Avoiding the Risk, Bearing the Cost: Evidence from General Health Screening in Korea during the COVID-19 Pandemic*

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Abstract

This study examines the unintended health consequences of voluntary responses to COVID-19. We focus on general health screening in Korea, using administrative data that link medical claims and screening records. At the national level, screening rates declined markedly in 2020, the first year of the pandemic, relative to counterfactual trends. Complementing this aggregate pattern, individual-level analysis reveals notable heterogeneity: declines were larger among those with higher predicted risk of chronic disease. We then assess the consequences of forgone screening, employing event study designs and propensity score matching. Our estimates show that, had they been screened, individuals who missed screening would have been more likely to initiate care for chronic diseases. The costs of missed screening were especially large among those at higher predicted risk of chronic disease. Such delays in management led to more advanced conditions at the time of care initiation. Taken together, the results show that, even without strict quarantine policies, voluntary responses to infection risk can undermine preventive care, with the burden concentrated among those who stand to benefit most. Policies during health crises should therefore balance infection control with the continuity of preventive care.

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1 Introduction

During the COVID-19 pandemic, individuals voluntarily adopted preventive behaviors—such as mask-wearing and avoiding public spaces—in response to infection risk (Gupta, Simon, and Wing 2020). This behavior is known in the economics literature as a prevalence response (Philipson 2000). Notably, recent evidence suggests that government-imposed interventions (e.g., lockdowns) played a relatively limited role, whereas these voluntary responses accounted for much of the observed reduction in infection rates (Cantor et al. 2022; Ziedan, Simon, and Wing 2020; Agrawal et al. 2023).

These same behaviors, however, may also have carried unintended harms. For example, the avoidance of health care facilities is likely to have disrupted routine and preventive care, such as health screening and chronic disease management, potentially worsening health outcomes for vulnerable populations (Bennett, Chiang, and Malani 2015). These concerns highlight the need for a clearer understanding of the unintended health consequences associated with voluntary responses, as such knowledge could help inform public health policies that better balance infection control with the mitigation of collateral harms. Yet empirical evidence on these consequences remains limited (Dorn et al. 2023). Indeed, economic research on prevalence responses has largely emphasized the benefits of self-protective behavior in slowing transmission, rather than the preventive care such behavior may crowd out.[1]

Such evidence is scarce in part because identifying the health impact of voluntary responses presents important empirical challenges. First, it is difficult to disentangle these individual-level decisions from the effects of public health interventions. In many countries, declines in non-COVID health care utilization occurred alongside government-imposed mobility restrictions. In addition, infection control policies, such as the temporary closure of facilities following confirmed patient visits, may have reduced the supply of non-COVID care. Second, even in the absence of such

1. Empirical prevalence-response research has concentrated on the benefits of voluntary responses: local pertussis outbreaks raised vaccination (Oster 2018b; Schaller, Schulkind, and Shapiro 2019), and H1N1 produced the unintended benefit of improved hygiene (Agüero and Beleche 2017; Hong, Lee, and Oh 2022). By contrast, this paper focuses on the cost side.

interventions, other pandemic-related factors, such as actual infection or income shocks, may have influenced health care demand, complicating efforts to isolate the effect of voluntary responses alone.

In this regard, Korea during the early phase of the COVID-19 pandemic offers a suitable setting for identifying the health impact of voluntary responses, for two reasons. The first is the nature of Korea's pandemic response. Unlike other high-income countries, Korea did not implement lockdowns in 2020 (Ariadne Labs 2025). Its health care system remained relatively stable, partly due to the low number of confirmed cases (about 1,200 per one million people by year-end, well below the global average) (Mathieu et al. 2020), and only a small number of health care facilities experienced temporary closures. These conditions minimize confounding from policy-induced constraints on health screening take-up. The second is the design of the general health screening program, which covers the entire population and imposes no out-of-pocket costs (Kang 2022). As a result, economic barriers to screening take-up are minimal, reducing potential confounding from income shocks.

We use administrative data from the National Health Insurance Service (NHIS), which administers the program as Korea's single insurer—an institutional feature that lets a single database link medical claims with health screening records. Beyond documenting the aggregate decline, this allows us to examine heterogeneity in individuals' responses to COVID-19 based on their health status, and to quantify the health consequences of missed screenings.

To examine the effect of COVID-19 on health screening participation, we conduct both a national-level and an individual-level analysis. At the national level, we use a modified interrupted time series approach, comparing actual screening rates in 2020 and 2021 against a counterfactual trend estimated from 2017–2019 data. Complementing this, the individual-level analysis investigates whether responses to COVID-19 varied according to individuals' risk of chronic disease. Higher risk raises both the value of screening and, through greater vulnerability to infection, the perceived cost of seeking it, leaving the direction of response ex ante unclear. To do so, we focus on individuals who had not used health care for hypertension or diabetes in the past three years,

and we predict their risk of chronic disease using prior screening records and claims data.

At the national level, screening rates fell sharply during the initial outbreak in early 2020 before recovering gradually, a pattern consistent with intertemporal substitution; by year-end, rates remained 7.52 percentage points below the counterfactual. In 2021, by contrast, screening rates tracked the counterfactual closely despite a higher caseload, indicating that the disruption was largely confined to the first year of the pandemic. Beneath this aggregate decline, the individual-level analysis reveals a clear gradient in chronic disease risk: among those without prior hypertension care, screening fell by 3.85 percentage points in the lowest-risk quintile and by 6.15 percentage points in the highest, with a similar but weaker pattern for diabetes risk. The groups that pulled away most (older adults, medical aid recipients, and those with elevated biomarkers) were precisely those for whom screening is most valuable.

To assess the health consequences of these missed screenings, we estimate how screening would have affected chronic disease care. Our research design centers on an event study that traces care utilization around the screening month. By testing for the absence of pre-trends and conducting placebo exercises on untreated individuals, it probes whether unobserved selection drives the results. The event study identifies the effect for those actually screened. To price the care forgone by those who did not screen, we report a distinct, policy-relevant estimand, the average treatment effect on the untreated (ATU), recovered by propensity-score matching on a rich set of covariates. The two designs rest on different, non-nested assumptions and yield a consistent picture.

The event study shows that care utilization rises sharply once individuals are screened, for both hypertension and diabetes, and stays elevated thereafter. The untreated show no such change around their pseudo screening month, and care is flat in the months before screening—evidence against unobserved selection. The dynamics thus point to a causal effect of screening on chronic disease care. Turning to the unscreened, the ATU implies that screening would have raised the probability of initiating care among the untreated by 2.58 percentage points (about 35% of their baseline), with comparable effects for hypertension and diabetes. Screening delays during the pandemic thus meaningfully set back the timely management of chronic disease. These effects,

too, are concentrated among the high-risk: the ATU rises steeply across risk quintiles, reaching 6.90 percentage points in the top quintile for hypertension—the same groups that cut screening most. Voluntary avoidance thus fell hardest on those who stood to gain the most. These delays also carried clinical costs. Among those who began care, screening would have reduced the chance of presenting with complications that had already developed, by 5.72 percentage points for hypertension (about 20% of the untreated mean) and 2.94 percentage points for diabetes (about 13%); this is evidence that forgone screening pushed disease management toward more advanced stages.

We make three contributions to the literature on health care disruptions during the COVID-19 pandemic. First, we document the health consequences of voluntary responses to infection risk, separately from the policy-induced disruptions with which they are usually entangled. The broad decline in pandemic-era utilization is well documented (Whaley et al. 2020; McBain et al. 2021; Danagoulian and Wilk 2022), but the voluntary component has received far less attention, in part because it is empirically challenging to separate from concurrent disruptions. Korea's setting overcomes this difficulty: with no lockdowns, no out-of-pocket screening cost, and stable care supply, it isolates the voluntary-avoidance channel. A close comparison with Ziedan, Simon, and Wing (2022) sharpens this contribution: whereas they analyze how the health care system prioritized services during the crisis, we examine how individuals prioritized their own health needs. While policy-driven shocks typically fade once restrictions are lifted, voluntary avoidance stems from individual perceptions of risk and thus calls for different policy responses.

Second, by focusing on a single type of care, general health screening, we can examine how individuals trade off the chronic disease risk that screening targets against the infection risk that COVID-19 introduces. The trade-off cuts both ways: the very condition that makes screening most valuable also raises the cost of infection (Geng et al. 2021; Centers for Disease Control and Prevention 2025), so whether high-risk individuals screen more or less during a pandemic is an ex ante empirical question. As Chandra and Skinner (2012) emphasize, the welfare impact of forgone care depends on whether it is high- or low-value; we show that high-risk individuals—whose

screenings are most likely high-value—were disproportionately likely to forgo them.

Finally, we provide more granular evidence on the health burden of the pandemic. As Chen and McGeorge (2020) argue, assessing the health consequences of pandemics requires attention to intermediate outcomes that shape longer-term mortality. Yet early research has focused primarily on excess mortality (Laliotis et al. 2023; Zhang 2021). We document an important channel through which disrupted health care translates into long-term health consequences, focusing on delays in the management of chronic conditions.

The rest of the paper is organized as follows. Section 2 provides background for our study. Section 3 describes the data. Section 4 analyzes changes in screening rates during the COVID-19 pandemic, and Section 5 examines the effects of health screenings on chronic disease management. Section 6 concludes.

2 Background

2.1 COVID-19 in Korea and Government Response

Two features of Korea's first pandemic year—a low caseload and the absence of strict containment measures—help isolate the health consequences of voluntary responses from confounding by infection or policy.

The low infection rate in the first year of the pandemic limits the possibility that either COVID-19 infection itself or constraints on health care provision significantly affected screening rates. Korea experienced three major waves of infection during 2020 (Kim et al. 2021). The first wave followed the country's first confirmed case on January 20, 2020, with 10,774 confirmed cases concentrated in two regions—Daegu and Gyeongsangbuk-do. The second wave emerged around August, centered on religious facilities and mass gatherings in the Seoul metropolitan area. The third wave, which began at the end of the year, was larger in scale than the previous two and spread nationwide. Despite these waves, as of the end of December 2020, Korea's cumulative number of confirmed cases was approximately 1,208 per one million population—substantially lower than the

global average of 12,720 (Mathieu et al. 2020). In addition, Korea continued to provide non-COVID health care during the pandemic, supported by its ample health care capacity (Oh et al. 2020; Her 2020).

The Korean government's approach to managing COVID-19 is another critical feature of the setting. Throughout 2020, Korea did not implement strict containment measures such as stay-at-home orders, which were widely adopted in other high-income countries (Ariadne Labs 2025). Instead, the government focused on large-scale diagnostic testing and contact tracing of confirmed cases, encouraging voluntary preventive behavior by sharing such information through regular public briefings (Her 2020). Consistent with this strategy, social distancing, which began on February 29, 2020, was also implemented as a public campaign. At the peak of the first wave, additional measures were introduced, including the temporary closure of multi-use facilities and public institutions. While the stringency of containment policies was adjusted several times depending on the spread of COVID-19, no nationwide mobility restrictions or limitations on access to health care facilities were imposed. In particular, although social distancing was applied to multi-use facilities, it did not formally restrict health care facilities, making it likely that the provision of health screening was not curtailed.[2]

2.2 National Health Screening Program in Korea

Korea's national health screening program, the setting for our analysis, consists of four major components, classified by target age group and purpose: general and cancer screenings for adults, screenings for adolescents, and screenings for infants and young children. This study focuses on

2. In some instances, health care facilities temporarily closed following visits by COVID-19 patients. However, such closures are unlikely to have significantly affected the overall supply of health care. According to data from loss-compensation applications submitted by health care facilities in 2020, only about 1.7% of facilities were officially closed, suspended, or ordered to disinfect due to confirmed-case exposure (Central Disaster and Safety Countermeasures Headquarters 2020a, 2020e, 2020d, 2020c, 2020b). If the degree of disruption were sufficient to restrict health care supply, we would expect to observe a subsequent increase in unmet care needs resulting from supply-side constraints. To examine this possibility, we refer to responses from the KNHANES regarding the incidence and reasons for unmet care needs (Appendix Figure A3). The proportion of individuals reporting unmet care needs in fact slightly declined, from 0.076 before the pandemic to 0.062 during the pandemic. At the same time, fear of infection became a more prominent reason for forgoing care—consistent with voluntary avoidance.

the general health screening, which serves as a primary route for diagnosing chronic conditions such as hypertension and diabetes.[3]

Since its introduction in 1980 for government employees and private school staff, Korea's national health screening program has gradually expanded its target population. This expansion, aimed at maximizing participation, applies to the general health screening as well (Kang 2022). All costs are fully covered by the NHIS, and eligible individuals can choose any time within the year to receive the screening. The number of screening institutions increased steadily from 16,411 in 2011 to 23,030 in 2019 (National Assembly Budget Office 2021), improving accessibility and minimizing supply-side constraints. As of 2020, regional subscribers (i.e., self-employed and non-employed individuals) are eligible for screening once every two years if they are household heads or household members aged 20 or older.[4] Among employee-insured individuals, non-office workers undergo screening annually, while office workers are screened once every two years (National Health Insurance Service 2021). Dependents of employee-insured individuals are also eligible for screening once every two years if they are aged 20 or older.

The institutional characteristics described above make the general health screening program a suitable context for analyzing the effects of voluntary responses to COVID-19. First, the program carries no out-of-pocket cost and remains highly accessible—over 23,000 institutions nationwide, with the timing left to the individual within the year—so that screening decisions during the pandemic are more likely driven by perceived infection risk than by cost or access barriers. Second, the NHIS, which administers the program as Korea's single insurer, maintains an integrated database that links each individual's health screening records—including biomarkers, health behaviors, and family history—with their medical claims. This integration provides two empirical advantages. It enables the prediction of chronic disease risk from these rich health indicators together with personal characteristics, allowing us to analyze heterogeneous responses to the pandemic. It also

3. According to the 2019 Korea National Health and Nutrition Examination Survey (KNHANES) (Korea Disease Control and Prevention Agency 2025), among the 72.7% of respondents aged 40 to 79 (n=4,270) who reported receiving a health screening within the past two years (excluding cancer screenings), 90.8% received the general health screening provided by the NHIS, while only 6.9% received a comprehensive screening paid out-of-pocket.

