the context of National Health Screening Program (NHSP) from South Korea that provides screening subsidies every other year ("biennial"). Specifically, this study addresses three research questions. First, what is the impact of biennial subsidies on screening take-up? Second, who responds to these subsidies and participate? Lastly, what is the impact of screenings on disease diagnoses, mortality and health care utilization?

[1] There are also downsides for the health care system as a whole in terms of resource allocation. Medical resources spent on screening low-risk healthy individuals could be potentially better used elsewhere. Annual physical is the most common reason that US patients seek care and it could potentially crowd out visits for more urgent care (Mehrotra and Prochazka, 2015).

To answer these questions, this study uses the research design that exploits biennial subsidies provided when one's age is even-numbered.[2] The South Korean National Health Insurance Service (NHIS) subsidizes 90 - 100 percent of the costs for the general health screening that assesses risk factors for chronic diseases such as high blood pressure, diabetes and high cholesterol, and 5 types of cancer screenings: stomach, breast, cervical, liver and colorectal screenings. General, stomach, breast and cervical screenings are subsidized biennially when one's age is even. The subsidies are provided after an age cutoff, usually from age 40. I show that the even-odd subsidy rule is random conditional on age and creates large variation in take-up between the even age group (eligible for subsidies) and the odd age group (ineligible for subsidies).[3]

A unique feature of this research design is that the experiments are repeated and everyone is ultimately eligible for subsidized screenings. Every year, treatment and control groups switch and a new experiment is conducted. This creates an incentive to reallocate screenings intertemporally such that people shift the screening timing from odd to even ages to be eligible for subsidies. This widens the take-up gap between the even and odd age groups without any net increase in take-up. Hence, the difference in take-up between even and odd age groups reflects both the net increase in screening participation due to subsidies and the effect of intertemporal substitution.

This study uses two complementary datasets; (i) Korean Health Panel Study Data, a nationally representative survey dataset that captures health screening take-up and (ii) National health insurance claims data, an administrative dataset that captures cancer diagnoses and deaths.

The survey data are annual, individual level survey data from 2008 to 2018, with an initial sample of around 7,000 households and 21,300 individuals. Despite yearly data collection, it includes visit-level health care usage information including detailed screening

[2]In this study, "age" refers to calendar age, calculated as the difference between the current year and the birth year. The policy also uses calendar age. Individuals are eligible for subsidies in a calendar year, from January 1 to December 31, when their calendar age is even-numbered. The policy rule verbatim states that those born in an even-numbered year are eligible for subsidies in an even-numbered year and those born in an odd-numbered year are eligible for subsidies in an odd-numbered year. This is equivalent to the rule that one is eligible for subsidies during a calendar year when one's age is an even number.

[3]I condition on age, since the two groups are never of the same age.

records.[4] This enables me to observe the exact date, type and the results of screenings. The survey data are used to analyze the impact of subsidies on screening take-up and selection into screening. However, due to its small size, it is not used for statistical analyses of rare outcomes such as cancer incidence and mortality.

The administrative data are insurance claims data from the NHIS, with the sample of about 640,000 randomly drawn individuals in the first year spanning 2002 to 2021. It provides comprehensive information on health care utilization, including data from the Coinsurance Reduction Program for Rare and Severe Diseases, which offers reduced coinsurance rates for conditions such as cancer. This allows me to observe all cancer diagnoses, regardless of whether they were identified through screenings. Leveraging the big size of the dataset, the administrative dataset is used to conduct statistical analyses on cancer incidences, mortality and health care utilization.[5]

This study presents three main findings. First, biennial screening subsidies significantly increase take-up at even ages and generate spillover across different types of screenings and between spouses. the take-up rate for stomach screenings increases from 8.3 percent at odd ages to 27.3 percent at even ages, a 19 percentage point increase. Screenings not directly subsidized also show larger take-up at even ages. Liver and colorectal screenings were subsidized every year and lung and prostate screenings were not subsidized. Despite having no reason to show systematic difference in take-up between even and odd ages, colorectal screening shows 3.3 percentage point increase in take-up at even ages compared to 2.7 percent at odd ages, and lung screening shows 0.6 percentage point increase compared to 0.9 percent at odd ages. This cross spillover is mainly driven by the tendency to receive multiple screenings on the same day. Examining married couples, I find that subsidies increase not only one's own take-up but also the spouse's take-up as well. Spouse's subsidy eligibility increases the partner's take-up by 1.6 percentage point. Instrumenting spouse's screening take-up with their subsidy eligibility

[4]Visit-level health care usage data was made possible through survey participants being required to record specifically designed health diaries and keep receipts from every visit to hospitals. See Section 4 for further details.

[5]The health insurance claims data could not be used to examine the effect of subsidies on take-up. It did not include private health screenings, which are not covered by subsidies. Since patients bear the full cost, it did not generate any insurance claim.

reveals that the spouse's screening take-up increases the partner's take-up by 8.1 percentage point, which corresponds to 38 percent of the direct subsidy effect. The spousal spillover is largely driven by the married couple's tendency to receive screenings on the same day.

Next, selection analyses reveal that subsidies induce participation of high-risk individuals from lower socioeconomic backgrounds. Comparing screening participants at even versus odd ages reveals that marginal participants, or compliers with subsidies, have lower household and individual income, lower education levels, and are less likely to be employed compared to those who participate even in the absence of subsidies, or always-takers. Consistent with lower socioeconomic status, compliers are more likely to find stomach-related diseases in stomach screenings. Due to strong income effect, I find compliers are less likely to smoke, drink and exercise (Cawley and Ruhm, 2011; Gallet and List, 2003; Gallet, 2007; Apouey and Clark, 2015; Armstrong et al., 2018; Thibaut et al., 2017). Comparing compliers to those who do not participate even in the presence of subsidies, or never-takers, I find compliers are positively selected on health behaviors. They are less likely to smoke or drink and more likely to exercise. This is consistent with previous studies showing correlations in various positive health behaviors (Oster, 2020; Cutler and Lleras-Muney, 2010).

In contrast, the cross spillover compliers exhibit an opposite pattern. Compared to individuals who only participate in biennial screenings, those who, in addition to participating in biennial screenings, further undergo annually subsidized or unsubsidized screenings are less likely to find a disease in biennial screenings. This suggests those who exhibit cross spillover have better overall health, and it can be explained by their better socioeconomic status. Compliers have higher individual and household income, higher educational attainment and are more likely to be working. However, the differences are larger for participants in unsubsidized screenings compared to participants in annually subsidized screenings. This suggests that subsidies mitigate the selection of healthy individuals with higher socioeconomic status in health screenings.

Selection patterns in spousal spillover depend on one's own subsidy eligibility. Among

even age individuals eligible for subsidies, those who are affected by the partner's screening and receive screenings have lower income and education levels compared to the entire even age group. Conversely, among odd age individuals ineligible for subsidies, compliers with spousal spillover exhibit higher incomes and education levels compared to the entire odd age group. This suggests that one's subsidy eligibility influences the selection not only through direct subsidy channel, but also indirectly through the spousal spillover channel. Overall, the selection patterns across three different margins highlight the importance of subsidies in encouraging screening participation among socially vulnerable individuals who bear higher burden of diseases.

Lastly, the analyses of the treatment effect of biennial subsidies indicate larger cancer diagnoses, lower mortality and increased health care utilization at even ages than at odd ages. Due to biennial subsidies and corresponding high take-up at even ages, there are more cancer diagnoses at even ages, including cancers whose screenings are subsidized every year or not at all. Cancer patients diagnosed at even ages are more likely to have earlier stage cancers and they show higher 5-year survival rates compared to the patients diagnosed at odd ages. Besides cancer incidences, even age group exhibits 1.68 percent lower mortality rate than odd age group, though the estimate is imprecisely estimated. Due to diagnoses of new diseases, even age group use health care more and show higher spending than odd age groups.

This study contributes to the literature on selection in preventive health care. People who opt in to a health program or adopt preventive health care are more motivated to improve their health and tend to engage in other healthy behaviors. This "healthy volunteer bias" has been documented in vaccinations, contraceptive use, workplace wellness programs and vitamin consumption (Thomas et al., 2021; Hungerford et al., 2016; Gafar et al., 2020; Jones et al., 2019; Oster, 2020; Cutler and Lleras-Muney, 2010). This study shows that similar selection bias occurs in health screenings and demonstrates how subsidies can mitigate these biases. This is important, since reducing selection bias could induce those more likely to benefit from health screenings to participate. Although this study does not directly estimate the long run effect of screenings on mortality, lower-

ing selection bias and having more high risk participants could lead to larger impact of screenings on reducing mortality.

This study complements and extends prior research on selection in health screenings (Kim and Lee, 2017; Einav et al., 2020; Kowalski, 2023; Bitler and Carpenter, 2016). Einav et al. (2020) examine recommended starting age for breast screenings at age 40 and find compliers have lower risk of true breast cancer than always-takers. Relative to Einav et al. (2020), this study focuses on the compliers with subsidies and highlights how different treatments or policies can attract different types of compliers. Kim and Lee (2017) examine subsidies for stomach and breast screenings at health insurance premium cutoffs in South Korea and find consistent selection pattern that compliers with subsidies are more likely to find cancers than always-takers.[6] Kowalski (2023) investigates overdiagnosis in breast screenings among Canadian women in their 40s and find that overdiagnosis is more prevalent among individuals with higher socioeconomic status who may undergo multiple screenings. This is in line with our study that underscores the importance of prioritizing screening for high risk group through subsidies. Our selection results also echo the finding from Bitler and Carpenter (2016) that prohibiting deductibles for mammogram led to sharper increase in take-up among high school dropouts and those with lower income.[7]

This study also contributes to the broader literature evaluating various health screening policies. Previous studies have examined various interventions, such as age recommendations, health insurance mandates, subsidies and workplace wellness program to encourage participation in health screening (Einav et al., 2020; Kadiyala and Strumpf, 2016; Bitler and Carpenter, 2016; Kim and Lee, 2017; Jones et al., 2019). These policies have been shown to raise screening take-up and lead to diagnoses of cancers and

[6] Kim and Lee (2017) examines the same National Health Screening Program from South Korea, where biennial subsidies cover 90 percent of the costs for stomach and breast screenings. For those below the health insurance premium cutoff, the remaining 10 percent copay was waived. Kim and Lee (2017) examines this cutoff to estimate the effect of copay on take-up and cancer detection.

[7] Another strand of literature on health screening focuses on health indicators reported during screenings and examine the discontinuity of indicators in defining health conditions, such as BMI cutoff for defining obesity or blood pressure cutoff for hypertension (Almond et al., 2010; Kim et al., 2019; Iizuka et al., 2021). They estimate the marginal value of health information or medical care and impact on future health outcomes and behaviors.

other adverse health conditions. In contrast to these studies that typically focus on one or a few screenings, this study considers examines many screenings together and find spillover in take-up across different types of screenings. Similar to our findings, Bitler and Carpenter (2016) also find increase in cervical screening take-up following mammogram mandates. These findings suggest that policies for various screenings should be developed in conjunction to maximize adherence to the recommended guideline.

The rest of the paper is organized as follows. Section 2 introduces the Korean health screening program and the subsidy schedule that provides random variation in take-up. Section 3 outlines the identification strategies using subsidy frequency as a source of exogenous variation. Section 4 discusses the two datasets used for this study. Section 5 presents the results and section 6 concludes.

2 Institutional background

The National Health Screening Program is a nationwide initiative in South Korea that promotes health screenings for all citizens, from infants to the elderly, with the goal of enhancing public health and welfare. The program offers 3 types of screenings: general health screenings, cancer screenings and infant/children health screenings. This study focuses on the general and cancer screenings.

General health screening comprises of tests to assess basic health conditions, such as measurements of weight, height, and blood pressure, along with chest X-ray, dental test, blood test, urine test and health risk evaluation. Chest X-rays are specifically used to detect tuberculosis.[8] Blood tests measure cholesterol and fasting blood sugar levels to assess heart disease risks. Additionally, various biomarkers for liver and kidney diseases are checked through blood and urine tests. Among general screening participants, those diagnosed to be at high risk of hypertension, diabetes or tuberculosis are recommended for follow-up tests and consultations.

[8]South Korea has the highest incidence and mortality rates of tuberculosis (TB) among the member countries of the Organization for Economic Cooperation and Development (Cho, 2018). So, TB screening is mandatory for students entering middle and high school. Adults receive biennial screenings as a part of general health screenings.

There are 5 types of cancer screenings covered by the National Health Insurance Service (NHIS) during the study period: stomach, breast, cervical, liver and colorectal screenings. This study additionally considers lung and prostate cancer screenings that are not subsidized.[9] The subsidized medical test for stomach screening is gastroscopy, inserting flexible tubes with a camera through mouth to examine throat, oesophagus and stomach. Breast screening uses X-ray mammography for both breasts and cervical screening uses cervical cytology, also known as a Pap test. Unlike the other cancer screenings available to all individuals, liver screening subsidies were limited to high risk groups.[10] Those not in the high risk group bear the full costs of liver screening. The subsidized test is liver ultrasound or Maternal Serum Alpha-Fetoprotein Screening (MSAFP). For colorectal screening, the subsidized test is fecal occult blood test (FOBT), which checks for hidden blood in a stool sample. If one tests positive on the FOBT, a subsidized colonoscopy follows. However, after this, colorectal screenings are not subsidized for the next 5 years. Receiving a colonoscopy without prior FOBT is not subsidized, and one patients must pay the full costs. Due to the low perceived risk and relatively low costs of colonoscopies in Korea, many people choose to receive a colonoscopy without first taking the FOBT (Baik and Lee, 2023).

Based on the frequency of subsidies, screenings can be grouped into 3 categories: biennial, annual, and no-subsidy screenings. Table 1 summarizes these subsidy rules. Biennial screenings, which include general health, stomach, breast and cervical screenings, are subsidized every other year. The biennial subsidies are provided 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 1968 would be eligible for subsidized screenings throughout 2024, since her age is 56 (=2024-1968), an even number. Next year in 2025, however, her age would be 57, an odd number, so she would need to pay

[9] Lung cancer screening began receiving subsidies in 2019, just after the study period.

[10] Liver screening was subsidized for high risk individuals up to twice a year. Non-high risk individuals could also undergo screening if they pay full costs. The survey data used in this study do not allow identifying the high risk group, so liver screening participants include both high risk and non-high risk individuals. The high risk group consists of (*i*) those with cirrhosis and chronic liver disease and (*ii*) those who tested positive in hepatitis B surface antigen test or hepatitis C virus HCV antibody test.

the full costs for the same screenings if she wants to receive them.[11] Annual screenings include liver and colorectal screenings, which are subsidized every year.[12] Finally, lung and prostate screenings are not subsidized and not covered by national health insurance.

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. Liver screening that follows the annual schedule is subsidized at ages 40, 41, 42, and onwards. The age cutoff for cervical screening is 30, while for colorectal screening, it is 50.[13] Since cervical screening follows a biennial schedule, subsidies are available at ages 30, 32, 34 and onwards. Colorectal screening following annual schedule is subsidized at ages 50, 51, 52 and onwards. There is no age cutoff for lung and prostate screenings, since they are not subsidized at all.[14]

The amount of subsidies is full coverage for the general health screening and 90% subsidy for 5 types of cancer screenings. So, one pays nothing for the general health screening, but needs to pay 10% of the costs for cancer screenings.[15] For those with low income, even the 10% copays are also subsidized, making cancer screenings free.[16]

The program was designed to maximize participation by ensuring easy access to subsidized screenings. People can receive screenings at public health clinics or at private

[11]The rule verbatim states that those born in even (odd) years are eligible for subsidies at even (odd) years. This is 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.

[12]Colorectal screening was subsidized biennially until 2011. It became annually subsidized in 2012.

[13]In 2016, the age cutoff for cervical screening was lowered to 20. Since the age cutoff was 30 for the majority of the study period, age 30 is used as the cutoff for cervical screening in this analysis.

[14]There are two exceptions to the subsidy rules for general health screening. First, one can be eligible for biennial subsidies for general screening before age 40 if one is formally employed or head of the household. Second, those with a "non-office" job are entitled to free general screening every year from age 40, not every other year. These exceptions apply only to general health screenings, and not to cancer screenings. Screening take-up data indicate that these two rules are not strictly followed. Therefore, this study abstracts away from these exceptions.

[15]The subsidies became more generous over time. During our study period, cervical screening was fully subsidized, and colorectal screening also became fully subsidized right after the study period.

[16]The 10% copay waiver was granted to individuals whose health insurance premium fell below the median. Since health insurance premium was determined by income and wealth level, the waiver primarily benefited those with lower income and wealth. Kim and Lee (2017) exploits the insurance premium cutoff for free cancer screenings to examine the effect of copays on take-up and cancer detection.

clinics and hospitals designated by the NHIS.[17] While appointments are typically not required for basic general screenings, they are required for more involved screenings, like colonoscopies. Reminder paper mails, and more recently mobile notifications, are sent to people eligible for subsidies and they include the types of screenings covered that year and available screening centers in the neighborhood.

3 Identification

This study exploits two sources of quasi-random variation in screening take-up generated by age cutoffs and biennial provision of subsidies. First, I bin ages by 2 years and measure the increase in take-up at age cutoffs as subsidies begin. Next, I unpack the 2 year age bins and measure the differential take-up between even ages with subsidies and odd ages without subsidies, while controlling for age.

Figure 1a illustrates the discontinuous increase in general screening take-up at age 40. General screening is subsidized at even ages from age 40. To abstract away from even and odd variation and focus on the effect of age cutoff, I bin ages by 2 years and plot the average screening take-up for each 2-year age bin.[18] Between age bins [38, 39] and [40, 41], the 2-year average take-up jumps from around 10 percent to 20 percent. This is because people are recommended to participate in general screening from age 40 and its subsidies kick in from age 40. To measure the discontinuous jump, I run regressions of the following econometric specification.

$$y_{it} = \alpha_0 + \alpha_1 \cdot k_{it} + \alpha_2 \cdot \mathbb{1}\{k_{it} > 0\} + \alpha_3 \cdot k_{it} \times \mathbb{1}\{k_{it} > 0\} + \varepsilon_{it} \tag{1}$$

The centered age variable is given as $k_{it} = (agebin_{it} - 39.5)$. Ages are binned in 2 years and I use samples 6 years before and after age 40. Hence, the age bins used are [34, 35], [36, 37], [38, 39], [40, 41], [42, 43], [44, 45] and the variable $agebin_{it}$ refers to the

[17] The number of designated private screening centers gradually expanded. In December 2023, there were around 5,800 private clinics and hospitals approved by the NHIS to provide health screenings, which translates into approximately one center for 900 people aged 40 and older.

[18] For each 2-year age bin, the numerator is the number of total screenings and the denominator is the number of individual-year pairs. Same person of different year is treated distinct.

midpoint of each bin, hence, 34.5, 36.5, ..., 42.5, 44.5.[19] The outcome variable y_{it} denotes the screening take-up for individual i in year t. The estimate of interest, $\hat{\alpha_2}$, captures the jump in take-up at the cutoff and the constant term, $\hat{\alpha_0}$, captures the take-up right before the jump. The standard errors, ε_{it}, are clustered at the individual level.

