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Abstract

Outreach raises public-program take-up, but its effects on targeting efficiency depend on who responds. We study voucher mailing in Japan's rubella immunization program, where pre-vaccination screening identifies antibody-negative individuals who should be vaccinated and allows evaluation of targeting efficiency. Exploiting an age-based mailing threshold in a regression discontinuity design with local administrative data, we find mailing increased antibody testing and vaccination by 19 and 5 percentage points, while maintaining average targeting efficiency at the population benchmark. Favorable selection concentrated immediately after mailing and attenuated over time. A complementary nationwide survey replicates these effects, highlighting awareness gains as a key mechanism.

Keywords: Public-program take-up; Targeting efficiency; Information frictions; Vaccination; Regression discontinuity design

JEL classification: D90, H75, I18

Public policy faces a "last-mile" problem: getting eligible individuals to actually take up the programs intended for them. In many settings, participation cannot be compelled, so take-up is voluntary and often incomplete. Previous research identifies several barriers to take-up, including application costs (Currie, 2006), limited knowledge about programs (Chetty and Saez, 2013; Kostøl and Myhre, 2021), inattention (Rees-Jones and Taubinsky, 2020), and stigma (Ko and Moffitt, 2022).

Mailing vouchers or informational materials to eligible individuals is a traditional intervention aimed at overcoming these barriers. This approach can lower the economic or time costs associated with take-up, deliver program-related knowledge, increase attention, and potentially reduce psychological costs by providing vouchers or simplifying procedures. This intervention has been widely used in practice, and its impact on take-up has been studied across various policy areas, including food stamps, health checkups, and tax filing (Chetty, Friedman and Saez, 2013; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Kostøl and Myhre, 2021; Goldin et al., 2022; Kacker et al., 2022).

However, an important question about this traditional approach remains underexplored—and is the focus of our paper: beyond raising take-up, does mailing reach those who most need the program? Building on Currie (2006)'s concern that application frictions may screen out the neediest and Kleven and Kopczuk (2011)'s formalization of the trade-off between screening, targeting efficiency, and incomplete take-up, this question becomes especially consequential when eligible individuals differ in their underlying returns to participation and this heterogeneity is revealed only after participation. In such settings, the welfare effect of outreach is theoretically ambiguous: if higher-return individuals face larger informational, attentional, or cost barriers, mailing disproportionately attracts them and improves targeting; if instead the marginal participants are lower-return, take-up rises while efficiency falls.

While much of the past literature asks whether outreach raises take-up, a smaller body asks a distinct question: whom outreach draws in, and how it reshapes the composition of participants relative to their underlying returns. In social-benefit programs, this composition has often shifted in an unintended, unfavorable direction, drawing in marginal participants of lower return: information and application assistance in SNAP (Finkelstein and Notowidigdo, 2019), automatic enrollment in cash transfers (Alatas et al., 2016), and social-worker assistance (Castell et al., 2025). Counterexamples include Deshpande and Li (2019), where lowering application costs in disability benefits drew in higher-return applicants, and Gupta (forthcoming), where application assistance in a cash-transfer program disproportionately increased take-up among more vulnerable individuals. However, the welfare reading of such selection is not settled: returns are typically observed through proxies such as predicted benefits, income, or risk scores, and because the welfare effect turns on marginal participants' behavioral biases, the direction of selection need not track welfare (Finkelstein and Notowidigdo, 2019). Health insurance sharpens this gap. Domurat, Menashe and Yin (2021) find that promoting take-up attracts low-risk eligibles, a favorable shift that mitigates adverse selection. Yet since high-risk individuals incur higher costs, these entrants may not be those who benefit most from coverage. Across these settings, outreach can shift composition either way, and because returns are proxied and may diverge from welfare, whether a shift is desirable often cannot be determined. This motivates evaluating outreach where returns are observed directly and aligned with welfare, so that selection and its welfare content, its targeting efficiency, can be read directly.