4. As of 2020, the eligible population for screening was 21,446,226—about 51% of the 42,041,406 individuals aged 20 or older (National Health Insurance Service 2022).

allows us to track each individual's health care utilization following a screening, and thus to estimate the potential costs of forgone screenings.

During the study period, two notable institutional changes occurred. First, in 2019, the eligible age for dependents of regional subscribers was lowered from 40 to 20, significantly altering the composition of the eligible population under the age of 40. This change makes it difficult to compare their screening rates before and after the pandemic, so individuals under the age of 40 are excluded from the analysis. Second, due to the COVID-19 pandemic, the screening period was temporarily extended to the end of June of the following year. The extension was intended to reduce infection risk by easing the year-end surge in examinees. As a result, focusing on screening uptake within the standard one-year period may overestimate the effect of the pandemic. We address this issue in Appendix B.

3 Data

This study uses the National Health Information Database from the NHIS (National Health Insurance Service 2025), which covers the entire population of health insurance enrollees in Korea. From this database, we draw a random sample, stratified by sex and age, of enrollees who were 20 years of age or older as of 2019.[5] The data include enrollee information such as sex, age, insurance type, and insurance premium, as well as medical claims data. For individuals who received a health screening, the dataset additionally contains biomarkers and self-reported survey information, such as health behaviors.

We impose two common restrictions on our study sample. First, we limit the sample to individuals who are at least 40 years old and younger than 80. This restriction reflects the fact that, prior to 2019, individuals under the age of 40 were generally not eligible for health screenings, except those covered by employee insurance. Second, we exclude all observations in which individuals received a health screening despite being ineligible.

5. The dataset includes 1,448,121 unique individuals as of 2019.

For each analysis, we apply additional restrictions depending on its specific purpose, summarized in Table 1. In Section 4, we aggregate samples from 2017 to 2021 to the weekly level for the national-level analysis. In the individual-level analysis, which examines heterogeneous responses to COVID-19, we use a sample of individuals who had not used health care for hypertension or diabetes in the past three years and had received a health screening within the past one to two years.[6] We use a dummy variable indicating whether an individual received a health screening as the outcome variable. In Section 5, we use samples from 2020 and 2021 to examine the effects of delayed health screening on health care utilization. To this end, we construct separate outcome variables for hypertension and diabetes, including care initiation and the number of related care visits for each condition.[7] We additionally examine whether the reduction in health screenings led to care initiation at more severe stages of chronic disease by including, for each condition, outcome variables indicating the presence of complications.[8]

Table 1: Summary of Sample Construction

Section	Sample construction	Unit of analysis	Outcome variable
Section 4. Changes in Health Screening Rates During the COVID-19 Pandemic	- Eligible individuals for health screening, aged 40 to under 80 years - Sample period: 2017–2021	National-level	Number of screenings per 100 eligible individuals
	- Eligible individuals for health screening, aged 40 to under 80 years - Excluding those who utilized health care for hypertension or diabetes in the past three years - Restricted to those who received a health screening within the past two years - Sample period: 2017–2021	Individual-level	Dummy variable for receiving a health screening
Section 5. Health Screening and Chronic Disease Management During the COVID-19 Pandemic	- Based on the individual-level sample in Section 4 - Restricted to the 2020–2021 period	Individual-level	Utilization of health care for: 1) hypertension or diabetes 2) related complications

6. We restrict the sample to recent screening recipients because predicting chronic disease risk requires prior biomarkers, which are observed only for individuals who have previously been screened.

7. Health care utilization related to hypertension and diabetes is identified using diagnosis codes I10–I13 for hypertension and E10–E14 for diabetes, based on the 7th revision of the Korean Standard Classification of Diseases (KCD-7).

8. For hypertension, complications include coronary artery disease (I20–I25), cerebrovascular disease (I60–I69), heart failure (I50), and chronic kidney disease (N18, N19) (National Health Insurance Service 2023). For diabetes, subcategories of E10–E14 that indicate diabetes with complications are included.

4 Changes in Health Screening Rates During the COVID-19 Pandemic

4.1 Empirical Approach

4.1.1 National-Level Changes in Health Screening Rates

Eligible individuals are allowed to choose when to receive screening within the designated screening period. As a result, concerns about COVID-19 infection may have led them to respond in two distinct ways. First, they may have avoided periods of high infection risk or heightened uncertainty and instead chosen to receive screening during safer periods, a pattern consistent with intertemporal substitution. Second, they may have chosen not to receive a screening at all during the year. Accordingly, we conduct an analysis at the weekly level to examine both types of responses.

To assess changes in health screening rates during the COVID-19 pandemic, we employ a modified interrupted time series approach. Specifically, we estimate the counterfactual trend in screening rates that would have been observed in the absence of the pandemic. To this end, we use data from 2017 to 2019 and estimate the following equation:[9]

$$Y_{wt} = \beta_0 + \beta_1 Year_t + \beta_2 Holiday_{wt} + \delta_w + \epsilon_{wt} \tag{1}$$

where the outcome variable (Y_{wt}) is the number of health screenings per 100 eligible individuals in week w and year t. To account for time trends in screening rates, we include a yearly linear trend ($Year_t$) in the model (Appendix Figure A4). The number of holidays ($Holiday_{wt}$) is added to control for holiday effects.[10] Since screening rates tend to be lower at the beginning of the year and increase toward the end, we include week fixed effects (δ_w) to account for seasonality. Standard

9. The results are robust to the choice of sample period used to estimate the counterfactual trend, as estimates based on equation 1, using samples from 2015–2019, 2016–2019, and 2017–2019, yield consistent results, which are available upon request.

10. We considered alternative functional forms for holidays, including a dummy variable for the presence of any holiday and categorical dummies based on the number of holidays. Among these, we selected the continuous variable for the number of holidays, as it yielded the lowest root mean squared error in the counterfactual estimation model.

errors are robust to heteroskedasticity.

Using the estimates from equation 1, we calculate counterfactual screening rates for 2020 and 2021. The difference between the observed and counterfactual screening rates can be interpreted as the effect of the pandemic on screening. For this interpretation to be valid, we assume that, in the absence of the pandemic, health screening rates in 2020 and 2021 would have followed the counterfactual trend. To support this assumption, we conduct falsification tests using samples from the pre-pandemic period (Appendix Figure A5).

To address the temporary extension of the screening period, we also estimate a complementary specification that follows each year's eligible individuals through June of the following year; the method and results are detailed in Appendix B.

4.1.2 Heterogeneous Responses by Chronic Disease Risk

As discussed, chronic disease risk raises both the benefit of screening (through earlier detection) and the perceived cost of seeking it, since those with chronic conditions face more severe COVID-19 infections and higher mortality. The net effect of higher risk on screening uptake is therefore an empirical question, which we examine by allowing the pandemic's effect to vary across quintiles of predicted risk.

For this analysis, we use the recent-screening sample defined in Section 3: individuals with no hypertension- or diabetes-related care in the past three years who received a screening within the past one to two years. The recent-screening restriction serves two purposes. First, predicting chronic disease risk requires prior biomarkers such as blood pressure and blood glucose, which are observed only for those previously screened. Second, excluding individuals who had not screened in recent years ensures that any change in screening uptake during the pandemic reflects a response to COVID-19 risk rather than pre-existing non-participation. The individual-level analysis uses the following equation:

$$Y_{it} = \beta_0 + \sum_{j=2}^{5} \beta_1^j 1[RiskQ_{it} = j] \times COVID_t + \beta_2 COVID_t + \beta_3 Year_t + \gamma' X_{it} + \epsilon_{it} \qquad (2)$$

where the outcome variable is an indicator that equals 1 if individual i received a screening in year t through June of $t + 1$, and 0 otherwise. $1[RiskQ_{it} = j]$ takes the value 1 if the predicted risk quintile for hypertension or diabetes is j. A detailed description of the risk prediction is provided in Appendix A.[11] The variable of interest, $COVID_t$, is a dummy variable that equals 1 if the individual was eligible for screening in 2020, and 0 if they were eligible between 2016 and 2018; individuals eligible in 2019 are excluded because their screening window overlapped with the onset of the pandemic. A linear year trend ($Year_t$) is also included. The model controls for individual characteristics (X_{it}) to account for changes in health status and age due to the panel structure of the data, as well as changes in sample composition. The control variables consist of dummy variables for predicted risk quintiles, demographic characteristics, and screening-related variables from previous screenings, as well as Elixhauser comorbidity conditions. More details on the control variables are provided in Appendix Table A1. Standard errors are robust to heteroskedasticity.

4.2 Changes in Health Screening Rates

4.2.1 Baseline Results

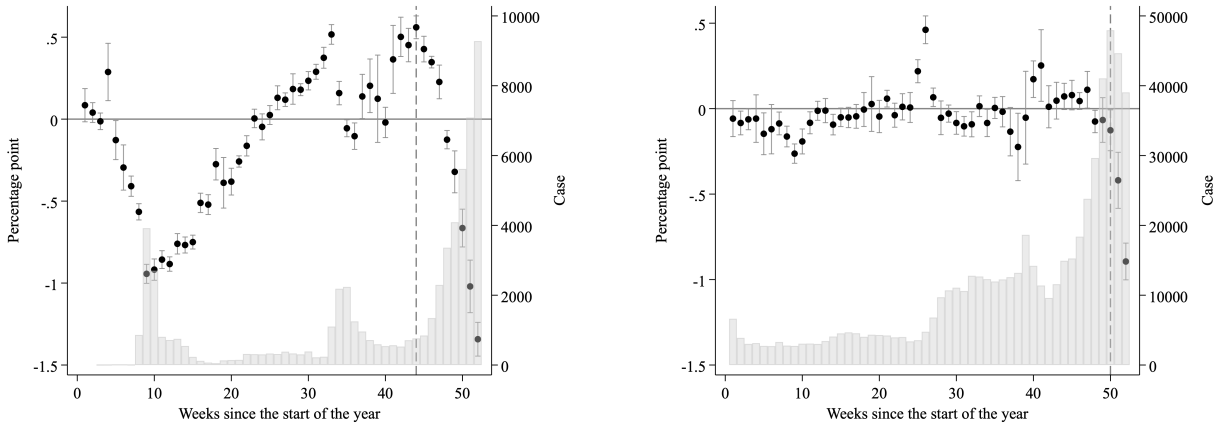
Figure 1 presents the weekly changes in health screening rates relative to the counterfactual, calculated using equation 1. Figure 1(a) shows that the decline in screening rates began around the time when the first confirmed case of COVID-19 was reported in Korea. During the initial wave of the pandemic (up to week 14 of 2020, early April), screening rates exhibited a clear dip. The largest drop occurred at the peak of the outbreak, with screening rates falling by approximately 1 percentage point compared to the counterfactual. As the first wave subsided, the magnitude of the decline diminished. Nevertheless, a significantly negative change persisted through week 22 of 2020 (late May), with the cumulative decline reaching 9.38 percentage points. In the weeks that followed, the screening rate exceeded the counterfactual. This pattern is consistent with intertemporal substitution, whereby individuals who postponed screenings during periods of

11. We estimate logistic regression models to predict the risk of developing hypertension and diabetes, applying the same sample restrictions to the 2015 sample. Model performance is evaluated using out-of-sample AUC, with 5-fold cross-validation to mitigate overfitting (Appendix Table A2).

heightened uncertainty received them later—when the perceived risk had decreased and the health care system had adapted. The recovery continued through week 47 of 2020 (in November), with the cumulative decline narrowing to 4.05 percentage points. Toward the end of the year, however, screening rates declined again, as the number of confirmed cases surged beyond earlier waves and the government announced an extension of the screening period (week 44). By the end of 2020, the overall screening rate had fallen by 7.52 percentage points.

Figure 1: Change in Health Screening Rates During the COVID-19 Pandemic

(a): Sample from 2020 (b): Sample from 2021



Notes: Figures 1(a) and 1(b) show weekly changes in health screening rates relative to the counterfactual for 2020 and 2021, respectively, along with 95% confidence intervals. The counterfactual values are derived by fitting equation 1 to data from 2017 to 2019. Gray bars indicate the number of newly confirmed COVID-19 cases per week, and vertical dashed lines mark the weeks when each year's screening period extension was announced. In Figure 1(a), cumulative reductions in screening rates amount to 9.38 percentage points by week 22 (end of the first-wave decline), 4.05 by week 47 (end of the recovery), and 7.52 by week 52 (year-end). Standard errors are robust to heteroskedasticity. Source of COVID-19 case data: World Health Organization and Various sources (2025).

Figure 1(b) shows that, unlike in 2020—when screening rates exhibited a clear pattern of decline and recovery corresponding to shifts in COVID-19 risk—changes in 2021 remained close to zero throughout the year. Despite the substantially higher number of confirmed cases in 2021 compared to 2020, the absence of marked deviation may reflect individuals' psychological adaptation to COVID-19 risk or pandemic fatigue, resulting in reduced behavioral elasticity in response to

infection risk (Droste and Stock 2021).[12] It may also reflect improvements in the management of COVID-19 risk through infrastructural adaptation (Ha and Kim 2024). As in 2020, the government announced an extension of the screening period toward the end of 2021, after which screening rates declined once again. These results suggest that the impact of the pandemic on screening uptake was largely confined to 2020, and they point to the need to account for the extended screening period.[13]

Because individuals who missed screenings in 2020 could still receive them during the extended period running through June 2021, the year-end decline may overstate the pandemic's true effect. To account for this, we track those eligible in 2020 through the extended window and compare actual to counterfactual rates over the full period. This group still shows a cumulative shortfall of 5.68 percentage points, similar to the baseline results. Full details of the method and results appear in Appendix B.