Figure 1b unpacks the 2 year age bins and plots the average take-up for each individual age. With subsidies provided at even ages from age 40, the take-up at even ages jumps to around 27 percent, while the take-up at odd ages remains steady around 10 percent. The take-up level for 2-year age bins shown in Figure 1a was the average of the even and odd age take-up level shown in Figure 1b, and hence is positioned in the middle of the even and odd curves. The differential take-up level between the even and odd ages after age 40 captures the effect of subsidies on screening take-up.

While the distinction between even and odd ages seems plausibly exogenous, the even age group (treatment group) is younger than the odd age group (control group) by design. This difference arises mechanically because subsidies begin at age 40, an even number.[20] I argue that age is the only difference between the treatment and control groups, so they should be balanced after controlling for age. To measure the differential take-up between even and odd ages, I estimate the effect using the following econometric specification.

$$y_{it} = \beta_0 + \beta_1 \cdot age\_even_{it} + \mathbf{f(age_{it})} + \epsilon_{it} \tag{2}$$

The treatment group variable is age_even_{it} that equals 1 when individual i has even age in year t. $\mathbf{f(age_{it})}$ is a function of age flexible enough to remove the age effect between the even and odd group. I use linear splines with 5 years interval as the main specification and provide robustness checks for using different length of interval in the Appendix section **??**. The standard errors, ϵ_{it}, is clustered at the individual level.[21]

[19] The reason I center age bins around 39.5 instead of 40 is that the midpoint between the two bins [38, 39] and [40, 41] is 39.5.

[20] Since the age distribution is roughly declining from age 40, the even age group is always younger than the odd age group in an analytical sample starting from age 40, regardless of the ending age. This creates imbalances in age between the treatment and the control group and in other covariates correlated with age.

[21] Despite the usage of panel data, the main specification does not include any panel method to make it clear that the identification strategy does not require panel structure. In the Appendix section **??**, robustness checks using individual fixed effects are presented.

Table 2 presents the conditional balance between the treatment and the control group using the survey data. The analytical sample is those with age from 40 to 89. First, note that the two groups are almost equally sized. This suggests that the even-odd rule divides the sample evenly into treatment and control groups. Column 3 further demonstrates that this assignment mechanism is random conditional on age. The differences in covariates conditional on age 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. Table A4 provides a similar balance check using the administrative health insurance claims data from the NHIS, showing that the even and odd age groups are well balanced conditional on age.

The identification strategy in this setup differs from a typical randomized controlled trial (RCT) in the sense that individuals switch between treatment and control intertemporally. This has two implications for our analysis. First, everyone ultimately receives subsidies for screenings. In any given year, individuals with odd age group who are not eligible for subsidies will reach an even age the following year and become eligible. Since everyone receives subsidized screenings within a two-year period, it is not feasible to estimate the long run effects of screenings. Second, the biennial subsidies create an incentive for forward looking individuals to shift their screenings from odd to even ages to take advantage of subsidy eligibility. This makes it hard to evaluate the cost and benefit of subsidy program by comparing even and odd age groups. The observed differences in screening take-up and subsequent cancer diagnosis between even and odd ages is the sum of net increase in screening due to monetary incentives and the change in screening timing from odd to even ages. While the net increase represents a marginal change from non-screening to screening, the timing shift represents a marginal change of screening one year earlier. Section 5.1 discusses the evidence for intertemporal substitution in more detail. This study embraces the substitution as a channel through which biennial subsidies magnify differences in take-up between even and odd ages, and focuses on the short run effect of subsidies on screening take-up, selection and disease diagnosis.

4 Data

This study uses two datasets: survey data on health care usage and administrative national health insurance claims data. The two datasets are complementary and each provides information that fills the gap in the other. The survey data provide detailed information on health screening take-up, but are not big enough to study rare outcomes, such as mortality or cancer incidence. The administrative insurance claims data capture a sufficient number of cancer incidences and deaths for statistical analysis, but do not capture private screenings received at odd ages. While the two datasets cannot be linked at the individual level, I use both to provide insight on how subsidies affect screening take-up and subsequent health outcomes.

This study utilizes the collection of Korean Health Panel Survey Data spanning the years 2008-2018.[22] It is a collection of yearly panel datasets that started with about 7,000 households and 21,300 individuals in 2008. To guarantee national representativeness in response to gradual attrition, the second cohort of 1,800 households with 5,000 individuals was added to the sample in 2014. Data were collected through face-to-face interviews. All household members were surveyed every year by survey enumerators using computer-assisted personal interviewing (CAPI), and hence, all variables are self-reported.

The collection of survey datasets includes rich information on demographic and socioeconomic characteristics, health care usage, health behaviors and so forth. The demographic and socioeconomic status variables, such as income and education level, are defined on a yearly basis. Health care usage dataset includes outpatient, inpatient and emergency care usage at the visit level.[23] For each visit to a hospital, the dataset contains the date of visit, hospital bills and drug expenditures incurred, diagnosis and whether the hospital visit was a first visit for a new illness.[24] Hospital visits for health screenings

[22] It is version 1.7.1 made jointly by Korean Institute for Health and Social Affairs (KIHSA) and National Health Insurance Services (NHIS).

[23] Although data collection was carried out annually, the unit of observations in these datasets is at the visit level. This was possible because survey participants were asked to keep a specifically designed health diary and leave detailed records of every visit to a hospital or a pharmacy with receipts. The enumerators collected the health diary and all the receipts at annual visits, compared each entry in the diary with receipts, and recorded the data. At every visit, they started with hospital visits from the last day of interview so that there is no missing period.

[24] Diagnoses were coded using Korean Classification of Diseases diagnosis code (Korean version of

included additional information about the type of screening received (general or specific type of cancer screenings), medical tests performed, screening results and diagnosis if any disease was found. The health behavior dataset includes annual information on smoking, drinking and exercising behavior. Each behavior contains information on the intensity in addition to whether one engages in such activity. For smoking and drinking, I consider current smoker or drinker status and additionally everyday smoker or drinker to take into account the intensity of the behavior.[25] Similarly for exercise, I consider 3 types by intensity: vigorous exercise, moderate exercise and walking.[26]

To complement the survey data, this study uses the insurance claims data from the NHIS. The sample consists of about 640,000 randomly drawn individuals from year 2002 to 2021. This results in observations of about 12.9 million individual-year pairs. The dataset includes basic demographic and health insurance information on beneficiaries, death, general and cancer screening records, and inpatient and outpatient hospital visit records.

One drawback of the insurance claims data is that the dataset does not capture private screening take-up. Private screenings fully paid by the patient do not generate any insurance claims. The implication for the study design is that most of the private screenings received at odd ages are not captured. Hence, the dataset is not appropriate for examining the impact of subsidies on screening take-up.

However, the insurance claims data include cancer diagnosis information regardless of screening take-up. I infer cancer diagnosis from the Coinsurance Reduction Program for Rare and Severe Diseases (CRP) run by the NHIS. The program reduces the coinsurance rate for hospital visits for rare and severe diseases, which usually ranges from 20-50%, to 0-10%. The list of diseases include cancers, cerebrovascular diseases, tuberculosis and other rare and incurable diseases that incur large health care expenditures. For newly diagnosed cancer patients, registering with the CRP is one of the first things to do and

ICD-10).

[25] These variables were defined based on the action in the previous one month of the time of interview.

[26] Specific examples for such activities were given at the survey. They are defined based on the action in the previous one week of the interview.

this generates an exhaustive list of new cancer diagnoses.[27] The CRP is implemented independently from the health screening programs, so it captures both cancer diagnoses made through screening and without screening. I infer new cancer diagnosis from this program and investigates whether subsidies at even ages lead to more cancer diagnoses at even ages.

5 Results

5.1 Effect of screening subsidies on take-up

5.1.1 Direct subsidy effect

First, I present the discontinuous increase in screening take-up at the cutoff ages where subsidies begin. General, stomach, breast and liver screenings are subsidized from age 40. Their 2-year average take-ups shown in Figure 1a, 1c, 1e, and 2a present the jump in take-up at age 40. Cervical and colorectal screenings are subsidized from age 30 and 50, respectively. There is a slight increase in cervical screening take-up at age 30, but it is much more muted. Colorectal screening, however, shows large increase in take-up at age 50. Both cervical and colorectal screenings exhibit another jump in take-up at age 40. This is the spillover from other screenings that have age cutoff at 40. I discuss the cross spillover in more detail in Section 5.1.2. Binning ages by two years remove the variation between the even and odd ages. I estimate the discontinuous jump at each cutoff age using Equation (1). Table 3 presents the estimation results. The 2-year average take-up increases by 8.6 to 11.2 percentage point for general, stomach and breast screenings at age 40. Compared to the constant term that represents the take-up before the jump, the estimates are both statistically and economically significant. Cervical screening displays a smaller, statistically insignificant 0.9 percentage point increase at age 30. Two types of annual screenings, liver and colorectal, also show significant increase in take-up (1.1-1.5

[27] The program also captures those who do not register as long as they visit hospital for cancer at least once. For cancer patients registered at the program, the coinsurance rate drops to 5%. For those who are not registered, the coinsurance rate drops to 10%. The reduction in coinsurance rate applies only to hospital visits related to cancer or a disease for which one is registered at the program. Unrelated hospital visits are exempt from the program.

percentage point) at age 40 and 50, respectively, as annual subsidies begin.

Next, I use the even-odd subsidy rule for biennial screenings and present the increase in take-up at even ages when subsidies are provided. Figure 1b, 1d, 1f and 1h plot the take-up for general, stomach, breast and cervical screenings at each individual age after unpacking the 2-year age bins. The take-up at even ages when subsidies are provided are much larger than that at odd ages without subsidies. Using Equation (2), I estimate this difference conditional on age. Table 4 presents the estimation results. Subsidy eligibility at even ages increases 1-year participation rate by 16 to 19 percentage point. Compared to the control group mean, this corresponds to 183 to 295 percent increase in take-up. The even-odd variation will later be used to estimate the treatment effect of health screenings. Therefore, I report the F-statistics for each outcome variable that indicate a strong first stage (Angrist and Pischke, 2009; Bound et al., 1995).

An important mechanism through which subsidies affect participation is intertemporal substitution. The knowledge of biennial subsidies at even ages allows forward-looking individuals to temporally reallocate screenings from odd to even ages. This change in timing widens the gap of take-up between even and odd ages without any net increase in participation. Hence, the differences in take-up between even and odd ages estimated in Table 4 are the sum of the net increase in take-up due to subsidies and the shift in screening timing from odd to even ages. The increases in take-up at cutoff ages estimated in Table 3 can also be magnified by intertemporal substitution if individuals expect subsidies kicking in from age 40 and shift screenings from 38, 39 to 40.

The take-up pattern for general, stomach and breast screenings, shown in Figure 1b, 1d and 1f, provides descriptive evidence for intertemporal substitution. First, the shift from before age 40 to after age 40 seems to be present but negligible in magnitude. Although there is a small decline at 39 and uptick at 40, these changes are barely discernible. Next, the substitution between even and odd ages after 40 exhibits variation across age groups. The slopes of the even and odd age take-up curve provide descriptive evidence. At ages in the 40s, the substitution effect is minimal, with take-up rates remaining stable or slightly increasing for both even and odd ages, indicating limited substitution. How-

ever, starting in the 50s, a divergence emerges: take-up rates decline at odd ages while increasing at even ages, suggesting a shift toward more subsidized screenings and fewer unsubsidized ones. In the 70s, take-up rates decrease for both even and odd ages, with a sharper decline at even ages. This trend may reflect a general decline in the demand for health screenings in older age groups, driven less by monetary incentives and more by medical needs that arise irrespective of whether the age is even or odd (Howard, 2005; Howard et al., 2009). Disentangling intertemporal substitution is challenging due to the lack of valid counterfactuals, but the overall take-up patterns indicate they exist and the extent of substitution varies with age.

In Appendix section C, I present evidence of substitution using monthly distribution of screening take-ups. It suggests the substitution is more pronounced by receiving screening early in December of even ages before the year changes than delaying screening from December of odd ages to the January of even ages. Appendix section B presents robustness checks with different functional forms, control variables and analytical samples. The resulting estimates are highly robust, boosting confidence in the exogeneity of the biennial subsidy criterion.

5.1.2 Cross spillover across different types of screenings

This section investigates spillover in screening take-up across different types of screening in two ways. First, using different age cutoffs for subsidies, I show that cervical and colorectal screenings exhibit large increase in take-up at age 40, despite their subsidies starting from age 30 and 50, respectively. Next, using different frequency of subsidies, I show that annual and unsubsidized screenings display larger take-up at even ages than at odd ages. This cross spillover leads to over- and under-utilization of screenings and has welfare implications. A potential mechanism is the tendency to receive multiple screenings on the same day or temporally close to each other.

Cervical and colorectal screenings display large discontinuous jump in take-up at age 40 as shown in Figure 1g and 2c, even though the subsidies for these screenings start from age 30 and 50, respectively. This is due to the fact that subsidies for most of the

screenings start from age 40. Table 5 estimates the jump in 2-year average take-up at age 40 for cervical and colorectal screenings using regression discontinuity design (RD) specification shown in Equation 1. The age 40 discontinuity leads to 7.4 percentage point (80 percent) and 1.4 percentage point (78 percent) increase in 2-year average take-up for cervical and colorectal screenings. While the age 30 discontinuity was insignificant for cervical screening, age 40 discontinuity is visibly larger and involves significant increase in take-up.[28]

Annually subsidized screenings and unsubsidized screenings have no reason to show systematic difference in take-up between even and odd ages. However, liver and colorectal screenings subsidized annually display larger take-up at even ages as shown in Figure 2b and 2d. Similarly, prostate and lung screenings that were not subsidized during our study period also display larger take-up at even ages as shown in Figure 2f and 2h. This is because most of the screenings, including the ones with the highest take-up like general and stomach screenings, follow biennial subsidy schedule. Table 5 columns 3-6 estimate the differential take-up between even and odd ages for annual and unsubsidized screenings using specification shown in Equation 2. All 4 screenings exhibit meaningfully larger take-up at even ages. Liver and colorectal screenings show 2.7-3.3 percentage point (94-124 percent) larger take-up at even ages than at odd ages. Prostate and lung screenings show 0.6-0.7 percentage point (67-81 percent) larger take-up at even ages.

One mechanism of cross spillover is receiving multiple screenings at the same time in one hospital visit. For illustration, Table A7 in Appendix section D presents the share of people who receive screenings on the same day with general screening, not just in the same year. Conditional on getting general screening, 85-96 percent of cancer screenings are received on the same day with the general screening. This could be due to fixed costs in visiting a hospital, such as time or travel costs. Since many screenings require fasting, it is better to receive multiple tests after a single fasting period, avoiding the need to fast separately for each screening. Another channel could be doctor's recommendation. During the general screening, the most basic screening, doctors could recommend receiv-

[28] Bitler and Carpenter (2016) find similar cross spillover in mammogram mandate that led to increase in not only mammogram take-up, but also clinical breast exams and cervical screening.

ing other subsidized screenings. Table A7 shows that when cancer screenings are not received on the same day with general screening, they are more likely to be received after the general screening than before.

A key policy implication is that interventions aimed at increasing screening participation should account for the tendency to undergo multiple screenings simultaneously. Stand-alone policies that deviate from this joint screening schedule are likely to have limited impact. The observation that annually subsidized liver and colorectal screenings show larger take-up at even ages could suggests under-utilization of subsidies at odd ages. This is because individuals are less inclined to visit hospitals solely for one type of screening at odd ages but are more likely to do so at even ages when they can combine multiple screenings. Appendix section D provides further evidence that cross spillover in colorectal screening is under-utilization of subsidies provided at odd ages by examining the change in subsidy frequency from biennial to annual in year 2012.[29] One potential solution is to subsidize alternative medical tests for colorectal screening. The current fecal occult blood test (FOBT) requires annual testing, resulting in poor adherence. In contrast, colonoscopy, with a recommended frequency of once every 10 years, aligns more closely with the existing biennial screening pattern and may improve participation rates. (Winawer et al., 2006).

5.1.3 Spousal spillover

This section examines spillover in screening take-up between spouses. The subsidy eligibility increases not only one's screening take-up, but also the spouse's take-up as well. A potential mechanism is the tendency for couples to receive screenings together.

Figure 3 plots the participation rate in any type of screening across four types of couples formed by the combination of one's own and the spouse's ages.[30] The "Even/Even" group consists of couples where both partners have even ages. The "Even/Odd" group

[29] A similar issue is observed with cervical screening subsidies for individuals aged 30 to 39.

[30] In constructing Figure 3, I include the currently married couples where both partners are between the ages of 30 and 80. For the regression analysis in Table 6, I restrict the sample to currently married couples where both the husband and wife are between the ages of 40 and 89, ensuring that both partners are subject to biennial subsidy program.

includes couples whose own age is even but the spouse's age is odd. The other groups are similarly defined. Two key patterns emerge from the figure. First, there is a jump in take-up at age 40 when one's age is even. This reflects the effect of subsidy eligibility on take-up. Second, within each even age group (colored in red) and odd age group (colored in blue), the group with even age spouse (solid line) shows larger take-up than the group with odd age spouse (dashed line). This suggests that a spouse's subsidy eligibility increases the other partner's own participation rate and vice versa.

To estimate the spousal spillover effects, the analytical sample is adjusted to currently married couples both of whose ages are between 40 and 89. There are four types of couples by the combination of their ages; (i) Even/Even, (ii) Even/Odd, (iii) Odd/Even, and (iv) Odd/Odd where the first even (odd) means one's own age is even (odd) and the second even (odd) means the spouse's age is even (odd). Equation (3) examines their participation rate.

$$y_{it} = \gamma_0 + \gamma_1 \cdot age\_even_{it} + \gamma_2 \cdot spouse\_age\_even_{it} + \gamma_3 \cdot age\_even_{it} \times spouse\_age\_even_{it} + \phi_{it} \tag{3}$$

The outcome variable, y_{it}, is screening take-up of individual i in year t. Our parameter of interest is γ_2 that captures whether one is more likely to get screening when the spouse has even age and is eligible for subsidies. The coefficient of the interaction term, γ_3, examines if there is any additional increase in take-up when both the husband and the wife are eligible for subsidies in the same year. The error term, ϕ_{it}, is clustered at the couple level.

Table 6 presents the positive spousal spillover in take-up. The outcome variable is one's own take-up of any type of screening. Column 1 presents positive and significant estimates for both one's own ($\hat{\gamma_1}$) and the spouse's subsidy eligibility ($\hat{\gamma_2}$). The direct effect of own subsidy eligibility measured in the married group is around 21 percentage point. The effect of spouse's subsidy eligibility is 1.6 percentage point and significant. However, there is no interaction effect ($\hat{\gamma_3}$). This implies that the spouse's subsidy eligibility increases one's own take-up by the same magnitude regardless of whether one is

eligible or ineligible for subsidies.[31] To account for the different ages between the even and odd groups, I control one's own and the spouse's ages using linear splines with 5 years interval, as used in Equation (2). Column 2 presents estimates controlling for ages and the results are robust.

To translate the subsidy effect into peer effect in screening, I instrument the spouse's screening participation with the spouse's subsidy eligibility. Column 3 measures the effect of spouse's screening take-up on one's own take-up using the two stage least square estimator. Spouse's take-up increases one's own take-up probability by 7.9 percentage point. This corresponds to around 37 percentage of the direct subsidy effects. Column 4 shows the estimates are robust to adding age control variables.