This paper contributes to this emerging literature by showing that, in a setting where returns are observed and more aligned with welfare, mailing interventions can raise take-up without inducing the unintended, unfavorable selection reported in the social-benefit settings above. Beyond this aggregate result, we offer a new perspective on the dynamics of targeting efficiency, which is relatively high immediately after mailing but attenuates over time. We document these findings using Japan's rubella antibody testing and vaccination program, where an antibody test first reveals each individual's medical need, and vaccinating the antibody-negative then delivers both individual and public-health benefits.

Immunity can be acquired not only through vaccination but also through natural infection, so the population mixes those who lack antibodies with those who already have them. Those who lack antibodies benefit directly from vaccination, and vaccinating them also advances herd immunity, so individual need and social welfare align. Because of this alignment, failing to reach antibody-negative individuals would raise take-up without a corresponding gain in antibody prevalence and would decrease cost-effectiveness. For example, some may be unsure whether they were infected or vaccinated years ago and take part rather than check, while others may suspect they are already immune but take part anyway if participation is free, drawing already-immune people in. Avoiding targeting inefficiency is therefore itself a meaningful goal for outreach interventions.

Targeting is nonetheless difficult in practice because infections can be asymptomatic, policymakers often do not observe individuals' immune status in advance, and even individuals themselves often have only imperfect knowledge of their own status. When antibody testing is available prior to vaccination, it becomes possible to identify those who lack immunity and to evaluate whether outreach increases participation among this medically relevant target group. However, many public programs (as in the COVID-19 case) offer vaccination without a screening step to identify who most needs it.

Japan's rubella immunization program offers a unique setting in which vaccination is preceded by antibody testing: individuals first take a test, and vaccination is recommended only for those who are antibody-negative, so underlying need is revealed through participation. The program began in 2019 for men born between April 2, 1962 and April 1, 1979 (ages 40–56 as of April 1, 2019), a cohort historically excluded from public rubella vaccination. In 2018, antibody prevalence among men in this group was 81% (vs. 98% among women), leaving Japan short of herd immunity (see Section I). Because rubella vaccine-induced immunity is long-lasting and antibody-negative individuals typically require only a single dose, the Ministry of Health, Labour and Welfare (MHLW) launched an additional measure in 2019 to raise antibody prevalence among these men from 81% to 90% by mailing vouchers that covered antibody testing (about JPY 5,000, equal to USD 45 at USD 1 = JPY 110) and vaccination (about JPY 10,000). The vouchers aimed first to increase testing, thereby identifying the 19% who lacked antibodies, and then to increase vaccination among those identified as antibody-negative.

This program provides two further key advantages for identifying the causal effect of mailing vouchers and exploring background mechanisms. First, voucher mailing followed a phased rollout: local governments mailed vouchers to men aged 40–46 (as of April 1, 2019) in Fiscal Year (FY) 2019, while men aged 47–56 began receiving mailed vouchers only after April 2020 and, during FY2019, could obtain vouchers only by applying to their local government. Thus, in FY2019, men aged 40–46 received vouchers by default, whereas men aged 47–56 faced an opt-in process involving transaction costs and information acquisition. We exploit this age-based cutoff in default mailing timing to identify the causal effect of mailing in a regression discontinuity design (RDD) framework (Lee and Card, 2008). The MHLW structured this two-stage design with antibody testing and the age-phased rollout of voucher mailing in response to constraints in rubella vaccine supply and implementation capacity.

Second, baseline awareness of the additional rubella immunization program was very low. Only 23.5% of men aged 47–56 (who were not targeted for mailing in FY2019) were aware of the program, consistent with Hori et al. (2021). This contrasts sharply with SNAP (Finkelstein and Notowidigdo, 2019): a Massachusetts survey reports that roughly 70% of low-income respondents were aware of SNAP[1], and 82% of eligible individuals participate nationally (Schanzenbach, 2023). We therefore evaluate mailing vouchers in an environment with low baseline awareness, which helps clarify the mechanisms through which outreach increases participation and how resulting participation changes map into targeting outcomes.