4.2.2 Response to COVID-19 by Predicted Chronic Disease Risk

Figure 2 plots β_1^j from equation 2 across quintiles of predicted risk for chronic diseases.[14] In Figure 2(a), as the predicted risk for hypertension increases, the pandemic-induced decline in the probability of receiving a health screening becomes larger relative to the first quintile. While the reference group (first quintile) shows a decline of 3.85 percentage points, the total decline reaches 4.75 percentage points (3.85 + 0.90) in the second quintile and 6.15 percentage points (3.85 + 2.30) in the fifth quintile. Although the interaction terms for each quintile are statistically significant, the confidence intervals across groups overlap. A qualitatively similar pattern appears in Figure 2(b), which examines heterogeneity based on predicted risk for diabetes, where the reference group

12. The average number of daily confirmed COVID-19 cases was 1,167.7 in 2020 and 11,048.6 in 2021.

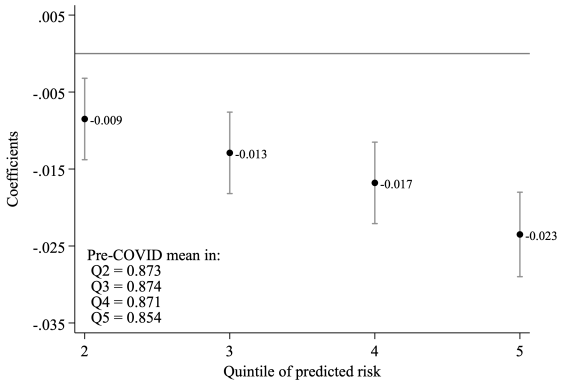
13. Interpreting the decline in screening rates observed in 2020 as an effect of the pandemic requires assuming that, had the pandemic not occurred, screening rates during the same period would have followed the counterfactual trend. To assess this assumption, we conduct a falsification test using pre-pandemic data from 2018 and 2019. If the decline in screening uptake is attributable to the pandemic, no systematic deviation from the counterfactual should appear during the pre-pandemic period. Appendix Figure A5 presents changes in screening rates relative to the counterfactual, obtained by estimating equation 1 using the sample from the preceding three years. Figures A5(a) and A5(b) show the results for the 2018 and 2019 samples, respectively. While some changes are statistically significant, they exhibit no systematic pattern and remain generally close to zero. These results support the interpretation that the decline in screening uptake observed in 2020 reflects the impact of the pandemic.

14. Baseline estimates from the individual-level analysis that do not allow for differences in responses by chronic disease risk are presented in Appendix Table A3.

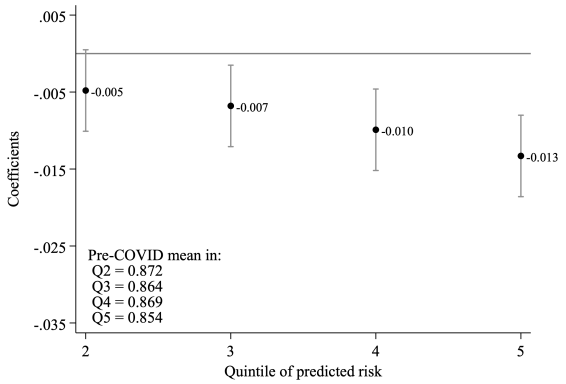
shows a decline of 4.38 percentage points; the between-group differences, however, are smaller in magnitude than those observed for hypertension. Taken together, these results suggest that individuals at higher risk for chronic disease experienced greater reductions in screening rates during the pandemic.

Figure 2: Heterogeneous Responses by Predicted Chronic Disease Risk

(a): By hypertension risk quintile



(b): By diabetes risk quintile



Notes: We estimate equation 2 using a sample of individuals who received a health screening within the past one to two years and had no health care utilization for either hypertension or diabetes in the previous three years. The outcome variable equals 1 if the individual received a screening between year t and June of year $t + 1$. Figures 2(a) and 2(b) present the estimated coefficients, allowing the change in the probability of receiving a screening during the pandemic to vary across quintiles of predicted risk for hypertension and diabetes, respectively. The method used to construct the predicted risk is described in Appendix A. The first quintile serves as the reference group and is omitted from the figures. The estimated change in screening probability for the reference group (first quintile) is –0.0385 for hypertension and –0.0438 for diabetes. Standard errors are robust to heteroskedasticity.

Examining the specific predictors behind these risk estimates sharpens the picture. The steeper declines are concentrated among higher-risk groups: older adults (aged 60 or older) and medical aid beneficiaries are the least likely to be screened despite their elevated risk, as are individuals flagged by biomarkers such as BMI, fasting blood glucose, and blood pressure, whose measured risk is higher. The same patterns hold when the analysis uses diabetes risk. The full decomposition by individual predictor is reported in Appendix Figure A6 for hypertension and Appendix Figure A7 for diabetes.

In the context of Korea, where strict quarantine policies, such as mobility restrictions, were not implemented, the results in this section suggest that voluntary responses to the COVID-19 pandemic reduced screening among those most likely to benefit from it.

5 Health Screening and Chronic Disease Management During the COVID-19 Pandemic

In this section, we analyze how delays in health screening during the COVID-19 pandemic affected the initiation of health care utilization related to hypertension and diabetes. This question is particularly relevant for two reasons. First, it allows us to quantify the cost of voluntary responses to the pandemic, which is meaningful in itself, as it captures the health consequences of individuals' decisions in response to infection risk. Second, it helps us understand whether the reduction in screening disproportionately affected individuals who were most likely to benefit from it. As shown in Section 4.2.2, the decline in screening rates during the pandemic was greater among those at higher risk for chronic diseases. Yet biomarkers from prior screenings, which play a critical role in predicting that risk (Appendix Table A2), are also observable to individuals themselves. High-risk individuals who cut back on screening may therefore have relied on this previously available information to manage their conditions. Whether such information substituted for screening, or whether the missed screenings still set back care, is the empirical question to which we now turn.

5.1 Empirical Approach

Our primary identification strategy is an event study that exploits variation in both the timing and receipt of health screenings across individuals, examining dynamic changes in care initiation around the screening month. The design follows each individual across the months surrounding their screening and asks how care-seeking evolves relative to the month just before screening, isolating the effect of screening from stable individual characteristics and from common time effects.

For this analysis, we construct an individual-month panel restricted to those with no prior care for either hypertension or diabetes in the past three years.[15] We then estimate the following model:

$$Y_{it} = \beta_0 + \sum_{\substack{j=-12 \\ j\neq -1}}^{6} \beta_1^j 1[t - t_i^* = j] + \gamma' X_i + \tau_t + \epsilon_{it} \tag{3}$$

where i indexes individuals and t denotes year-months. We estimate the model using two types of outcome variables (Y_{it}): a dummy variable for the initiation of health care use and a count variable for the number of such uses, each defined separately for hypertension and diabetes. $1[t - t_i^* = j]$ is a dummy variable that equals 1 when the observation month is j months from the screening month. One month prior to screening ($j = -1$) is used as the reference period; thus, β_1^j captures the change in the outcome relative to this baseline. To control for individual characteristics associated with health care utilization, we include X_i, which consists of the same covariates used in equation 2. τ_t denotes year-month fixed effects. Standard errors are clustered at the individual level.

The coefficient of interest, β_1^j, in equation 3 captures the causal effect of screening, under the identifying assumption that the outcome variables would have followed a smooth trend over time in the absence of screening. To put this assumption to the test, we provide two pieces of evidence. First, the estimated coefficients β_1^j are close to zero during the pre-screening period, indicating that treated individuals were not already on a differential trajectory in care initiation before screening. Second, we conduct a placebo test by assigning each untreated individual a pseudo (false) screening month drawn from a comparable treated individual identified by propensity-score matching, as described in Appendix C. Since that procedure uses 1:4 nearest-neighbor matching, the false screening month is randomly drawn from one of the four matched treated individuals; a flat profile of placebo coefficients suggests that time-varying confounding or selection on unobservables is unlikely to drive the estimated effects.

Because individuals are screened in different months, treatment timing is staggered in our setting. Recent work shows that, with staggered timing, dynamic estimates can be biased when

15. For a balanced sample, the observation period is limited to 12 months before and 6 months after the health screening.

treatment effects are heterogeneous across cohorts. We examine this issue by estimating a two-way fixed-effects (TWFE) specification and applying the heterogeneity-robust interaction-weighted (IW) estimator of Sun and Abraham (2021); the comparison is reported in Section 5.2.1 (Figure 4).

The event study identifies whether screening causes care initiation and traces its dynamics, but its coefficients describe individuals who were actually screened—an effect on the treated. Our cost framing instead requires a single magnitude for those who were not screened: the care they would have initiated had they been screened. We therefore report the average treatment effect on the untreated (ATU) as our target estimand. We recover the ATU using the propensity-score matching from our placebo procedure (details in Appendix C and D). The validity of this estimate rests on conditional independence given the matched covariates—an assumption distinct from, and non-nested with, the smooth-counterfactual-trend assumption underlying the event study. Because the two designs identify their respective effects under different assumptions, their agreement is mutually corroborating: the event study, with its directly testable pre-trends and placebo checks, lends credibility to the matching design, while the ATU supplies the policy-relevant magnitude that the event-study coefficients alone do not deliver.

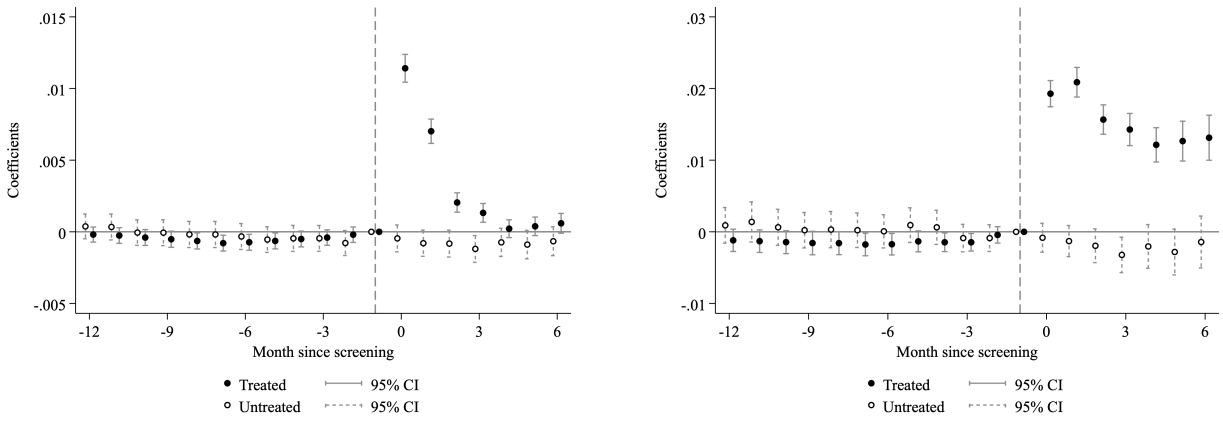
5.2 Effects of Health Screening on Chronic Disease Care

5.2.1 Event Study Results

Figure 3 presents the estimated coefficients from equation 3 for hypertension, measured relative to the reference period, the month immediately before screening ($j = -1$). We estimate changes in care initiation and the number of care visits following screening, using a sample of individuals with no prior care for either hypertension or diabetes in the past three years. The probability of care initiation increases by 1.14 percentage points immediately after screening, then drops sharply. The number of care visits also rises by 0.019 in the month of screening and slightly declines thereafter, though it remains elevated. In contrast, the untreated show no meaningful changes either before or after the false screening month. Analogous results for diabetes are reported in Appendix Figure A8.

Figure 3: Effects of Screening on Health Care Use: Event Study Results (Hypertension)

(a): *Care initiation* (b): *Number of care visits*



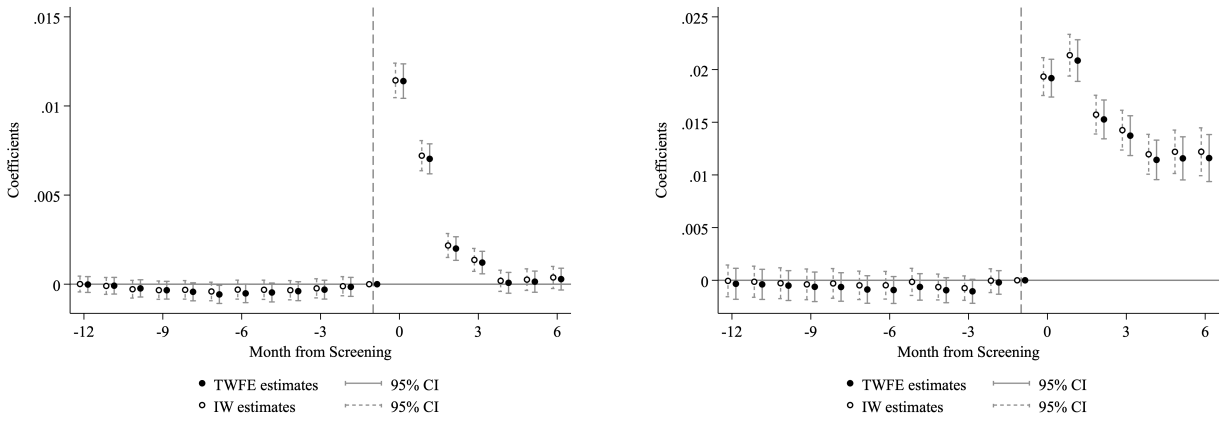
Notes: Figure 3 presents estimates from equation 3 for hypertension, illustrating how health care utilization evolves before and after screening. All coefficients are estimated relative to the reference period, the month immediately before screening ($j = -1$). For the untreated group, a false screening month is assigned by randomly selecting one of the four matched treated individuals. Figures 3(a) and 3(b) use *Care initiation* (a dummy variable indicating the first instance of health care use) and *Number of care visits* (the total number of hypertension-related visits) as outcome variables. Analogous results for diabetes are reported in Appendix Figure A8. Standard errors are clustered at the individual level.

As discussed in Section 5.1, individuals differ in the month they received screening, so we examine whether staggered treatment timing affects our findings by comparing the TWFE specification with the IW estimator of Sun and Abraham (2021). For this analysis, we combine the treated and untreated groups into a single sample. Following the implementation of the IW estimator in Sun and Abraham (2021), untreated individuals—who never received a screening—serve as the baseline cohort: because no screening month is defined for them, all event-time dummies (leads and lags) equal zero. For hypertension, the IW estimates are nearly identical to the TWFE estimates (Figure 4), indicating that heterogeneous treatment timing does not materially affect our findings; the corresponding results for diabetes are reported in Appendix Figure A9.

