

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

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

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1 Introduction

Health screenings are widely considered an essential form of preventive care that reduces premature deaths from heart disease and cancer, the two leading causes of death in developed countries (Cutler, 2008). Based on clinical trials demonstrating their effectiveness, many governments have introduced public screening programs (CanScreen5, 2022). Yet the population-level impact of these programs is often limited by poor targeting (Woolhandler and Himmelstein, 1988; Zonderman et al., 2014). Screening participation is typically lower among individuals with lower socioeconomic status (SES), who face higher cancer and all-cause mortality, raising concerns about programs’ effectiveness (Pill et al., 1988; Waller et al., 1990; Khang et al., 2004; Jung-Choi et al., 2011; Bender et al., 2015; Jones et al., 2019; Carethers and Doubeni, 2020). Underuse among high-risk individuals reduces opportunities for early detection, while overuse among low-risk individuals raises risks of false positives, overdiagnosis, and overtreatment (Welch et al., 2016; Autier et al., 2017; Kowalski, 2023). Therefore, improved targeting could enhance early diagnosis, reduce unintended harms, and help narrow health disparities across socioeconomic groups.

Prior studies on selection into screenings and their causal effects often rely on local variation (Kadiyala and Strumpf, 2016; Kim and Lee, 2017; Einav et al., 2020; Kowalski, 2023). Most exploit either age cutoff at the recommended starting age or income cutoff for income-based subsidies. While such designs credibly identify selection patterns and causal effects around the cutoff, their findings may not generalize to older populations or to individuals with lower SES, groups in which cancer incidence is higher. Yet most of the world’s largest screening programs, including the Affordable Care Act’s preventive care mandate in the United States and Europe’s organized screening programs, offer universal and unconditional access. Without variation generating population-level compliers, we still know little about whether these large-scale screening efforts succeed in reaching high-risk individuals.¹

¹When large-scale programs have been studied, researchers often rely on difference-in-differences designs exploiting staggered rollouts (Bitler and Carpenter, 2016; Van Ourti et al., 2020; Guthmuller et al., 2023). However, these approaches typically do not identify the characteristics of compliers relative

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

South Korea’s NHSP subsidizes 90–100% of the costs for stomach, breast, and cervical cancer screenings. It also covers a comprehensive cardiovascular and general health examination, commonly referred to as a “general screening”. Subsidies begin at age 30 for cervical screening and at age 40 for other screenings, and are provided during the calendar year when an individual reaches an even-numbered age.² I show that this rule generates quasi-random variation in subsidy eligibility, conditional on a flexible function of age,³ and creates large variation in screening take-up between treatment (even-aged individuals eligible for subsidies) and control (odd-aged individuals ineligible for subsidies) groups. The identifying assumption is that, conditional on age controls, there are no systematic differences between treatment and control groups other than subsidy eligibility. As I show later, this variation captures both net increases in screening participation and intertemporal substitution, as individuals may shift screenings from unsubsidized to subsidized ages.

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

to always- or never-takers.

²The policy rule verbatim states that individuals born in an even (odd) year are eligible for subsidies in even (odd) calendar years. This is equivalent to eligibility in the calendar year when one turns an even-numbered age.

³Age controls adjust for mechanical imbalance in an analytical sample starting at age 40 (an even number), which makes even-aged individuals younger on average than odd-aged individuals.

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

I find that subsidy eligibility increases screening take-up by 16-19 percentage points (183–295%) for general, stomach, breast, and cervical screenings, corresponding to arc elasticities of -0.48 to -0.72 .⁴ In addition, I document sizable spillovers across screening types. Subsidy eligibility increases take-up of annually subsidized liver and colorectal screenings by 2.7–3.3 percentage points (94-124%) and unsubsidized lung and prostate screenings by 0.6-0.7 percentage points (67-81%). These cross-screening spillovers are much larger in magnitude than the cross-vaccine spillovers documented by [Carpenter and Lawler \(2019\)](#) and [Humlum et al. \(2024\)](#).

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

Next, using National Health Insurance Service claims data, I estimate the intent-to-treat (ITT) effects of subsidy eligibility on cancer diagnoses. Subsidy eligibility increases the one-year overall cancer detection rate by 0.16 percentage points (18%), with significant gains in six of the seven cancer types examined. Notably, stomach cancer diagnoses

⁴These effects are substantially larger than those found in prior studies on preventive care, including the RAND Health Insurance Experiment, the Oregon Medicaid Experiment, and a related study on screening subsidies ([Newhouse, 1993](#); [Finkelstein et al., 2012](#); [Kim and Lee, 2017](#)). I later show that intertemporal substitution partly explains this difference.

rise by 0.08 percentage points (41%), and breast cancer diagnoses by 0.05 percentage points (11%). The effects extend beyond cancers directly targeted by biennial subsidies. For example, colorectal cancer diagnoses increase by 0.02 percentage points (10%). While the main analysis focuses on ITT effects, local average treatment effects (LATE) of one screening can be calculated by dividing ITT estimates by increases in take-up rates estimated from survey data. The resulting LATEs are 0.0041 percentage points (213%) for stomach cancer, 0.0026 percentage points (56%) for breast cancer, and 0.0055 percentage points (287%) for colorectal cancer. These LATE estimates represent the cancer detection rates among compliers and are substantially large, consistent with the finding that compliers are disproportionately high-risk individuals. For context, [Guthmuller et al. \(2023\)](#) estimate a LATE of 0.0010 for breast cancer in Europe’s organized screening programs, less than half of my estimate, 0.0026.

Medical studies emphasize the role of early detection of cancers in decreasing morbidity and mortality ([Nicholson et al., 2024](#); [Davidson et al., 2021](#); [Krist et al., 2021](#); [Grossman et al., 2018](#); [Curry et al., 2018a](#); [Park et al., 2015](#); [Etzioni et al., 2003](#)). I break down aggregate diagnoses by cancer stage and show that screening increases detection of both early stage in-situ (stage 0) and invasive (stage 1-3) cancers. For instance, subsidy eligibility increases invasive breast cancer diagnoses by 0.03 percentage points (8%) and in-situ diagnoses by 0.016 percentage points (24%). Consistent with earlier detection, breast cancer patients diagnosed in the treatment group exhibit 1.7 percentage points (1.8%) higher five-year survival rate than those diagnosed in the control group.

Finally, screenings increase the use of preventive care for precancerous conditions and cardiovascular risk factors. Beyond the detection of cancers, screenings help prevent cancer development by identifying precursors such as *Helicobacter pylori* infection and colon polyps, which are strongly linked to stomach and colorectal cancers. Treatments for these conditions increase by 0.003–0.720 percentage points (15–62%). Screenings also detect cardiovascular risk factors, leading to higher medication use for high blood pressure, diabetes, and high cholesterol by 0.02–0.42 percentage points (0.17–3.01%). Among other conditions covered by screenings, prescriptions increase for osteoporosis by

0.11 percentage points (4%) but show no significant change for tuberculosis.

A unique feature of the research design is alternating treatment. Individuals switch between treatment and control groups each year, creating incentives to shift screening from control to treatment ages to receive subsidized screenings. This intertemporal substitution widens the take-up gap between treatment and control groups without net increase in overall participation. I find that biennial subsidies generate both net increase in take-up and substitution, by analyzing two sharp changes in subsidy eligibility: the age 40 eligibility cutoff and January/December of subsidized years. Moreover, comparing the magnitude of bunching in January versus December reveals that advancing screenings is more common than delaying them.

Intertemporal substitution presents limitations on analysis, but also analytical leverage. On the one hand, the two-year eligibility cycle complicates the assessment of long-term impacts. On the other hand, it amplifies the effect of subsidies on take-up, providing larger variation to identify selection patterns and causal effects. Furthermore, it allows treatment and control years within the same individuals to serve as empirical counterfactuals, enabling classification of compliance groups at the individual level. Using this approach, I show that individuals who respond to subsidies by increasing participation share similar characteristics with those who respond by shifting screening timing.

This study contributes to two strands of literature. First, it adds to the literature on selection into treatment in the context of health screenings. Prior studies mostly rely on local variation, such as income thresholds or age-based eligibility cutoffs, to characterize compliers (Kim and Lee, 2017; Einav et al., 2020; Kowalski, 2023). These regression discontinuity designs restrict compliers to individuals near the cutoff, limiting analysis of how selection varies across broader demographic and socioeconomic dimensions.⁵ In contrast, I exploit even- and odd-age eligibility rules, which generate broad, population-wide variation. This design enables the study of selection across age, income, education, and other dimensions closely related to cancer incidence, and better reflects the structure of large-scale screening programs worldwide that provide universal access.

⁵While Kowalski (2023) examine an influential clinical trial on mammography in Canada, the sample is restricted to women aged 40-49.

This advantage naturally extends to the estimation of causal effects. Compared to prior studies focusing on impacts around specific age or income cutoffs (Decker, 2005; Kadiyala and Strumpf, 2012, 2016; Kim and Lee, 2017; Myerson et al., 2020; Sun et al., 2022), the population-wide variation in this study allows for the estimation of both local and population-level effects. I document substantial heterogeneity in the impact of screening on cancer detection, with much larger effects among older and lower-income individuals. Accounting for this heterogeneity provides a more complete picture of the benefits of screenings and offers new evidence on the population-wide consequences of introducing universal screening programs.

Finally, this study expands the scope of outcomes examined in the literature on causal effects of screening. While prior research largely evaluates single screening, most often mammography, in isolation, I jointly examine eight screening types (Decker, 2005; Kadiyala and Strumpf, 2012, 2016; Bitler and Carpenter, 2016, 2017; Kim and Lee, 2017; Kim et al., 2019; Myerson et al., 2020; Van Ourti et al., 2020; Iizuka et al., 2021; Sun et al., 2022; Guthmuller et al., 2023). This allows for comparisons of heterogeneous effects across screenings within the same setting and reveals spillovers in participation and diagnosis. I further extend outcomes beyond aggregate cancer detection by distinguishing in-situ from invasive cancers, showing that screenings increase both early- and late-stage diagnoses. In addition, I document effects on precancerous conditions and chronic risk factors, highlighting the role of public screening programs as an entry point into broader preventive care.

The rest of the paper is organized as follows. Section 2 describes the disease burden in South Korea and provides an overview of the National Health Screening Program. Section 3 outlines the identification strategy using even/odd age-based subsidies. Section 4 discusses the survey and administrative datasets used for this study. Section 5 presents the results on take-up, selection into screenings, and their causal effects. Section 6 concludes.

2 Institutional background

2.1 Disease burden and screenings in South Korea

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

To address this burden, the National Health Screening Program (NHSP) offers two types of publicly financed screenings. The first is a comprehensive set of tests commonly referred to as a “general health screening” that targets cardiovascular and metabolic risk factors, including high blood pressure, blood glucose, and cholesterol. It also includes urine tests to assess kidney and liver function, and chest X-rays to screen for tuberculosis.⁶ Additional age-specific tests include osteoporosis screening, mental health assessments, and evaluations of functional capacity in older adults. Individuals identified as high-risk for hypertension, diabetes, or tuberculosis are referred for follow-up testing and consultation.

Second, five types of cancer screenings are subsidized under the NHSP: stomach, breast, cervical, liver, and colorectal. These screenings target cancers that are relatively common in South Korea and for which early detection is known to improve health outcomes. Stomach cancer is particularly prevalent in Korea and East Asia more broadly, and it has consistently ranked among the top five cancers in both incidence and mortality for men and women.⁷ It is screened using upper endoscopy (gastroscopy). Breast cancer is the most frequently diagnosed cancer among Korean women and is screened using bilateral mammography. Cervical cancer incidence has declined steadily over the past

⁶South Korea has the highest incidence and mortality rates of tuberculosis (TB) among OECD countries (Cho, 2018). TB screening is mandatory for students entering middle and high school, and adults receive biennial TB screening as part of the general health screening.

⁷The age-standardized incidence rates per 100,000 were 27 for South Korea and 27.6 for Japan in 2022, compared to 4.1 in the United States and 6.4 in Germany (World Cancer Research Fund, 2024).

two decades, and is screened with Pap smears. Liver cancer, while a major contributor to cancer mortality, is screened only for high-risk individuals, such as those with chronic liver disease or hepatitis, and is not universally subsidized. Screening is performed using liver ultrasound. Colorectal cancer, the second most common cancer in 2022, is initially screened using a fecal occult blood test (FOBT), with colonoscopy subsidized only for individuals who test positive. However, due to relatively low costs and low perceived risk, many individuals opt to undergo colonoscopy directly without prior FOBT (Baik and Lee, 2023).

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

2.2 Subsidy schedule and program design

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

Screenings can be grouped into three categories based on their subsidy frequency: biennial, annual, and unsubsidized. Table 1 summarizes the rules. Biennial screenings, including general, stomach, breast, and cervical, are subsidized every other year. A key feature of the program is that subsidies are provided throughout the calendar years when one’s age is even-numbered. Here, “age” refers to calendar age, calculated as the difference between the current year and birth year.