Specifically, we collaborated with a Japanese local government and used administrative records of antibody testing and vaccination for 27,745 men aged 40 to 56 as of April 1, 2019. Using an age-based cutoff at 47 years, we implement a RDD to estimate the effects of voucher mailing on take-up of antibody testing and vaccination, and to assess how well the induced take-up is targeted toward antibody-negative individuals. In addition, in collaboration with the MHLW, we conducted a nationwide online survey of 3,444 men in the same age group and apply the same RDD framework to assess the geographic external validity of the administrative-data estimates.

Three main findings emerge. First, voucher mailing increased take-up of antibody testing and vaccination. In the local administrative data, voucher mailing raises antibody testing and vaccination by 19.3 and 5.3 percentage points, respectively. Given baseline rates of 1.6% and 0.3% among men outside the FY2019 mailing target, these effects represent large changes relative to pre-mailing participation. We obtain similar estimates in the nationwide survey, providing supportive evidence on the geographic external validity of the local administrative results.

Second, 29 percent of compliers, the marginal individuals induced to test by voucher

[1] `https://projectbread.org/research/barriers-to-snap` (Accessed May 15, 2026).

mailing, lack antibodies. This point estimate exceeds the 22 percent benchmark implied by population antibody prevalence at the cutoff; the difference is not statistically significant. Voucher mailing thus raises take-up while maintaining targeting efficiency at this benchmark. Favorable selection concentrates immediately after mailing, where the negative rate among compliers reaches 40 percent (Section D).

Third, low take-up among men outside the FY2019 mailing target is consistent with limited program awareness. In the nationwide survey, only 23.5% of non-targeted men report being aware of the rubella immunization program.[2] Furthermore, mailing the vouchers increases this awareness by 37.4 percentage points. Therefore, this mailing intervention improves program awareness and boosts take-up of the rubella immunization program.

Related literature and our contributions — Our paper contributes to the literature on program take-up, targeting efficiency, and vaccination policy. First, building on mixed evidence from social benefit programs on whether reducing frictions increases take-up among high-return individuals or mainly attracts lower-return participants (Alatas et al., 2016; Finkelstein and Notowidigdo, 2019; Deshpande and Li, 2019; Domurat, Menashe and Yin, 2021; Castell et al., 2025), we study a public health program in which eligibility for vaccination is revealed only after antibody testing, which provides a single, objectively observed measure of returns rather than a constructed proxy such as predicted benefits or risk scores. We demonstrate that a low-cost mailing intervention substantially increases take-up while maintaining targeting efficiency at the population benchmark. Moreover, we provide supportive evidence on mechanisms and dynamics: much of the take-up effect operates through alleviating information frictions, and individuals induced by mailing are at least as likely (and possibly more likely) to lack antibodies. Using timing variation, we show that this favorable selection is strongest immediately after mailing and attenuates over time.

These findings speak to a broader literature on targeting individuals with the highest returns. Two targeting approaches are common: one uses observable characteristics, such as demographics, past behavior, or administrative records, to predict whom to pri-

[2]Hori et al. (2021) similarly found that only 27% of those who did not receive a voucher were aware that the government recommends rubella vaccination.

oritize (Kitagawa and Tetenov, 2018; Athey and Wager, 2021), while the other relies on self-selection, treating enrollment or response as a signal of underlying returns (Heckman and Vytlacil, 2005; Heckman, 2010). Our setting offers a clean test of the self-selection approach: in Japan's rubella program, policymakers observe almost nothing about immunity status prior to test participation, and individuals' beliefs about their own immunity are uncertain. In this environment, where true returns are not fully observed by either side, targeting policies that combine self-selection with antibody testing as a screening tool can be effective.