These event study results yield two key implications. First, the sharp rise in care initiation at the screening month, together with the flat placebo profile, supports a causal interpretation of the effect of screening on chronic-disease care. Second, the elevated number of care visits in the months

Figure 4: Comparing Screening Effects: TWFE vs. IW Estimator (Hypertension)

(a): *Care initiation* (b): *Number of care visits*



Notes: Figure 4 presents estimates from the two-way fixed-effects (TWFE) and interaction-weighted (IW) estimators for hypertension, following Sun and Abraham (2021), to compare changes in health care utilization before and after screening. Since no screening month is defined for the untreated group, all event time dummies (leads and lags) are equal to zero, allowing this group to serve as the baseline cohort in the estimation. Figures 4(a) and 4(b) use *Care initiation* (a dummy variable equal to 1 if the individual initiated health care use) and *Number of care visits* (the total number of hypertension-related visits) as dependent variables. The corresponding results for diabetes are reported in Appendix Figure A9. Standard errors are clustered at the individual level.

after screening—not just the initial visit—suggests that missed screenings disrupted not only the initiation of care but also its continued management.

5.2.2 ATU Estimates

The event study in Section 5.2.1 provided evidence consistent with a causal effect of screening on the initiation of chronic-disease care. This subsection quantifies the magnitude of that effect for the population our cost framing targets—those who did not screen—by estimating the ATU.

Table 2 reports the ATU of screening on chronic-disease care utilization. In the baseline model, screening raises the probability of initiating hypertension-related care by 2.58 percentage points—about 35% of the untreated mean—and increases the number of care visits by 0.217, or roughly 46% of the untreated mean. For diabetes, screening raises the probability of initiating care by 2.42 percentage points and the number of visits by 0.098, equivalent to 35% and 41% of the respective

untreated means.[16]

These ATU magnitudes are consistent with the event-study results in Section 5.2.1, corroborating the screening effect under a different identifying assumption and supporting our interpretation of forgone screening as a real cost borne by the untreated.[17] The estimates are robust to two checks for reverse causality: Model (2) excludes individuals whose care for the relevant condition began before the screening month, and Model (3) adds the month of the previous screening as a matching covariate.[18]

Two channels rationalize these magnitudes. First, screening increases contact with providers, who may bill for same-day visits when they deliver additional services—diagnoses or prescriptions—beyond the screening itself. Second, the information conveyed through screening shapes subsequent utilization (Iizuka et al. 2021; Kim, Lee, and Lim 2019; Oster 2018a; Zhao, Konishi, and Glewwe 2013); this informational channel is especially relevant here, since the sample includes individuals with biomarkers above diagnostic thresholds. The estimates are not directly comparable to the regression-discontinuity literature that exploits diagnosis cutoffs, but they remain in a plausible range. Iizuka et al. (2021) report diabetes care increases of 4.7 percentage points at the alert cutoff (fasting glucose of 110 mg/dL) and 4.0 percentage points at the risk cutoff (126 mg/dL); these exceed our estimates, as expected, since they capture local effects around specific thresholds while the ATU averages over the untreated without restricting to high-risk individuals.

5.2.3 Effects of Health Screening by Predicted Chronic Disease Risk

Chronic diseases tend to progress gradually, suggesting that health screenings may have larger effects for individuals at higher risk of developing these conditions. Therefore, we examine whether the effects of health screenings differ by chronic disease risk. Specifically, we divide the sample into quintiles based on predicted risk for hypertension and diabetes, and estimate the ATU within each

16. Appendix Figure A12 shows robustness to varying the number of matched neighbors (1 to 8) and using matching without ties. These specifications yield results consistent with the baseline estimates.

17. Balance diagnostics confirm common support and standardized differences below the conventional 20% threshold after matching (Appendix D, Figure A2).

18. Details of these two approaches are provided in Appendix C.

Table 2: Average Treatment Effects of Screening on Health Care Use for Chronic Diseases

	(1) Baseline			(2) Excluding sample diagnosed before screening			(3) Matching on previous screening month		
	N	Mean (untreated group)	Estimate	N	Mean (untreated group)	Estimate	N	Mean (untreated group)	Estimate
Panel A: hypertension									
Outcome variable:									
Care initiation	198720	0.0733	0.0258***	194967	0.0533	0.0266***	198720	0.0733	0.0253***
			(0.0021)			(0.0019)			(0.0021)
Number of care visits	198720	0.4655	0.2170***	194967	0.2790	0.2015***	198720	0.4655	0.2244***
			(0.0208)			(0.0158)			(0.0216)
Panel B: diabetes									
Outcome variable:									
Care initiation	198720	0.0692	0.0242***	195287	0.0513	0.0263***	198720	0.0692	0.0249***
			(0.0020)			(0.0018)			(0.0020)
Number of care visits	198720	0.2377	0.0979***	195287	0.1427	0.1070***	198720	0.2377	0.1024***
			(0.0137)			(0.0098)			(0.0138)

Notes: Table 2 reports estimates of the average treatment effect on the untreated (ATU) obtained from propensity score matching. The variable *Care initiation* is a dummy equal to 1 if the individual initiated health care use for the respective condition (hypertension or diabetes). The variable *Number of care visits* refers to the total number of related visits. All outcome variables are measured using medical claims data from 2020 to 2021. Model (2) excludes individuals who initiated care for the respective condition prior to receiving a screening. Model (3) additionally includes the timing of the most recent screening as a matching covariate. Standard errors are calculated following Abadie and Imbens (2006). *, **, *** denote significance at the 10%, 5%, 1% levels, respectively.

quintile.

Estimates using the whole sample mask substantial heterogeneity. Figure 5(a) shows that the magnitude of the estimates increases markedly with risk level. In the lowest risk quintile (Q1), the probability of initiating hypertension-related care would increase by 0.40 percentage points if the untreated received a health screening. However, this estimate is not statistically significant at the 95% confidence level, and a similar result is observed in Q2. From Q3 to Q5, the ATU estimates rise steeply, reaching 6.90 percentage points in Q5—equivalent to approximately 43% of the untreated mean. A comparable gradient is observed in Figure 5(b) for the number of care visits. Figures 5(c) and 5(d) display qualitatively consistent patterns for diabetes-related outcomes.

5.3 Health Consequences of Delay in Care Initiation

In the previous analyses, we showed that screening increased the likelihood of initiating care for chronic conditions. A relevant follow-up question is whether screening also facilitated earlier detection—specifically, whether untreated individuals began care at a more advanced stage of disease. To address this, we examine the presence of complications associated with hypertension and diabetes.

Table 3 reports the effects of health screening on the presence of chronic disease complications among new health care users. In the baseline model, receiving a health screening would have reduced the probability of initiating hypertension-related care with complications by 5.72 percentage points, equivalent to approximately 20% of the untreated mean. This effect is statistically significant. Similarly, for diabetes, the probability of initiating care with complications would have decreased by 2.94 percentage points, or about 13% of the untreated mean.[19] As in Table 2, which examines the effects of screening on care initiation, we address the issue of reverse causality here as well. Model (2) excludes individuals who began health care for hypertension or diabetes before screening. Model (3) includes the timing of the previous screening as a matching covariate. The

19. Matching details for the baseline model are provided in Appendix Figure A13. The results support the common support assumption and indicate that covariate imbalance is eliminated after matching. Similar findings hold for Models (2) and (3); results available upon request.

results are robust across both specifications.

This study analyzes the effects of screening within a two-year observation window. While this period is relatively short given the typically long asymptomatic course of chronic diseases, prior research highlights the importance of early detection. For instance, in the case of diabetes, the time from disease onset to diagnosis can range from four to seven years (Harris et al. 1992). Nonetheless, timely diagnosis and initiation of lifestyle or pharmacological interventions can substantially improve outcomes. Harris and Eastman (2000) emphasize that diabetes-related complications may advance significantly during the undiagnosed period, underscoring the value of screening. Even a one-year delay in treatment intensification can elevate the risk of complications, including cardiovascular events (Reach et al. 2017). Similarly, early detection and treatment of hypertension are critical. Martín-Fernández et al. (2019) show that all-cause mortality increases markedly when the interval between diagnosis and blood pressure control exceeds 125 days. The findings in Table 3 suggest that, had the untreated been screened, chronic disease management might have begun at a less severe stage.

6 Conclusion

This study provides a comprehensive analysis of how individuals' voluntary responses to the COVID-19 pandemic influenced general health screening, a key component of preventive health care. Using a modified interrupted time series approach, we find that participation among eligible individuals declined substantially in 2020, the first year of the pandemic. The richness of the NHIS data allows us to move beyond aggregate patterns and examine heterogeneity in responses based on individuals' predicted risk of chronic disease. We find that screening participation declined more sharply among higher-risk individuals with no prior care for hypertension or diabetes, suggesting greater reductions among those most likely to benefit.

We next examine how reduced screening participation during the pandemic affected chronic disease management. Our findings show that individuals who missed screenings in 2020 would

Table 3: Average Treatment Effects of Screening on Complications from Chronic Diseases

	(1) Baseline			(2) Excluding sample diagnosed before screening			(3) Matching on previous screening month		
	N	Mean (untreated group)	Estimate	N	Mean (untreated group)	Estimate	N	Mean (untreated group)	Estimate
Panel A: among individuals initiating hypertension care									
Complications	17018	0.2853	-0.0572***	12722	0.2332	-0.0595***	17018	0.2853	-0.0712***
			(0.0122)			(0.0138)			(0.0121)
Panel B: among individuals initiating diabetes care									
Complications	16724	0.2270	-0.0294**	13292	0.2267	-0.0372***	16724	0.2270	-0.0257**
			(0.0115)			(0.0136)			(0.0118)

Notes: Table 3 presents average treatment effect on the untreated (ATU) estimates from propensity score matching, based on a sample of individuals who initiated health care use for hypertension or diabetes in 2020. The outcome variables are two separate dummy variables indicating whether the individual used health care related to complications from hypertension and from diabetes, respectively. For hypertension, complications include coronary artery disease (I20–I25), cerebrovascular disease (I60–I69), heart failure (I50), and chronic kidney disease (N18, N19), based on the Korean Standard Classification of Diseases (KCD). For diabetes, we include only subcategories within E10–E14 that indicate diabetes with complications. The outcomes are measured using medical claims data from 2020 to 2021. In Model (2), individuals who initiated care prior to screening are excluded. Model (3) additionally includes the timing of the most recent screening as a matching covariate. Standard errors are calculated following Abadie and Imbens (2006). *, **, *** denote significance at the 10%, 5%, 1% levels, respectively.

have been more likely to initiate care for hypertension and diabetes had they been screened. Delays in disease management can lead to worse health outcomes. Specifically, individuals who missed screenings were more likely to present with hypertension- and diabetes-related complications when initiating care. Based on our estimates, reduced screening in 2020 led to approximately 262 missed hypertension cases and 246 missed diabetes cases in our sample. Given that the sample represents roughly 3.3% of the population aged 20 or older, this implies approximately 7,939 missed hypertension cases and 7,455 missed diabetes cases nationwide.[20] These missed cases are estimated to have resulted in approximately 454 hypertension cases and 219 diabetes cases involving complications.[21] A diagnosis made only after complications have developed is not merely a later diagnosis: the clinical literature discussed in Section 5.3 suggests that complications emerging during the undiagnosed interval can set the course for longer-term morbidity. The shift toward more advanced disease at the point of detection thus represents a health burden in its own right. We do not attempt to translate this burden into monetary terms, but the evidence on diagnostic delay points to tangible consequences, as detection at a more advanced stage is linked to worse long-term health outcomes.

New infectious diseases are arriving with growing frequency, and the high uncertainty that accompanies each novel pathogen can translate into heightened perceptions of risk (Smith et al. 2014). Voluntary responses are therefore likely to play an important role in future health crises, even where governments stop short of mandatory restrictions. Yet such responses come at

20. We performed a back-of-the-envelope calculation, multiplying the number of eligible individuals by the estimated reduction in screening participation during the pandemic (0.0511) and the ATU for care initiation (0.0258 for hypertension, 0.0242 for diabetes). While these figures illustrate the costs of missed screenings, we do not attempt a formal cost–benefit calculation. Reliable hazard estimates linking diagnostic delays to health outcomes are scarce. Any net-impact calculation would therefore hinge on unverifiable assumptions about the functional relationship between diagnostic delay and disease progression. To avoid over-interpretation, we refrain from speculative monetization.

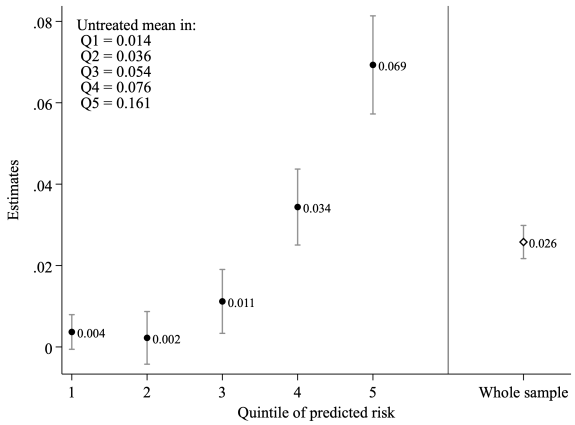
21. Applying the estimated ATU for complications to the counterfactual group of missed cases requires the assumption that, in terms of disease progression, these individuals resemble those who initiated care without screening during the observation window. This assumption can be challenged in both directions. On the one hand, missed cases may present with more advanced disease due to longer delays, in which case the effect of screening would be larger in this group than in our study sample. On the other hand, they may exhibit lower severity due to weaker symptoms or slower disease progression that did not yet necessitate care, in which case the screening effect in this group would be smaller than in our study sample. For this reason, the calculation is not intended to provide a point estimate of complications that could have been mitigated. Instead, it serves to illustrate the potential scale of health consequences arising from reduced screening participation.

a cost: our findings show they can reduce the uptake of preventive care and, ultimately, worsen health outcomes, with the largest declines often concentrated among those who stand to benefit most. Even during a crisis, then, sustaining health care utilization among high-risk groups remains important. Public health efforts targeted at preserving preventive care for these individuals, rather than uniform messaging, can help mitigate the trade-off between infection control and the continued management of other conditions.