For example, someone born in 1970 would be eligible for subsidized screenings throughout 2020, since her age is 50 ($= 2020 - 1970$), an even number. Next year in 2021, however, her age would turn 51, an odd number, so she would need to pay the full costs for the same screenings if she wants to receive them.⁸ Annual screenings include liver and colorectal cancer screenings, which are subsidized every year.⁹ Lung and prostate cancer screenings are not subsidized under the NHSP during the study period.

For each subsidized screening, whether biennial or annual, there is a cutoff age where subsidies begin. For general, stomach, breast and liver screenings, subsidies start at age 40. There are no subsidies before age 40, even if one’s age is even. Hence, general, stomach, and breast screenings that follow the biennial schedule are subsidized at ages 40, 42, 44 and so on.¹⁰ Liver screening that follows the annual schedule is subsidized at ages 40, 41, 42, and onward. The age cutoff for cervical screening is 30, while for colorectal screening, it is 50.¹¹ Since cervical screening follows a biennial schedule, subsidies are available at ages 30, 32, 34 and onward. Colorectal screening following annual schedule is subsidized at ages 50, 51, 52 and onward. No age thresholds apply to lung and prostate

⁸The policy rule verbatim states that those born in even (odd) years are eligible for subsidies at even (odd) years. This is mathematically equivalent to the rule where one is eligible for subsidies at even ages, defined as the difference between the current year and birth year. This is because the difference between two even numbers or two odd numbers is always even.

⁹Colorectal screening was biennially subsidized until 2011 and annually thereafter. Because it was annual for the majority of the study period, it is treated as such in the main analysis. I explicitly examine this policy change in Appendix Section E.

¹⁰Two minor exceptions exist in the general health screening program: (i) some employee- and self-employed–insured individuals may receive biennial subsidies before age 40, and (ii) non-office workers are eligible for annual rather than biennial screenings. These exceptions apply only to general screening, and survey evidence suggests limited adherence to the annual schedule. Since our analysis begins at age 40, they do not affect identification, and we abstract from them.

¹¹In 2016, the age cutoff for cervical screening was lowered to 20. Since age 30 was the cutoff for most of the study period, this analysis adopts age 30 threshold.

cancer screenings, as they were not subsidized.

The amount of subsidies is full coverage for the general health screening and 90% subsidy for 5 types of cancer screenings.¹² As a result, individuals pay nothing for general screening and only 10% of the cost for cancer screenings.¹³ For lower-income individuals, even the 10% copay is waived, effectively making all screenings free.¹⁴ Excluding liver screening, which is subsidized only for high-risk individuals, the total amount of available screening subsidies one can receive at even ages is approximately \$117.5 for men and \$168.5 for women.

The program was designed to maximize participation by ensuring easy access to subsidized screenings. Screenings can be obtained at public health clinics or designated private hospitals and clinics approved by the NHSP.¹⁵ While appointments are generally not required for general screening, they are typically necessary for more specialized procedures such as gastroscopy or colonoscopy. Eligible individuals receive paper mail or mobile reminders, which specify the available subsidized screenings and list nearby participating providers.

3 Identification

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

¹²Under the Occupational Safety and Health Act, employers are required to ensure their employees undergo general health screening. When employees do not participate, employers face financial penalty of approximately \$40. This mandate applies only to general screening, not to cancer screenings.

¹³Subsidy generosity increased over time. For example, cervical screening was fully subsidized during the study period, and colorectal screening became fully subsidized shortly afterward.

¹⁴The copay waiver applies to individuals whose health insurance premium falls below the median. Because the premium is income- and asset-based, the waiver primarily benefits lower-income individuals. See [Kim and Lee \(2017\)](#) for an analysis of take-up and detection effects at the premium threshold.

¹⁵As of December 2023, approximately 5,800 private facilities were designated for NHSP screening, equivalent to one center for every 900 adults aged 40 and older ([National Health Insurance Service, 2023](#)).

While the distinction between even and odd ages seems plausibly exogenous, the even-age group (treatment) is mechanically younger than the odd-age group (control), because subsidies begin at age 40, an even number.¹⁶ I argue that age is the only difference between the treatment and control groups, so they should be balanced after controlling for age. Hence, I estimate the following econometric specification.

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

The treatment group indicator, $treat_{it}$, equals 1 if individual i has even age in year t . Note that the treatment group is not individual-specific, but varies with years. Age control variable, $f(age_{it})$, is a function of age flexible enough to remove the age effect between the treatment and the control group. The main specification uses linear splines with 5-years intervals, and robustness checks with alternative age controls are reported in the Appendix Section ???. Standard errors are clustered at the individual level.¹⁷

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

The identification strategy in this study differs from a standard randomized controlled trial (RCT), because individuals alternate between treatment and control intertemporally. This creates an incentive to shift screenings from control to treatment ages to align

¹⁶Since the age distribution roughly declines with age starting from 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 also creates imbalances in covariates correlated with age.

¹⁷Despite using panel data, the main specification does not include any panel method to make it clear that the identification strategy does not require panel structure. Robustness checks using individual fixed effects are provided in Appendix Section ??.

take-up with subsidy eligibility. This intertemporal substitution amplifies the take-up gap between treatment and control groups, which makes it easier to detect statistically significant effects of screenings on downstream outcomes. It also provides a chance to test whether individuals are forward-looking and responsive to anticipated price changes by advancing or delaying screenings. However, this feature imposes two restrictions on our analysis. First, it precludes estimation of long-term effects, since everyone ultimately belongs to the treatment group within a two-year window. Second, it makes it challenging to isolate the net increase in screening take-up attributable to subsidies, since the observed take-up gap reflects both new screenings and shifts in timing.

While this study cannot fully separate net increase from intertemporal substitution, Section 5.1 provide suggestive evidence that both channels play a role, with advancing screenings more common than delaying. This study embraces substitution as a mechanism through which biennial subsidies magnify the take-up gap and focus on selection into screening and its short-term effects.

4 Data

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

The survey data come from the Korean Health Panel Survey, spanning the years 2008-2018.¹⁸ It is a longitudinal household survey with around 7,000 households and 21,300 individuals. To maintain national representativeness in response to gradual attri-

¹⁸It is version 1.7.1 made jointly by Korean Institute for Health and Social Affairs and National Health Insurance Services.

tion, a second cohort of 1,800 households (5,000 individuals) was added to the sample in 2014. Data were collected annually through face-to-face interviews using computer-assisted personal interviewing (CAPI). All household members were surveyed each year, and all variables are self-reported.

The survey datasets include annual data on demographic and socioeconomic characteristics, health care utilization, and health behaviors. Socioeconomic status variables include yearly income and educational attainment. Health care utilization data are recorded at the visit level for outpatient, inpatient, and emergency services.¹⁹ For each hospital visit, the dataset records the date, total expenditures on hospital and drug services, and diagnosis codes.²⁰ Hospital visits for health screenings include additional information on the type of screening (general or cancer-specific), tests performed, results, and any diagnosis made. The dataset also includes information on health behaviors such as smoking, drinking, and physical activity. For each behavior, the dataset indicates participation and frequency. For smoking and drinking, I consider both current use and daily use to capture behavioral intensity.²¹ Physical activity is classified by intensity into vigorous exercise, moderate exercise, and walking.²²

To complement the survey data, this study utilizes national health insurance claims data, specifically the Standard Cohort Database (2002-2019) and the Customized Cohort Database (2002-2021). The former includes a random sample of one million individuals, while the latter includes a separate random sample of approximately 640,000 individuals. Both datasets provide demographic information and health care utilization records. Additionally, the Standard Cohort Database provides data on prescription and cause-specific mortality, whereas the Customized Cohort Database includes cancer incidence

¹⁹Although data collection interviews were conducted 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 save all receipts from medical visits and pharmacy purchases. Survey enumerators collected these records during annual visits, and cross-checked diary entries against receipts. Each interview began by documenting medical visits since the date of the previous interview, ensuring no missing period.

²⁰Diagnoses were coded using the Korean Classification of Diseases, a Korean version of ICD-10.

²¹Behavioral status is based on activity in the month preceding the interview.

²²Specific examples for such activities were provided at the survey. They are measured based on activity during the week prior to the interview.

data. Given that the two samples are both random samples and exhibit similar average demographic and health insurance characteristics, they are collectively referred to as health insurance claims data in this study.

The claims data contain cancer diagnosis records regardless of whether the diagnosis was made through screening. Cancer diagnoses are inferred using information from the Coinsurance Reduction Program for Rare and Severe Diseases (CRP), administered by the National Health Insurance Service (NHIS) from year 2004. Under this program, coinsurance rates for hospital visits, which normally range from 20-50%, are reduced to 0-10% for patients with rare and severe diseases, including cancers, stroke, tuberculosis, and other rare or incurable conditions that incur large health care expenditures. For newly diagnosed cancer patients, registering with the CRP is typically one of the first steps, making it a reliable source for identifying true cancer diagnoses, not including false positives.²³ The CRP is implemented independently from the health screening programs, so it captures cancer diagnoses regardless of how the cancer was detected, whether through screening or not. I use this program to infer new cancer diagnoses and investigate whether subsidies at even ages lead to more cancer diagnoses at even ages.

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

²³Even individuals who do not formally register can be identified if they visit a hospital for cancer treatment at least once. For CRP-registered cancer patients, the coinsurance rate is reduced to 5%. For unregistered patients, it drops to 10%. These reductions apply only to visits related to the registered condition. Unrelated visits are not covered by the program.

5 Results

5.1 Effect on screening take-up

5.1.1 Effect on biennial screenings

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

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

5.1.2 Intertemporal substitution

One mechanism through which subsidies affect participation is intertemporal substitution. The knowledge of subsidy schedule allows forward-looking individuals to temporally reallocate screenings to subsidized years. Such timing shifts can widen the take-up

gap between subsidized and unsubsidized ages without any net increase in participation. While estimating the magnitude of intertemporal substitution is challenging due to the lack of variation in targets, this study provides suggestive evidence that biennial subsidies generate both net increases in participation and shifts in screening timing, with advancing screenings more common than delaying them. Appendix Section D provides detailed analysis supporting these findings. Here, I summarize the key findings.

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

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

5.1.3 Cross spillover

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

A likely mechanism of cross spillover is bundling of multiple screenings during a single hospital visit, reflecting shared fixed costs (e.g., time, travel, or fasting requirements for medical tests). Appendix Table A10 shows that 96% of individuals receiving both general and stomach screenings do so on the same day. Additionally, screenings not performed on the same day tend to occur after the general screening, consistent with doctors recommending further tests. Between doctors and patients, patients are more likely to be a driver of this bundling behavior according to selection patterns presented in A12 in Appendix Section E. Individuals who, after getting a biennial screening, further participate in annual or unsubsidized screenings are healthier and from higher socioeconomic backgrounds than those who only get a biennial screening. These results echo prior findings on cross spillover across vaccinations (Carpenter and Lawler, 2019; Humlum et al., 2024).

5.2 Selection into screening

5.2.1 Cross-sectional inference

A common criticism against health screening programs is that they tend to attract relatively healthy individuals, while those at higher risk, such as people with lower in-

come or education levels, are less likely to participate (Pill et al., 1988; Waller et al., 1990; Bender et al., 2015; Jones et al., 2019).²⁴ This pattern can reduce the effectiveness of screenings and increase the likelihood of false positives, overdiagnosis, and overtreatment (Kowalski, 2023; Welch et al., 2016; Autier et al., 2017). As a result, improving participation among higher-risk individuals is crucial for maximizing the benefits and minimizing the harms of screening programs.

To understand whether subsidized screenings successfully reach high-risk individuals, I analyze the types of people whose behavior changes in response to the policy. These individuals, who would not have been screened without subsidies but opt in when screenings are subsidized, are referred to as compliers, following the terminology of Imbens and Angrist (1994) and Angrist et al. (1996). Understanding who these compliers are is essential, as they represent the population whose behavior is most responsive to the policy and thus critical for assessing its effectiveness. I compare compliers with two other groups: always-takers, who always participate even in the absence of subsidies, and never-takers, who never participate even in the presence of subsidies.²⁵ Since the goal of screening is to detect diseases at an early stage, the policy should ideally target unhealthy individuals who are at higher risk of having undiagnosed conditions.

One approach to characterizing compliers relative to always-takers is to restrict the analytical sample to screening participants and compare treatment and control groups.²⁶ Under this restriction, the participants in the treatment group are either always-takers or compliers, while the participants in the control group must be always-takers. The differences between participants in the treatment and the control come from group composi-

²⁴The pattern of preventive care take-up by healthier individuals extends beyond screenings. Similar selection pattern is observed in the use of vaccinations, contraception, workplace wellness programs, and vitamin supplements (Thomas et al., 2021; Dalton et al., 2020; Jones et al., 2019; Oster, 2020). In developing countries, selection in response to the price of preventive health products has been shown for antimalarial medications, bednets, and water purification kits (Cohen and Dupas, 2010; Cohen et al., 2015; Dupas, 2014; Tarozzi et al., 2014; Ashraf et al., 2010).