Second, we contribute to the literature on vaccination policy in settings with partial immunity, where the challenge is to identify and vaccinate individuals who lack immunity. Prior work has mainly evaluated financial incentives (Barham and Maluccio, 2009; Campos-Mercade et al., 2021; Barber and West, 2022; Brehm, Brehm and Saavedra, 2022) and behavioral interventions such as defaults or framed messages (Chapman et al., 2010; Dai et al., 2021; Milkman et al., 2021; Serra-Garcia and Szech, 2021), typically in universal-offer contexts where underlying returns are not directly observed. We show how policy design interacts with heterogeneity in immunity, and that a low-cost outreach intervention can increase vaccination uptake without a net reduction in how well participation aligns with true returns. Because rubella vaccination is delivered as MR/MMR, our findings also speak to measles policy amid renewed outbreaks, including in countries with historically high reported coverage (Suran, 2024; WHO, 2024*a*).

The remainder of this paper is organized as follows. In Section I, we describe the rubella immunization program. In Section II, we present the results derived from local administrative data. In Section III, we present the complementary results derived from nationwide online survey data. We discuss our results in Section IV. Finally, we conclude the paper in Section V.

I The Two-Stage Immunization Program and Voucher Mailing

Rubella, similar to influenza, is an infectious disease transmitted via respiratory droplets. The infection is most consequential for pregnant women, as it can give rise to congenital

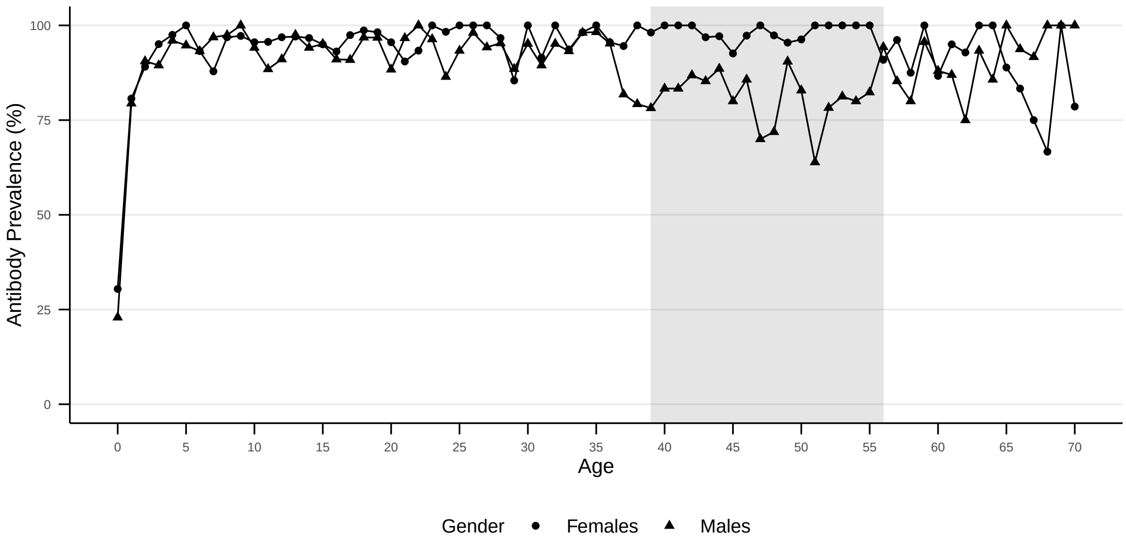

Figure 1: Prevalence of Rubella Antibodies by Age and Gender in Japan.
Note: The gray area is the intended population for the rubella immunization program since FY2019. Data source: National Institute of Infectious Diseases "2018 National Epidemiological Surveillance of Vaccine-Preventable Diseases."

rubella syndrome in infants, marked by ocular and auditory abnormalities. In recognition of these risks, the World Health Organization identifies rubella as a leading vaccine-preventable cause of birth defects (WHO, 2024*b*), and recent declines in vaccination coverage in the United States and Europe have raised concerns about renewed measles-rubella outbreaks (Seither et al., 2024; Béraud et al., 2018). The Japanese government also designated rubella as a disease for which immunization is required to safeguard public health.