Appendix section E presents further analyses on potential mechanisms and heterogeneity in spousal spillover. Table A11 reveals the spousal spillover is pronounced from wives to husbands, but muted from husbands to wives. Husbands are more likely to participate when wives are eligible for subsidies, but not vice versa. Table A12 presents evidence that taking screening together is one mechanism of spousal spillover. Conditional on both husband's and wife's participation, more than 40 percent receive screenings on the same day.

5.2 Selection into screening

A common criticism against health screening is that it is usually healthy people who receive screenings. On the other hand, those at higher risk of cancer, such as individuals with lower income and education levels, tend to have lower participation rates. Furthermore, critics argue that screening healthy people is a waste of precious medical resources and it increases the risk of false positives and overdiagnoses (Rubin, 2019; Kowalski, 2023). Therefore, it is important to encourage participation among individuals likely to have a disease.

This section examines the characteristics of marginal individuals who respond to sub-

[31] It may seem puzzling that the magnitude of spousal spillover remains similar regardless of one's own subsidy eligibility. Section 5.2 provides one explanation from the selection analysis, that is, the individuals who exhibit spousal spillover (i.e., the "compliers") differ depending on whether or not the individual is eligible for subsidies. For a more detailed analysis of this selection effect, see Section 5.2.

sidies and participate. They are the ones who would not receive screening without subsidies, but choose to do so when subsidies are available. Drawing on the terminology of Imbens and Angrist (1994) and Angrist et al. (1996), they are called "compliers" and their characteristics determine the effectiveness of the policy. Since the goal of screening is to detect diseases at an early stage, the policy should ideally target unhealthy compliers who are at higher risk of having undiagnosed conditions.

This section examines selection across three margins. First, compliers with the biennial subsidies are characterized by comparing the participants at even ages to those at odd ages. They are the ones who receive subsidized screenings at even ages but do not receive unsubsidized ones at odd ages. Second, compliers in cross spillover are characterized by analyzing those who, after receiving subsidized screenings at even ages, further proceed to receive annual or unsubsidized screenings. Finally, compliers in spousal spillover are examined. These are individuals who do not receive screenings when their spouse is at an odd age but do so when their spouse is at an even age. The analysis focuses on comparing the characteristics of compliers when own subsidies are available versus when they are not.

5.2.1 Compliers with screening subsidies

One way to characterize and compare compliers with always-takers is to restrict the sample to screening participants and compare even with odd age groups.[32] After the sample restriction, those who participate in subsidized screening at even ages are either always-takers or compliers. On the other hand, those who participate in unsubsidized screening at odd ages must be always-takers.[33] Then, I examine differences between the participants at even ages with those at odd ages. Any observed differences come from group composition, that is, the presence of compliers in the even age group. Any treatment effect of screening cancels out, since they are all screening participants. Hence,

[32]Complier characterization is straightforward in settings with only one-sided noncompliance. For example, in a setting with only always-takers but no never-takers, the control group can be used to distinguish between always-takers and compliers. Participants in the control group are always-takers, while the nonparticipants are untreated compliers. Similar comparison can be made in the treatment group when there are only never-takers but no always-takers.

[33]I impose monotonicity assumption that rules out the existence of defiers.

by comparing the even with the odd age group among screening participants, I can compare treated compliers with always-takers. Similarly, once can restrict the sample to screening nonparticipants and compare even with odd age group. This allows comparing untreated compliers with never-takers.

Figure 4a, 4b and 4c shows that compliers with biennial subsidies are more likely to be diagnosed with a stomach-related disease through stomach screening, have lower household income and are less likely to be college graduate than always-takers. Figure 4a plots the share of participants who reported they were diagnosed with a stomach disease among stomach screening participants.[34] The diagnosis rate is consistently higher at even ages than at odd ages. This suggests the marginal participants who respond to subsidies and participate at even ages (compliers) are more likely to be diagnosed with a disease compared to the inframarginal participants who participate regardless of subsidies (always-takers). This difference reflects the groups' distinct underlying health conditions prior to screening, rather than any treatment effect of the screening itself. Similarly, Figure 4b and 4c plot the household income and the share of college graduates among participants in any type of screening.[35] The even age participants shows lower household income and the share of college graduates compared to the odd age participants. This suggests that compliers with biennial subsidies have lower household income and share of college graduates than the always-takers, implying the lower socioeconomic status of compliers.

The figure provides intuitive and visually clear evidence but is limited to qualitative comparisons. To enable formal comparisons, I infer complier characteristics from the group of participants at even ages, which is a convex combination of the characteristics of always-takers and compliers. By using the shares of always-takers and compliers estimated from the first stage regression (Equation (2)) and the always-takers characteristics estimated from the participants at odd ages, I back out the compliers' characteristics.

[34]The survey asked screening participants whether they had been diagnosed with any disease through screening. This includes not only cancer but also milder conditions, such as stomach inflammation or ulcers, in the case of stomach screening. Appendix section F details the most common diagnoses for each type of screening.

[35]The unit of household income is 10,000 Korean Won.

Specifically, I run the regression (Equation (8)) containing an interaction term between the screening take-up and the even age dummy. This approach allows me to estimate the characteristics of the three compliance groups and statistically test the difference in their means. It also distinguishes between compliers in the treatment group, treated compliers, and compliers in the control group, untreated compliers. By comparing always-takers with treated compliers and never-takers with untreated compliers, I cancel out any causal effect of screening, isolating the selection effect. The same method was used in Einav et al. (2020), Kowalski (2023) and Kim and Lee (2017) to characterize compliers in the health screening context. Appendix section G provides more detailed estimation steps.

Table 7 and Figure 5a presents the relative characteristics of treated compliers compared to always-takers. With respect to diagnoses from the screenings, compliers are more likely to be diagnosed with stomach-related diseases through stomach screening than always-takers.[36] However, for diagnoses from breast, cervical and colorectal screenings, the differences were not statistically significant.

An analysis of socioeconomic status reveals that individuals from lower social backgrounds are more likely to respond to biennial subsidies and participate in screenings. Compliers have lower individual and household incomes than always-takers. This could be because they are less likely to be currently working. They also have lower educational attainment and are less likely to be college graduates. This selection pattern suggests that subsidies effectively induce participation of those from lower socioeconomic background. This could be because lower income individuals who were financially constrained in screening participation are more sensitive to subsidies and are more likely to participate given subsidies. This is consistent with the finding from Bitler and Carpenter (2016) that showed prohibiting deductibles for mammogram led to increase in take-up and the effect was concentrated among high school dropout with lower income.

In terms of health behaviors, compliers are less likely to smoke, drink or exercise compared to always-takers. Specifically, compliers are less likely to be current or everyday

[36]Table **??** in Appendix section G explores specific stomach diseases. While the estimated ratios are imprecise, compliers show a higher likelihood of detecting Helicobacter Pylori (*H. pylori*), a key predictor of stomach cancer, as well as stomach inflammation and ulcers.

smoker, less likely to be current or everyday drinker, and less likely to engage in vigorous or moderate exercise.[37] These differences are primarily driven by negative selection on income. Beneficial health behavior, such as exercise, exhibit a positive and monotonic relationship with income across the distribution of income. Individuals with higher income are more likely to exercise, so compliers, who have lower income than always-takers, are also less like to exercise. Current drinker variable also displays similar relationship with the income. Harmful health behaviors, such as everyday drinking or smoking, also show a positive correlation with income, driven by the income effect. Previous studies on health behaviors consistently found positive income elasticity of demand for cigarettes and alcohol (Cawley and Ruhm, 2011; Gallet and List, 2003; Gallet, 2007). However, this positive relationship masks a non-monotonic pattern. Despite overall positive correlation, everyday drinking and smoking display an inverted U-shaped relationship with income. While higher income initially correlates with greater likelihood of these behaviors, after a certain threshold point, further income increases are associated with reduced smoking and lower probability of being an everyday drinker. The margin captured in this study may fall before the peak in this inverted U-shaped curve, where higher income is still correlated with higher probability of smoking and everyday drinking.

Table 7 and Figure 5b 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.[38] Specifically, compliers are less likely to smoke and drink but more likely to exercise. Kowalski (2023) confirms similar positive selection in health behaviors in the Canadian National Breast Screening Study, an influential RCT on mammogram that informed the USPSTF guideline. Similarly, Jones et al. (2019) report comparable selection pattern in a workplace wellness program that includes screening component. These results are consistent with prior studies that highlight the correlation between positive health behaviors (Oster, 2020; Cutler and Lleras-Muney, 2010).

[37] If screening has any causal effect on health behaviors, it would cancel out in this comparison, as both treated compliers and always-takers participate in screening.

[38] 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.

5.2.2 Compliers in cross spillover

This section investigates the characteristics of compliers in cross spillover. Section 5.1.2 discusses the spillover from biennial screenings to annually subsidized and unsubsidized screenings. Specifically, despite annual subsidies for liver and colorectal screenings and no subsidy for prostate and lung screenings, these screenings display higher take-up at even ages from age 40. This section analyzes the 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 no-subsidy screenings are a subset of biennial screening participants. Among annual or no-subsidy 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 no-subsidy screenings. Almost no one receives a colorectal screening, an annual screening, without also receiving the general screening, a biennial screening. This one-sided noncompliance simplifies the selection analysis. One only needs to examine who participate in annual or no-subsidy screening among biennial screening participants. I run the following regression to estimate the characteristics of compliers in spillover.

$$y_{it} = \delta_0 + \delta_1 \cdot screen_{it} + \varepsilon_{it} \tag{4}$$

The sample is restricted to biennial screening participants at even ages.[39] 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 8 presents the characteristics of compliers in cross spillover. Compared to individuals who only participate 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 can be explained

[39]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.

by their better socioeconomic status. Compliers have higher individual and household income, higher educational attainments and are more likely to be working. The differences are larger for the participants in unsubsidized screenings compared to the participants in annually subsidized screenings. This suggests that subsidies mitigate the selection of healthy individuals with higher socioeconomic status in health screenings.

5.2.3 Compliers in spousal spillover

This section analyzes the characteristics of compliers in spousal spillover in screening take-up. As shown in Section 5.1.3, a spouse's subsidy eligibility increases the partner's screening participation by approximately 1.6 percentage point. Interestingly, this increase is similar in magnitude regardless of whether the individuals themselves are eligible for subsidies or not.[40] This similarity in absolute magnitude is unexpected, as one might anticipate a smaller increase when individuals are not eligible for subsidies, given that these compliers must bear the full cost of screenings. To explore this further, this section investigates whether the characteristics of spousal spillover compliers vary depending on the individual's own subsidy eligibility.

To explore the compliers in spousal spillover, I compute the spousal spillover effect across various subsamples and compare the first stage coefficients. The ratio of the first stage coefficient for a certain group to that of the entire sample is equal to the relative likelihood that a complier belongs to that certain group (Angrist and Pischke, 2009). For example, the relative likelihood that a complier is male is calculated as follows:

$$\text{Relative likelihood} = \frac{Pr(male \mid compliers)}{Pr(male)} = \frac{\text{1st stage among male}}{\text{1st stage in the entire sample}}$$

Specifically, I estimate the following econometric specification with the male subsample and with the entire sample. To take into account potentially different compliers based

[40] When considering the relative increase — calculated as the absolute increase in take-up divided by the average take-up rate — the effect is notably larger when the individual is not eligible for subsidies. Specifically, the absolute increase remains 1.6 percentage points in both cases. The average take-up is 12.8 percent for the Odd/Odd group and 34 percent for the Even/Odd group. Therefore, the relative increase is 12.5 percent ($= 100 * 1.6/12.8$) for the Odd/Odd group and 4.7 percent ($= 100 * 1.6/34$) for the Even/Odd group.

on one's subsidy eligibility, the subsample is further divided into the ones with even ages and the ones with odd ages.

$$y_{it} = \eta_0 + \eta_1 \cdot spouse\_screen_{it} + \mathbf{f}(\mathbf{spouse\_age_{it}}) + \varepsilon_{it} \tag{5}$$

The specification is similar to the Equation (2), except that it estimates the effect of the spouse's subsidy eligibility rather than one's own subsidy eligibility. The outcome variable, y_{it}, is one's own screening take-up. The parameter of interest, η_1, captures the spousal spillover in take-up. To account for age effects, the regression includes linear splines of the spouse's age at 5 year intervals. This regression is estimated separately for even-aged and odd-aged subsamples. The ratio of the first stage coefficients among male with even ages to that among even ages provides the relative likelihood that a complier in the even age group is male.[41] Standard errors are clustered at the couple level.

Table 9 presents the first stage coefficients for spousal spillover across various subsamples. The spousal spillover in the entire sample was 1.9 percentage point in the even age group and 1.6 percentage point in the odd age group. For each age group, first stage coefficients are estimated in the subsample of male, currently working, college graduate and households with income above the median. Taking the ratio of the first stage coefficient in the subsample to that of the entire sample yields the relative likelihood that a complier belongs to a certain group. Figure 6 plots these relative likelihoods for the even and odd age groups separately.

In both age groups, compliers are more likely to be male and currently working. This is consistent with the direction of spousal spillover results, shown in Table A11, that the spousal spillover is stronger from wives to husbands than from husbands to wives. However, there is opposite selection pattern in terms of education and income. In the even age group where one is eligible for subsidized screenings, those affected by the spouse and subsequently participate in screenings are from lower socioeconomic backgrounds. They

[41] Spousal spillover involves two-sided noncompliance, hence both always-takers and never-takers. Ideally, the same method used for characterizing compliers with direct subsidy effect should be used here as well to compare compliers with always-takers and never-takers. However, due to the small magnitude of first stage coefficient, inferring complier characteristics became imprecise and unstable. As a result, I rely on comparisons of first stage coefficients across subsamples to infer complier characteristics.

are less likely to be college graduates and their household income is less likely to be above median compared to the entire even age group. On the other hand, in the odd age group where one has to pay the full costs of screenings, compliers display the opposite pattern. They are more likely to be college graduate and their household income is more likely to be above median. The contrasting selection patterns suggest that screening subsidies help attract socially vulnerable participants not only through direct subsidy channel, but also through spousal spillover channel as well.

5.3 Effect of health screening

This section examines the causal effect of health screenings on cancer diagnoses, mortality and health care utilization by using the exogenous variation in screening take-up between even and odd ages.[42] The analysis uses insurance claims data from the Korean National Health Insurance Service, which includes detailed information on cancer diagnoses, deaths and health care utilizations. As explained in Section 4, the insurance claims data do not capture private screenings that are fully paid for by screening participants. As a result, the take-up at odd ages or before 40 is virtually zero, making it infeasible to run a first stage regression. Hence, all subsequent analyses are conducted in a reduced form framework, examining differences in cancer diagnoses, mortality and health care usage between the even and odd age groups. The sample consists of individuals aged 40 to 89. To estimate the effects of biennial subsidies on new cancer diagnoses, deaths, and health care utilization, the same econometric specification used to evaluate the impact of subsidies on screening take-up, outlined in Equation (2), is applied.

5.3.1 Effect on cancer diagnoses

Biennial subsidies provided at even ages lead to more cancer diagnoses at even ages as shown in Figure 7. New cancer diagnoses are identified using data from the Coinsurance Reduction Program for Rare and Severe diseases in South Korea, which offers reduced coinsurance rates for the treatment of rare and severe diseases, including all types of

[42] Due to the coarseness of the running variable, a regression discontinuity design at age 40 is not used.

cancer. The program provides detailed information on the timing of diagnoses and the associated health care utilization for treatment. Figures 7b, 7c and 7d plot the share of new stomach, breast and cervical cancer diagnoses where the associated screenings were biennially subsidized at even ages. Consistent with the subsidy schedule, there are more stomach, breast and cervical cancers diagnosed at even ages than at odd ages. Table 10 presents the differences, showing that diagnoses of stomach, breast and cervical cancers are 41%, 11% and 13% higher, respectively, at even ages compared to odd ages.

The differences in cancer diagnoses are also evident for cancers associated with annually subsidized or unsubsidized screenings. Figures 7e and 7f present the liver and colorectal cancer diagnoses for which the corresponding screenings were subsidized annually. These figures reveal a higher number of diagnoses at even ages. Similarly, lung and prostate cancer diagnoses, shown in Figure 7g and 7h, also display higher number at even ages. Table 10 confirms that the differences are statistically significant, with the exception of prostate cancer diagnoses.

5.3.2 Characteristics of cancer diagnoses

The primary goal of cancer screenings is to prevent premature deaths through early detection of cancers. Previous analysis demonstrated that cancer screenings significantly increase cancer diagnoses. This section extends the analysis by examining whether cancers identified through screenings are diagnosed at earlier stages compared to those detected without screening.

To measure early detection, this analysis considers two metrics: the 5-year survival rate and the share of in-situ cancers for each diagnosis. The 5-year survival rate represents the share of cancer patients who survive at least 5 years after the diagnosis, with higher rates indicating a greater likelihood of survival. However, interpreting survival rates requires caution. Higher survival rate should not be confused with a causal effect on mortality. Lead time bias can inflate survival rates through earlier diagnoses without reducing overall mortality (Gordis, 2013; Welch et al., 2000). While effective treatments that follow after the diagnosis may extend lifespans, survival rate measures are influenced

by both early diagnosis and the mortality effect.

In-situ cancers, also known as precancers or stage 0 cancers, are early stage cancers in which abnormal cells are formed but are confined to the original location and have not invaded other tissues. A higher share of in-situ cancers suggests detection of earlier stage cancers. These cancers are most commonly detected in breast, cervical, and colorectal cancer screenings due to the sensitivity of current screening technologies for these cancers.

Table 11 presents the 5-year survival rates and the share of in-situ cancers for each type of cancer screening. The sample is restricted to new cancer diagnoses. Differences in 5-year survival rates and the share of in-situ cancers between even and odd ages are estimated using Equation (2). The number of observations reported in column 4 is the number of new cancers diagnoses. For stomach, breast and cervical cancers, where screenings are biennially subsidized, patients diagnosed at even ages show statistically significant improvements in 5-year survival rates and higher share of in-situ cancers. This suggests that screenings lead to detection of earlier stage cancers. For liver and colorectal cancers, where screenings are subsidized annually but showed lower take-up, diagnoses at even ages also show higher survival rates and higher share of in-situ cancers. However, these estimates are imprecisely estimated. By contrast, for lung and prostate cancers, which are not subsidized, there is no observed gain in survival rates or the share of in-situ cancers. This is consistent with the rationale for the lack of subsidies: there are no effective screening tests for these cancers that reliably enable earlier diagnosis and treatment to reduce mortality.

5.3.3 Effect on short-run mortality

This section investigates the effect of screening subsidies on short-term mortality. Specifically, it compares the proportions of deaths between even- and odd-age groups within the same year to determine whether subsidized screenings for the even-age group result in lower mortality. Due to the recurring subsidy schedule, this analysis is limited to examining deaths occurring in the same year and does not estimate the long-term effects of screenings. Using the same specification outlined in Equation (2), I estimate

the impact of screening subsidies on 1-year mortality.

Table 12 presents the results of the 1-year mortality analysis. The findings indicate that screening leads to a small reduction in mortality. The 1-year mortality rate among odd age group was 0.32 percent. Even age group exhibits 0.005 percentage point (1.68 percent) lower mortality rate compared to the odd age group. However, this estimate was statistically insignificant.