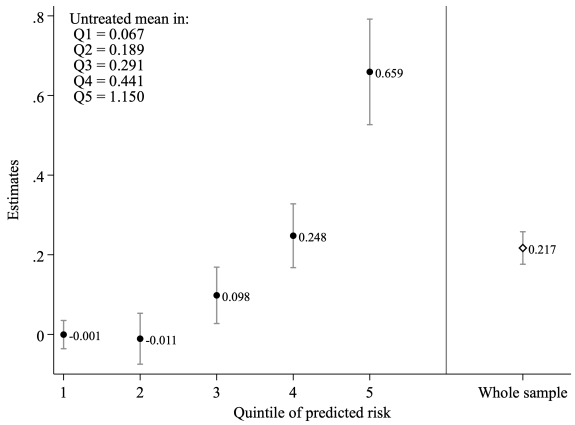
While our findings benefit from the institutional setting of Korea's National Screening Program (characterized by universal coverage, zero cost, and high accessibility), this context also limits external validity. The results are particularly relevant to health systems with national screening programs, such as those in England and Japan (Tanner et al. 2022; Fujimaru, Tachi, and Nakajima 2019). By contrast, in countries with greater cost-sharing and more fragmented access, the pandemic's impact on screening participation and chronic disease management may differ both in magnitude and in mechanism. Our findings are therefore specific to settings with minimal barriers to care and point to the need for further research on less supportive systems.

Figure 5: Heterogeneous Effects of Health Screening by Chronic Disease Risk

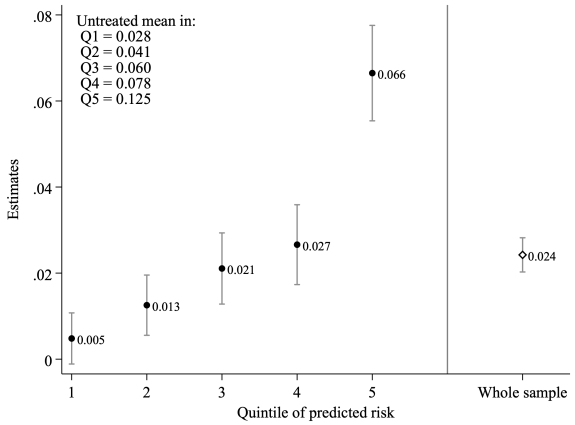
(a): Hypertension - *Care initiation*



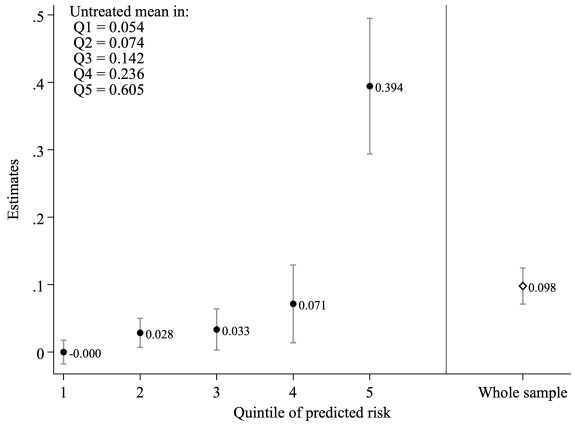
(b): Hypertension - *Number of care visits*



(c): Diabetes - *Care initiation*



(d): Diabetes - *Number of care visits*



Notes: Figure 5 presents average treatment effect on the untreated (ATU) estimates from propensity score matching, stratified by quintiles of predicted risk for hypertension and diabetes. The outcome variable, *Care initiation*, is a dummy equal to 1 if the individual initiated health care use for the respective condition, and 0 otherwise. *Number of care visits* refers to the total number of such visits. Outcome variables are measured using medical claims data from 2020 to 2021. Figures 5(a) and 5(b) show results for hypertension-related health care use, and Figures 5(c) and 5(d) present the corresponding results for diabetes. The method for constructing predicted risk is described in Appendix A. Standard errors are calculated following Abadie and Imbens (2006).

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Appendix

A Risk Prediction for Hypertension and Diabetes

Risk prediction models for hypertension and diabetes typically incorporate a wide range of variables, including biomarkers (e.g., blood pressure and fasting glucose), health behaviors, and family history of these conditions (Collins et al. 2011; Sun et al. 2017; Nusinovici et al. 2020). Since the National Health Insurance Service (NHIS) collects detailed health screening data, our dataset includes many of the key predictors commonly used in the existing literature.

The variables included in our prediction model are summarized in Table A1. We begin with demographic variables: age group (in 5-year intervals), sex, insurance premium (in 20 quantile groups), and type of health insurance. The health screening database additionally provides information on biomarkers (BMI, waist circumference, blood pressure, and fasting blood glucose), health behaviors (alcohol consumption, smoking, and physical activity), and family history of chronic diseases. Biomarkers are categorized based on clinical diagnostic criteria. We also incorporate health care utilization related to Elixhauser comorbidity conditions into the model (Khan, Uddin, and Srinivasan 2018; Uddin et al. 2022).

Logit models perform comparably to machine learning approaches in predicting chronic disease risk (Nusinovici et al. 2020). Accordingly, we use logit models to predict the risk of hypertension and diabetes. The outcome variable is a dummy equal to 1 if an individual initiates health care utilization for hypertension or diabetes as the primary condition in a given year, and 0 otherwise. The model is estimated using a sample of individuals aged 40 to 79 in 2015 who (i) had no prior health care utilization related to hypertension or diabetes in the preceding three years, and (ii) had received a health screening within the past one to two years. To evaluate model performance, we compare the area under the receiver operating characteristic curve (AUC) across specifications. To guard against overfitting, we use 5-fold cross-validation during model training. The dataset is randomly partitioned into five folds, with each fold serving once as the test set and the remaining four used for training. The AUC values reported in Table A2 reflect the out-of-sample performance,

averaged across folds.

Table A2 reports the AUC for each specification. Columns (1) to (5) sequentially add groups of predictors. Notably, the inclusion of biomarkers in Column (2) leads to a substantial improvement in AUC. The subsequent addition of health behaviors, family history, and Elixhauser comorbidity conditions in Columns (3) to (5) yields incremental gains in model performance. In Columns (6) and (7), comorbidity conditions are constructed using information from the past two and three years, respectively. Column (7), which yields the highest AUC, is used in the heterogeneity analysis by chronic disease risk level.

B Accounting for the Screening Period Extension

As noted in Section 2.1, the health screening period was extended at the end of 2020. If the effects of this extension are not taken into account, the decline in screening rates attributed to the pandemic may be overestimated. Because screenings are typically concentrated toward the end of the year, the extension may have allowed individuals to avoid the heightened infection risk during that period; if individuals instead received screening during the extended window, the decline observed during the regular screening period would overstate the true reduction. To address this concern, we extend the analysis window to 2020–2021 for individuals who were eligible in 2020. Specifically, we estimate equation 1 using the 2016–2018 cohorts of eligible individuals, with the number of weekly health screenings from year t through June of $t+1$ as the outcome variable. We use the 2016–2018 sample rather than the 2019 cohort to avoid contamination from the pandemic period in year $t+1$ of the 2019 cohort. Based on this model, we calculate the counterfactual rates for individuals eligible in 2020 and measure the deviation between the actual and counterfactual rates over the 2020–2021 period.

As shown in Figure A1, the results are similar to those presented in Section 4.2.1. At the end of 2020, screening rates declined sharply following the announcement of the screening period extension. In 2021, screening rates rose above the counterfactual and increased steadily until the end of June, when the extended period ended, resulting in a cumulative change of 0.89 percentage points during weeks 53–78 and –5.68 percentage points overall (through week 78). It remains unclear, however, whether the extension mitigated the decline in screening rates caused by the pandemic. Its effectiveness depends on both the anticipatory effect of the extension and the increase in screenings during the extended period. Unfortunately, the anticipatory effect cannot be separately identified from individuals' responses to the pandemic itself. Nevertheless, accounting for the extended period helps mitigate the overestimation of the pandemic's effect.

C Propensity Score Matching

This appendix details the propensity-score matching procedure that underlies both the placebo test and the complementary ATU estimate reported in Section 5.1, and formalizes the ATU estimand introduced there. As established in Section 5.1, the event study is our primary identification strategy; the matching described here is a complementary tool that supplies the placebo comparison and produces the average treatment effect on the untreated (ATU), the magnitude relevant to our cost framing. In the context of this study, the ATU represents how the outcome would have changed if individuals who did not receive a health screening had received one.[22] Formally, the ATU is defined as follows:

$$\tau_{ATU} = E[\tau \mid D = 0] = E[Y(1) \mid D = 0] - E[Y(0) \mid D = 0] \tag{4}$$

where τ is the treatment effect; D denotes the treatment (in this case, health screening), and $Y(1)$ and $Y(0)$ are the potential outcomes of receiving and not receiving the treatment, respectively.[23] We cannot observe $E[Y(1) \mid D = 0]$ in equation 4, that is, the potential outcome for individuals who did not receive a health screening, had they received one. What we do observe in the data are $E[Y(1) \mid D = 1]$ and $E[Y(0) \mid D = 0]$. When we compare these two observable outcomes:

$$E[Y(1) \mid D = 1] - E[Y(0) \mid D = 0] = \tau_{ATU} + \{E[Y(1) \mid D = 1] - E[Y(1) \mid D = 0]\} \tag{5}$$

If $E[Y(1) \mid D = 1] - E[Y(1) \mid D = 0] \neq 0$, the ATU estimate will be biased due to selection. Because individuals can choose whether to receive a health screening, systematic differences may exist between those who were screened during the pandemic and those who were not. These differences may include health status, prior health care utilization, health behaviors, and other factors that influence chronic disease risk. The selection bias term is therefore expected to be

22. This section follows Cunningham (2021) and Shin (2022).

23. Based on the results in Section 4.2.2, individuals who received a health screening between January 2020 and June 2021 are classified as treated, while those who did not are classified as untreated.

non-zero, which makes matching necessary.[24]

A single 1:4 nearest neighbor propensity score matching procedure serves both purposes described in Section 5.1. For the placebo test, it pairs each untreated individual with comparable treated individuals from which a pseudo (false) screening month is drawn; for the complementary ATU estimate, the same matched treated counterparts furnish each untreated individual's missing potential outcome $E[Y(1) \mid D = 0]$. Propensity score matching can estimate the ATU under two identifying assumptions. The first is the conditional independence assumption, which implies that, conditional on the propensity score, treatment assignment is as good as random. Although this assumption is inherently untestable, we argue that it is plausible in our setting, as propensity score matching balances a rich set of covariates related to health screening and chronic disease between treated and untreated individuals. The second assumption is common support, which requires that, for each value of the propensity score, there exist both treated and untreated individuals. To assess whether this condition is met, we examine the overlap in the distribution of propensity scores between the two groups.

There are two key considerations in selecting covariates for estimating the propensity score. First, following Heckman and Navarro-Lozano (2004), we include covariates that are strongly correlated with the outcome variables, namely, hypertension and diabetes. Second, as emphasized by Caliendo and Kopeinig (2008), only covariates that are not affected by the treatment should be used in estimating the propensity score. Taken together, these considerations support the use of the covariates employed in our risk prediction model for propensity score matching. These covariates are highly predictive of hypertension and diabetes, as demonstrated by model performance (Appendix Table A2), and are measured prior to the receipt of health screening.

Next, we describe the matching algorithm used in our analysis. We employ 1:4 nearest neighbor matching, in which each individual in the reference group (the untreated in the ATU framework) is matched to the four treated individuals with the closest propensity scores. Because the number

24. According to Appendix Table A4, individuals who did not receive a health screening tend to have a higher average risk of chronic disease. Therefore, if these individuals had received a screening, their health care utilization related to chronic disease would likely have been higher than that of those who were actually screened. In this case, the direction of selection bias would be negative.

of treated individuals exceeds that of untreated individuals, we allow for replacement to avoid sensitivity to the ordering of observations. To restrict the matching distance, we apply a caliper of 0.2 standard deviations of the propensity score, following Austin (2011). Standard errors are computed using the method proposed by Abadie and Imbens (2006).

Reverse causality between health screening and health care use for chronic conditions is a potential concern. Specifically, if an individual initiates health care use for a chronic condition prior to receiving a screening, the incentive to undergo screening may diminish, reducing the likelihood of uptake (Appendix Figure A10). In such cases, the estimated effect of health screening may be downward biased. To address this concern, we adopt two strategies. First, we exclude individuals whose health care utilization began before the month of their screening. For untreated individuals, we exclude those whose health care use began before the month of their previous screening.[25] Second, we include the month of the previous screening as a matching covariate. Individuals screened later in the year have more time for chronic conditions to naturally emerge. Therefore, matching on the month of the previous screening enables comparisons across groups with similar probabilities of natural disease onset.

25. In the 2016–2019 sample of individuals who received a screening, Appendix Figure A11 shows a strong association between the timing of the previous screening (1–2 years earlier) and that of the current screening.

D Matching Details

We begin by assessing the validity of the common support assumption for the matching procedure described in Appendix C through a visual inspection of the propensity score distribution (Figure A2(a)). As expected, the distribution for the untreated group is somewhat more concentrated at lower values of the propensity score. A common approach to evaluating the common support assumption is to compare the minimum and maximum propensity scores across groups (Caliendo and Kopeinig 2008). This involves removing observations whose scores fall outside the range of the other group. Applying this criterion results in the exclusion of only two treated individuals.