However, Japan did not achieve herd immunity against rubella in 2019, the starting point of this study, because the antibody prevalence among men in their 40s and 50s was still approximately 81% owing to no vaccination opportunities through routine immunization and the low likelihood of natural infection (see Figure 1).[3] Kinoshita and Nishiura (2016) suggested that Japan could achieve herd immunity against rubella if the antibody prevalence in all generations exceeded 90%.

In April 2019, the MHLW launched an immunization program providing free rubella antibody testing and vaccination for men born between April 2, 1962 and April 1, 1979

[3]The antibody prevalence among men and women aged 56 years and older is 91.3% and 89.4%, respectively, because they are most likely to have not received the routine rubella vaccination but had antibodies from natural infection. Men and women under the age of 38 years have antibody prevalence of 91.3% and 94.0%, respectively, because they have received at least one dose of the rubella vaccine through routine immunization.

(ages 40–56 as of April 1, 2019).[4] This program follows a two-step structure. The first step is antibody testing as a screening, which identifies the 19% of eligible men who lack antibodies. The second step is vaccination, which is offered only to those who tested negative in the first step.

Local governments followed the MHLW's guideline and mailed vouchers for free antibody testing and vaccination to eligible men in two phases, beginning in FY2019 and continuing after FY2020. Each phase's mailing eligibility was determined by the individual's date of birth. Specifically, with ages measured as of April 1, 2019, the FY2019 eligibility was defined as follows:

1. April 2, 1972 – April 1, 1979 (ages 40–46): automatically received the vaccination vouchers by mail in FY2019;
2. April 2, 1962 – April 1, 1972 (ages 47–56): automatically received the vaccination vouchers by mail after April 2020 but had to apply to get them before April 2020.

The design of this program provides a uniquely suitable setting for our study, offering three features that we exploit: First, voucher eligibility in FY2019 was determined by an age cutoff. This design feature provides a sharp threshold that allows us to apply a RDD to identify the causal effect of mailing, as shown in Sections II and B.

Second, the vouchers simultaneously reduced information acquisition costs and application costs. In light of previous studies emphasizing the importance of these frictions, the reduction of both costs is therefore expected to raise take-up. First, mailing vouchers lowered information acquisition costs. Men in the former group received an official letter from their local government notifying them of the program's launch, outlining its procedures, and listing the medical institutions where the test and vaccination were available (see Figure B1 in Supplemental Appendix B). Men in the latter group had to search for this information independently, often with limited awareness. Second, it lowered application costs. Men in the former group automatically received free vouchers without any effort, whereas men in the latter group were required to either visit the city hall or local health centers in person, or fill out an application form and mail it.

[4]Official MHLW materials often describe this birth cohort as ages 40–57 in FY2019. Throughout this paper, we report completed ages as of April 1, 2019, the reference date used to construct our RD running variable.

Third, the program incorporated a two-step structure: antibody testing followed by vaccination. This design uniquely allows us to move beyond the conventional focus on participation outcomes and directly evaluate targeting efficiency. Antibody testing identifies the subset of men who actually require immunization. If mailing vouchers increases participation while maintaining or raising the proportion of antibody-negative individuals among those who take up the program, the intervention can be judged as enhancing efficiency. Conversely, if the proportion of antibody-negative individuals declines, it would suggest that easier access primarily drew in men who already had immunity, thereby lowering efficiency.