²⁵I impose monotonicity assumption, meaning that subsidies weakly increase the probability of screening. This assumption rules out defiers, who would get screened only in the absence of a subsidy.

²⁶Complier characterization is straightforward in settings with one-sided noncompliance. For example, in a setting with only always-takers but no never-takers, 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.

tion, specifically, the presence of compliers in the treatment group. Since all individuals in this restricted sample are screening participants, any observed differences cannot reflect treatment effects but instead reflect pre-existing characteristics. Similarly, restricting the sample to nonparticipants and comparing treatment and control allows for comparisons between untreated compliers and never-takers.²⁷

Figure 2a, 2b, and 2c show that, compared to always-takers, compliers are more likely to be diagnosed with a stomach-related disease, have lower household income, and are less likely to be college graduates. Figure 2a plots the share of stomach screening participants who report finding a stomach-related disease.²⁸ Diagnosis rates are consistently higher among participants in treatment than those in control, which suggests that compliers have worse underlying health conditions prior to screening than always-takers. Similarly, Figure 2b and 2c show that screening participants in the treatment group have lower household income and are less likely to have a college degree. Together, these patterns imply that compliers are systematically of lower socioeconomic status relative to always-takers.

While the figures illustrate the selection patterns intuitively, they are limited to qualitative comparisons. To formally test and quantify the selection patterns observed in the descriptive figures, I estimate the average characteristics of compliers, always-takers, and never-takers in three steps. First, I estimate the characteristics of always-takers using screening participants in the control group. Due to the exogeneity of the even/odd subsidy rule, always-takers in the treatment group are comparable to the always-takers in the control group. Similarly, I estimate the characteristics of never-takers using nonparticipants in the treatment group. Second, I infer complier characteristics from the pool of screening participants in the treatment group, which is a convex combination of complier and always-taker characteristics, where the relative weights are derived from first-stage

²⁷In this analysis, I use the term “always-takers” or “never-takers” in the cross-sectional sense. In a given year, participants in the control group are “always-takers” in the sense that if they were in treatment group this year, they must have also participated in screening. I do not use panel structure in this analysis, that is, whether an individual actually participates all the years during the study period. I use the panel structure for selection analysis in the next section.

²⁸The survey asked screening participants whether they had been diagnosed with any condition through screening. For stomach screening, this includes cancer as well as less severe conditions, such as inflammation or ulcers. See Appendix Section F for common diagnoses.

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

Table 5 reports the estimated characteristics of always-takers, never-takers, treated compliers, and untreated compliers. It also presents the ratio of treated compliers to always-takers and of untreated compliers to never-takers for comparison where the null hypothesis is that the ratio is equal to one. Figure 3a plots the ratio of treated compliers to always-takers for each outcome, along with 95 percent confidence intervals. Regarding screening diagnoses, compliers are 68 percent more likely to report being diagnosed with a stomach-related disease through stomach screening than always-takers, suggesting that compliers face higher underlying health risks.²⁹ In contrast, differences in diagnosis rates for breast, cervical, and colorectal screenings are not statistically significant.³⁰

The socioeconomic characteristics of compliers further underscore their relative disadvantage. Compliers have 40 percent lower individual income and 12 percent lower household income compared to always-takers. Furthermore, compliers are 13 percent less

²⁹This pattern is not driven by differences in the quality or quantity of testing. Both subsidized and unsubsidized screenings are available at the same hospitals and are typically administered by the same medical staff, suggesting minimal variation in test quality. Moreover, as shown in Table A14 (Appendix Section G), always-takers actually receive more medical tests than compliers, including unsubsidized procedures such as sonograms and CT scans. Therefore, the higher disease detection rate among compliers is not attributable to more extensive or higher-quality testing.

³⁰There were too few disease diagnoses from liver, lung, and prostate screenings, which renders comparison infeasible.

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

In terms of health behaviors, compliers are neither more nor less likely to smoke, drink, or exercise compared to always-takers. To account for substantial gender differences in these behaviors, I report results separately for male and female samples.³¹ Table 5 and Figures 3a and 3b present the characteristics of male compliers, while Appendix Section G shows similar results for female compliers. Although the levels differ across genders, the comparison patterns between compliers and always-takers are consistent. Both among men and women, I find no statistically significant differences in health behaviors between compliers and always-takers.³²

Figure 3b provides the relative characteristics of untreated compliers compared to never-takers. While the selection based on socioeconomic status is not pronounced, compliers demonstrate clear positive selection in health behaviors.³³ Specifically, compliers are less likely to smoke and more likely to exercise. Compared to always-takers, male compliers shown in Table 5 are 29 percent less likely to smoke and 18 percent more likely to do vigorous exercise, while female compliers shown in Table A13 are 65 percent less likely to smoke and 29 percent more likely to do vigorous exercise. This pattern of positive selection relative to never-takers is also observed from prior studies on screenings ([Einav et al., 2020](#); [Kowalski, 2023](#); [Jones et al., 2019](#)). These findings align with broader

³¹For example, smoking is prevalent primarily among men: 39 percent of men are smokers compared to just 3 percent of women. Similarly, 13 percent of men report daily alcohol consumption, while only 1 percent of women do.

³²Any causal effect of screening on health behaviors would cancel out in this comparison, as both treated compliers and always-takers receive screenings.

³³Note that it is not possible to infer the health outcomes of never-takers through screening results, since they do not get screened by definition.

evidence on the correlation of health behaviors, in which individuals who engage in one form of preventive behavior are more likely to engage in others (Oster, 2020; Cutler and Lleras-Muney, 2010).

A related study by Einav et al. (2020) examines breast cancer screening around age 40 and finds that compliers with clinical guidelines are less likely to have true positive breast cancer than always-takers, but observes no systematic socioeconomic differences between the two groups. My findings complement this evidence by highlighting a different pattern when the treatment is financial incentives, rather than medical recommendations. In my setting, compliers exhibit a slightly higher risk of stomach disease, and they come from lower socioeconomic backgrounds than always-takers. Taken together, these studies suggest that patterns of selection may vary across both health risk and socioeconomic dimensions, depending on the nature of the intervention and the population under study.³⁴

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

Appendix Section G outlines the detailed methodology for selection analysis and presents robustness checks. First, around the age 40 eligibility cutoff, individuals starting screening at age 40 exhibit lower socioeconomic status and higher incidence of stomach diseases relative to those who begin earlier, consistent with the main findings. Second, workplace wellness programs in large firms, which often provide annual health screenings, may make some employees always-takers. Although this alternative source of free

³⁴Lawler (2020) examines national meningococcal vaccination recommendation for high school-aged adolescents and find similar selection pattern with Einav et al. (2020) that the adolescents from households with high education and high income were more responsive to the recommendation. This finding suggests that recommendation alone fails to reach those with low socioeconomic status.

screening does not change the selection results, it may affect the interpretation that individuals respond to lower prices. Table A15 shows that even among the non-working population, significant differences in diagnoses and household income remain, suggesting that workplace wellness programs do not fully account for the observed selection.³⁵

5.2.2 Longitudinal inference

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

The unique alternating subsidy schedule creates a repeated experiment every year. Over 10 years, each person faces five subsidized (even) and five unsubsidized (odd) ages. I interpret this setting as the one where treatment and control states are observed within the same individual over time and use experiments at subsidized ages as empirical counterfactuals for the experiments at unsubsidized ages, and vice versa.³⁶ Using 10 years screening history, I classify individuals into four groups. Always-takers who screen at both subsidized and unsubsidized ages, compliers who screen at subsidized ages, but not at unsubsidized ages, defiers who screen at unsubsidized ages, but not at subsidized ages, and never-takers who screen at neither. This classification assumes stability in compliance behavior over time. In reality, screening behavior may respond to transitory shocks, such as illness of oneself or of a family member, introducing noise and creating “almost-compliers” or “almost-always-takers” (Fadlon and Nielsen, 2019; Hodor, 2021; Jeon and

³⁵Jones et al. (2019) find that participants in workplace wellness programs are positively selected and have better health than nonparticipants. Therefore, workplace wellness program, if anything, underscores the importance of the public health screening programs that can attract high-risk participants.

³⁶The concept of compliers in Angrist et al. (1996) and Imbens and Angrist (1994) is grounded in the potential outcomes framework, where individuals are either in a treatment or control group. Compliers are defined by their unobserved behavior in a counterfactual state. For example, someone who takes up treatment when assigned to the treatment group but would not have done so if assigned to control is a complier. In this study, the alternating subsidy schedule creates a unique repeated experiment in which the same individual is observed across both treatment (even ages) and control (odd ages) conditions over time. This panel structure allows the use of observed screening behavior at odd ages as a proxy for the counterfactual to behavior at even ages, and vice versa.

Pohl, 2017; Hoagland, 2025; García-Gómez et al., 2013). To the extent that compliance behavior persists over time, this classification offers useful insight into selection into screening.

I restrict the sample to 5,701 individuals who are aged 40 or above and are observed in all 10 years.³⁷ I define the empirical screening probabilities at even ages as follow.

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

The term $screen_{ik}$ equals one if individual i of age k participated in a screening. The probability of screening at odd ages can be calculated in a similar way. Probabilities range from 0 to 1 in 0.2 increments. Figure 4 shows the joint distribution of even and odd screening probabilities. For exhaustive definition, I arbitrarily delineate the four groups and define those with $(Pr(screen_even) > 0.5, Pr(screen_odd) > 0.5)$ as always-takers, those with $(Pr(screen_even) < 0.5, Pr(screen_odd) > 0.5)$ as defiers, those with $(Pr(screen_even) < 0.5, Pr(screen_odd) < 0.5)$ as never-takers, and those with $(Pr(screen_even) > 0.5, Pr(screen_odd) < 0.5)$ as compliers. Based on these definitions, around 29.4 percent falls in the category of compliers, 65.9 percent are never-takers, 2.5 percent are always-takers, and 2.2 percent are defiers.

Table 6 compares characteristics across compliance groups. The patterns are highly consistent with the ones from cross-sectional analysis. Column 5 provides comparison between compliers and always-takers. Panel A shows compliers are 41 percent more likely to find a stomach-related disease than always-takers. This is consistent with negative selection in income and education as shown in Panel B. I do not find significant differences in health behaviors, except that compliers are slightly less likely to exercise than always-takers.³⁸

Comparison with never-takers also shows consistent pattern. Panel C shows compliers display better health behaviors than never-takers. They are less likely to smoke and more

³⁷Although the dataset spans 11 years, I exclude the first year, 2008, because the data collection start date varies across individuals in that year.

³⁸Consistent with the cross-sectional analysis, I only use male sample for health behaviors to account for large differences in smoking and drinking behaviors between male and female.

likely to exercise. As shown in the cross-sectional analysis, the differences in socioeconomic status presented in Panel B are ambiguous and do not indicate clear evidence of either positive or negative selection.

The panel approach identifies defiers, which the cross-sectional analysis cannot detect due to the monotonicity assumption. The estimated share of defiers is small, supporting the plausibility of the monotonicity assumption. One plausible mechanism is spousal spillover. Some individuals may get screened at unsubsidized ages if their spouse is eligible (Kim et al., 2024). To examine this, I restrict the sample to 4,704 married individuals and calculate the share whose age is even when their spouse’s age is odd, or vice versa. Consistent with the hypothesis, this off-age combination is most common among defiers and least common among compliers.

The comparison between compliers and defiers provides insight into compliance through the intertemporal substitution channel. On average, both groups undergo the same number of screenings, but the timing differs. Compliers concentrate participation at subsidized ages, while defiers do so at unsubsidized ages. Movement from defiers to compliers therefore largely reflects a shift in timing rather than an increase in overall screenings. Column 6 of Table 6 shows that the characteristics of compliers relative to defiers mirror those observed relative to always-takers. In both cases, compliers are more likely to be diagnosed with stomach disease and to come from lower socioeconomic backgrounds. This consistency suggests that the selection patterns among compliers are similar whether participation reflects net increases in screening or intertemporal substitution across years.

5.3 Effect of health screenings

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

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

5.3.1 Effect on cancer diagnoses

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

I find that subsidy eligibility leads to a significant increase in cancer diagnoses.³⁹ Figure 5b, 5c, and 5d plot detection rates for stomach, breast, and cervical cancer at each age, regardless of whether detected through preventive screening or not. Detection rates are consistently higher in the treatment group beginning at age 40, consistent with subsidy eligibility. Table 7 first rows for each cancer report ITT effects. Subsidy eligibility increases diagnosis rates by 0.077 percentage points (41%) for stomach cancer, 0.05

³⁹In this study, cancer refers to both invasive and in-situ cases unless otherwise specified. False positive results and benign tumors are excluded.

percentage points (11%) for breast cancer, and 0.02 percentage points (13%) for cervical cancer.