The analysis of 2-year death rates allows me to estimate the impact of intertemporal substitution, or shifting the screening timing 1 year earlier, on mortality. One mechanism through which biennial subsidies generate large variation in screening take-up between even and odd ages is substitution: individuals shifting screening from odd to even ages. Does receiving a subsidized screening one year earlier have any impact on mortality? To answer this, I examine 2-year mortality rates. Both even and odd age groups can get subsidized screenings over a 2 year period. However, the timing differs: the even age group is eligible for subsidized screenings in the first year, while the odd age group is eligible in the second year. Therefore, comparing 2-year mortality rates allows me to estimate the impact of receiving subsidized screenings one year earlier rather than later. This comparison is not about whether individuals receive subsidized screenings, but rather about the timing of when they receive them.

Table 12 also presents the results for 2-year mortality.

5.3.4 Effect on health care utilization

Finally, this analysis explores the impact of screening subsidies on health care utilization. As a measure of health care usage, I examine the number of outpatient visits, days of inpatient care, number of surgeries and the total outpatient and inpatient spending. The spending includes out-of-pocket payments by the patients and payments from the National Health Insurance Service.

In addition to total health care usage, I analyze specific components related to cancers, cancer precursors, chronic conditions and potential harms of screening. Health care usage for cancers includes follow-up testings and the treatments following cancer diagnoses. I

also examine usage related to cancer precursors, which are conditions identified during screenings that, if untreated, could progress to cancer. Treating these precursors can prevent cancer development, which aligns with the preventive goals of screening. A primary goal of general screening is to assess the risk of chronic conditions, such as high blood pressure, diabetes and high cholesterol. Therefore, I also examine health care usage for managing these chronic conditions. Lastly, I evaluate the impact of screening subsidies on health care usage related to the treatment of potential harms of screenings. For example, colonoscopy screenings may result in direct health risks, such as perforations in the colon wall, if not performed properly. This analysis investigates whether biennial subsidies provided at even ages lead to increased health care usage due to these types of adverse outcomes.

Table 13 presents estimates for increase in health care usage related to cancers at even ages. The results show a significant rise in cancer-related outpatient visits and days of hospitalization, both increasing by approximately 2.5 percent. Additionally, the number of surgeries performed for cancer increases by 11 percent. Correspondingly, total outpatient and inpatient spending on cancer treatment rises by 2.5 percent at even ages compared to odd ages.

When combining all components, the biennial subsidies provided at even ages result in a significant increase in overall health care utilization at even ages relative to odd ages. The number of outpatient visits, days of inpatient stays, and total spending all increase by 1–1.5 percent, while the number of surgeries rises by 6.7 percent. It is important to note that these estimates capture the immediate, one-year effects of the subsidies and do not reflect the long-term impact of health screenings on health care utilization.

6 Conclusion

This paper studies South Korea's National Health Screening Program that offers subsidies for the general and cancer screenings biennially to individuals of even ages. The even age group eligible for subsidies is comparable to the odd age group ineligible for sub-

sidies, apart from the subsidy eligibility. This provides an opportunity to estimate the causal effects of these subsidies. Using a combination of survey data and administrative insurance claims from a nationally representative population, this study investigates the impact of subsidies on screening take-up, selection into screenings, and the downstream effects on disease diagnoses, mortality, and health care utilization.

The analysis reveals that biennial subsidies significantly increase screening participation. It documents spillover in take-up across screenings due to the tendency to receive multiple screenings on the same day. This suggests that bundling screenings and facilitating same-day screenings can increase overall participation rates. Additionally, spillover effects between spouses were observed driven by the tendency for couples to receive screenings together.

Subsidies were shown to attract high-risk individuals with lower socioeconomic status. Those who respond to subsidies and participate have lower income and education levels than those who voluntarily choose to get screened in the absence of subsidies. Conversely, those who additionally participate in unsubsidized screenings tend to have higher income and education attainment. Spousal spillover effect exhibits distinct selection patterns based on one's own subsidy eligibility. When one is eligible for subsidies, spousal compliers have lower income and education levels, whereas the reverse is true when one is ineligible for subsidies. Overall, the evidence supports the role of subsidies in reducing disparities in screening participation by attracting socially vulnerable participants who are at higher risk of heart diseases and cancers.

The analysis also finds that subsidies leads to diagnoses of cancers at an earlier stage, with patients diagnosed through screenings exhibiting higher survival rates. These gains in survival rates are observed only for cancers with subsidized screenings, consistent with the reasons they are subsidized in the first place. Additionally, the increased diagnosis of diseases leads to greater health care utilization as people seek subsequent medical care.

This study underscores the importance of subsidies in improving access to health screenings, particularly among disadvantaged populations. By encouraging earlier detection and increasing survival rates for high-risk individuals, subsidies play a critical role in

addressing health disparities and improving population health outcomes. Policymakers should consider designing guidelines that bundle screenings and facilitate participation with families, leveraging spillover effects to maximize the impact of subsidy programs. Further research could explore the long-term effects of such policies on mortality and health care expenditures.

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7 Tables

Table 1: Screening schedule

	Biennial subsidy				Annual subsidy		No subsidy	
	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%
Copay ($)	0	7	3.5	0	10	5	110	20
Target			Female	Female	High risk group			Male
Subsidized medical tests		Gastroscopy, biopsy	Mammogram	Pap smear	Ultrasound, MSAFP	Fecal occult blood test, colonoscopy, biopsy		

Notes: This table summarizes the subsidy schedules for health screenings in Korea. Biennial screenings are subsidized once every two years when one's age (=current year - birth year) is even-numbered. Annual screenings are subsidized every year. No-subsidy screenings are not subject to any subsidy by the Korean National Health Insurance Service during the study period. Liver screening is subsidized up to twice a year. The subsidy starting age for cervical screening was lowered to 20 in 2016. The colorectal screening used to be biennially subsidized at even ages from age 50 until 2011. It became an annually subsidized screening from year 2012. Colonoscopy is subsidized only for those with positive result from fecal occult blood test.

Table 2: Balance table

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

Notes: This table reports the 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 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 3: Effect of subsidies on 2-year take-up using age cutoff

	(1)	(2)	(3)	(4)	(5)	(6)
	Biennial				Annual	
	General	Stomach	Breast	Cervical	Liver	Colorectal
$Age \geqslant 40$	0.086***	0.105***	0.112***		0.011***	
	(0.007)	(0.006)	(0.009)		(0.004)	
$Age \geqslant 30$				0.009		
				(0.007)		
$Age \geqslant 50$						0.015***
						(0.005)
Constant	0.095***	0.061***	0.064***	0.029***	0.024***	0.039***
	(0.005)	(0.004)	(0.005)	(0.004)	(0.002)	(0.003)
N	34713	34713	17725	12168	34713	34819
Adj R^2	0.020	0.032	0.037	0.015	0.004	0.003
Percentage increase	91	173	175	30	47	40
Sample age range	[34, 45]	[34, 45]	[34, 45]	[24, 35]	[34, 45]	[44, 55]
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 after binning ages by 2 years. All estimates are around the respective subsidy starting age for each screening given in the table. 6 years (3 bins) before and after the subsidy starting age are used as analytical sample. The econometric specification is given in Equation (1). The outcome variable is the 2-year average take-up. Coefficients for $Age \geqslant c$ measures the jump in take-up at age c and the constant measures the take-up right before the jump. The percentage increase refers to the relative size of the jump at the cutoff compared to the constant. Standard errors are clustered at the individual level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table 4: Effect of subsidies on 1-year take-up using Even/Odd variation

	(1)	(2)	(3)	(4)
	General	Stomach	Breast	Cervical
Age even	0.187***	0.190***	0.191***	0.164***
	(0.003)	(0.003)	(0.004)	(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: This table reports the effect of biennial subsidies on 4 types of biennial screening take-up by comparing even age group with odd age group. The analytical sample is those subject to biennial subsidy rule, so from age 40 for general, stomach and breast screenings and from age 30 for cervical screening. The econometric specification is given in Equation (2). The outcome variable is the average take-up at each age. The estimates measure the effect of screening subsidies on take-up 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 5: Cross spillover in screening take-up

	(1)	(2)	(3)	(4)	(5)	(6)
	RD		Even vs Odd			
	Cervical	Colorectal	Liver	Colorectal	Prostate	Lung
$Age \geqslant 40$	0.074***	0.014***				
	(0.010)	(0.003)				
Constant	0.093***	0.018***				
	(0.006)	(0.002)				
Age even			0.027***	0.033***	0.007***	0.006***
			(0.001)	(0.001)	(0.001)	(0.001)
N	17725	34713	107183	107183	50260	107183
Adj R^2	0.013	0.004	0.008	0.011	0.002	0.002
Control group mean			0.028	0.027	0.009	0.009
Percentage increase	80	78	94	124	81	67
Sample age range	[34, 45]	[34, 45]	[40, 89]	[40, 89]	[40, 89]	[40, 89]
Subsidy starting age	30	50	40	50		
Age controls			Y	Y	Y	Y

Notes: This table reports the cross spillover in take-up generated due to different subsidy starting age and different frequency of subsidies Column 1 and 2 report cross spillover at age 40 for cervical and colorectal screenings that are subsidized from age 30 and 50, respectively. The analytical sample is 6 years before and after 40, that is, [34, 45]. Ages are binned by 2 years and the outcome variable is 2-year average take-up. The econometric specification using regression discontinuity design (RD) is given in Equation (1). Column 3 to 6 report cross spillover for annually subsidized screenings (liver, colorectal) and unsubsidized screenings (prostate, lung). The analytical sample is those with age from 40 to 79. The outcome variable is the average take-up at each age. The econometric specification is given in Equation (2). The estimates are measured conditional on the linear splines of age with 5 years interval. Control group mean reports the average take-up for the odd age group in the age range [40, 79]. Standard errors are clustered at the individual level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table 6: Spousal spillover in screening take-up

	(1)	(2)	(3)	(4)
	Outcome var: Own screening take-up of any kind			
Age even	0.214***	0.212***	0.213***	0.212***
	(0.006)	(0.006)	(0.004)	(0.004)
Spouse age even	0.016***	0.016***		
	(0.005)	(0.005)		
Age even	0.001	0.002		
× Spouse age even	(0.009)	(0.009)		
Spouse screening			0.079***	0.081***
			(0.017)	(0.017)
N	79790	79790	79790	79790
Odd/Odd group mean	0.128	0.128	0.128	0.128
Age controls		Y		Y
Estimator	OLS	OLS	2SLS	2SLS

Notes: This table reports the spillover effect in screening take-up between spouses. Outcome variable is one's own screening take-up of any kind. The sample consists of currently married couples both of whose age is in [40, 89]. Odd/Odd group mean refers to the average take-up when both one's own and the spouse's ages are odd. Age control variables include linear splines of one's own age and the spouse's age in 5 years interval. In column 3 to 4, spouse screening variable is instrumented by spouse age even variable. Standard errors are clustered at the couple level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table 7: Compliers with subsidies comparing even and odd groups after age 40

	(1)	(2)	(3)	(4)	(5)	(6)
	Average value				Ratio	
	Always-takers	Treated compliers	Untreated compliers	Never-takers	CP_1/AT	CP_0/NT
Panel A. Diagnoses						
Stomach	0.174	0.293	-	-	1.684***	-
	(0.006)	(0.008)	-	-	(0.084)	-
Breast	0.018	0.022	-	-	1.228	-
	(0.003)	(0.003)	-	-	(0.354)	-
Cervical	0.067	0.061	-	-	0.906	-
	(0.007)	(0.006)	-	-	(0.154)	-
Colorectal	0.212	0.252	-	-	1.190	-
	(0.011)	(0.023)	-	-	(0.161)	-
Panel B. SES						
Individual income	1741	1037	1098	1341	0.596***	0.818***
	(48)	(47)	(45)	(36)	(0.029)	(0.027)
Household income	4985	4379	4425	4209	0.878***	1.051**
	(84)	(87)	(108)	(66)	(0.018)	(0.021)
Years of education	10.393	10.081	10.080	9.795	0.970***	1.029***
	(0.092)	(0.095)	(0.095)	(0.079)	(0.009)	(0.007)
College graduate	0.151	0.090	0.099	0.097	0.596***	1.021
	(0.009)	(0.008)	(0.008)	(0.007)	(0.057)	(0.069)
Working status	0.713	0.619	0.640	0.670	0.868***	0.954***
	(0.010)	(0.012)	(0.013)	(0.009)	(0.016)	(0.014)
Panel C. Health behaviors						
Current smoker	0.145	0.097	0.089	0.210	0.668***	0.425***
	(0.010)	(0.009)	(0.010)	(0.008)	(0.058)	(0.037)
Everyday smoker	0.136	0.094	0.082	0.202	0.690***	0.406***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.060)	(0.038)
Current drinker	0.663	0.623	0.613	0.631	0.939***	0.971*
	(0.011)	(0.012)	(0.013)	(0.010)	(0.017)	(0.016)
Everyday drinker	0.078	0.060	0.070	0.085	0.776***	0.825***
	(0.006)	(0.006)	(0.007)	(0.005)	(0.077)	(0.067)
Vigorous exercise	0.258	0.211	0.229	0.212	0.817***	1.085*
	(0.009)	(0.009)	(0.011)	(0.007)	(0.040)	(0.046)
Moderate exercise	0.440	0.407	0.409	0.373	0.925***	1.096***
	(0.010)	(0.010)	(0.013)	(0.008)	(0.027)	(0.034)
Walking	0.836	0.831	0.820	0.799	0.994	1.027*
	(0.008)	(0.008)	(0.011)	(0.006)	(0.012)	(0.014)

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. 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 (8). 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. 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.

Table 8: Compliers in cross spillover

	(1)	(2)	(3)
	Participants in screenings		
	Annual subsidy	No-subsidy	Sample mean
Panel A. Diagnoses			
Stomach	–0.028***	–0.087***	0.228
	(0.006)	(0.010)	
Breast	–0.007**	–0.020***	0.022
	(0.003)	(0.004)	
Cervical	–0.024***	–0.034**	0.067
	(0.006)	(0.015)	
Panel B. SES			
Individual income	874***	1499***	1592
	(49)	(110)	
Household income	1012***	1393***	4564
	(66)	(145)	
Years of education	0.975***	1.342***	10.769
	(0.073)	(0.129)	
College graduate	0.074***	0.131***	0.196
	(0.007)	(0.014)	
Working status	0.063***	0.141***	0.635
	(0.008)	(0.012)	

Notes: This table reports the relative characteristics of biennial screening participants who further participate in annual and no-subsidy screenings. Table 1 shows the list of biennial, annual and no-subsidy 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 screening and column 2 reports the difference for those who further receive unsubsidized screening. 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.

Table 9: Compliers in spousal spillover

	(1)	(2)	(3)	(4)
	Own age even		Own age odd	
Subsample	First stage	Relative likelihood	First stage	Relative likelihood
Entire sample	0.019***		0.016***	
	(0.007)		(0.005)	
Male	0.031***	1.623	0.019***	1.197
	(0.008)		(0.006)	
Working	0.023***	1.186	0.022***	1.364
	(0.008)		(0.006)	
College graduate	0.005	0.283	0.028**	1.767
	(0.015)		(0.012)	
Household income top 50%	0.009	0.461	0.025***	1.554
	(0.009)		(0.007)	

Notes: This table reports the characteristics of compliers in spousal spillover. The sample consists of currently married couples both of whose age is in [40, 89]. Column 1 and 2 examine compliers when own age is even and eligible for subsidies. Hence, the sample is restricted to individual-year with even age. Column 3 and 4 examine compliers when own age is odd and ineligible for subsidies. Hence, the sample is restricted to individual-year with odd age. Column 1 and 3 reports first stage coefficients for the entire sample and subsamples. Column 2 and 4 reports the relative likelihood a complier belongs to the given subsample. Following Angrist and Pischke (2009), the relative likelihood is given by the ratio of the first stage coefficient for the subsample to the overall first stage. Age controls of the spouse are included in the regressions. For age controls, linear splines of age with 5 years interval are used. Standard errors are clustered at the couple level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table 10: Effect on cancer diagnoses

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
Any cancer	0.0092	0.0017***	17.947	7,449,256
		(0.0001)		
Stomach cancer	0.0019	0.00079***	41.468	7,449,256
		(0.00006)		
Breast cancer	0.0047	0.0005***	10.918	3,503,656
		(0.0001)		
Cervical cancer	0.0015	0.00020***	13.073	3,503,656
		(0.00007)		
Liver cancer	0.0009	0.00014***	16.231	7,449,256
		(0.00004)		
Colorectal cancer	0.0019	0.00021***	10.688	7,449,256
		(0.00005)		
Lung cancer	0.0009	0.00014***	15.258	7,449,256
		(0.00004)		
Prostate cancer	0.0016	0.00010	6.472	3,945,600
		(0.00007)		

Notes: This table reports the treatment effect of health screening subsidies. 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 perecentage 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 11: Characteristics of cancer diagnoses

	(1)	(2)	(3)	(4)
	Odd age group	Even - Odd difference	Percent relative to odd	N
Panel A. Stomach cancer				
5 year survival rate	0.839	0.028***	3.307	16,777
		(0.007)		
Share of in-situ cancers	0.023	0.003	12.641	16,777
		(0.003)		
Panel B. Breast cancer				
5 year survival rate	0.911	0.016***	1.803	17,037
		(0.006)		
Share of in-situ cancers	0.138	0.025***	18.324	17,037
		(0.008)		
Panel C. Cervical cancer				
5 year survival rate	0.935	0.013	1.341	5,608
		(0.009)		
Share of in-situ cancers	0.500	0.064***	12.882	5,608
		(0.020)		
Panel D. Liver cancer				
5 year survival rate	0.554	0.019	3.419	6,934
		(0.017)		
Share of in-situ cancers	0.005	0.002	47.085	6,934
		(0.002)		
Panel E. Colorectal cancer				
5 year survival rate	0.832	0.015*	1.797	14,963
		(0.008)		
Share of in-situ cancers	0.112	0.001	1.265	14,963
		(0.007)		
Panel F. Lung cancer				
5 year survival rate	0.528	–0.009	–1.753	7,453
		(0.016)		
Share of in-situ cancers	0.005	–0.002	–36.604	7,453
		(0.002)		
Panel G. Prostate cancer				
5 year survival rate	0.834	–0.013	–1.504	6,215
		(0.013)		
Share of in-situ cancers	0.006	–0.001	–20.131	6,215
		(0.003)		

Notes: This table reports the characteristics of cancer diagnosis between the diagnoses made at even ages with the ones made at odd ages. The sample consists of cancer diagnosis made at age in [40, 89]. Column 1 reports the mean of the odd age (control) group cancer characteristics. Column 2 reports the difference in cancer characteristics of even age group compared to odd age group controlling for age. Column 3 reports the relative size of the difference (column 2) in percentage compared to the control group mean (column 1). Surgeries, inpatient days and spending refer to the number of surgeries, inpatient days and spending in the year of diagnosis. Standard errors are clustered at the individual level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table 12: Effect on mortality

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
1-year death	0.0032	–0.00005	–1.679	7,913,868
		(0.00004)		

Notes: This table reports the treatment effect of health screening subsidies. 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 perecentage 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 13: Effect on health care utilization for cancers

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
Outpatient visits for cancers	0.4015	0.010***	2.525	7,913,868
		(0.001)		
Inpatient days for cancers	0.3005	0.008***	2.562	7,913,868
		(0.002)		
Surgeries for cancers	0.0266	0.0029***	10.939	7,913,868
		(0.0001)		
Spending for cancers	156,649	3,877***	2.475	7,913,868
		(1,016)		

Notes: This table reports the treatment effect of health screening subsidies. 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 perecentage 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.