It is worth noting that in September 2025, the WHO verified rubella elimination in Japan (WHO, 2025), even though the actual antibody testing rate (program participation rate in our context) did not reach the MHLW's goal. According to national administrative data provided by the MHLW, throughout the program period, the actual antibody testing rate is 32.2%, well below the MHLW's 60% goal (see Figure A1 in Supplemental Appendix A). Moreover, under the counterfactual assumption that antibody testing take-up is independent of antibody status and those whose test results are negative are vaccinated, the predicted vaccination rate falls below the rate actually observed (see Figure A2 in Supplemental Appendix A). This gap implies that testing take-up was not independent of antibody status. If participants were disproportionately those who had antibodies, that is, low targeting efficiency, the predicted vaccination rate would have exceeded, not fallen below, the observed rate. Thus, the observed gap is consistent with high targeting efficiency, in which participants were disproportionately those who lacked antibodies. This high targeting efficiency offers one hypothesis for how Japan achieved rubella elimination despite the low aggregate testing rate. In the following section, we examine this possibility more precisely.

II Take-Up and Targeting Efficiency

We cooperated with Tsukuba City, Ibaraki Prefecture, Japan, to acquire data on the rubella immunization program. Administrative data accurately reflect antibody testing and vaccination behavior, allowing us to precisely estimate the effectiveness of mailing vaccination vouchers compared to survey data, which are mainly based on self-reports. This section presents the results of the analysis of the local administrative data.

A Administrative Data

In early June 2022, we obtained anonymized personal data from Tsukuba City for eligible men ($N = 30,936$), excluding those who had already moved out at the time of data collection. The dataset included masked addresses, dates of birth, and move-in dates. For individuals who had moved into Tsukuba City before March 31, 2019, Tsukuba City identified the 2019 mailing targets based on guidelines established by the MHLW. We therefore limited our analysis sample to eligible men who had moved into Tsukuba City before March 31, 2019 ($N = 27,745$).

As the personal data contained birth dates, we calculated age in days as of April 1, 2019, and created a treatment variable indicating the 2019 mailing target.[5] The treatment variable is a dummy variable that takes the value 1 for individuals born between April 2, 1972, and April 1, 1979 (age in days less than 17,166 days, or age less than 47 years). Tsukuba City sent vaccination vouchers to these individuals on April 12, 2019 ($N = 12,731$, 46% of the analysis sample).

We also received data on the rubella immunization program from Tsukuba City to construct the outcome variables. These data recorded the date of antibody testing and vaccination (April 1, 2019–February 28, 2022) for those who received at least antibody testing ($N = 8,043$).[6] Eligible men not in this data had received neither antibody testing nor vaccination within the data period. We created dummy variables indicating eligible men who underwent antibody testing or vaccination during our defined analysis periods (April 1, 2019–March 17, 2020, as detailed later) for use as outcomes.

Figure 2 splits the analysis sample by treatment status and plots the transition in the sample average of the outcome variable (cumulative take-up rates) for each subsample. By default, men subject to the 2019 mailing target received vouchers on April 12, 2019, whereas men outside the 2019 mailing target received vouchers on April 1, 2020.[7] Anti-

[5]Our analysis sample already excludes one observation with an incomplete birth date.

[6]We excluded samples with incomplete date of antibody testing ($N = 1$).

[7]As its own policy, Tsukuba City resent vaccination vouchers on June 14, 2021, to those who had not taken an antibody test by March 31, 2021, or to the negatives who had not been vaccinated by March 31,