Subsidy eligibility also increases diagnoses for cancers not directly targeted by biennial subsidies. Figure 5e, 5f, 5g, and 5h show detection rates for liver and colorectal cancers, which were subsidized annually, and for lung and prostate cancers, which were not subsidized. In all four cases, detection rates are higher in the treatment group. Estimated effects indicate that subsidy eligibility increases diagnosis rates by 0.013 percentage points (16%) for liver cancer, 0.018 percentage points (10%) for colorectal cancer, 0.014 percentage points (15%) for lung cancer, and 0.01 percentage points (6%) for prostate cancer. Considering all seven cancer types together, subsidy eligibility raises the overall diagnosis rate by 0.16 percentage points (18%). These patterns suggest that spillover in screening take-up, examined in Section 5.1, leads to spillover in cancer diagnosis as well.

While the use of separate datasets to estimate participation and cancer diagnoses complicates the calculation of local average treatment effects (LATE), they can be manually calculated by dividing the ITT estimates by the first-stage increases in screening take-up from survey data.⁴⁰ The resulting LATEs are 0.0041 percentage points ($= 0.00077/0.19$) for stomach cancer, 0.0026 ($= 0.0005/0.191$) for breast cancer, and 0.0055 ($= 0.00018/0.033$) for colorectal cancer, corresponding to 213%, 56%, and 287% of the respective control group means. These estimates are large relative to other quasi-experimental evidence.⁴¹ For example, Guthmuller et al. (2023) report a LATE of 0.0010 for breast cancer in Europe’s organized screening programs, less than half of my estimate. Similarly, in Kim and Lee (2017), LATEs for stomach and breast cancers were both around 0.0025, while my estimate for stomach cancer is substantially higher. Since LATEs capture detection rates among compliers, these results align with my selection analysis indicating that compliers in this setting are disproportionately high-risk individuals.

The effectiveness of screening in detecting cancer exhibits strong age heterogeneity. At

⁴⁰These estimates should be interpreted with caution, as differences in data sources and time periods may introduce bias.

⁴¹As emphasized by Angrist and Hull (2023), LATEs provide a more meaningful basis for comparison across studies than ITT estimates.

younger ages, effects are small, but they increase sharply with age. In the earlier analysis of screening participation, I documented a sharp discontinuous increase in take-up at age 40, when eligibility for subsidized screenings begins (Detailed analysis in Appendix Section ??). This discontinuity was evident even for cervical and colorectal screenings, which were subsidized from ages 30 and 50, respectively. By contrast, figures on cancer diagnosis rates show no corresponding jump at age 40, suggesting that individuals who initiate screening at this threshold are generally healthy and face relatively low underlying cancer risk. At older ages, however, the opposite pattern emerges. The effect of subsidies on screening take-up diminishes, while the effect on cancer detection grows larger.⁴² This divergence implies that the chance of detecting cancer through screening, or the LATE of screening on cancer diagnosis, rises sharply with age. This finding implies that research designs exploiting discontinuities at recommended starting ages tend to capture effects among relatively young, low-risk individuals who are less likely to be diagnosed with cancer (Kadiyala and Strumpf, 2016; Einav et al., 2020). As a result, these designs can underestimate the broader effectiveness of screening, particularly for older adults, who account for the majority of cancer cases.⁴³

Unpacking aggregate cancer diagnoses into in-situ and invasive cases reveals that screenings disproportionately increase early stage in-situ cancer diagnoses.⁴⁴ Table 7 shows that screenings increase both invasive and in-situ cancer diagnoses, with relatively larger effects on in-situ cancers for most sites, except for lung and prostate cancers. The second and third rows for each cancer in Table 7 report the ITT effects on invasive and in-situ diagnoses. Once the ITT estimates are normalized by the control group mean, the percentage increase is generally larger for in-situ cancers, suggesting that screenings

⁴²Two exceptions are breast and cervical cancers. Figure 5c and 5d show that breast cancer incidence typically rises in the 30s, peaks in the 40s and 50s, and then gradually declines, while cervical cancer incidence peaks slightly earlier, in the 30s and 40s. These trends are consistent with Korea’s Cancer Registry data, which includes all cancer diagnoses nationwide (Kang et al., 2020; Chung et al., 2006).

⁴³While early detection in younger individuals may yield more life-years saved per case, the overall population-level impact of screening is concentrated among older adults, given their higher underlying cancer risk and greater incidence. Thus, the magnitude of effect observed at guideline start ages may not generalize across the age distribution.

⁴⁴In-situ cancers, also referred to as precancer or stage 0 cancer, are characterized by abnormal cells that have not yet invaded neighboring tissues. It can progress to invasive stages if left untreated. Although many cancers have an in-situ stage, it is particularly common in breast and cervical cancers.

disproportionately increase early stage cancer diagnosis.

The pattern of early diagnosis is further supported by higher survival rates among patients diagnosed in the treatment group. I restrict the sample to individuals diagnosed with one of the seven major cancers listed in Table 8 and compare their survival rates by whether the diagnosis occurred in the treatment (even age) or control (odd age) group. Figure 6 plots survival trajectories from 1 to 10 years post-diagnosis, and Table 8 reports the commonly used 5-year survival rates. Among 52,962 cancer cases, the 5-year survival rate is 80.1 percent in the control group versus 81.3 percent in the treatment group, a 1.2 percentage point (1.47%) increase. This difference suggests earlier cancer detection in the treatment group, consistent with screening-induced lead time. However, this improvement in survival should not be confused with reduced mortality, since early diagnosis can improve survival rates without changing actual lifespan.⁴⁵ This survival advantage appears across most cancers, except for lung and prostate cancers. These exceptions are consistent with prior RCT evidence showing that screening for these two cancer types does not reliably detect earlier stage disease, explaining why these screenings are not subsidized by NHIS (Krist et al., 2021; Grossman et al., 2018).⁴⁶ Table 8 also shows a larger share of in-situ cancers among treatment group diagnoses, consistent with larger percentage increase in in-situ cancer diagnoses compared to invasive cancers observed in Table 7. Taken together, these results indicate that screenings effectively shift diagnoses toward earlier stages.

5.3.2 Effect on preventive care use

Beyond increasing cancer diagnoses, health screenings also lead to treatment of cancer precursors, thereby reducing long-term cancer risk. One key example is *Helicobacter pylori* (*H. pylori*) infection, a well established risk factor for stomach cancer (Polk and Peek Jr, 2010; Uemura et al., 2001; Butt and Epplein, 2019). During stomach screening,

⁴⁵This is called “lead time bias” in health screening. Refer to Morrison (1982); Welch et al. (2000); Duffy et al. (2008); Gordis (2013) and Yang et al. (2021) for detail.

⁴⁶NHIS began subsidizing lung cancer screening for high-risk individuals only after the study period. The subsidized test is a low-dose CT scan, whereas during the study period the dominant method for lung screening was chest X-ray.

such as gastroscopy, a biopsy sample can be collected to test for *H. pylori* infection. If an infection is detected, treatment typically involves a combination of antibiotics and a proton pump inhibitor, administered orally. I define *H. pylori* treatment as the concurrent prescription of two antibiotic classes and a proton pump inhibitor during a single hospital visit. Table 9 Panel A shows that subsidy eligibility increases treatment by 0.003 percentage points (15%), suggesting that stomach screening leads to diagnosis and treatment of the strong stomach cancer precursor.

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

Health screenings also increase the diagnosis and treatment of cardiovascular risk factors, such as high blood pressure, diabetes, and high cholesterol, which are examined during the general screening. Table 9 Panel B shows that subsidy eligibility increases medication use by 0.05 percentage points (0.19%) for high blood pressure, 0.02 percentage points (0.17%) for diabetes, and 0.4 percentage points (3.01%) for high cholesterol.⁴⁷ The magnitude is larger for high cholesterol drugs compared to the other two conditions, reflecting the fact that cholesterol levels are less likely to be measured outside of formal screenings, whereas blood pressure and blood sugar level can be more easily measured during routine visits to general practitioners. These results suggest that screenings increase the diagnosis of chronic conditions and prompt greater take-up of medication for disease management.

The general screening program also includes tuberculosis (TB) detection, especially important in South Korea, which has among the highest TB incidence rates in the OECD (Cho, 2018). Chest X-rays are used for initial screening, followed by sputum tests and nucleic acid amplification if abnormalities are detected. TB treatment typically includes

⁴⁷Appendix Section ?? provides a list of medications for high blood pressure, diabetes, and high cholesterol and reports the impact on each drug.

medications, including isoniazid, rifampin, pyrazinamide, and ethambutol. Table 9 Panel C shows that subsidy eligibility had no significant change in TB-related prescriptions.

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

5.3.3 Effect on mortality

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

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

window and cannot capture potential longer-term benefits of screening.

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

6 Conclusion

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

A policy implication is that providing subsidized screenings not only increase participation, but also improve targeting of health screenings, particularly among low SES pop-

ulations. This study suggests that blanket subsidies, as opposed to subsidies conditional on low income, are effective in improving access among people from low socioeconomic backgrounds. This study also showed that bundling screenings such that they can be received in a single hospital visit can help increase participation in screenings. It can also make subsidies more salient, as people perceive the total amount of subsidies available for various screenings.

An open question is the long-term effects of screenings. The current even/odd age-based design does not allow examining long-term effects due to alternating subsidies. However, to fully capture the benefits of screenings, it is important to examine long-term effects, as benefits of screenings may take time to materialize. Important long-run outcomes to examine are decrease in late-stage cancers, mortality, and health care expenditures. The short-term effects from this study have shown that screenings disproportionately increase early-stage cancer diagnoses. If these early diagnoses represent prevention from progress to late-stage cancers, there should be decline in late-stage cancers in the long run. Failure to find decline in late-stage diagnoses would imply overdiagnosis, that is, detecting early-stage cancers that would not have caused harm during one's lifetime. Similarly, our ultimate goal would be whether screening actually led to decline in mortality. Finally, screenings increase health care spending in the short-run, due to follow-up testing and treatment. We need to find out if this leads to long run drop in spending.

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7 Tables

Table 1: Screening subsidy schedule

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

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

Table 2: Balance table

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

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

Table 3: Effect of subsidies on take-up

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

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

Table 4: Cross spillover across different screening types

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

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

Table 5: Compliers with subsidies

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

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

Table 6: Compliers with subsidies using panel information

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

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

Table 7: Effect on cancer diagnosis

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

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

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

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

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

Table 9: Effect on preventive care use

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

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

Table 10: Effect on one-year mortality

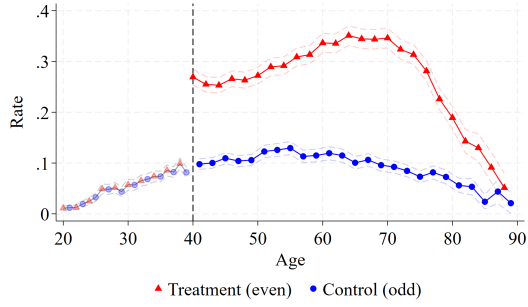
	(1)	(2)	(3)	(4)
	Control group mean	ITT	Percent relative to control	N
Total death	0.009084	−0.000048 (0.000072)	−0.530	6,740,415
Cancer death	0.002805	−0.000015 (0.000041)	−0.548	6,740,415
Stomach cancer death	0.000334	−0.000002 (0.000014)	−0.515	6,740,415
Breast cancer death	0.000153	−0.000011 (0.000013)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