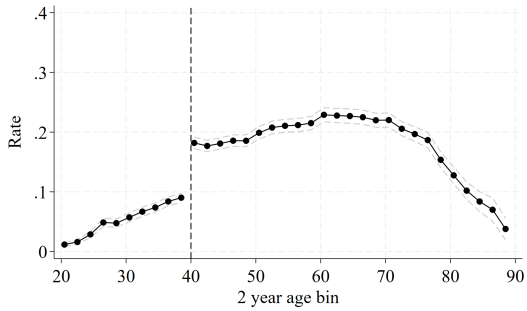
Table 14: Effect on total health care utilization

	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
Outpatient visits	13.4089	0.168***	1.252	7,913,868
		(0.005)		
Inpatient days	2.3401	0.024***	1.012	7,913,868
		(0.006)		
Surgeries	0.2361	0.0156***	6.621	7,913,868
		(0.0004)		
Total spending	908,473	14,213***	1.565	7,913,868
		(1,722)		

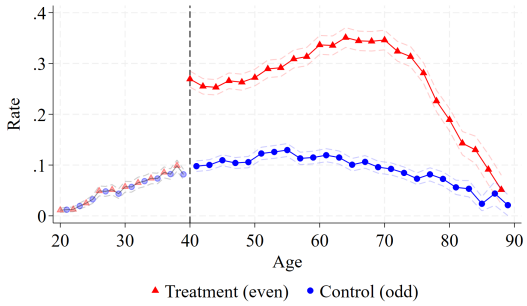
Notes: This table reports the treatment effect of health screening subsidies. The sample consists of individuals with age in [40, 89]. Column 1 reports the mean of the odd age (control) group. Column 2 ITT estimates report the effect of biennial subsidies on outcome variables by comparing even with odd age groups controlling for age. Column 3 reports the relative size of the ITT effect (column 2) in perecentage 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.

8 Figures

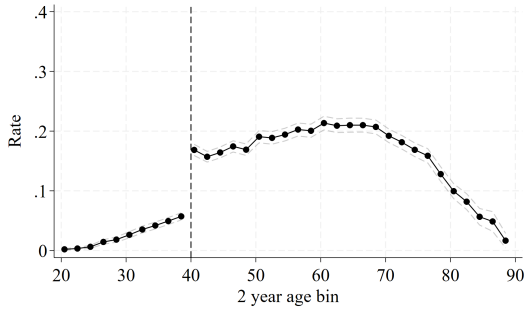
Figure 1: Screening rates for biennial screenings



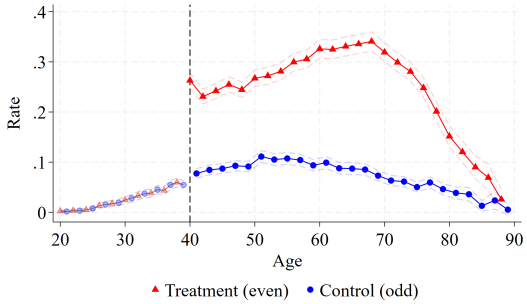
(a) General screening with 2 year age bins



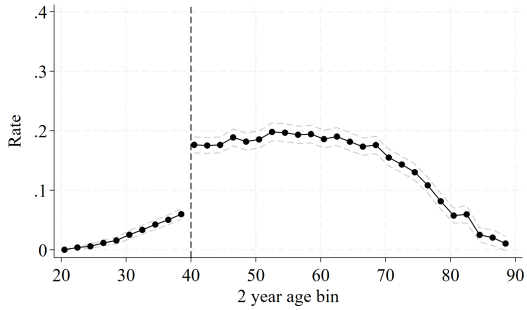
(b) General screening



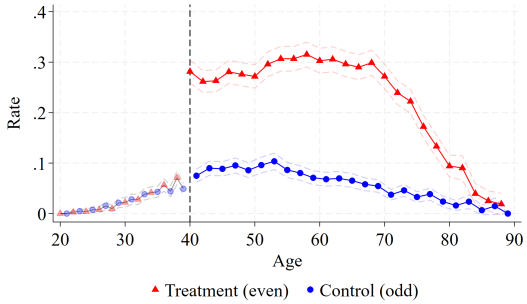
(c) Stomach screening with 2 year age bins



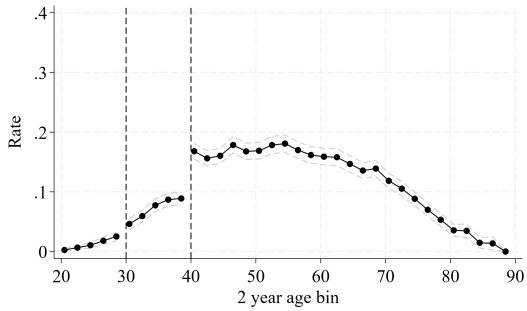
(d) Stomach screening



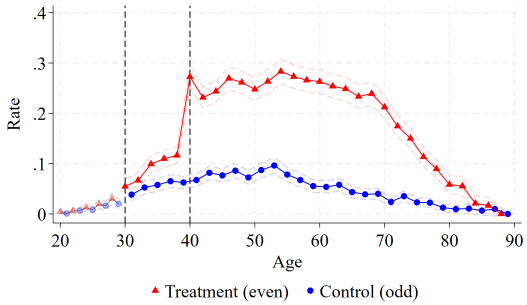
(e) Breast screening with 2 year age bins



(f) Breast screening



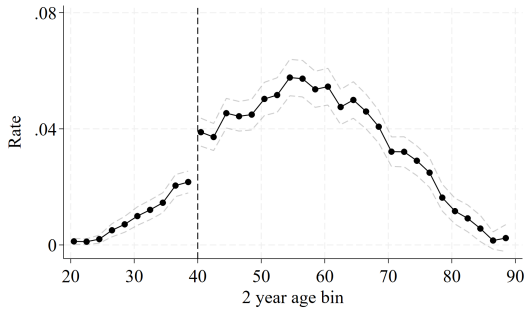
(g) Cervical screening with 2 year age bins



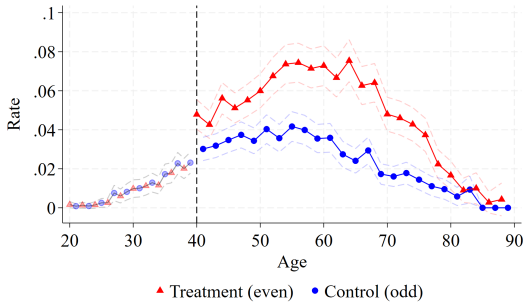
(h) Cervical screening

Notes: Figures show the take-up rate for 4 types of biennially subsidized screenings. Figures on the left side plot the average take-up for 2-year age bins, while the ones on the right side plot the average take-up for each age. In the right figures, even ages are colored in red and odd ages are colored in blue. Dashed vertical lines show the subsidy starting age and age 40. Only cervical screening has subsidy starting age at 30 and the rest starts from age 40. Confidence intervals at 95 percent are shown in dashed lines.

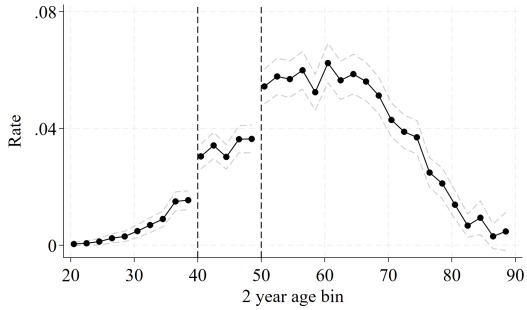
Figure 2: Screening rates for annual and no-subsidy screenings



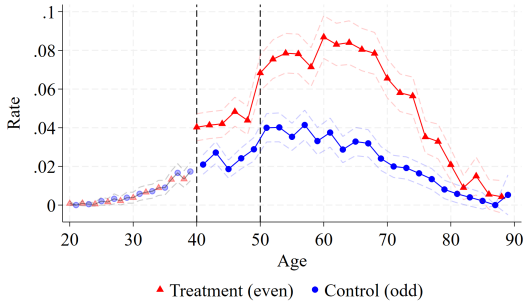
(a) Liver screening with 2 year age bins



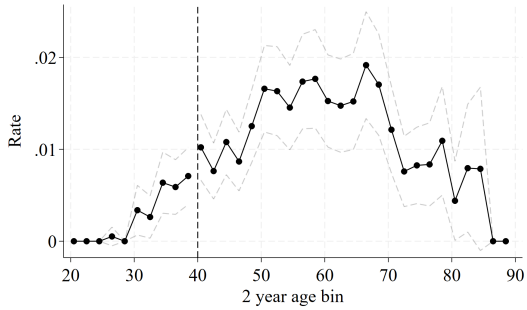
(b) Liver screening



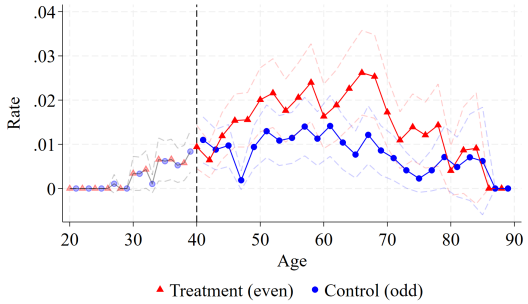
(c) Colorectal screening with 2 year age bins



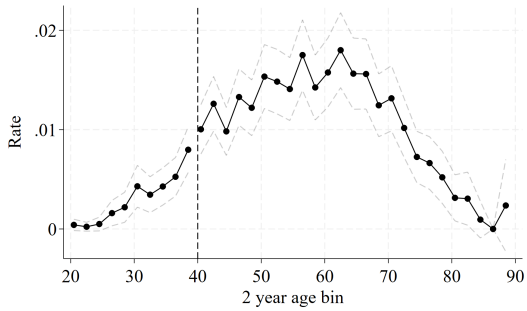
(d) Colorectal screening



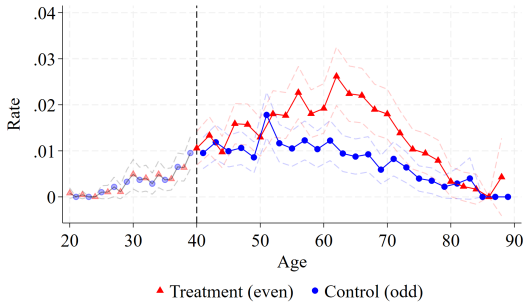
(e) Prostate screening with 2 year age bins



(f) Prostate screening



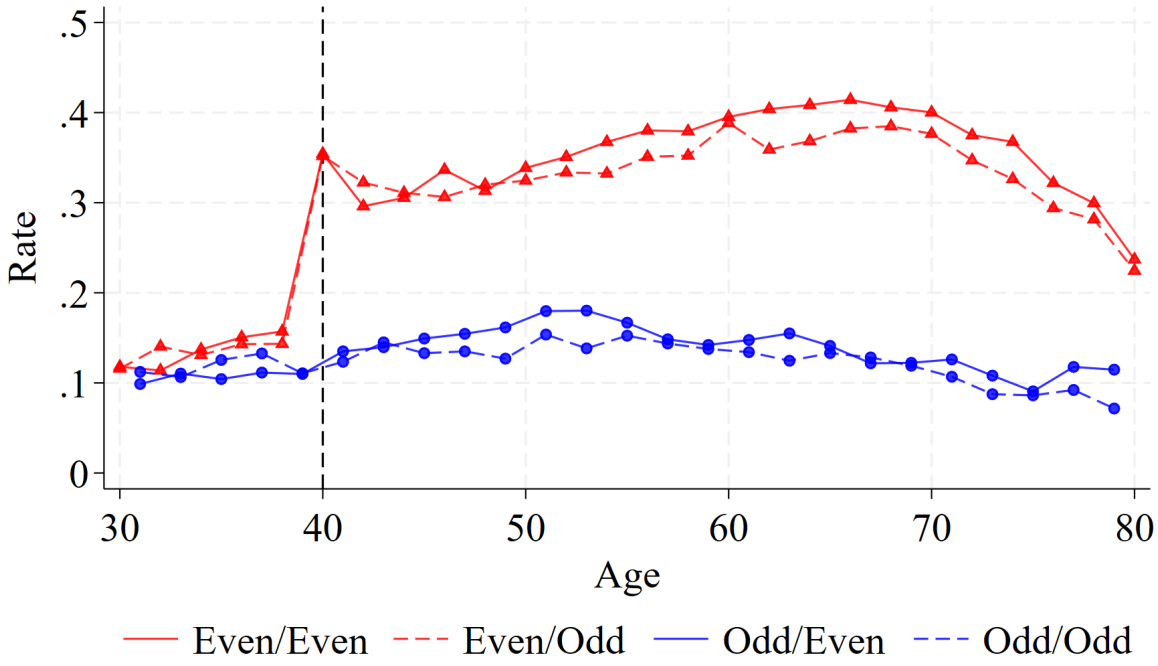
(g) Lung screening with 2 year age bins



(h) Lung screening

Notes: Figures show the take-up rate for liver and colorectal screenings subsidized annually and prostate and lung screenings without any subsidy. Figures on the left side plot the average take-up for 2-year age bins, while the ones on the right side plot the average take-up for each age. In the right figures, even ages are colored in red and odd ages are colored in blue. Dashed vertical lines show the subsidy starting age and age 40. Colorectal screening has subsidy starting age at 50 while the liver screening starts from age 40. Confidence intervals at 95 percent are shown in dashed lines.

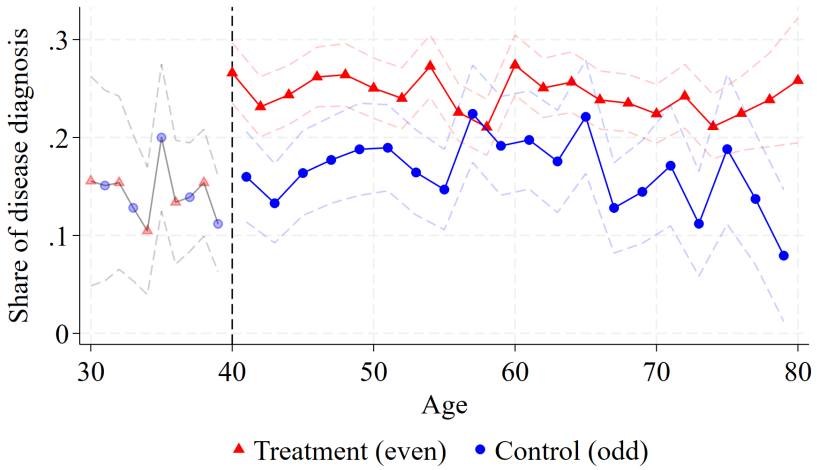
Figure 3: Spillover in screening take-up between spouses



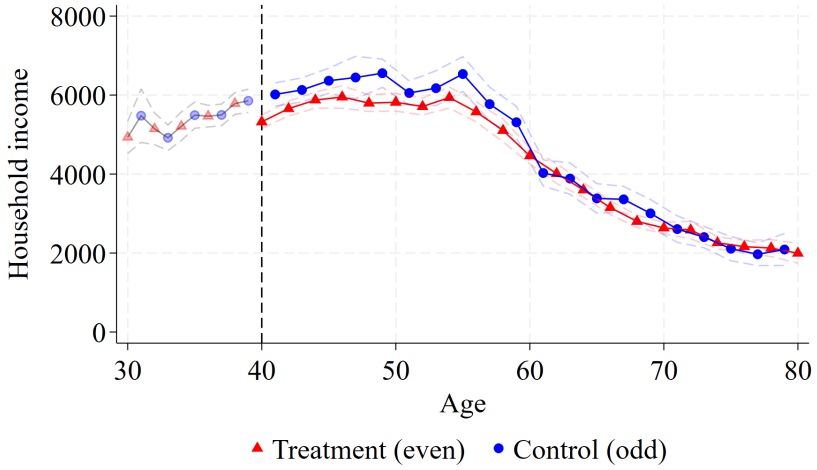
Notes: The figure plots the take-up of screening of any kind for 4 groups of the currently married people. The sample consists of individuals who are currently married and whose age is in the range [30, 80]. The legend Even/Odd refers to a group where one's own age is even, hence eligible for subsidies, and the spouse's age is odd, not eligible for subsidies. The other 3 groups are similarly defined. Those whose age is even are colored in red, while those whose age is odd are colored in blue. Those whose spouse's age is even are shown in solid lines, while those whose spouse's age is odd are shown in dashed lines.