Next, we assess whether the matching procedure effectively balances covariates between individuals who received health screenings and those who did not; this assessment supports the balance reported in the main text. To do so, we use the standardized bias measure proposed by Rosenbaum and Rubin (1985):

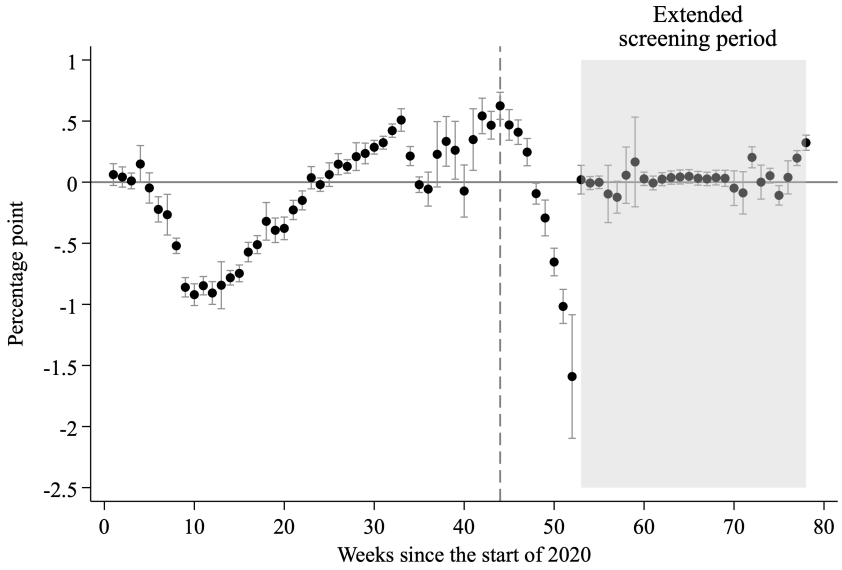
$$\frac{\bar{x}_{i1} - \bar{x}_{i0}}{\sqrt{(s_{i1}^2 + s_{i0}^2)/2}} \times 100 \tag{6}$$

where $\bar{x}_{i1}$ and $\bar{x}_{i0}$ denote the means of covariate i in the treated and untreated groups, respectively, and s_{i1} and s_{i0} are their corresponding standard deviations.

Although there is no theoretical justification for this threshold, an absolute standardized difference greater than 20% is conventionally considered too large (Shin 2022). The differences in covariates between the treated and untreated groups, observed before matching, are substantially reduced after matching and fall well below the 20% threshold (Figure A2(b)). These results suggest that the assumption of conditional independence is likely to hold, given that rich covariates related to chronic disease care are used in the matching process and are successfully balanced.

E Figures

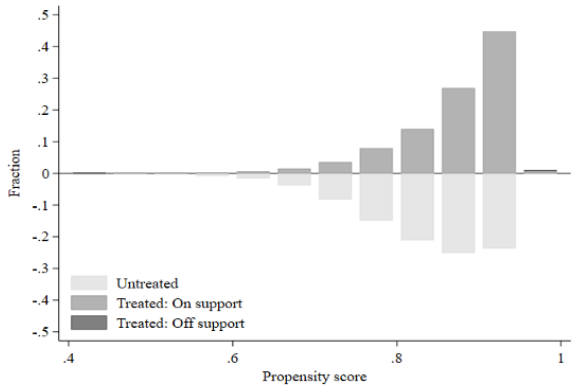
Figure A1: Change in Health Screening Rates for 2020 Cohort with Extended Screening Period



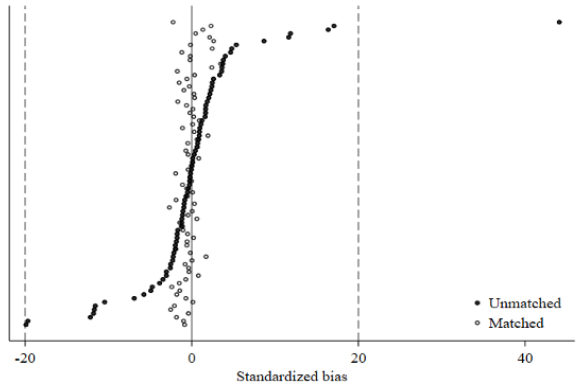
Notes: Figure A1 shows weekly changes in health screening rates in 2020 relative to the counterfactual rates, along with 95% confidence intervals. Vertical dashed lines mark the week when the screening period extension was announced. The counterfactuals are estimated by fitting equation 1 to weekly data from individuals eligible in 2016–2018, tracking screening uptake from the eligible year through June of the following year. The cumulative change in screening rates reached –6.57 percentage points by week 52. During the extended screening period (shaded area), screening rates increased by 0.89 percentage points, resulting in a total reduction of 5.68 percentage points by week 78. Standard errors are robust to heteroskedasticity.

Figure A2: Validity of Propensity Score Matching

(a): Distribution of propensity score



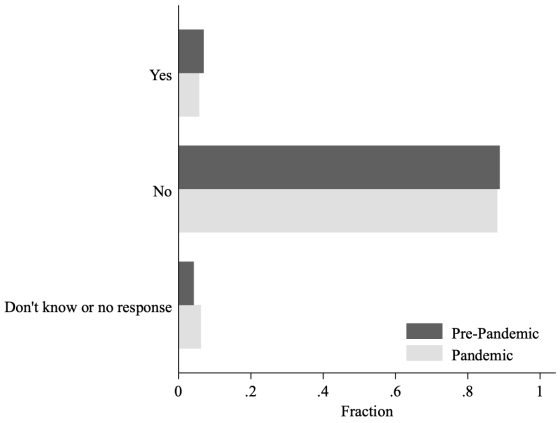
(b): Balance in covariates: standardized bias



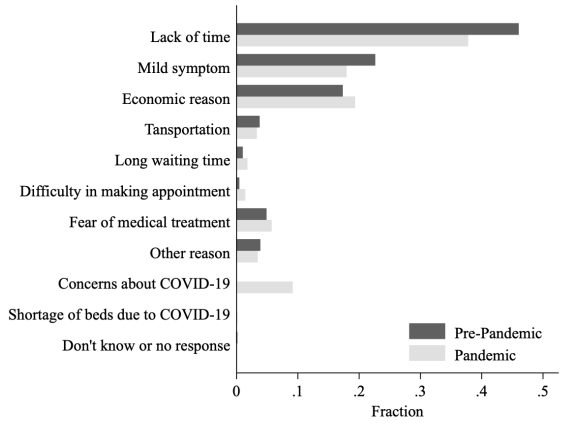
Notes: Figure A2(a) shows the distribution of propensity scores for the treated and untreated groups. Following Caliendo and Kopeinig (2008), we assess the common support assumption by comparing the minimum and maximum values of the propensity score distributions across groups. The matched sample includes 172,508 treated individuals (2 excluded due to lack of common support) and 26,212 untreated individuals (none excluded). Figure A2(b) presents the standardized bias of covariates before and after matching. Matching was performed using the `psmatch2` module in Stata. Variable names are not labeled because the plot cannot display labels for more than 30 covariates.

Figure A3: COVID-19 Pandemic and Unmet Health Care Needs

(a): Status of Unmet Health Care Needs

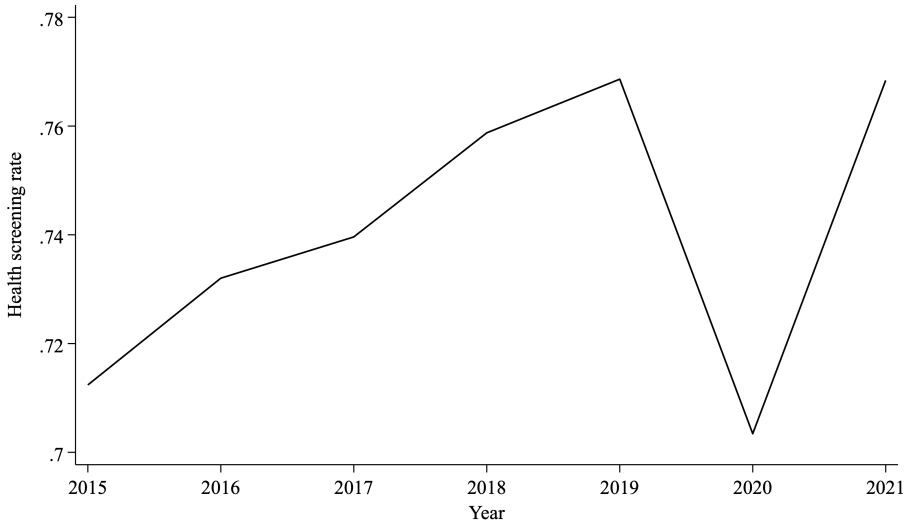


(b): Causes of Unmet Health Care Needs



Notes: Figure A3(a) reports the share of individuals who reported needing but not receiving health care in the past year. Figure A3(b) presents the main reasons cited for forgoing care. All proportions are weighted using survey sampling weights. The analysis uses data from the Korea National Health and Nutrition Examination Survey (KNHANES) for the years 2017 to 2021, restricted to individuals aged 40 to 79. The pre-pandemic period is defined as 2017–2019, and the pandemic period as 2020–2021.

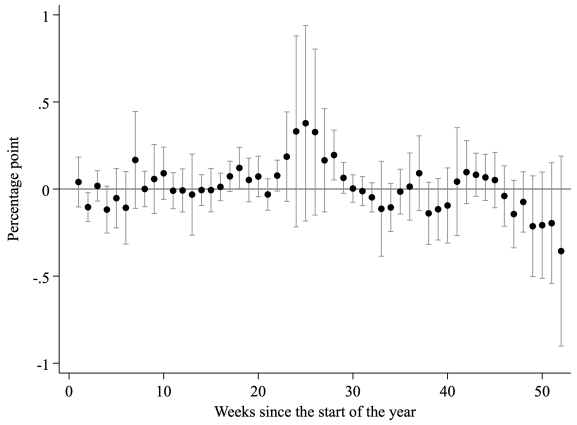
Figure A4: Trends in Health Screening Rates



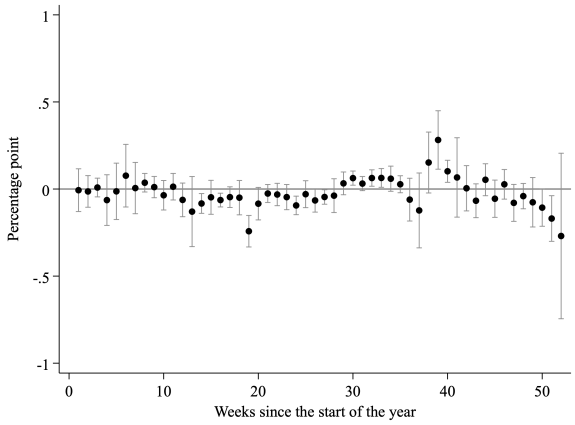
Notes: Figure A4 presents annual trends in the proportion of eligible individuals aged 40 to 79 who received a health screening.

Figure A5: Falsification Test Using Pre-Pandemic Samples

(a): Sample from 2018



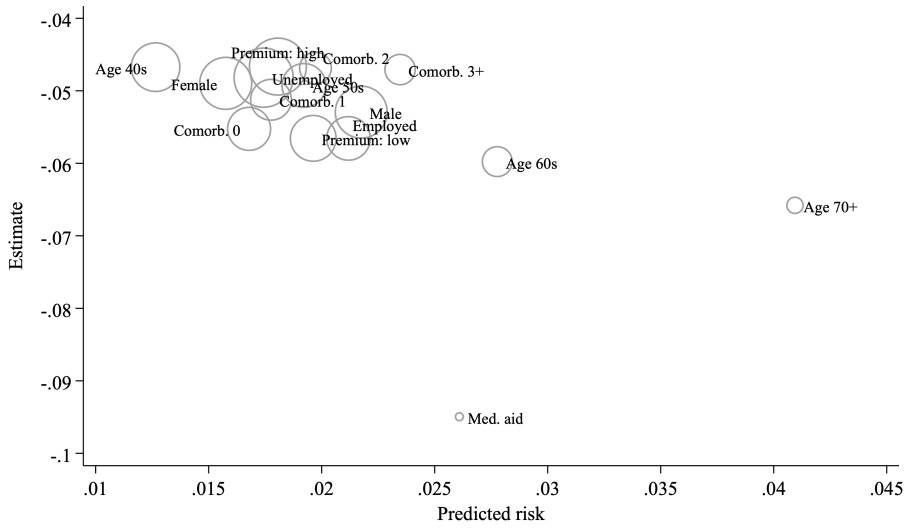
(b): Sample from 2019



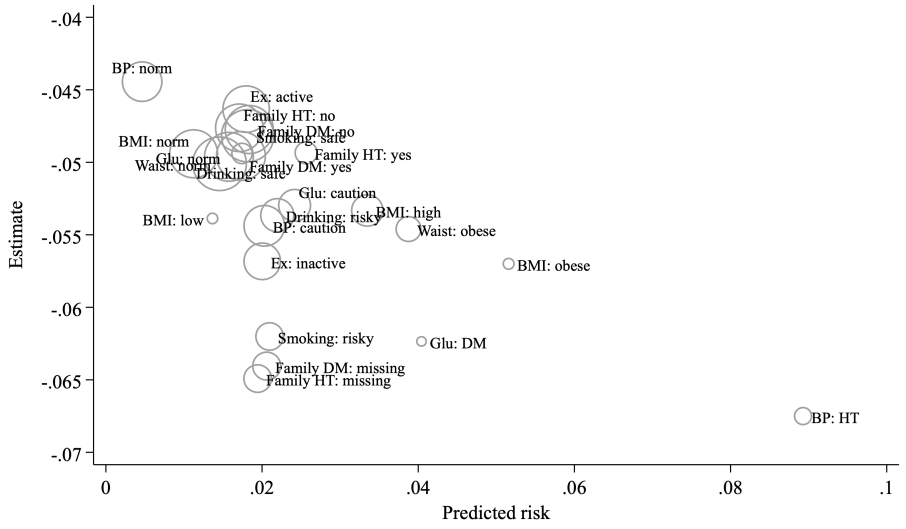
Notes: Figures A5(a) and A5(b) show weekly changes in health screening rates relative to the counterfactual for 2018 and 2019, respectively, along with 95% confidence intervals. The counterfactual values are estimated by fitting equation 1 to data from the preceding three years for each corresponding year (i.e., 2015–2017 for 2018, and 2016–2018 for 2019). Standard errors are robust to heteroskedasticity.