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

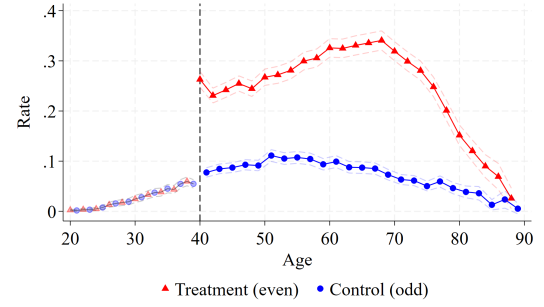
8 Figures

Figure 1: Screening rates

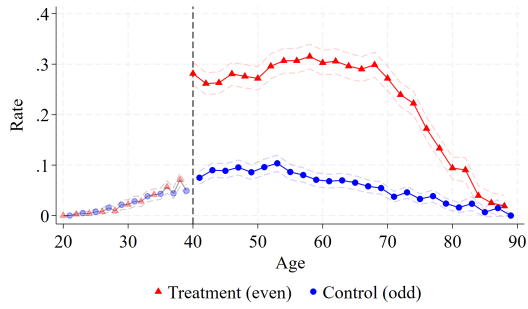
(a) General screening



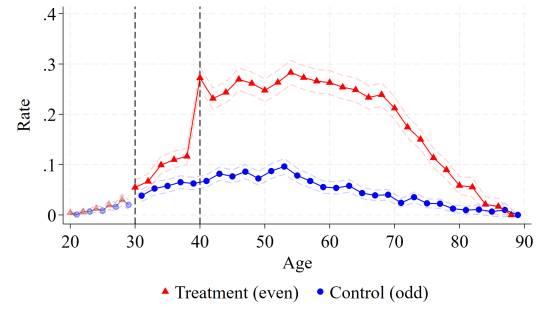
(b) Stomach screening



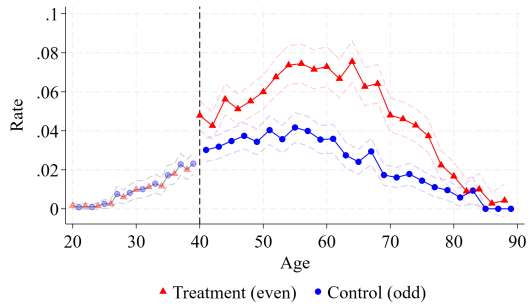
(c) Breast screening



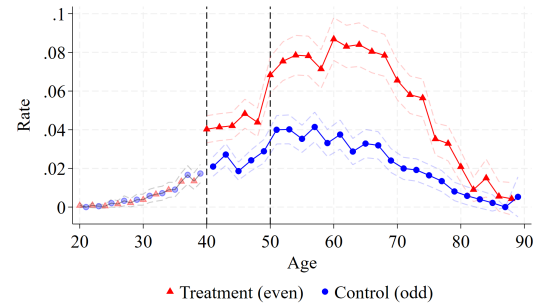
(d) Cervical screening



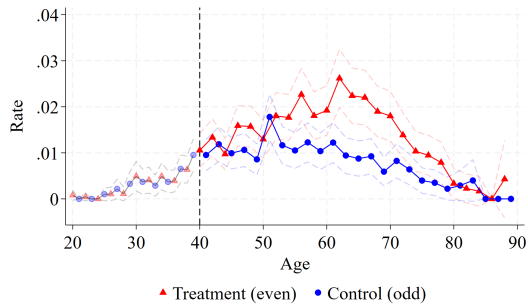
(e) Liver screening



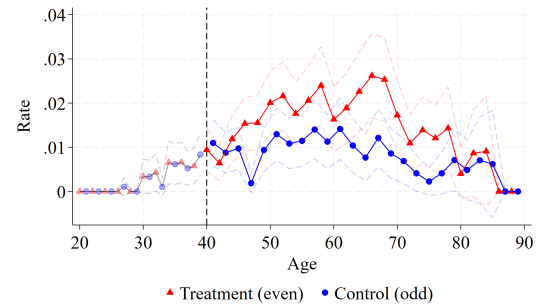
(f) Colorectal screening



(g) Lung screening



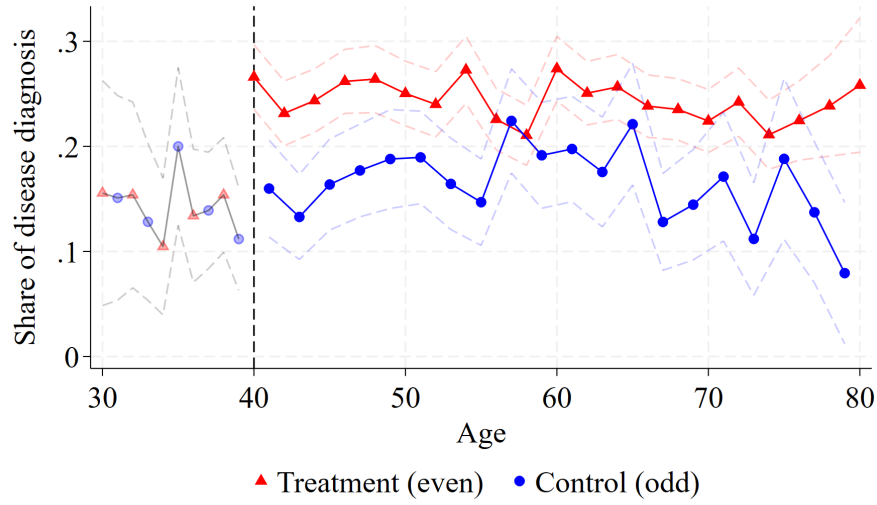
(h) Prostate screening



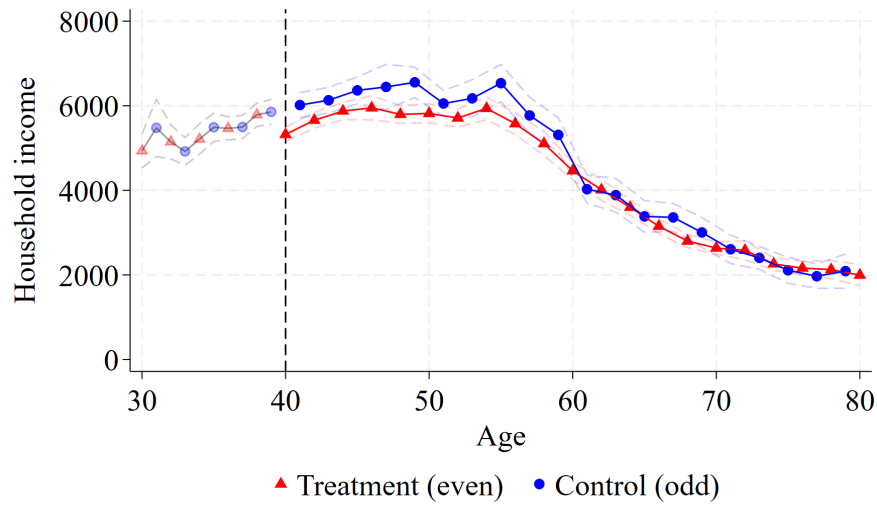
Notes: The figures plot take-up rates for 4 biennially subsidized screenings (general, stomach, breast, cervical), 2 annually subsidized screenings (liver, colorectal), and 2 unsubsidized screenings (lung, prostate) using survey data. Even ages are shown in red, odd ages in blue. Dashed vertical lines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

Figure 2: Screening participants characteristics

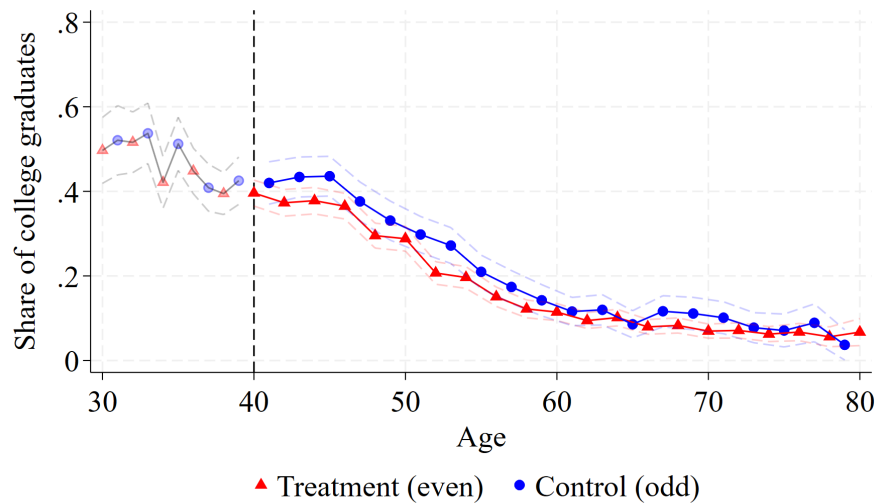
(a) Share of stomach disease diagnosis



(b) Household income



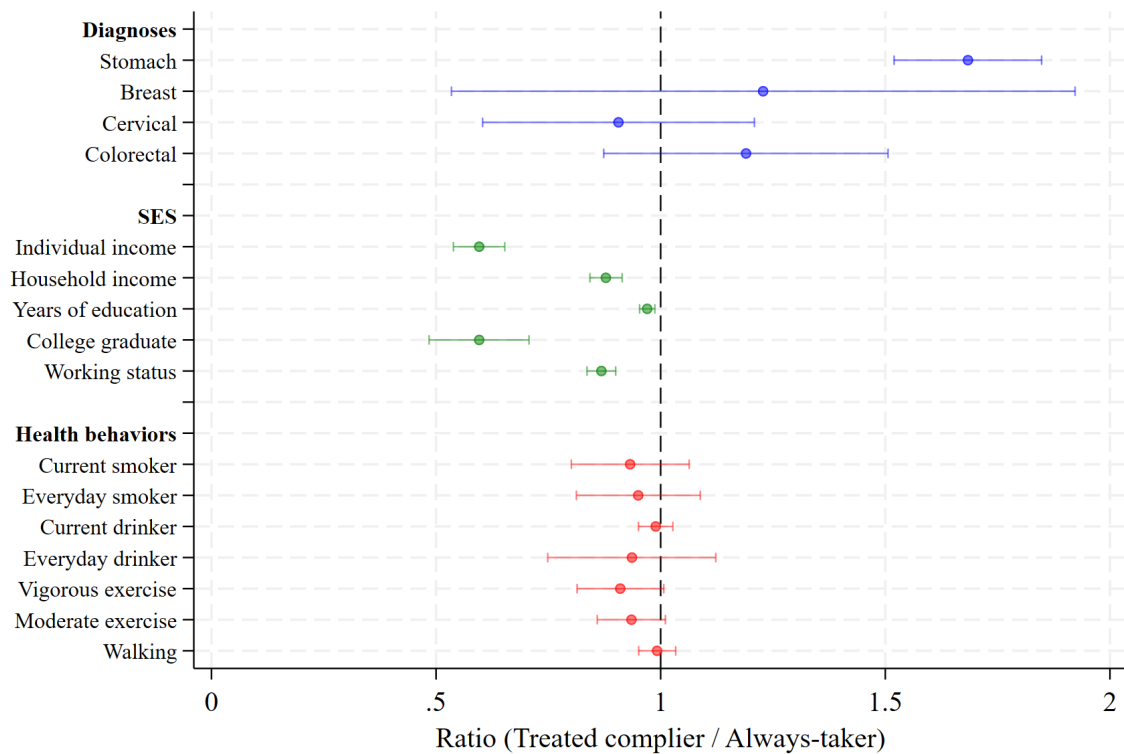
(c) Share of college graduates



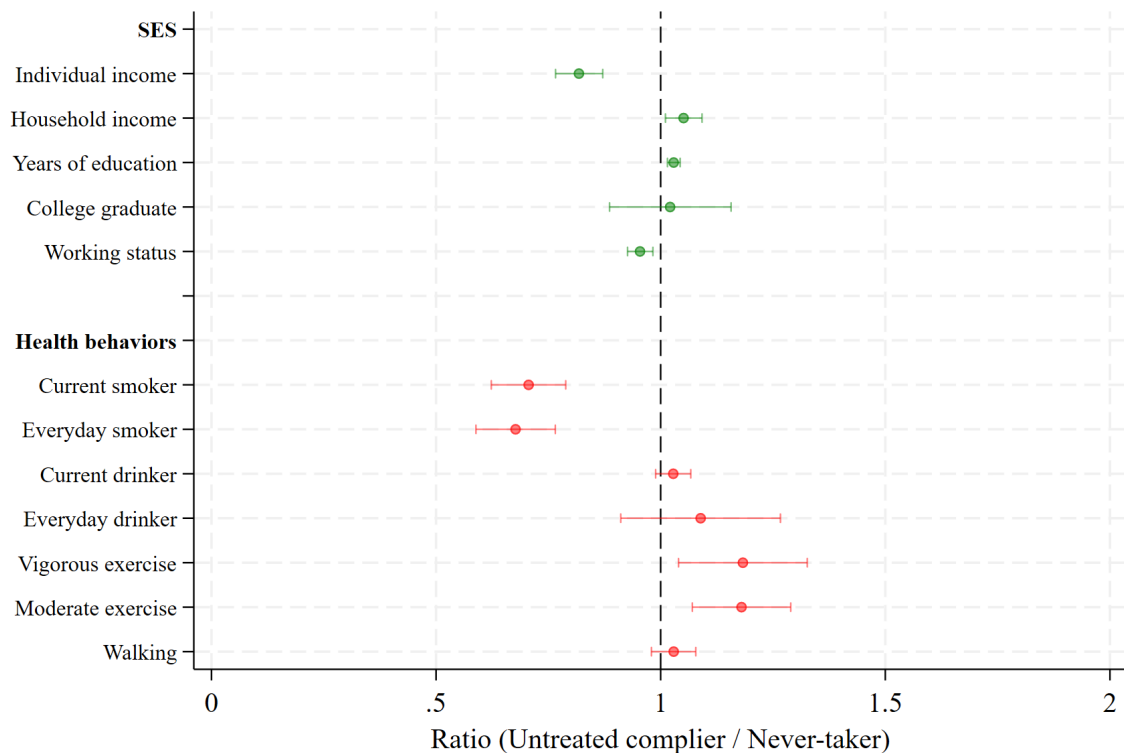
Notes: The first figure plots the share of stomach screenings in which participants were diagnosed with a disease. The sample is restricted to stomach screening participants. Diagnoses are coded using ICD-10, with examples provided in Appendix Section F. The second and third figures plot average household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