Figure 4: Screening participants characteristics

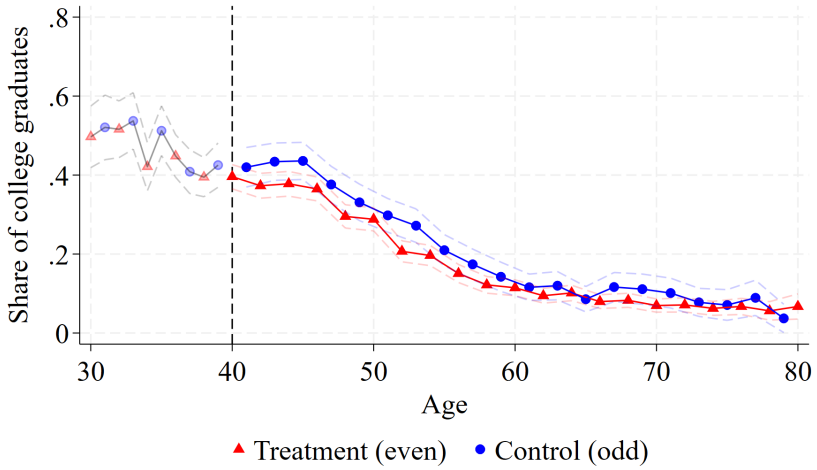
(a) Share of stomach disease diagnosis



(b) Household income



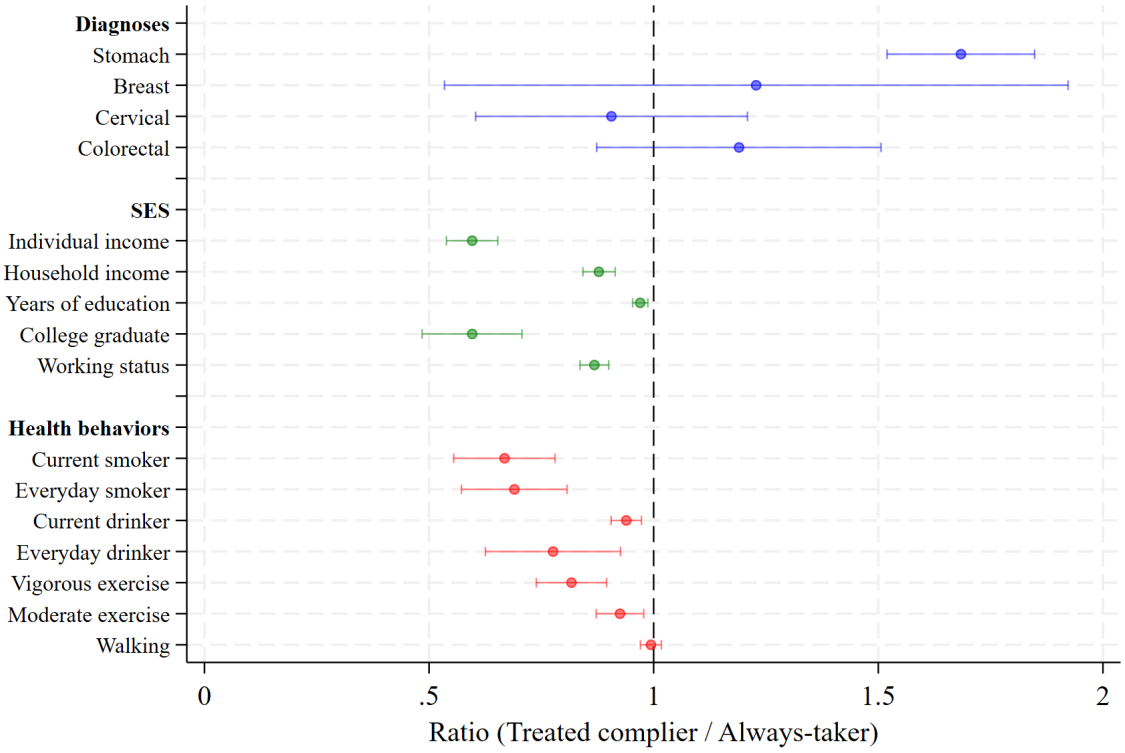
(c) Share of college graduates



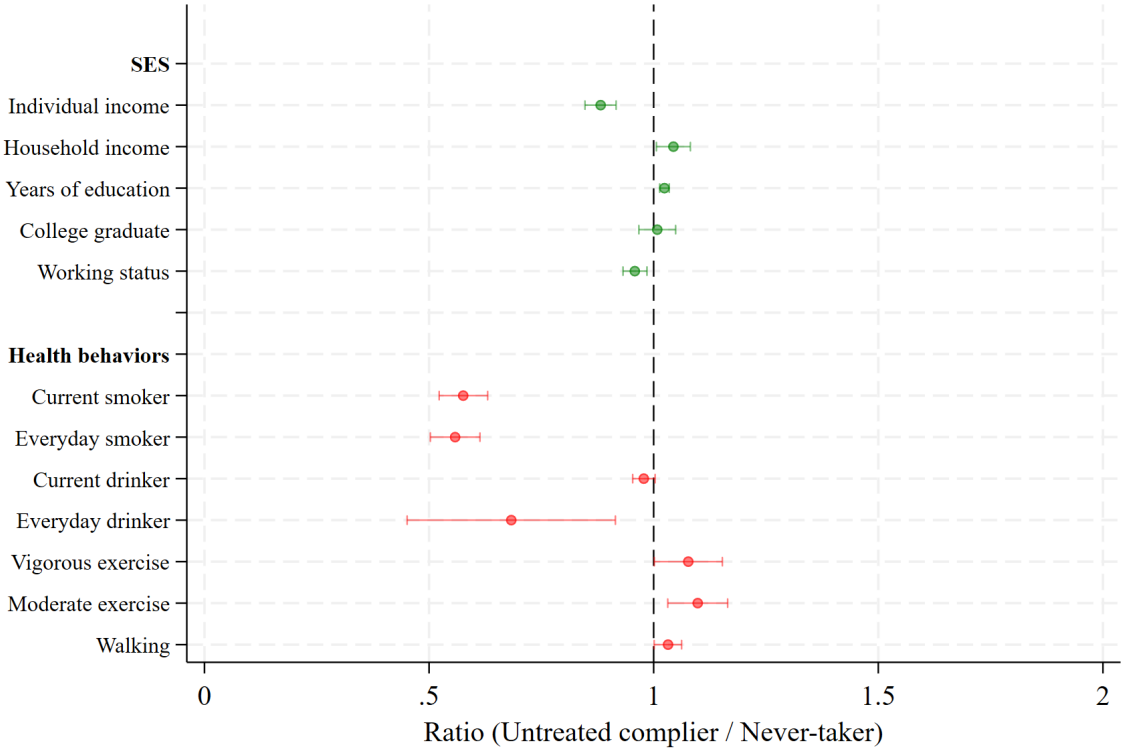
Notes: The first figure plots the share of stomach screenings where participants were diagnosed with a disease. The sample is restricted to stomach screening participants. The types of diseases found were coded using International Classification of Diseases Tenth Revision and some examples are reported in Appendix section F. The second and third figures plot average household income and the share of college graduates among participants in any type of screening. The unit of household income is 10,000 Korean Won. Even ages are colored in red and odd ages are colored in blue. Dashed vertical line shows the subsidy starting age at 40. Confidence intervals at 95 percent are shown in dashed line

Figure 5: Compliers characterization

(a) Comparing compliers with always-takers

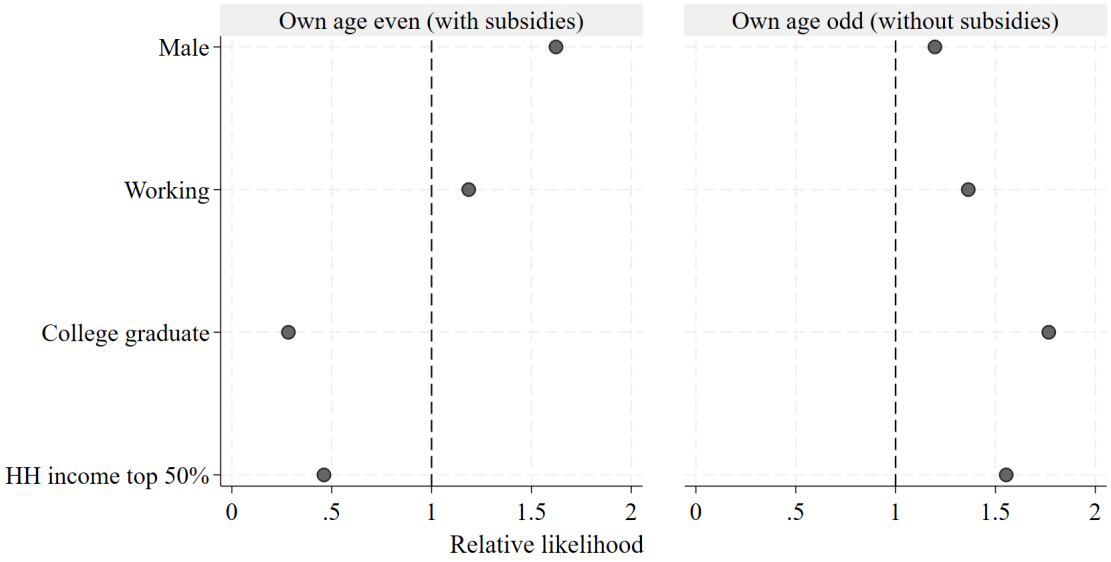


(b) Comparing compliers with never-takers



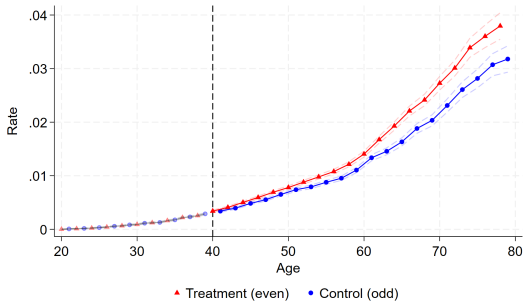
Notes: Figures plot the ratio of average characteristics of treated compliers to always-takers with 95 percent confidence intervals. Treated compliers are compliers in the treatment group who participate in screening. The average values and ratios are calculated from the estimation of equation (8) and are reported in Table 7. The standard errors are calculated using bootstrap with 500 replications clustered at the individual level. Diagnoses refer to whether a screening participant was diagnosed with or showed manifestations of a disease that require further examinations. Health behavior variables are dummy variables for engaging in such activities.

Figure 6: Relative likelihood of spousal spillover compliers

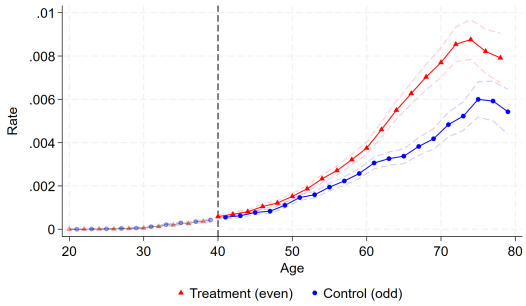


Notes: The figure plots the relative likelihood that spousal spillover compliers belong to a certain group. Following Angrist and Pischke (2009), the relative likelihood is given by the ratio of the first stage coefficient for the subsample to the overall first stage. They are calculated from the estimation of equation (5) across subsamples and are reported in Table 9. HH income top 50% refers to the group with above median household income.

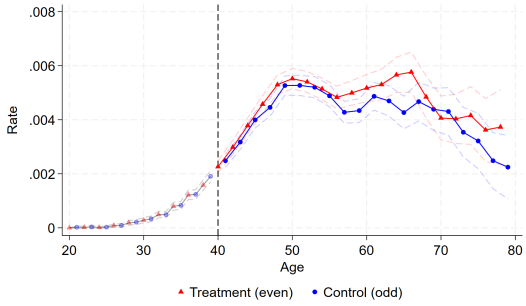
Figure 7: Cancer diagnoses by age



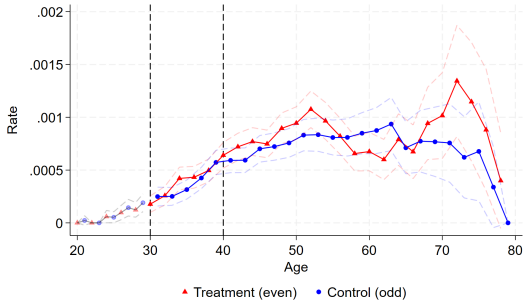
(a) Any cancer



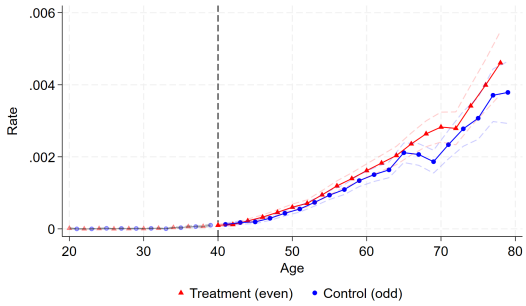
(b) Stomach cancer



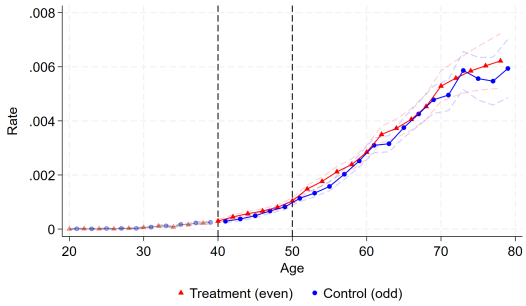
(c) Breast cancer



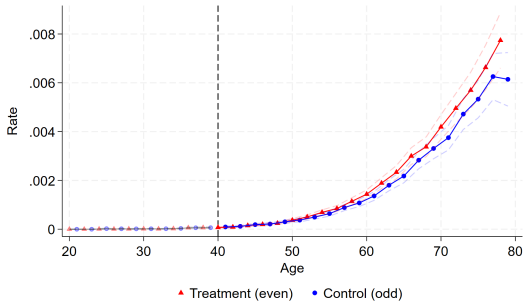
(d) Cervical cancer



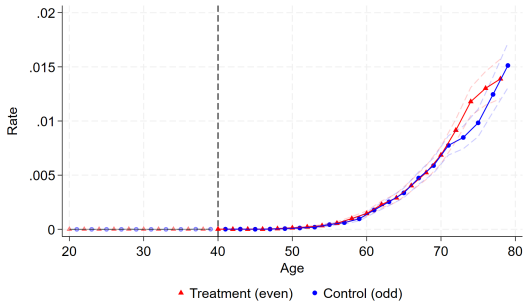
(e) Liver cancer



(f) Colorectal cancer



(g) Lung cancer



(h) Prostate cancer

Notes: Figures plot the cancer diagnoses rate by age separately for even age group (treatment) and the odd age group (control). Cancer diagnoses are identified from the Coinsurance Reduction Program for Rare and Severe Diseases and they include diagnoses made with and without screenings.

Appendix A Balance tests

Appendix presents robustness checks and auxiliary analyses not presented in the main paper. This section presents 4 additional tables on the balance test presented in section 3.

Table A1 presents the same balance table as in 2 with the additional unconditional differences between the even and odd age groups. Unconditional differences in column 3 are the difference between even and odd age groups, while the conditional differences in column 4 are the ones after adjusting for age. The age adjustment makes the point estimates for the differences smaller in absolute value and also reduces the standard errors.

Table A2 presents the balance table with additional functions of age as a robustness check. In addition to using linear splines of age with 5 years interval to adjust for age, it additionally presents estimates using 3 and 7 years intervals. The results are robust to the choice of age functions.

Table A3 presents the unconditional differences between even age odd age groups with samples of different starting ages: age [39, 89], [40, 89] and [41, 89]. As argued in section 3, the sample starting age is creating mechanical imbalance between the treatment and the control group. Using this finding, I later provide robustness checks for main results, running all the regressions without the age controls but with three different 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 A4 presents the balance table using the South Korea's national health insurance claims dataset. The difference between the even and odd groups are small and the age adjustment using linear splines with 5 years interval makes the conditional difference smaller.

Table A1: Balance test with unconditional and conditional differences

	(1)	(2)	(3)	(4)
	Even age group	Odd age group	Unconditional difference	Conditional difference
Age	58.697	59.240	–0.543***	-
	(12.532)	(12.353)	(0.026)	-
Female	0.530	0.532	–0.002**	–0.002*
	(0.499)	(0.499)	(0.001)	(0.001)
Currently married	0.799	0.798	0.0009	–0.0011
	(0.401)	(0.402)	(0.0009)	(0.0008)
Years of education	10.320	10.227	0.093***	–0.003
	(4.510)	(4.538)	(0.009)	(0.008)
Working status	0.610	0.608	0.001	–0.003*
	(0.488)	(0.488)	(0.002)	(0.001)
Individual income	1446.3	1425.7	20.607***	2.762
	(2081.6)	(2068.1)	(5.508)	(5.185)
Household income	4104.4	4086.7	17.735	3.221
	(3708.6)	(3737.9)	(14.555)	(14.267)
Own a house	0.734	0.737	–0.002*	–0.0002
	(0.442)	(0.441)	(0.001)	(0.0011)
Number of household members	3.067	3.051	0.016***	–0.004
	(1.317)	(1.317)	(0.003)	(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 test with different age controls

	(1)	(2)	(3)
	3 years	5 years	7 years
Female	–0.002*	–0.002*	–0.002*
	(0.001)	(0.001)	(0.001)
Currently married	–0.001*	–0.001	–0.001
	(0.001)	(0.001)	(0.001)
Years of education	–0.003	–0.003	–0.003
	(0.008)	(0.008)	(0.008)
Working status	–0.003**	–0.003*	–0.003*
	(0.002)	(0.001)	(0.001)
Individual income	1.1	2.8	1.2
	(5.3)	(5.2)	(5.2)
Household income	0.6	3.2	–4.5
	(15.4)	(14.3)	(14.1)
Own a house	–0.000	–0.000	0.000
	(0.001)	(0.001)	(0.001)
Number of household members	–0.004	–0.004	–0.004*
	(0.003)	(0.003)	(0.003)

Notes: This table reports the difference between the treatment group (even age group) and the control group (odd age group) conditional on linear splines of age with 3, 5 and 7 years interval. The sample consists of those with age in [40, 79]. Standard errors are clustered at individual level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table A3: Bounding estimates for balance test

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

Notes: This table reports the balance check between the treatment group (even age group) and the control group (odd age group) using samples with different starting age (39, 40, 41). It does not include any control variable. The coefficients report the average value of the treatment group relative to the control group. Standard errors are clustered at individual level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table A4: Balance test with NHIS data

	(1)	(2)	(3)	(4)
	Treatment (even)	Control (odd)	Unconditional difference	Conditional difference
Panel A. Demographics				
Age	51.855	52.493	–0.638***	-
	(8.989)	(8.876)	(0.002)	-
Female	0.471	0.469	0.003***	0.0002***
	(0.499)	(0.499)	(0.000)	(0.0000)
Panel B. Health insurance				
Insurance premium (KRW)	100,067	100,694	–627***	65**
	(106,080)	(106,994)	(31)	(31)
Self-employed insurance: head	0.205	0.207	–0.002***	–0.0003***
	(0.404)	(0.405)	(0.000)	(0.0001)
Self-employed insurance: dependent	0.147	0.147	0.0008***	0.00010
	(0.355)	(0.354)	(0.0001)	(0.00010)
Employee insurance: head	0.389	0.385	0.003***	0.0007***
	(0.487)	(0.487)	(0.000)	(0.0001)
Employee insurance: dependent	0.237	0.238	–0.001***	–0.0005***
	(0.425)	(0.426)	(0.000)	(0.0001)
Medical aid insurance: head	0.016	0.016	–0.0006***	0.00003
	(0.124)	(0.126)	(0.0000)	(0.00003)
Panel C. City of residence				
Living in a metropolitan city	0.451	0.452	–0.0002***	0.0003***
	(0.498)	(0.498)	(0.0001)	(0.0001)
Population/1,000	420.546	419.851	0.695***	–0.048
	(261.301)	(261.662)	(0.046)	(0.043)
Panel D. Work				
Working	0.608	0.606	0.002***	0.0003*
	(0.488)	(0.489)	(0.000)	(0.0001)
Agriculture, forestry and fishery	0.004	0.004	–0.00002	0.00003
	(0.065)	(0.065)	(0.00003)	(0.00003)
Manufacturing	0.363	0.361	0.002***	–0.0003*
	(0.481)	(0.480)	(0.000)	(0.0002)
Panel E. Disability				
Have disability	0.057	0.059	–0.002***	0.00001
	(0.231)	(0.235)	(0.000)	(0.00003)
Disability grade	5.971	5.998	–0.028***	0.00005
	(4.816)	(4.849)	(0.003)	(0.00320)
External physical disability	0.898	0.899	–0.0010***	–0.0002
	(0.302)	(0.301)	(0.0002)	(0.0002)
Internal physical disability	0.060	0.060	–0.00001	0.00002
	(0.238)	(0.238)	(0.00018)	(0.00018)
Developmental disability	0.019	0.018	0.0008***	0.0003***
	(0.136)	(0.133)	(0.0001)	(0.0001)
N	4,004,921	3,836,454		
Share	(0.49)			
F-statistic				F(17, 567301) = 5.17
Prob > F				0.000

Notes: This table reports the 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, 79]. Column 3 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 Effect of subsidies on take-up

This section provides 2 robustness checks for the first stage regression results shown in Table 4. Table A5 presents the same estimates using different age functions: linear splines of age with 3 and 7 years interval. Additionally, I present regression results with additional control variables. Given age controls are enough to guarantee balance, no more control variables are needed for internal validity. However, they can be useful for increasing precision of the estimates. I present the estimation results using linear splines of age with 5 years interval and additionally full control variables shown in the balance table, Table 2, or individual fixed effect terms. These additional control variables should not affect the point estimates.