Figure A6: Breakdown by Variables Used in Risk Prediction (Hypertension)

(a): Demographic variables



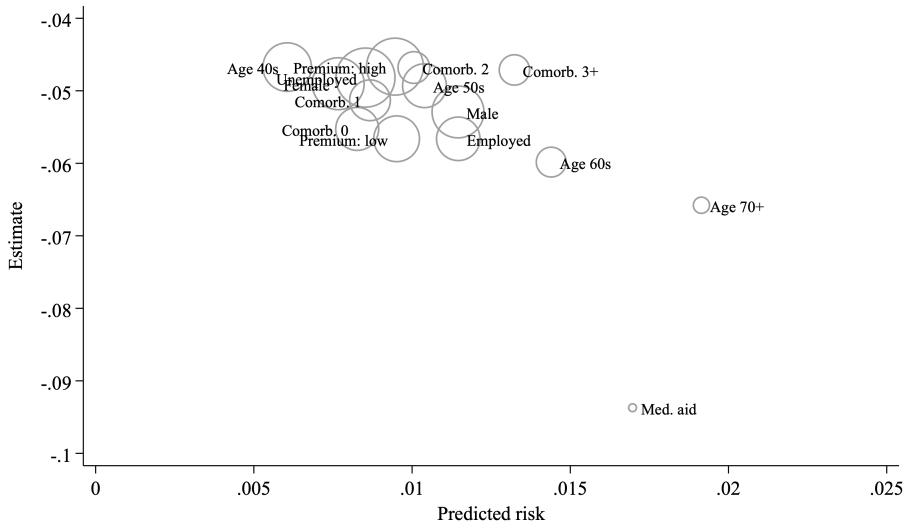
(b): Variables from past health screenings



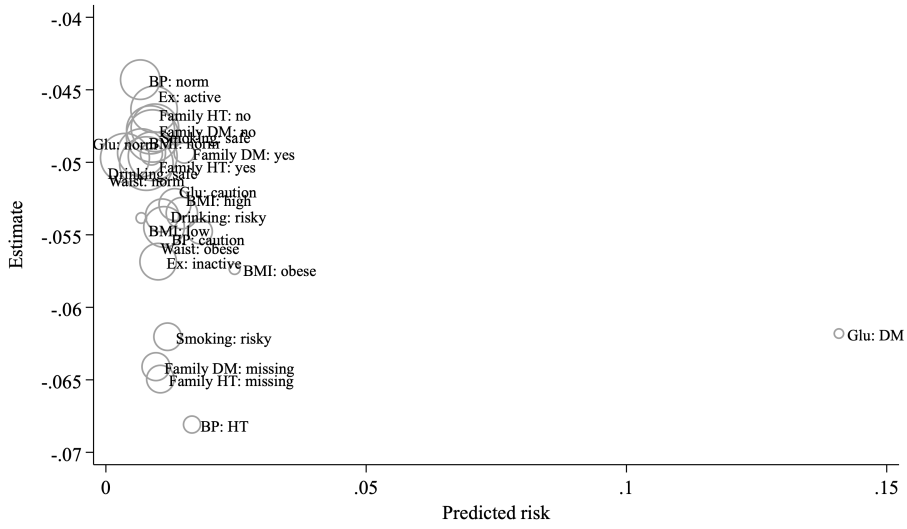
Notes: Figure A6 shows the results of a heterogeneity analysis based on the variables used to predict hypertension risk. For each variable, we estimate equation 2, allowing the effect of the pandemic on the probability of receiving a screening to vary across groups. The horizontal axis represents the average predicted risk for each group, and the vertical axis indicates the estimated change in screening probability during the pandemic, relative to the pre-pandemic period. For presentation purposes, the original variables used in the risk prediction model (see Appendix Table A1) are grouped into simplified categories. The size of each circle is proportional to the sample size of the corresponding group.

Figure A7: Breakdown by Variables Used in Risk Prediction (Diabetes)

(a): Demographic variables



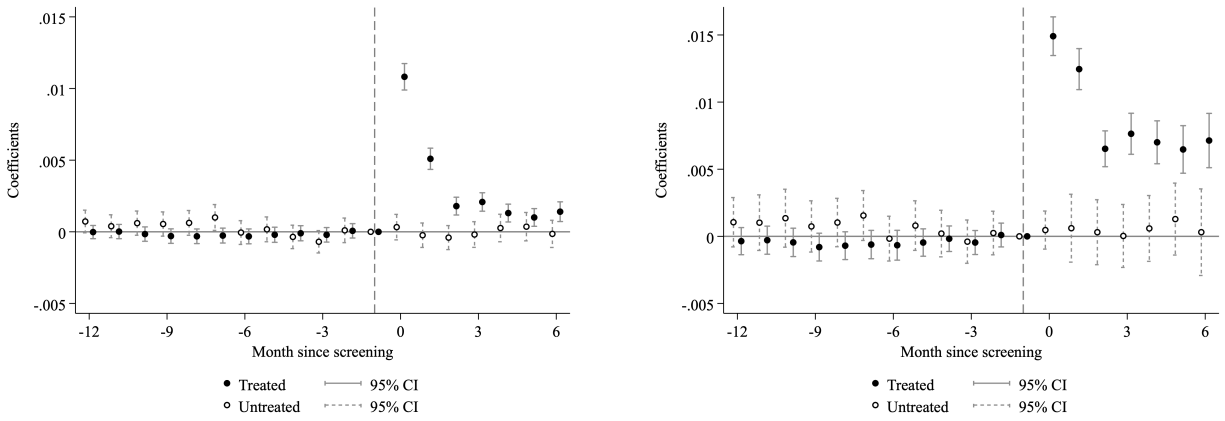
(b): Variables from past health screenings



Notes: Figure A7 shows the results of a heterogeneity analysis based on the variables used to predict diabetes risk. For each variable, we estimate equation 2, allowing the effect of the pandemic on the probability of receiving a screening to vary across groups. The horizontal axis represents the average predicted risk for each group, and the vertical axis indicates the estimated change in screening probability during the pandemic, relative to the pre-pandemic period. For presentation purposes, the original variables used in the risk prediction model (see Appendix Table A1) are grouped into simplified categories. The size of each circle is proportional to the sample size of the corresponding group.

Figure A8: Effects of Screening on Health Care Use: Event Study Results (Diabetes)

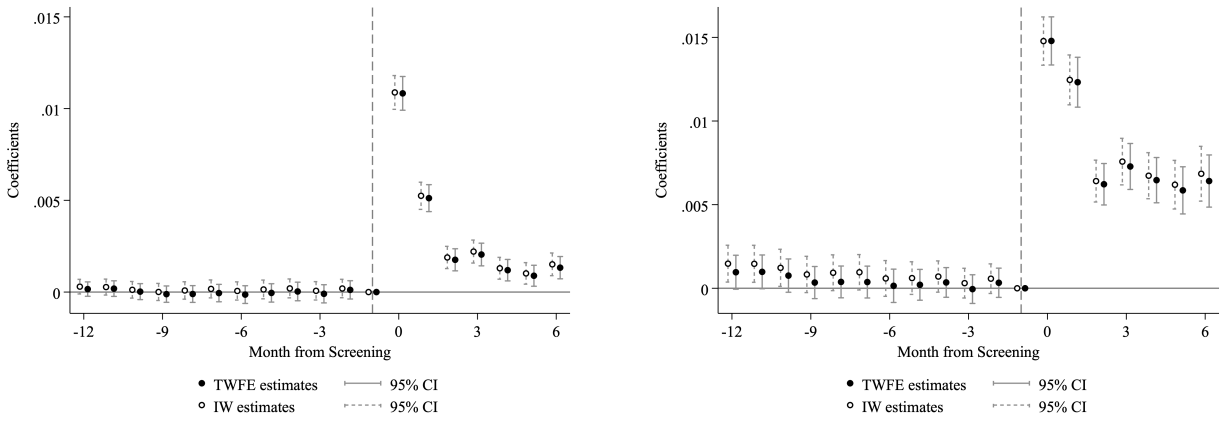
(a): *Care initiation* (b): *Number of care visits*



Notes: This figure presents estimates from equation 3 for diabetes, the counterpart to the hypertension event study reported in Section 5.2.1, illustrating how health care utilization evolves before and after screening. All coefficients are estimated relative to the reference period, the month immediately before screening ($j = -1$). For the untreated group, a false screening month is assigned by randomly selecting one of four treated individuals matched through propensity score matching. The two panels use *Care initiation* (a dummy variable indicating the first instance of health care use) and *Number of care visits* (the total number of diabetes-related visits) as outcome variables. Standard errors are clustered at the individual level.

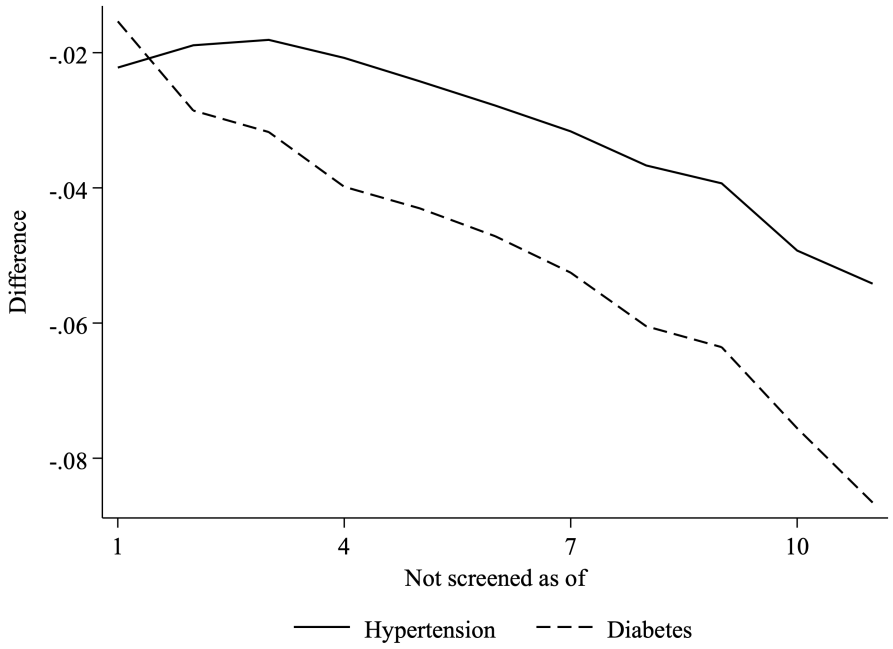
Figure A9: Comparing Screening Effects: TWFE vs. IW Estimator (Diabetes)

(a): *Care initiation* (b): *Number of care visits*



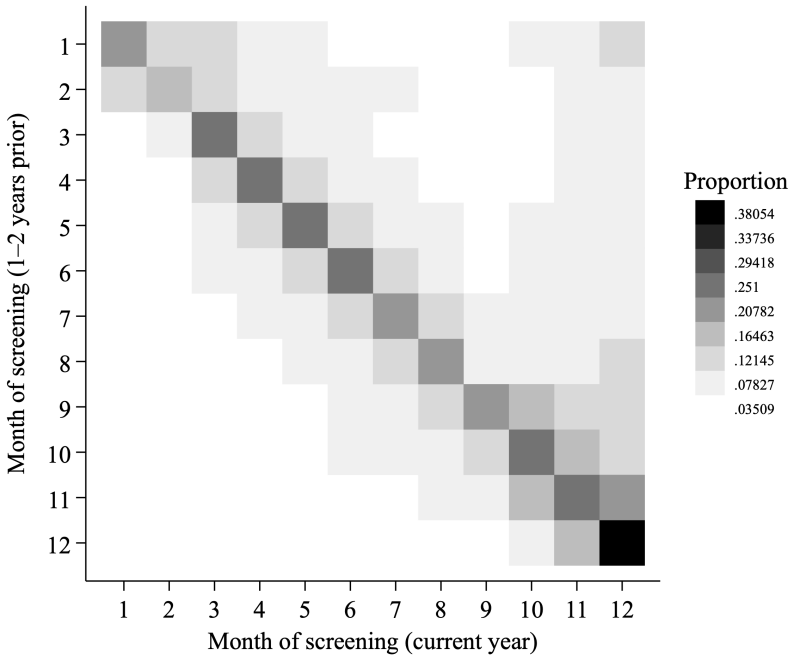
Notes: This figure presents estimates from the two-way fixed-effects (TWFE) and interaction-weighted (IW) estimators for diabetes, following Sun and Abraham (2021), the counterpart to the hypertension results in Section 5.2.1, to compare changes in health care utilization before and after screening. Since no screening month is defined for the untreated group, all event time dummies (leads and lags) are equal to zero, allowing this group to serve as the baseline cohort in the estimation. The two panels use *Care initiation* (a dummy variable equal to 1 if the individual initiated health care use) and *Number of care visits* (the total number of diabetes-related visits) as dependent variables. Standard errors are clustered at the individual level.

Figure A10: Health Screening Rates by Care Initiation Status for Chronic Diseases



Notes: Figure A10 presents differences in screening rates during the designated screening period between individuals who initiated health care for hypertension or diabetes and those who did not, conditional on not having received a screening by each calendar month. The analysis uses pre-pandemic data from 2016 to 2019 and focuses on individuals aged 40 to 79 who had received a screening within the past one to two years and had no health care use related to hypertension or diabetes in the preceding three years.