Figure 3: Compliers characterization

(a) Comparing compliers with always-takers

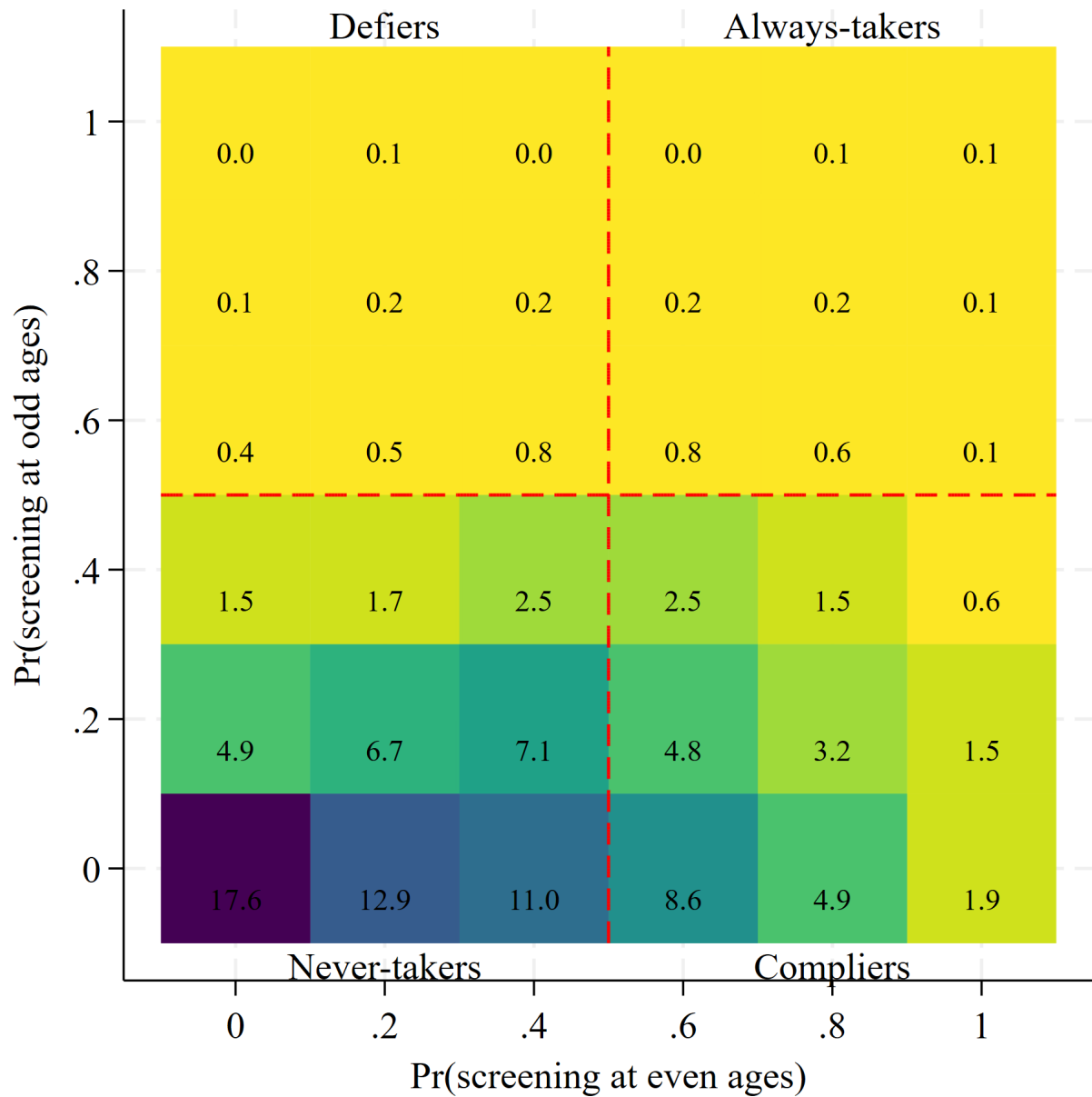


(b) Comparing compliers with never-takers



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

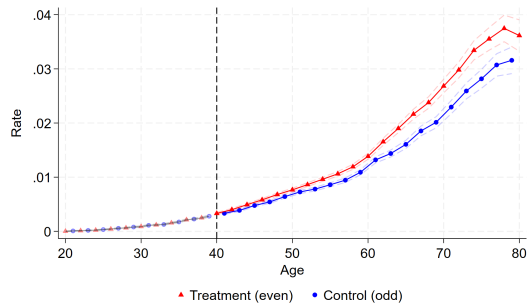
Figure 4: Joint distribution of the screening probability at even and odd ages



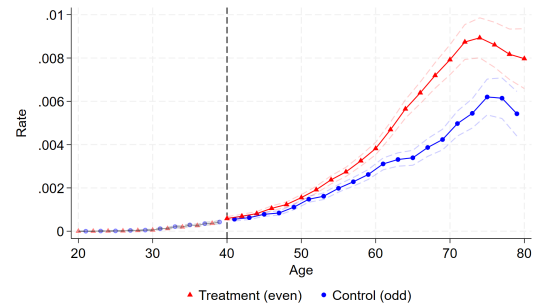
Notes: The figure plots the empirical joint distribution of screening probabilities at even and odd ages, based on 10-year health screening history in the survey data. The sample includes 5,701 individuals aged 40 or above in 2009 who participated in the survey all years from 2009–2018. Compliance groups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

Figure 5: Cancer detection rates

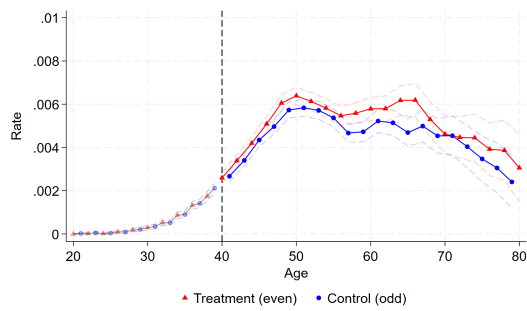
(a) Any cancer



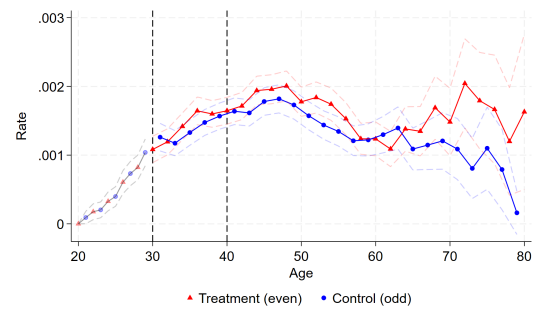
(b) Stomach cancer



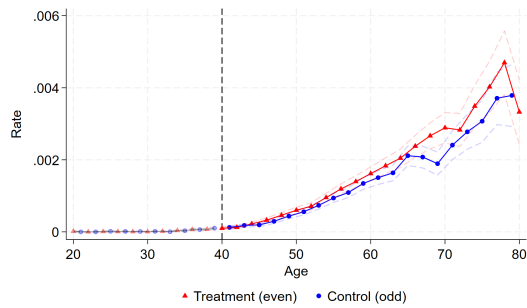
(c) Breast cancer



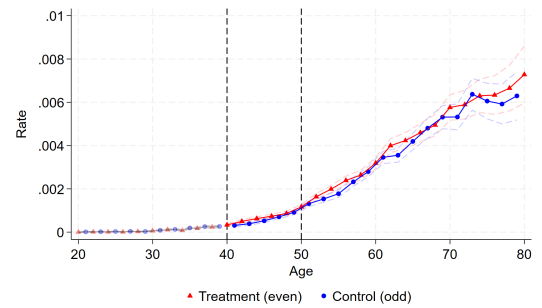
(d) Cervical cancer



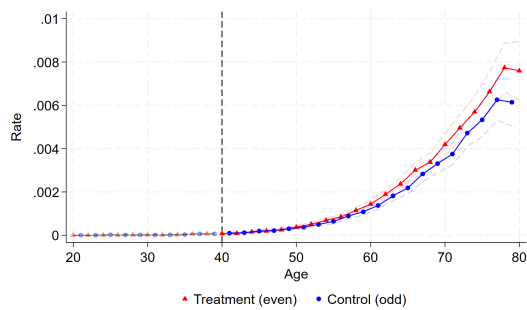
(e) Liver cancer



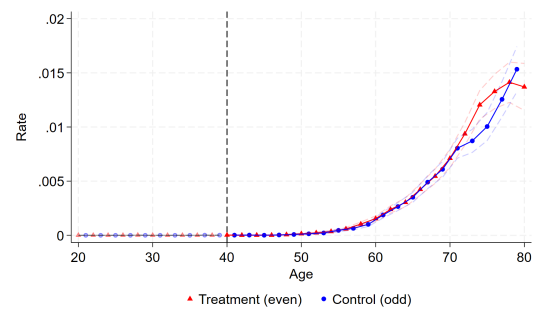
(f) Colorectal cancer



(g) Lung cancer

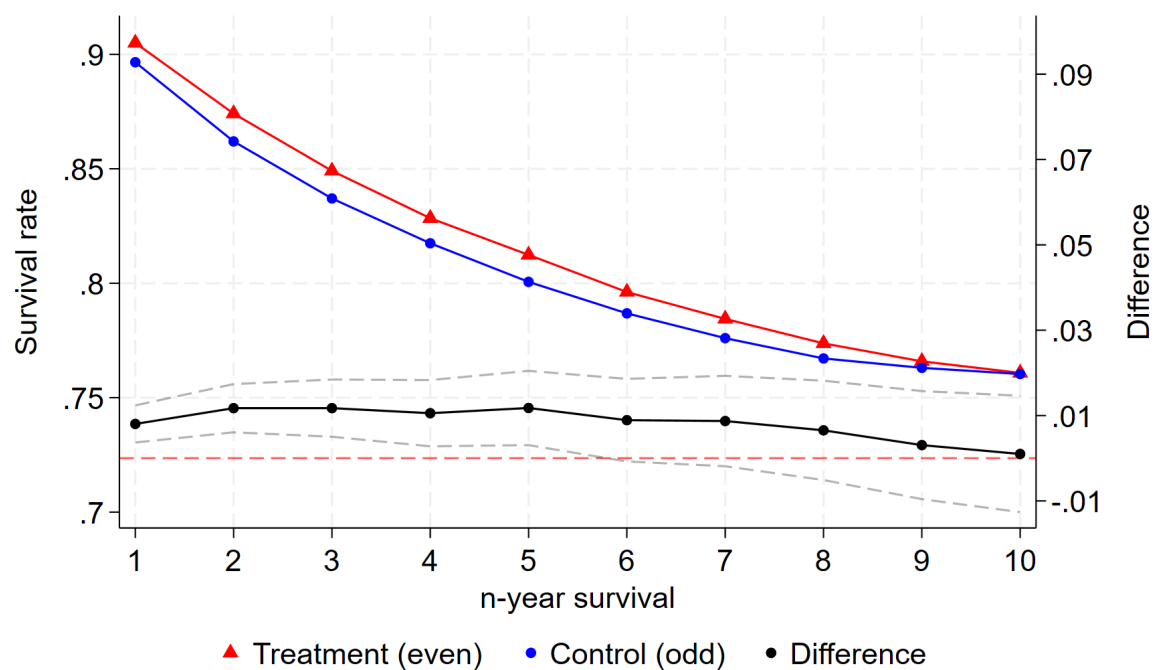


(h) Prostate cancer



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

Figure 6: Cancer survival rates by window



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

Appendix A Balance tests using insurance claims data

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

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

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

Table A1: Balance test with unconditional and conditional differences

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

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

Table A2: Balance table with NHIS Customized Cohort Database

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

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

Table A3: Balance table with NHIS Standard Cohort Database

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

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

Appendix B Robustness checks for age controls

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

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

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

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

Table A4: Robustness check for balance table

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

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

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

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

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

Table A6: Cross spillover with different controls

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

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

Appendix C Robustness checks with different analytical samples

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

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

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

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

Table A7: Balance test with different starting ages

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

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

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

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

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

Table A9: Bounding estimates for cross spillover

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

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

Appendix D Intertemporal substitution

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

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

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

Panel A and B in Figure A2 present stomach and breast screening take-up pattern for participants at age 36, 37, 38 and 39 separately. By definition, age 36 participants show take-up rate of one at age 36.⁴⁸ Comparing before and after age 40, one can see that take-up at unsubsidized ages after 40 is not any lower than pre-40 take-up level, but the take-up at subsidized ages are clearly much higher. This suggests that as one passes age 40, take-up at subsidized ages rise due to subsidies and it does not come at the cost of drop in take-up at unsubsidized ages.

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

⁴⁸One reason I do not examine those who participated at least once before age 40 is due to age 39 participants. By definition, they show average take-up of one at age 39. Therefore, the take-up pattern for those who participated at least once before 40 shows abnormally large take-up at age 39 and makes it hard to compare before and after 40.