Table A6 presents the bounding estimates of the first stage regressions. They are the unconditional differences between the even and odd age groups on the three samples: age [39, 89], [40, 89] and [41, 89]. Given that the imbalances run in opposite direction, the resulting estimates provide nonparametric bounds for the effect of subsidies on take-up without the need to specify a functional form for the age control variables.

Table A5: First stage regressions with different controls

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

Notes: This table reports the effect of biennial subsidy on 4 types of screening take-up with different control variables. Screenings reported in column 1 to 4 are subject to biennial subsidy when ages are even. 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: Bounding estimates for the first stage regressions

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

Notes: This table reports first stage using samples with different starting age (39, 40, 41). It does not include any control variable. The coefficients report the effect of subsidy on screening take-up. Standard errors are clustered at individual level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Appendix C Intertemporal substitution

This section presents further descriptive analyses on intertemporal substitution. The screening schedule that provides subsidies at even ages gives individuals an incentive to shift screening timing from odd to even ages when one is eligible for subsidies. The goal of this section is to find evidence of intertemporal substitution at two different margins: (*i*) around age 40 cutoff and (*ii*) January and December where ages change from odd to even or even to odd.

First, I focus on cohorts around age 40 to examine how screening take-up changes as one passes age 40 threshold and becomes eligible for subsidies. A sufficient sign of intertemporal substitution would be drop in take-up at odd ages after age 40. However, as new people start to participate from age 40, regardless of even or odd, this opposing force could make it hard to detect a drop in screening rate. Therefore, I focus on those who were already participating before 40 to examine if they exhibit any drop at odd ages after 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.[43] Comparing before and after 40, one can see that take-up at odd ages after 40 is not any lower than pre-40 take-up level, but the take-up at even ages are clearly much higher. This suggests that as one passes age 40, take-up at even ages rise due to subsidies and it does not come at the cost of drop in take-up at odd ages.

Next, I examine monthly distribution of take-up in stomach and breast screening. If there were intertemporal substitution, then it would be most pronounced in January or December of the year when ages change. One can get screening a couple of weeks early and receive it in December of even age instead of January of odd age. Similarly, one can delay it a couple of weeks and receive it in January of even age instead of December of

[43] 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.

odd age. Hence, we expect lower number of screenings in January and December of odd age compared to months in the middle.

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 the underlying distribution of monthly take-ups 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.

$$\begin{aligned} screen_{imt} =& \theta_0 + \theta_1 \cdot after40_{imt} + \theta_2 \cdot age\_even_{imt} + \sum_{m=2}^{12} month_m \\ &+ \theta_3 \cdot after40_{imt} \cdot age\_even_{imt} + \sum_{m=2}^{12} month_m \cdot after40_{imt} \\ &+ \sum_{m=2}^{12} month_m \cdot age\_even_{imt} + \sum_{m=2}^{12} month_m \cdot after40_{imt} \cdot age\_even_{imt} + \varepsilon_{imt} \end{aligned} \tag{6}$$

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 category. 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 clsutered 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 ages before 40. There should be no systematic difference, since it is 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 ages 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 ages before and after 40.

The first set of figures in each panel, Figure A2a and A2e present the average number of stomach and breast screening take-ups for even and odd ages in an analytical sample

of ages [40, 89], where everyone is eligible for biennial subsidies. For easy comparison, I present even ages again after the end of odd ages. The take-up is larger at even ages across all months. While take-up at odd ages show little variation across months, take-up at even ages exhibit an upward trend from January to December, with an additional local spike in March.

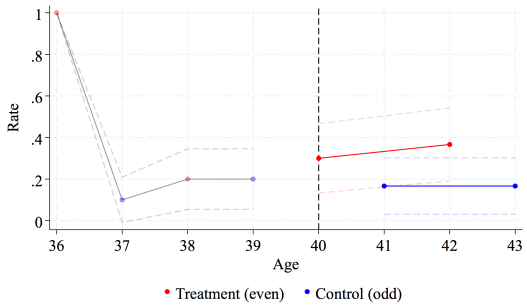
The second set of figures in each panel, Figure A2b and A2f provide comparison of monthly take-up between even and odd ages for ages [20, 39] before subsidies apply. These are the coefficients of the term $age\_even_{imt} + \sum_{m=2}^{12} month_m \cdot age\_even_{imt}$. For both stomach and breast screenings, there is no systematic difference in monthly take-up between even and odd ages in this younger age group, consistent with the absence of subsidies during this period.

The third set of figures in each panel, Figure A2c and A2g present the comparison of monthly take-up of odd ages before and after age 40. These are coefficients of the term $\sum_{m=2}^{12} month_m \cdot after40_{imt}$. The results reveal no systematic monthly variation in take-up. If substitution effects were particularly pronounced in January and December, we would expect lower growth in these months compared to other months in the middle. However, no such pattern is observed.

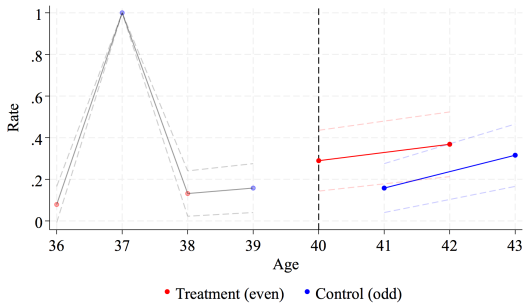
The fourth set of figures in each panel, Figure A2d and A2h present the comparison of monthly take-up of even ages before and after age 40. These are coefficients of the term $\sum_{m=2}^{12} month_m \cdot after40_{imt} + \sum_{m=2}^{12} month_m \cdot after40_{imt} \cdot age\_even_{imt}$. As shown in the figure figures, there is a noticeable increase in March and December, compared to January. 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 are also eligible for certain screenings, like general, liver or colorectal screenings. The large spike in December suggests substitution behavior, as individuals with even ages rush to complete their screenings before the year ends.

Figure A1: Screening take-up for participants before 40

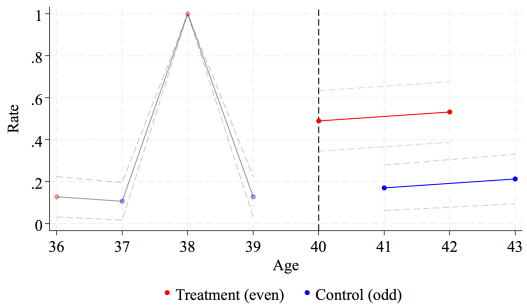
Panel A. Stomach screening participants



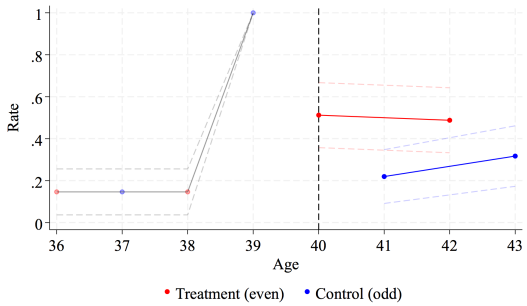
(a) Age 36 participants



(b) Age 37 participants

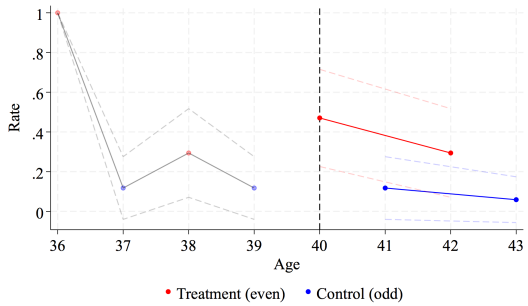


(c) Age 38 participants

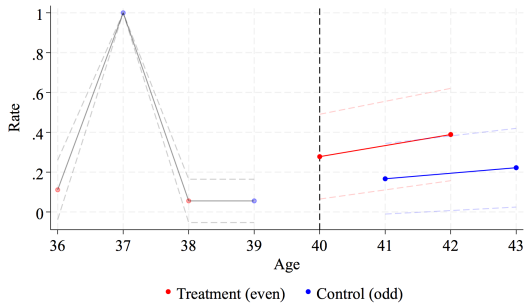


(d) Age 39 participants

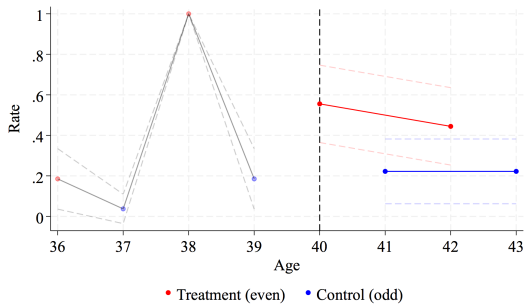
Panel B. Breast screening participants



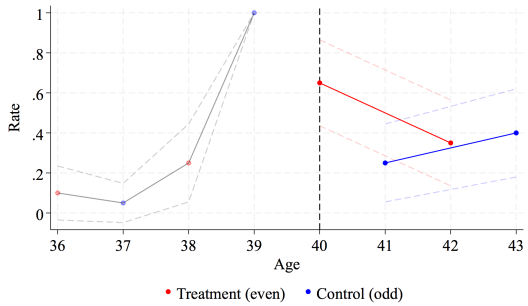
(e) Age 36 participants



(f) Age 37 participants



(g) Age 38 participants

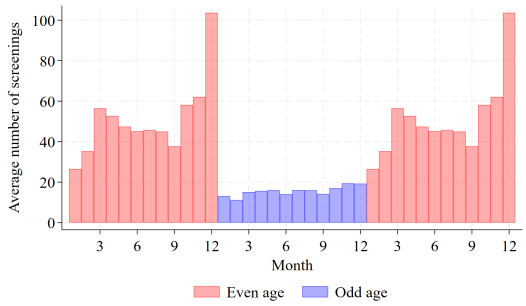


(h) Age 39 participants

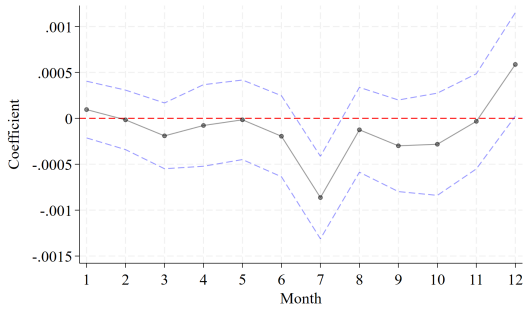
Notes: Figures plot the stomach and breast screening take-up for those who already participate in screening before age 40. The sample is restricted to four age cohorts around age 40. Each figure plots the take-up among either the stomach or breast cancer participants at age 36, 37, 38 or 39. Even ages 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

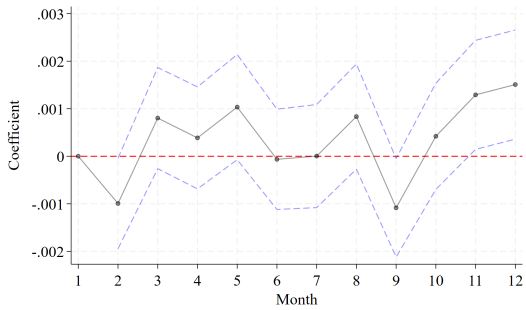
Panel A. Stomach screening



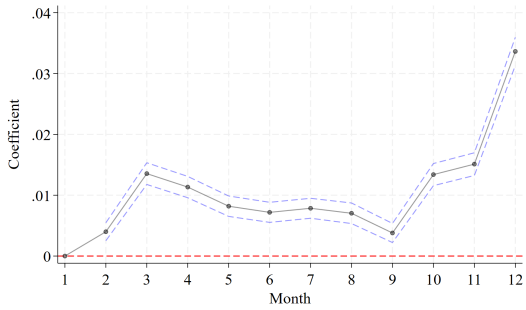
(a) Average monthly take-up



(b) Even vs odd age take-up before 40

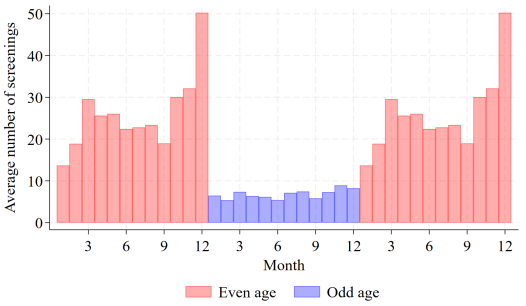


(c) Odd age take-up before and after 40

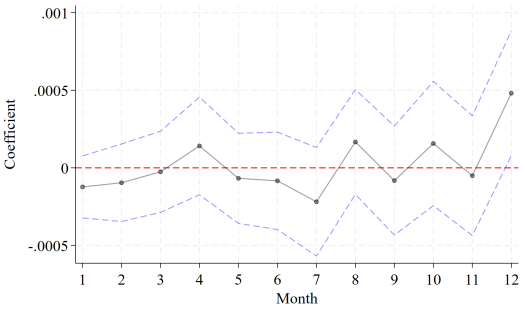


(d) Even age take-up before and after 40

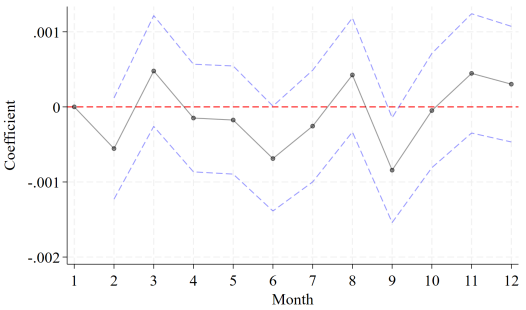
Panel B. Breast screening



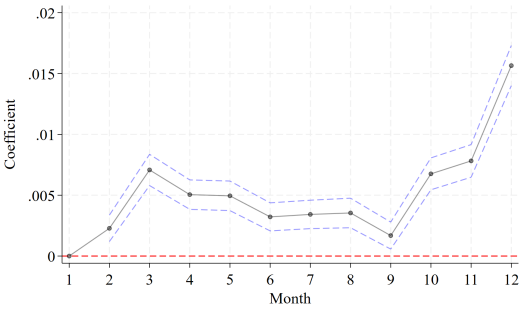
(e) Average monthly take-up



(f) Even vs odd age take-up before 40



(g) Odd age take-up before and after 40



(h) Even age take-up before and 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 at even and odd ages. Even age group is repeated after odd age to provide an easy comparison. Second figures plot the monthly differences in take-up between even and odd age group before age 40. Third figures plot the monthly differences in take-up in odd age group before and after age 40 using January as the reference month. Fourth figures plot the monthly differences in take-up in even age group before and after age 40 using January as the reference month.

Appendix D Cross spillover

This section presents 4 additional analyses on the cross spillover. Table A7 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 A8 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 A8 does not support this hypothesis. If anything, they seem to be slightly smaller for the 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.

Table A9 presents robustness checks for cross spillover by using different age control functions or controlling for additional variables. The robustness in resulting estimates suggests that the results are not driven by a choice of a particular functional form. It also shows that the age difference between even and odd age groups are the only difference between the two groups.

Table A10 presents bounding estimates for cross spillover by comparing the take-up between even and odd age groups without any age adjustment. The unconditional differences estimated on three samples are similar and they provide nonparametric bounds for the effect of subsidies.

Figure A3 presents the results of difference-in-differences analysis using the change in colorectal screening subsidy frequency. Before 2012, colorectal screenings were subsidized

bienially at even ages from age 50. Starting from 2012, the subsidies became annual from age 50. To examine how take-up changed, I estimate the following econometric specification among those with ages 50 to 89.

$$y_{it} = \theta_0 + \theta_1 \cdot age\_even_{it} + \theta_2 \cdot After_{it} + \theta_3 \cdot age\_even_{it} \times After_{it} + \varepsilon_{it} \tag{7}$$

The outcome variable y_{it} is colorectal screening take-up for individual i in year t. The variable $After_{it}$ is an indicator variable that equals to 1 when $t \geqslant 2012$. Standard errors are clustered at the individual level.

The estimate for θ_1 was 0.03 and significant at 1 percent. It means that even age group showed 3 percentage point larger take-up than odd age group before 2012 when subsidies were biennial. The difference between even and odd age groups after 2012 is estimated by $\theta_1 + \theta_3$, which was equal to 0.04. Given that the policy change started offering subsidies both at even and odd ages, the take-up gap between the two groups should decrease after 2012. However, it increased. The difference between the two differences before and after 2012, estimated by θ_3, was 0.02 and significant at 1 percent. This implies that subsidies additionally provided at odd ages did not increase take-up at odd ages.

Table A7: Share of screenings by screening date

	(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 liver, colorectal, lung and prostate screening in each column. General first (later) means the screening concerned is received after (before) the general screening within 30 days.

Table A8: Gender difference in cross spillover

	(1)	(2)	(3)
	Liver	Colorectal	Lung
Age even	0.025***	0.036***	0.007***
	(0.002)	(0.002)	(0.001)
Age even × Female	0.003	–0.005*	–0.002
	(0.003)	(0.003)	(0.001)
Female	–0.017***	–0.012***	–0.0078***
	(0.002)	(0.002)	(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. Those with even age are entitled to free general screening and 90% subsidized stomach screening. Women are additionally entitled to subsidized breast and cervical screenings. Liver screening is subsidized every year from age 40. Colorectal screening is subsidized every year from age 50. Lung screening is not subsidized. Standard errors are clustered at individual level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table A9: Cross spillover with different controls

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

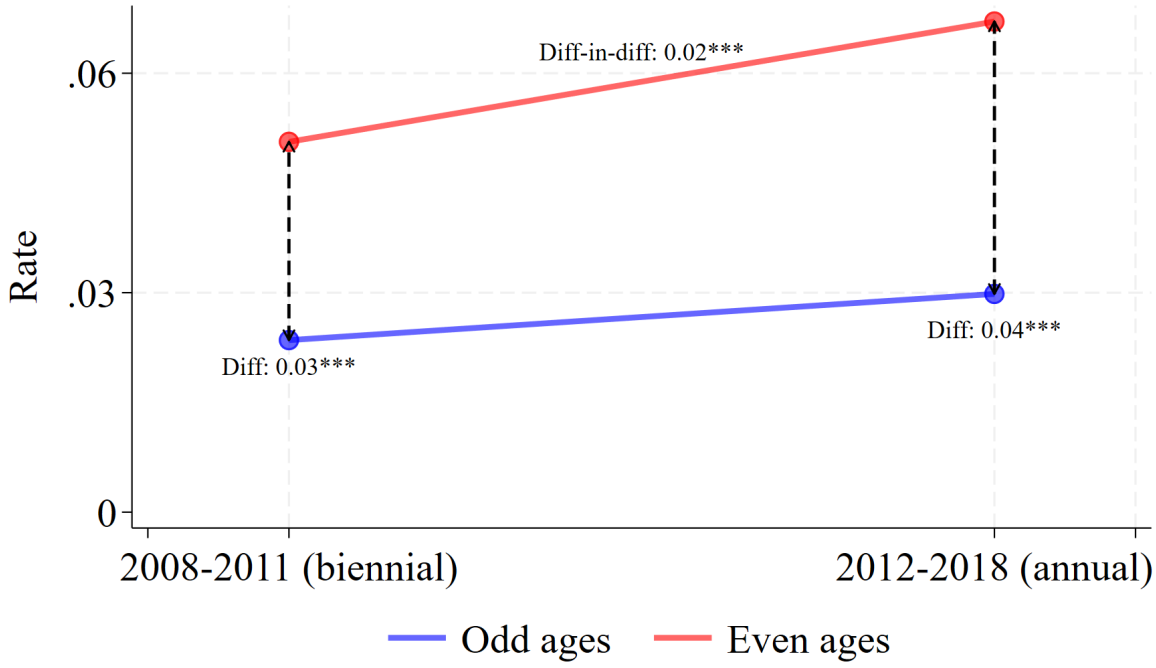
Notes: This table reports the effect of biennial subsidy on 4 types of screening take-up with different control variables. Liver and colorectal screenings are subject to annual subsidies, while prostate and lung screenings are not subsidized. Panel A uses linear splines of age with 3, 5, and 7 years 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.