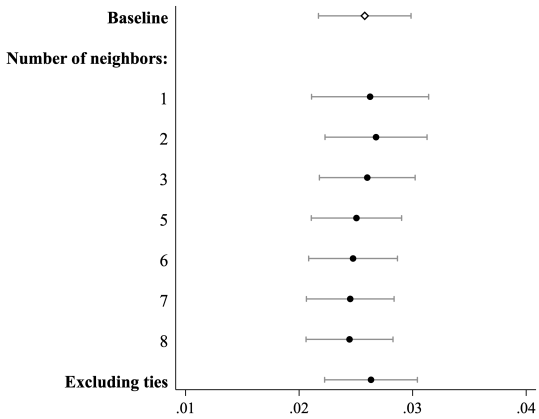
Figure A11: Tabulation of Screening Months in Current and Prior Years



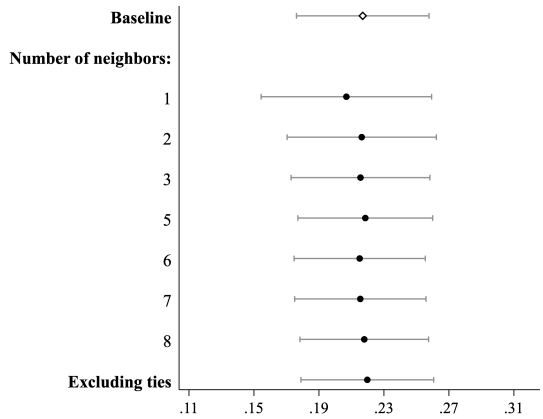
Notes: Figure A11 presents a tabulation of individuals' screening months, comparing the month of screening in the current year with that in the previous one to two years. The analysis uses data from individuals aged 40 to 79 who received health screenings between 2016 and 2019, restricted to those who had undergone a screening within the past one to two years and had no health care use related to hypertension or diabetes in the preceding three years.

Figure A12: Robustness of ATU Estimates for Health Care Use

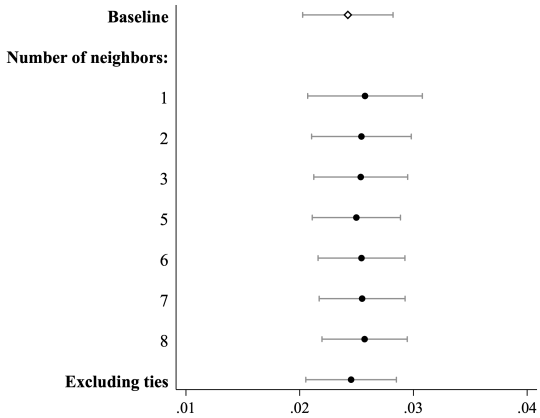
(a): Hypertension - *Care initiation*



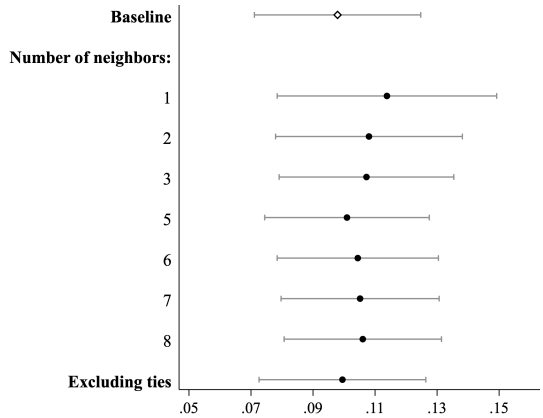
(b): Hypertension - *Number of care visits*



(c): Diabetes - *Care initiation*



(d): Diabetes - *Number of care visits*

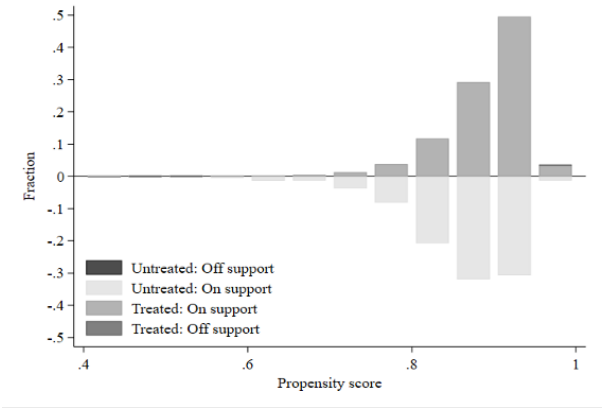


Notes: Figure A12 presents average treatment effect on the untreated (ATU) estimates from propensity score matching, with 95% confidence intervals. The variable *Care initiation* is a dummy equal to 1 if the individual used health care for the respective condition (hypertension or diabetes), while *Number of care visits* denotes the total number of such visits. Outcomes are measured using medical claims data from 2020 to 2021. The baseline specification uses four nearest neighbors and allows for matching with all ties. The group labeled "Number of neighbors" presents estimates obtained by varying the number of neighbors from 1 to 8. The estimate labeled "Excluding ties" is based on matching exactly four neighbors while excluding ties. Standard errors are calculated using the method of Abadie and Imbens (2006).

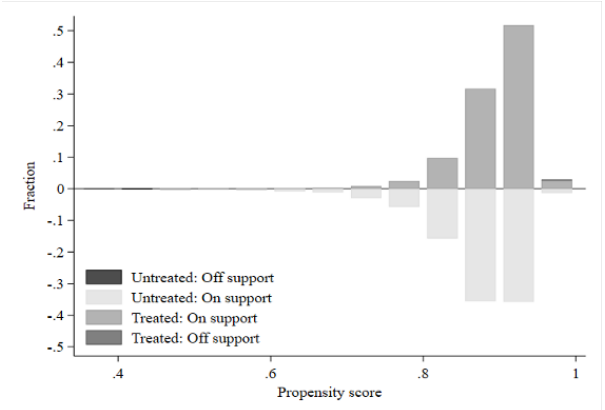
Figure A13: Validity of Propensity Score Matching (Section 5.3)

Distribution of propensity score

(a): Hypertension sample

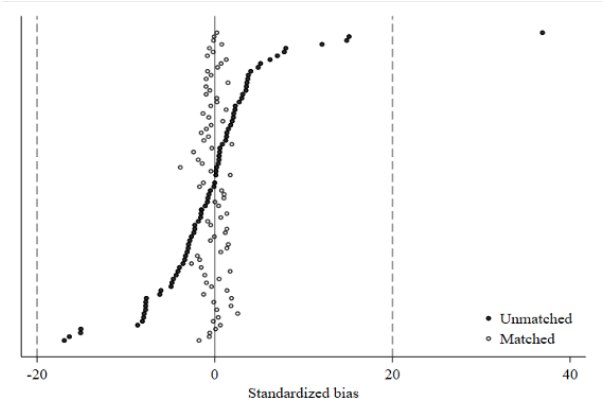


(b): Diabetes sample

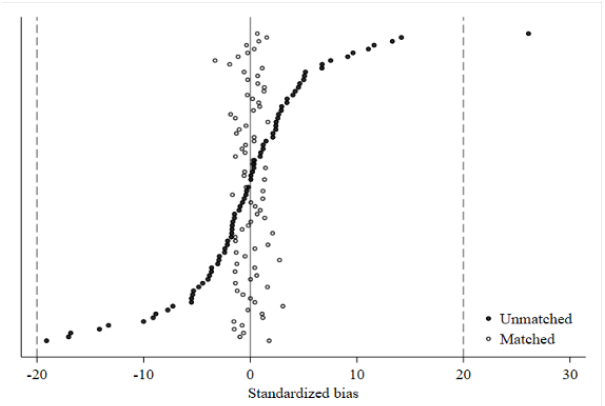


Balance in covariates: standardized bias

(c): Hypertension sample



(d): Diabetes sample



Notes: Figure A13(a) shows the distribution of propensity scores for the treated and untreated groups used in the baseline matching for Table 3. Following Caliendo and Kopeinig (2008), we assess the common support assumption by comparing the minimum and maximum values of the propensity score distributions across groups. The matched sample includes 15,097 treated individuals (20 excluded due to lack of common support) and 1,921 untreated individuals (1 excluded). Figure A13(b) shows the same assessment for the diabetes sample, consisting of 14,909 treated individuals (37 excluded) and 1,815 untreated individuals (2 excluded). Figures A13(c) and A13(d) present the standardized bias of covariates before and after matching. Matching was performed using the `psmatch2` module in Stata. Variable names are not labeled because the plot cannot display labels for more than 30 covariates.

F Tables

Table A1: List of Variables Used in Risk Prediction

Category	Variable	Description
Outcome variable	Health care use related to hypertension, diabetes	Dummy variable indicating health care utilization with hypertension or diabetes as the primary diagnosis
Demographic variable	Age	Dummy variables for age groups in 5-year intervals
	Health insurance premium	Dummy variables for each of the 21 groups, composed of 20 insurance premium quintiles and medical aid recipients
	Health insurance type	Dummy variable representing employee subscribers
	Sex	Dummy variable equal to 1 for females
Past health screening	BMI	Dummy variables for underweight, normal, cautious, and suspected obesity
	Waist circumference	Dummy variables for normal and suspected abdominal obesity
	Fasting blood sugar	Dummy variables for normal, cautious, and suspected diabetes
	Blood pressure	Dummy variables for normal, cautious, and suspected hypertension
	Physical activity	Dummy variable indicating the need for physical activity
	Smoking	Dummy variable indicating the need for smoking cessation
	Alcohol consumption	Dummy variable indicating the need for abstaining from alcohol
	Family history of hypertension, diabetes	Dummy variable indicating a family history of hypertension, diabetes
Elixhauser Comorbidity Condition	27 disease groups	Dummy variables indicating health care utilization due to the respective condition

Table A2: Comparison of Prediction Model Performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: hypertension							
AUC	0.6356	0.7864	0.7869	0.7892	0.7906	0.7909	0.7911
Panel B: diabetes							
AUC	0.6339	0.8148	0.8150	0.8178	0.8219	0.8247	0.8254
Predictors:							
Demographics	Y	Y	Y	Y	Y	Y	Y
Biomarkers		Y	Y	Y	Y	Y	Y
Health behavior			Y	Y	Y	Y	Y
Family history				Y	Y	Y	Y
Elixhauser comorbidity - 1 year					Y		
Elixhauser comorbidity - 2 years						Y	
Elixhauser comorbidity - 3 years							Y

Notes: Table A2 reports the area under the receiver operating characteristic curve (AUC) from prediction models estimating the initiation of health care utilization for hypertension (Panel A) and diabetes (Panel B). The outcome variable is a dummy equal to 1 if an individual initiated health care use for the corresponding condition as the primary condition in a given year, and 0 otherwise. The models are estimated using logistic regression, with AUC values computed based on 5-fold cross-validation. Each column incrementally adds a group of predictors, including demographics, biomarkers, health behaviors, family history, and Elixhauser comorbidity conditions constructed using data from the past one to three years. A detailed description of each predictor group is provided in Appendix Table A1.

Table A3: Effect of the COVID-19 Pandemic on Health Screening Participation: Baseline Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
COVID	-0.0117***	-0.0518***	-0.0534***	-0.0531***	-0.0513***	-0.0511***	-0.0511***
	(0.0009)	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0017)
Controls:							
Year trend		Y	Y	Y	Y	Y	Y
Demographics			Y	Y	Y	Y	Y
Biomarkers				Y	Y	Y	Y
Health behavior					Y	Y	Y
Family history						Y	Y
Elixhauser comorbidity							Y
Adjusted R-squared	.0002	.0011	.0288	.0302	.0315	.0331	.0344
Observations	770815	770815	770815	770815	770815	770815	770815

Notes: Table A3 reports estimates from a series of linear probability models assessing the effect of the COVID-19 pandemic on the likelihood of receiving a health screening among eligible individuals. The outcome variable is a dummy equal to 1 if the individual received a health screening between year t and June of year $t + 1$, and 0 otherwise. The variable of interest, $COVID$, is a dummy equal to 1 if the individual was eligible for screening in 2020 and 0 if eligible in 2016–2018. Individuals eligible in 2019 are excluded, as their time window partially overlapped with the early stage of the pandemic. Control variables are added sequentially from Columns (1) to (7), with definitions provided in Appendix Table A1. Standard errors are robust to heteroskedasticity. *, **, *** denote significance at the 10%, 5%, 1% levels, respectively.

Table A4: Sample Characteristics (Section 5)

	Untreated		Treated		Difference
	Mean	SD	Mean	SD	
Outcome variables					
Care initiation (hypertension)	0.073		0.088		-0.014
Number of care visits (hypertension)	0.466	2.505	0.590	2.923	-0.124
Care initiation (diabetes)	0.069		0.087		-0.017
Number of care visits (diabetes)	0.238	1.665	0.286	1.688	-0.049
Demographic characteristics					
Age	53.110	9.008	52.513	8.713	0.597
Health insurance premium (20-quantiles)	11.411	6.305	11.948	5.991	-0.537
Recipient of medical aid	0.020		0.006		0.014
Employee-insured status	0.492		0.703		-0.211
Female	0.524		0.503		0.021
Previous screening results (1–2 years prior)					
Obesity risk status (BMI)					
Underweight	0.033		0.027		0.006
Normal	0.635		0.652		-0.018
Caution	0.291		0.287		0.004
At-risk	0.041		0.034		0.007
Obesity risk status (waist circumference)					
Normal	0.801		0.820		-0.019
At-risk	0.199		0.180		0.019
Diabetes risk status (fasting blood glucose)					
Normal	0.656		0.673		-0.017
Caution	0.316		0.306		0.009
At-risk	0.028		0.021		0.008
Hypertension risk status (blood pressure)					
Normal	0.415		0.458		-0.043
Caution	0.472		0.464		0.008
At-risk	0.112		0.078		0.034
Smoking cessation needed					
No	0.765		0.812		-0.047
Yes	0.235		0.188		0.047
Drinking cessation needed					
No	0.650		0.654		-0.004
Yes	0.350		0.346		0.004
Physical activity needed					
No	0.462		0.521		-0.059
Yes	0.538		0.479		0.059
Family history (diabetes)					
No	0.612		0.693		-0.081
Yes	0.135		0.134		0.001
Missing	0.253		0.173		0.080
Family history (hypertension)					
No	0.592		0.671		-0.079
Yes	0.163		0.164		-0.001
Missing	0.245		0.165		0.080
N	26212		172508		

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