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

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

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

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

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

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

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

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

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

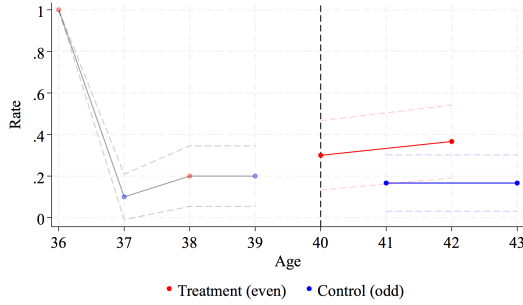
screening take-up. The increase in March is likely driven by reminder mails from regional offices of National Health Insurance Service, typically sent in March and April to inform people of the screenings they should receive. These reminders are sent every year, since odd-aged individuals could also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

Figure A1: Screening take-up for participants before 40

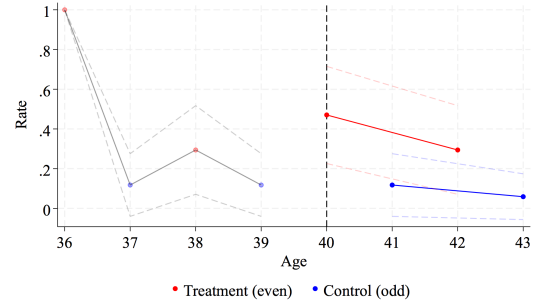
Panel A. Stomach screening participants

Panel B. Breast screening participants

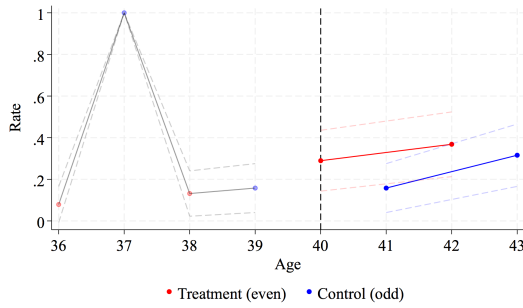
(a) Age 36 participants



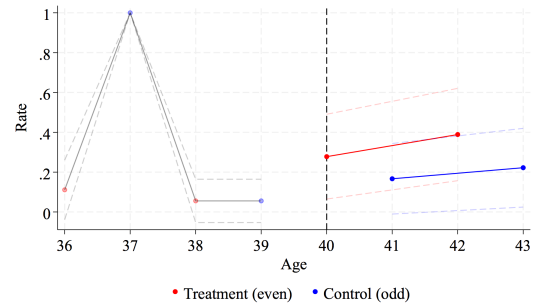
(e) Age 36 participants



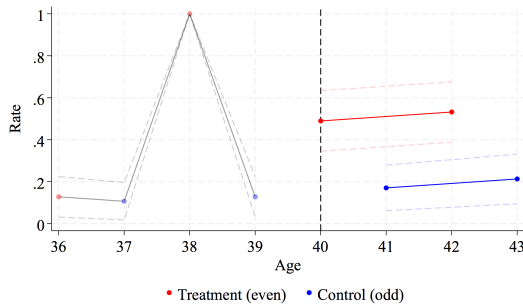
(b) Age 37 participants



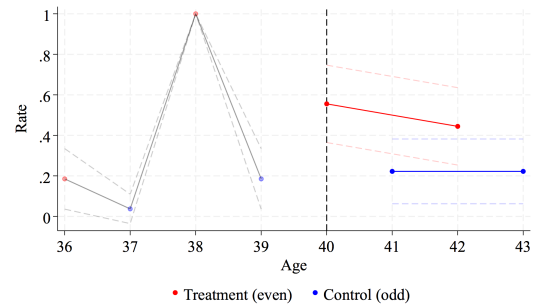
(f) Age 37 participants



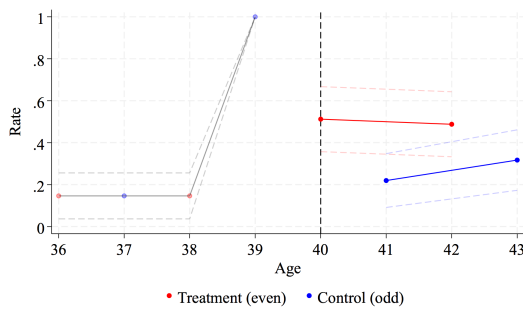
(c) Age 38 participants



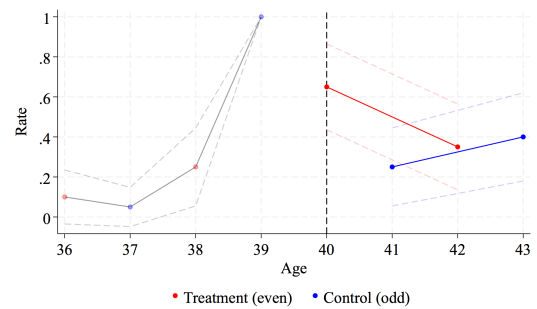
(g) Age 38 participants



(d) Age 39 participants



(h) Age 39 participants

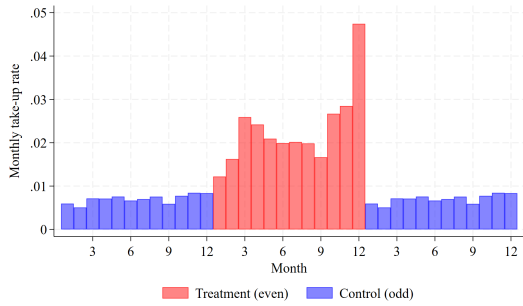


Notes: Figures plot the stomach and breast screening take-up for those who already participate in screening before age 40. The sample is restricted to four age cohorts around age 40. Each figure plots the take-up among either the stomach or breast cancer participants at age 36, 37, 38 or 39. Even ages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

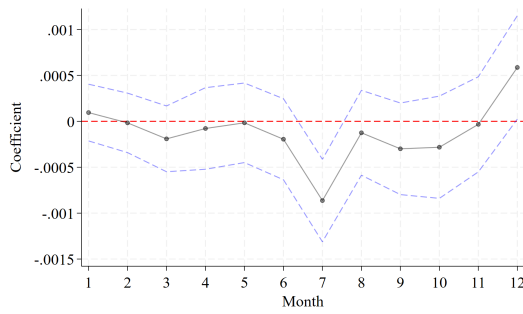
Figure A2: Monthly screening take-up

Panel A. Stomach screening

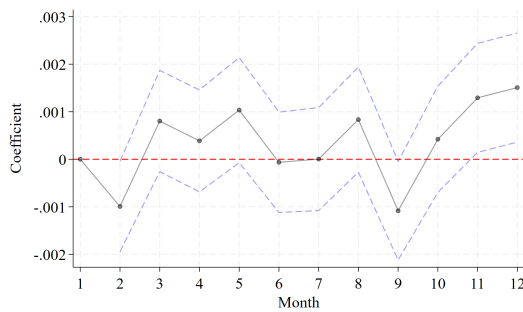
(a) Average monthly take-up



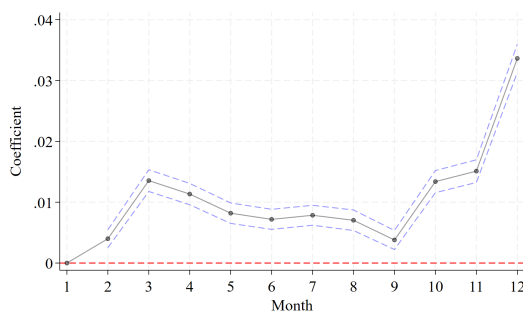
(b) Treatment before 40 vs Control before 40



(c) Control before 40 vs Control after 40

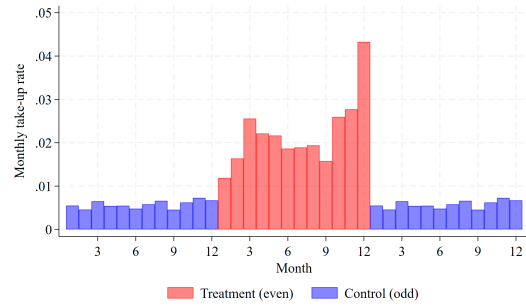


(d) Treatment before 40 vs Treatment after 40

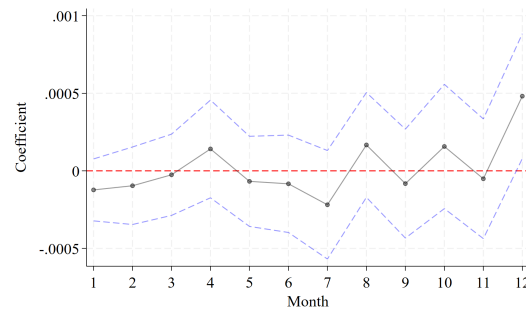


Panel B. Breast screening

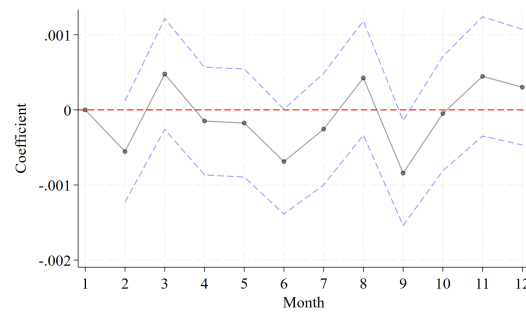
(e) Average monthly take-up



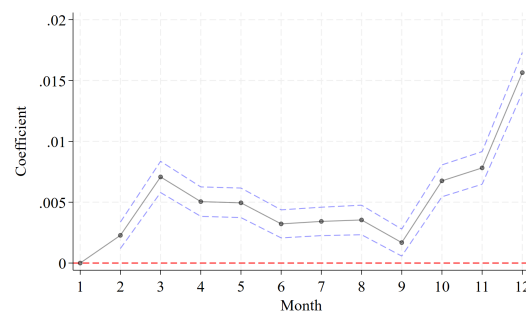
(f) Treatment before 40 vs Control before 40



(g) Control before 40 vs Control after 40



(h) Treatment before 40 vs Treatment after 40



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

Appendix E Additional analyses on cross spillover

This section presents additional analyses on the cross spillover, discussed in Section 5.1 in the main text. Table A10 presents the share of screenings that happen on the same day with general screening. For instance, among stomach screening participants, 88 percent also receive general screening in the same year. Among participants in general and stomach screenings, 96 percent receive the two screenings on the same day. If they are not received on the same day, 2.7 percent receive general screening first followed by stomach screening within 30 days. On the other hand, only 0.8 percent receive stomach screening first followed by general screening.

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

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

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

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

The sample is restricted to biennial screening participants at even ages.⁴⁹ The explanatory variable, $screen_{it}$, is an indicator variable for participating in any of the annual or unsubsidized screenings. Standard errors are clustered at the individual level.

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

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

Table A10: Share of screenings taken on the same day

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

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

⁴⁹When the outcome variable is a diagnosis of a stomach disease, the sample is restricted to stomach screening participants, with similar restrictions applied to breast and cervical screenings. For the other outcome variables, the sample is restricted to participants in any of the four biennial screenings.

Table A11: Heterogeneity in cross spillover effects across gender

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

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

Table A12: Compliers in cross spillover

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

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

Appendix F Disease diagnoses from the survey data

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

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

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

Appendix G Complier selection

G.1 Methodology for characterizing compliers

To formally estimate the average group characteristics and make comparisons between always-takers, compliers and never-takers, I follow the approach used in [Kim and Lee \(2017\)](#); [Einav et al. \(2020\)](#) and [Kowalski \(2023\)](#).⁵⁰ The key idea is to infer always-takers' characteristics from screening participants in the control group and infer never-takers' characteristics from screening nonparticipants in the treatment group. The exogeneity of the assignment mechanism guarantees that the characteristics of always- and never-takers will be the same in both the treatment and the control group. The compliers' characteristics can be backed out from the equation where the characteristic of screening participants (nonparticipants) in the treatment (control) group is a weighted sum of always-takers' (never-takers') and compliers' characteristics, with the weights corresponding to the relative share of each group.

I present detailed steps to infer complier characteristics in the even/odd subsidy design.⁵¹ I estimate the following equation to estimate group characteristics.

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

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

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

⁵⁰[Marbach and Hangartner \(2020\)](#) gives a nice summary of the methodology.

⁵¹Appendix in [Einav et al. \(2020\)](#) present the detailed steps to characterize compliers at age 40 in the regression discontinuity setting.

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

To calculate the characteristics of treated compliers, denote the average characteristic of treated compliers as $g_C^1(y)$, untreated compliers as $g_C^0(y)$, and the screening participants in the treatment group as $g_T(y)$. The screening participants in the treatment group are always-takers and treated compliers whose average is given by $g_T(y) = \frac{\pi_{AT}}{\pi_{AT} + \pi_C} g_{AT}(y) + \frac{\pi_C}{\pi_{AT} + \pi_C} g_C^1(y)$, where π_{AT} and π_C are share of always-takers and compliers, respectively. Imposing $age_even_{it} = 1$ and $screen_{it} = 1$, we get $g_T(y) = \hat{\lambda}_1 + \hat{\lambda}_2 + \hat{\lambda}_3 + \hat{\lambda}_4' \mathbf{f}(\mathbf{age}_{it})$. The share of always-takers and compliers can be calculated from the first stage regression given in (1) as $\pi_C = \hat{\beta}_1$, $\pi_{AT} = \hat{\beta}_0$ adjusting for age.⁵² 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.⁵³

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, $\frac{g_C^0(y)}{g_{NT}(y)}$.⁵⁴ 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.