Table A10: Bounding estimates for cross spillover

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

Notes: This table reports the ITT estimates of cross spillover results using samples with different starting age (39, 40, 41). It does not include any control variable. The coefficients report the effect of biennial subsidy on the take-up of annual-subsidy and no-subsidy screenings. Standard errors are clustered at individual level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Figure A3: Change in colorectal screening subsidy frequency



Notes: The figure plots the take-up of colorectal screening for even and odd ages before and after 2012. The subsidy frequency for colorectal screenings changed from biennial to annual in the beginning of year 2012. Before 2012, subsidies were available only at even ages from age 50 but from 2012, it became available at every ages from age 50. The difference coefficients show the difference in take-up between even and odd ages before and after 2012. The diff-in-diff coefficient show the difference in the take-up gap coefficients.

Appendix E Spousal spillover

This section presents 3 auxiliary analyses on spousal spillover. Table A11 presents the spousal spillover, separately for male and female. It shows that the spousal spillover is stronger from wives to husbands than husbands to wives.

Table A12 presents the probability of couples receiving screening on the same day. Conditional on both wife's and husband's participation, more than 40 percent receive screening on the same day. This is more likely to be the case when both the husband and the wife are eligible for subsidies or when both are ineligible for subsidies.

Table A13 presents the estimates of spillover effect for each screening type. It shows that spousal spillover exists in most screenings, but they are muted in liver, lung, breast, cervical and prostate screenings. This could be because liver screening is subsidized only for high risk group. So, unless both the husband and the wife are both high risk group, they are less likely to be received together. Another reason is that they are gender specific screenings. Hence, so they are less likely to be received together.

Table A11: Spousal spillover directions

	(1)	(2)	(3)	(4)
	Among wives (husband ⇒ wife)		Among husbands (wife ⇒ husband)	
Age even	0.260***	0.258***	0.168***	0.167***
	(0.005)	(0.005)	(0.005)	(0.004)
Spouse age even	0.009*		0.025***	
	(0.005)		(0.005)	
Spouse screening		0.056*		0.097***
		(0.030)		(0.017)
N	39895	39895	39895	39895
Odd/Odd group mean	0.134	0.134	0.122	0.122
Age controls	Y	Y	Y	Y
Estimator	OLS	2SLS	OLS	2SLS

Notes: This table reports the direction of spousal spillover in screening take-up. Outcome variable is the take-up in any kind of screening. The sample consists of currently married couples both of whose age is in [40, 89]. Column 1 and 2 examine spousal spillover among married females, while column 3 and 4 examine spillover among married males. In column 2 and 4, spouse screening variable is instrumented by spouse even age variable. Age controls of both oneself and the spouse are included in the regressions. For age controls, linear splines of age with 5 years interval are used. Standard errors are clustered at the couple level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Table A12: Spousal spillover: share of same day screenings

	(1)	(2)	(3)	(4)	(5)
	Total	Even/Even	Even/Odd	Odd/Even	Odd/Odd
Pr(same day | both participate)	0.528	0.570	0.433	0.433	0.652
Pr(Spouse first | both participate)	0.139	0.149	0.134	0.127	0.121
Pr(Spouse later | both participate)	0.139	0.149	0.127	0.134	0.121

Notes: This table reports the share of spouses getting screening on the same day given that both participate in screening in a year. Take-up before or after spouse considers 30 day window before and after the screening day.. Column 1 reports the share of the couples who participate in a same year. Columns 2 to 5 reports the same share of couples by each even and odd combination.

Table A13: Spousal spillover by screening types

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	General	Stomach	Liver	Colorectal	Lung	Breast	Cervical	Prostate
Age even	0.195***	0.198***	0.028***	0.037***	0.008***	0.208***	0.185***	0.008***
	(0.005)	(0.005)	(0.003)	(0.003)	(0.001)	(0.006)	(0.006)	(0.002)
Spouse age even	0.014***	0.016***	0.002	0.006***	0.002*	0.009*	0.005	–0.0007
	(0.004)	(0.004)	(0.002)	(0.002)	(0.001)	(0.004)	(0.004)	(0.0015)
Age even	0.002	0.004	0.001	–0.001	–0.003	–0.007	–0.008	0.001
× Spouse age even	(0.008)	(0.008)	(0.005)	(0.004)	(0.002)	(0.009)	(0.008)	(0.003)
Age controls	Y	Y	Y	Y	Y	Y	Y	Y
N	79790	79790	79790	79790	79790	39895	39895	39895

Notes: This table reports the spousal spillover effect for different types of screenings. Outcome variable is one's own screening take-up. The sample consists of currently married couples both of whose age is in [40, 89]. Age controls of both oneself and the spouse are included in the regressions. For age controls, linear splines of age with 5 years interval are used. Standard errors are clustered at the couple level and reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Appendix F Specific disease diagnoses

This section provides a list of ICD-10 disease diagnosis codes that were reported as a disease found through screenings. 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).[44] The key idea is to infer always-takers' characteristics from those who participate in screening in the control group and infer never-takers' characteristics from those who do not participate 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 (non-participants) 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 vs odd design. [45] I estimate the following equation to estimate group characteristics.

$$y_{it} = \lambda_1 \cdot age\_even_{it} + \lambda_2 \cdot screen_{it} + \lambda_3 \cdot age\_even_{it} \times screen_{it} + \boldsymbol{\lambda_4'}\mathbf{f}(\mathbf{age_{it}}) + \varepsilon_{it} \quad (8)$$

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 treatment group as a weighted sum with always-takers or the screening non-participants

[44] Marbach and Hangartner (2020) gives a nice summary of the methodology.

[45] Appendix in Einav et al. (2020) present the detailed steps to characterize compliers at age 40 in the regression discontinuity setting.

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 π_{AT} and π_C are share of always-takers and compliers, respectively. Imposing $age\_even_{it} = 1$ and $screen_{it} = 1$, we get $g_T(y) = \hat{\lambda_1} + \hat{\lambda_2} + \hat{\lambda_3} + \hat{\boldsymbol{\lambda_4}}' \mathbf{f}(\mathbf{age_{it}})$. The share of always-takers and compliers can be calculated from the first stage regression given in (2) as $\pi_C = \hat{\beta}_1$, $\pi_{AT} = \hat{\beta}_0$ adjusting for age.[46] 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.[47]

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, $\frac{g_C^0(y)}{g_{NT}(y)}$.[48] 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.

[46] Under monotonicity, the share for never-takers is $\pi_{NT} = 1 - \pi_{AT} - \pi_C = 1 - \hat{\beta}_0 - \hat{\beta}_1$

[47] I examine selection pattern at age 60 by imposing $age_{it} = 60$. The results are robust to the different choices of age.

[48] 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 Selection using panel information in even-odd design

This section provides an alternative way of characterizing compliers taking advantage of the unique feature of the setting: the repeated experiments. Biennial subsidy rule provides screening subsidies when one's age is even and this affects screening take-up pattern of not only the biennial screenings, but also annual and no-subsidy screenings. Compliers are the ones who respond to subsides and participate in screening when subsidies are provided but do not participate when there is no subsidy. Hence, another way, or perhaps a more intuitive way, to define compliers is to use the history of 10 years information altogether and find those who participated in even ages and not in odd ages. Other compliance groups can be defined similarly. Always(Never)-takers are the ones who (do not) participate both at even and odd ages. Defiers can be defined as ones who participate at odd ages but do not participate at even ages.

An advantage of the alternative definition is that it allows checking if the compliance behavior holds over time. The compliance group defined originally in Imbens and Angrist (1994) and Angrist et al. (1996) is based on potential outcome framework and does not take into account any panel structure. In our setting, we have the same experiments happening over time and panel data to capture the evolution of compliance behaviors. An individual receives subsidies at age 42, one experiment between even and odd ages. The same experiment happens next year at age 43, this time in the control group, and again at 44, back in the treatment group. If one participated at age 42 implying that she is either an always-taker or a complier, does she also participate at age 44? Whether repeated experiments happening at different times can serve as a valid counterfactual is by no means theoretically grounded, but it is an interesting exercise and provides a nice robustness check on cross sectional analysis.

To avoid attrition issue, I restrict the sample to unattrited survey participants that show up in the dataset for the entire 10 years.[49] I also impose the age condition such that the age in the first year is 40 or above. This leaves unique 5,701 individuals. For

[49]While the entire dataset spans 11 years, I drop the first year, 2008. This is because having an even number of years such that there are 5 years of even and odd ages simplifies the analysis.

classification, I calculate the empirical probability of screening at even and odd ages.

$$Pr(screen\_even)_i = \frac{1}{5}\sum_k \mathbb{1}\{screen_{ik} = 1\}, \qquad k \text{ even} \tag{9}$$

The term $screen_{ik}$ is whether individual i of age k has participated in screening. Given 10 years of data, there will be 5 even ages and 5 odd ages. The probability of screening at odd ages can be calculated in a similar way. Then, the probability of screening at even or odd ages will range from 0 to 1 with 0.2 increments.

Perfect compliance means participating at all the even ages and not participating at all the odd ages, which gives us probability at even age of 1 and probability at odd age of 0. Defiers will have the opposite pattern. Always-takers will have both the even and odd age probability of 1. Never-takers will have both even and odd age probability of 0. Figure A4 presents the bivariate distribution of the two empirical probabilities. For exhaustive definition, I define those in the upper right quadrant ($Pr(screen\_even) > 0$, $Pr(screen\_odd) > 0$) as always-takers, upper left quadrant ($Pr(screen\_even) < 0$, $Pr(screen\_odd) > 0$) as defiers, lower left quadrant ($Pr(screen\_even) < 0$, $Pr(screen\_odd) < 0$) as never-takers and lower right quadrant ($Pr(screen\_even) > 0$, $Pr(screen\_odd) < 0$) as compliers. Based on these definitions, around 29 percent falls in the category of compliers, 66 percent are never-takers, 2 percent are always-takers and 3 percent are defiers.

Table A14 presents the characteristics of four compliance groups and the ratios compared to compliers. The selection pattern is highly similar to the one observed in cross sectional analysis. Column 5 provides comparison between compliers and always-takers. Panel A shows compliers are more likely to find a stomach-related disease than always-takers. This is consistent with negative selection in income and education as shown in Panel B. Due to negative selection in income, Panel C shows compliers are less likely to smoke, drink and exercise.

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, less likely to drink everyday and more likely to exercise. In terms of socioeconomic status as shwon in Panel B, we again get mixed finding and unclear selection pattern. Panel

A shows that compliers are slightly more likely to find a stomach-related disease than never-takers. However, this would be clearly sensitive to classification. Never-takers, by definition, do not participate in screening. Their diagnosis effect is identified off of those who are on the margin with other compliance groups. With stricter classification, the number of screenings participated by never-takers would be small making the comparison difficult.

One different finding from the cross sectional analysis is the existence of defiers, which was assumed away with monotonicity assumption in Section 5.1. Given that they are the ones who participate in odd years and do not participate in even years, they would be classified as either always-takers or never-takers in the cross sectional analysis. The share of defiers is small suggesting validity of the monotonicity assumption. One explanation for defiers comes from spousal spillover discussed in Section 5.1.3. Despite odd age, if a spouse has even age, it increase one's probability of screening giving rise to defiance behavior. I further restrict the sample to 4,704 married individuals and check the share of people whose age is even when the spouse's age is odd or whose age is odd when the spouse's age is even. Consistent with our expectation, the share of off-age combination is highest among defiers and lowest among compliers.

Overall, the selection pattern from using panel information is similar to the one from using cross sectional information reported in Table 7. This gives confidence to my results and also suggest that compliance behavior may hold over time.

Figure A4: Bivariate distribution of the even and odd score

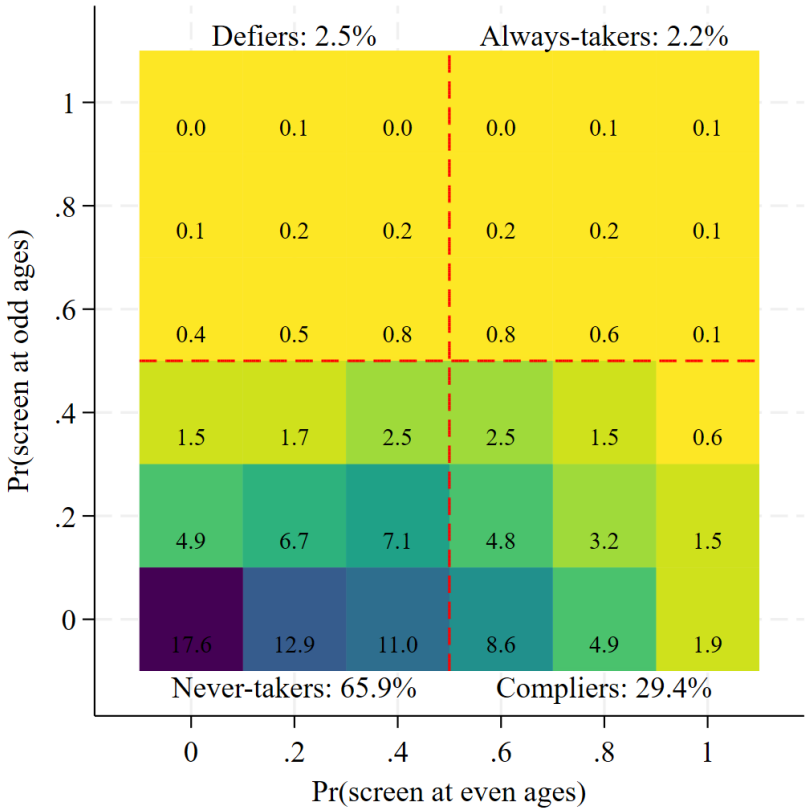


Table A14: Compliers with subsidies using panel information in even-odd design

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Always	Compliers	Defiers	Never	Compliers / Always	Compliers / Defiers	Compliers / Never
Panel A. Diagnoses							
Stomach disease diagnosis	0.181	0.250	0.176	0.232	1.384***	1.418**	1.079*
	(0.018)	(0.007)	(0.020)	(0.007)	(0.144)	(0.164)	(0.045)
Breast disease diagnosis	0.017	0.021	0.030	0.014	1.201	0.694	1.448
	(0.008)	(0.003)	(0.015)	(0.003)	(0.551)	(0.354)	(0.362)
Cervical disease diagnosis	0.075	0.051	0.039	0.053	0.681*	1.302	0.970
	(0.018)	(0.005)	(0.016)	(0.007)	(0.178)	(0.534)	(0.149)
Colorectal disease diagnosis	0.168	0.210	0.165	0.229	1.250	1.272	0.916
	(0.031)	(0.013)	(0.032)	(0.015)	(0.242)	(0.258)	(0.080)
Panel B. SES							
Individual income	2688	1043	2405	1288	0.388***	0.434***	0.810***
	(247)	(37)	(222)	(28)	(0.038)	(0.043)	(0.034)
Household income	6099	4000	5589	3764	0.656***	0.716***	1.063***
	(310)	(67)	(284)	(46)	(0.035)	(0.038)	(0.022)
Years of education	12.184	9.876	11.486	9.585	0.811***	0.860***	1.030**
	(0.312)	(0.098)	(0.349)	(0.075)	(0.022)	(0.027)	(0.013)
College graduate	0.270	0.123	0.291	0.142	0.456***	0.423***	0.869**
	(0.039)	(0.008)	(0.038)	(0.006)	(0.072)	(0.062)	(0.066)
Working status	0.753	0.560	0.733	0.603	0.744***	0.764***	0.930***
	(0.030)	(0.010)	(0.032)	(0.007)	(0.033)	(0.036)	(0.020)
Panel C. Health behaviors							
Current smoker	0.152	0.104	0.225	0.212	0.685**	0.464***	0.491***
	(0.026)	(0.007)	(0.031)	(0.006)	(0.125)	(0.071)	(0.034)
Everyday smoker	0.145	0.100	0.208	0.204	0.688**	0.480***	0.489***
	(0.025)	(0.006)	(0.030)	(0.006)	(0.128)	(0.076)	(0.035)
Current drinker	0.743	0.612	0.708	0.599	0.824***	0.865***	1.022
	(0.032)	(0.009)	(0.031)	(0.007)	(0.038)	(0.041)	(0.019)
Everyday drinker	0.060	0.055	0.097	0.080	0.923	0.572***	0.694***
	(0.015)	(0.004)	(0.019)	(0.003)	(0.243)	(0.120)	(0.059)
Vigorous exercise	0.301	0.206	0.282	0.193	0.685***	0.730***	1.067*
	(0.022)	(0.005)	(0.021)	(0.004)	(0.053)	(0.056)	(0.034)
Moderate exercise	0.503	0.389	0.473	0.340	0.773***	0.822***	1.146***
	(0.020)	(0.006)	(0.023)	(0.004)	(0.034)	(0.042)	(0.022)
Walking	0.831	0.820	0.821	0.769	0.986	0.998	1.066***
	(0.016)	(0.004)	(0.014)	(0.003)	(0.020)	(0.017)	(0.007)
Panel D. Married subsample							
Pr(even/odd or odd/even)	0.509	0.484	0.577	0.500			
Share	0.022	0.294	0.025	0.659			

Notes: This table reports the average characteristics of compliance groups defined using the history of health screenings in 10 years. Compliance groups are defined using the rule shown in Equation (9) based on how many times one participated in screening at even and odd ages. Compliers are the ones who participate more than half at even ages and less than half at odd ages. Always-takers are the ones who participate more than half at both the even and odd ages. Never-takers are the ones who participate less than half at both the even and odd ages. Defiers are the ones who participate more than half at odd ages and less than half at even ages. The sample is restricted to those who do not show any attrition and participated in the survey for all 10 years. It is also restricted to those whose age in year 2009 is 40 or above. After classification, the sample consists of 5,701 individuals and all the characteristic variables are averages of 10 year values. The null hypotheses used for ratios are $H_0 : CP/AT = 1$, $H_0 : CP/NT = 1$, and $H_0 : CP/DF = 1$ for comparison with always-takers, never-takers and defiers, respectively, where AT = Always-takers, NT = Never-takers, DF = Defiers, and CP = compliers. Pr(even/odd or odd/even) refers to the probability that one's age and the spouse's age is even-odd or odd-even. For this share calculation, the sample is further restricted to 4,742 individuals who are married. Share reports the share of each compliance group using the whole sample. Robust standard errors are used. They are reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.