⁵²Under monotonicity, the share for never-takers is $\pi_{NT} = 1 - \pi_{AT} - \pi_C = 1 - \hat{\beta}_0 - \hat{\beta}_1$

⁵³I examine selection pattern at age 60 by imposing $age_{it} = 60$. The results are robust to the different choices of age.

⁵⁴The reason I differentiate treated and untreated compliers is the possibility that health screening may affect health behaviors. While demographic variables are in general pre-specified and not likely to be affected by health screening, health behaviors such as smoking or drinking can be affected by health screening. Comparing treated compliers with never-takers may be contaminated since treated compliers have received screening while never-takers have not. Same applies to comparison between untreated compliers with always-takers. Therefore, I compare always-takers with treated compliers, both of whom participated in screening, and never-takers with untreated compliers, both of whom did not participate in screening.

G.2 Health behaviors among female sample

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

Table A13: Health behaviors among female compliers with subsidies

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

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

G.3 Additional analyses on selection

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

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

Panel B presents tests not covered by NHIS. Since these tests are not covered, they are usually paid out-of-pocket by patients.⁵⁵ Compliers are less likely to use these tests than always-takers. This finding is consistent with negative selection of compliers in terms of socioeconomic status. It also suggests that compliers' worse health condition, as evidenced by higher probability of finding a disease through screening, is not because compliers receive more medical tests.

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

⁵⁵CT is Computed Tomography scan, MRI is Magnetic Resonance Imaging scan, PET is Positron Emission Tomography scan, EEG is Electroencephalogram, and EKG is Electrocardiogram. While bone density test is part of general screening, it is subsidized only for women at age 54 and 66. For male or women at different ages, the test should be paid out-of-pocket.

Table A14: Comparing medical tests received

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

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

Table A15: Compliers with subsidies among non-working population

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

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

Appendix H Cancer diagnosis

This section provides detailed survival rates for cancer patients. For each cancer examined in this study, Table A16 and A17 present 1, 3, 5, 7, and 10 year survival rates, separately for those diagnosed in the treatment (even age) and the control (odd age) group.

Table A16: Survival rate comparisons

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

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

Table A17: Survival rate comparisons

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

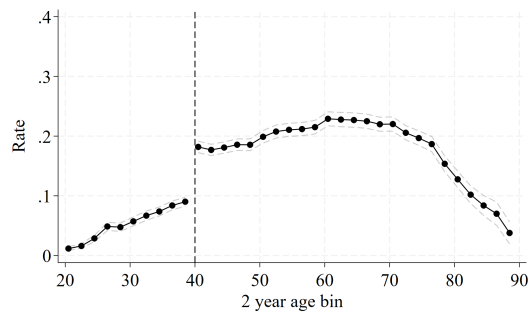
Notes: This table compares the survival rates of cancers diagnosed at even ages with the ones diagnosed at odd ages. The sample consists of cancer diagnosis made at age in [40, 89]. Column 1 reports the survival rates of the cancers diagnosed at odd ages. Column 2 reports the difference in survival rates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

Appendix I Regression discontinuity analysis using age cutoffs

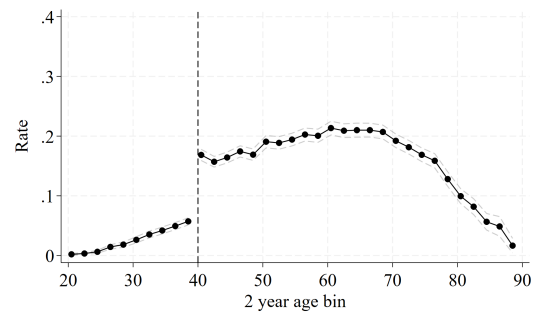
This section examines screening take-up, selection into screening, and causal effects of screenings around the age cutoffs using a regression discontinuity (RD) design. Subsidy eligibility begins at age 40 for most screenings, with exceptions at age 30 for cervical and 50 for colorectal screenings. Since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

Figure A3: 2-year average screening rates

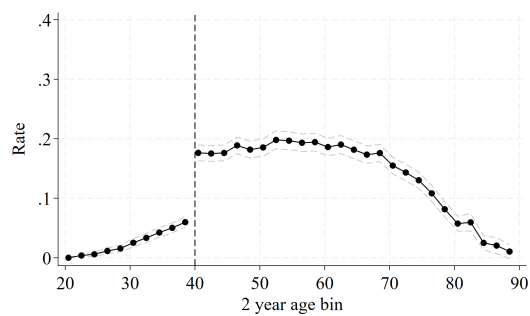
(a) General screening



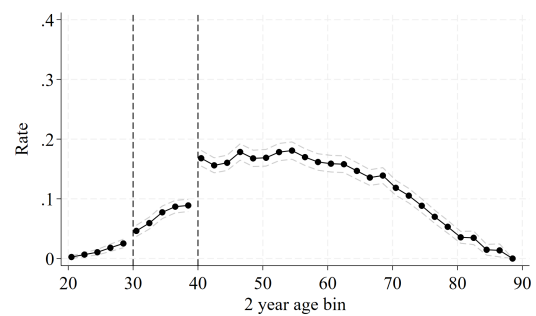
(b) Stomach screening



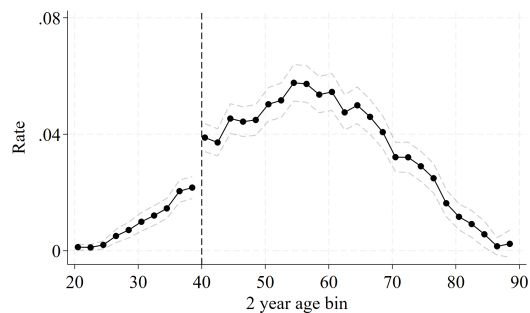
(c) Breast screening



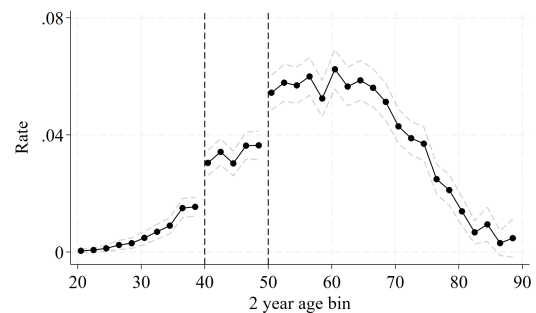
(d) Cervical screening



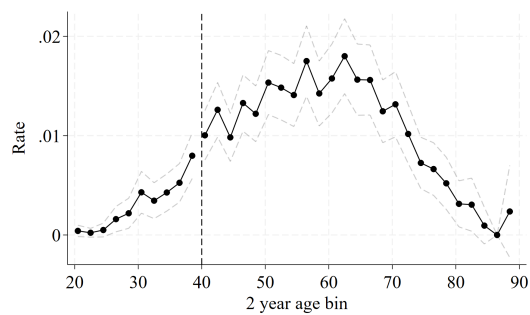
(e) Liver screening



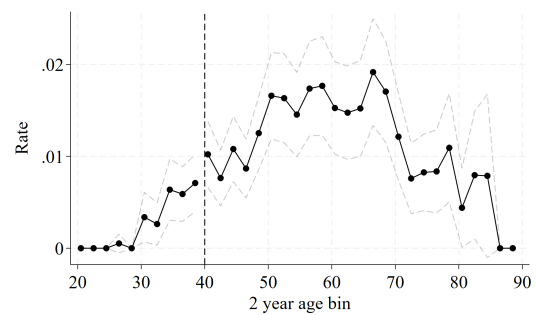
(f) Colorectal screening



(g) Lung screening



(h) Prostate screening



Notes: Figures show the 2-year average screening rates using the survey data. Ages are binned by two years and the average screening rates within each 2-year bins are plotted with 95% confidence intervals. Dashed vertical lines show the subsidy starting age and/or age 40.

Table A18: 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
<i>Age</i> \geq 40	0.086*** (0.007)	0.105*** (0.006)	0.112*** (0.009)		0.011*** (0.004)	
<i>Age</i> \geq 30				0.009 (0.007)		
<i>Age</i> \geq 50						0.015*** (0.005)
Constant	0.095*** (0.005)	0.061*** (0.004)	0.064*** (0.005)	0.029*** (0.004)	0.024*** (0.002)	0.039*** (0.003)
N	34713	34713	17725	12168	34713	34819
Adj <i>R</i> 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 subsidized screenings using the survey data. Regression discontinuity design is used 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 (??). Coefficients for *Age* \geq *c* measures the jump in take-up at age *c* and the constant term 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 A19: Complementarity in screening take-up

	(1)	(2)
	Cervical	Colorectal
<i>Age</i> ≥ 40	0.074*** (0.010)	0.014*** (0.003)
Constant	0.093*** (0.006)	0.018*** (0.002)
N	17725	34713
Adj <i>R</i> ²	0.013	0.004
Sample age range	[34, 45]	[34, 45]
Subsidy starting age	30	50
Age controls		
Control group mean		
Percentage increase	80	78

Notes: This table reports the cross spillover in take-up generated due to different subsidy starting age. 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 (??). Standard errors are clustered at the individual level and reported in parentheses. A */**/** indicates significance at the 10/5/1% levels.

Table A20: Compliers with subsidies using age 40 cutoff

	(1)	(2)	(3)	(4)	(5)	(6)
	Average value				Ratio	
	Always-takers	Treated compliers	Untreated compliers	Never-takers	CP_1/AT	CP_0/NT
Panel A. Diagnoses						
Stomach	0.125 (0.024)	0.302 (0.028)	- -	- -	2.410** (0.670)	- -
Breast	0.027 (0.014)	0.028 (0.016)	- -	- -	1.047 (7.810)	- -
Cervical	0.166 (0.028)	0.030 (0.053)	- -	- -	0.181** (0.373)	- -
Colorectal	0.143 (0.044)	0.179 (0.113)	- -	- -	1.248 (1.820)	- -
Panel B. SES						
Individual income	2522 (110)	1887 (204)	1767 (368)	2179 (46)	0.748** (0.100)	0.811 (0.176)
Household income	5906 (139)	4763 (255)	4581 (704)	4744 (67)	0.806*** (0.054)	0.966 (0.158)
Years of education	14.169 (0.109)	13.812 (0.194)	13.990 (0.370)	13.658 (0.051)	0.975 (0.018)	1.024 (0.029)
College graduate	0.392 (0.025)	0.414 (0.041)	0.397 (0.078)	0.323 (0.010)	1.057 (0.151)	1.228 (0.257)
Working status	0.780 (0.021)	0.655 (0.039)	0.578 (0.085)	0.758 (0.009)	0.840** (0.064)	0.763** (0.116)
Cancer insurance	0.296 (0.023)	0.341 (0.041)	0.322 (0.075)	0.251 (0.009)	1.153 (0.195)	1.281 (0.322)
Panel C. Health behaviors						
Current smoker	0.197 (0.021)	0.199 (0.038)	0.384 (0.082)	0.288 (0.011)	1.012 (0.276)	1.335 (0.306)
Everyday smoker	0.184 (0.021)	0.191 (0.037)	0.371 (0.082)	0.272 (0.010)	1.041 (0.295)	1.364 (0.323)
Current drinker	0.869 (0.019)	0.776 (0.036)	0.832 (0.076)	0.819 (0.009)	0.894** (0.052)	1.017 (0.096)
Everyday drinker	0.038 (0.009)	0.039 (0.019)	0.070 (0.046)	0.047 (0.005)	1.035 (0.778)	1.489 (1.078)
Vigorous exercise	0.297 (0.024)	0.233 (0.046)	0.295 (0.088)	0.266 (0.009)	0.785 (0.202)	1.107 (0.354)
Moderate exercise	0.404 (0.026)	0.456 (0.050)	0.348 (0.104)	0.396 (0.010)	1.128 (0.183)	0.879 (0.276)
Walking	0.791 (0.022)	0.803 (0.042)	0.892 (0.092)	0.755 (0.009)	1.015 (0.072)	1.181 (0.130)

Notes: This table reports the average values of screening diagnoses, socioeconomic status, and health behaviors among always-takers, never-takers, treated compliers and untreated compliers. The compliance groups are defined using the age 40 cutoff. Ages are binned by two years to average out the substitution effect between the even and odd years after the age 40. Treated compliers are compliers in the treatment group who participate in screening. Untreated compliers are compliers in the control group who do not participate. Diagnoses are not reported for untreated compliers and never-takers, since by definition, they do not receive screening. The null hypotheses used for ratios are $H_0 : CP_1/AT = 1$ and $H_0 : CP_0/NT = 1$ for comparison with always-takers and never-takers, respectively, where AT = Always-takers, NT = Never-takers, CP_1 = Treated compliers and CP_0 = Untreated compliers. Standard errors are calculated using bootstrap with replications and are clustered at individual level. They are reported in parentheses. A */**/** indicates significance at the 10/5/1% levels.