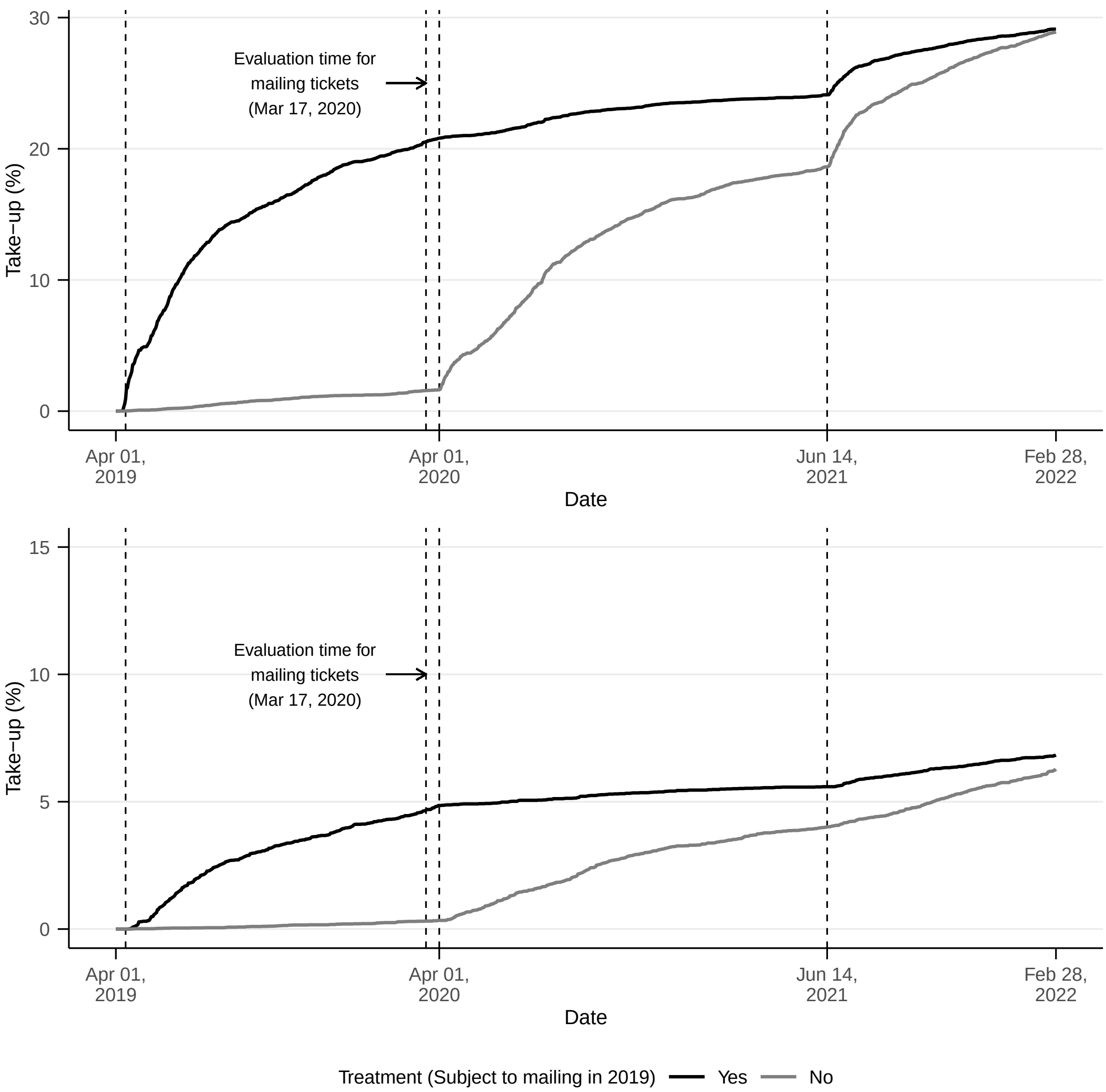

Figure 2: Cumulative Take-Up of Antibody Testing and Vaccination by Treatment Status. *Note:* Data source: Local administrative data.

body testing and vaccination take-up rates were concave for both groups after the default mailing date. In other words, antibody testing and vaccination were more frequent immediately after Tsukuba City mailed vouchers; however, this momentum waned over time.

We evaluated the effect of mailing vaccination vouchers on the outcome variable as of March 17, 2020 (analysis period: April 1, 2019–March 17, 2020). This analysis period was defined for two reasons: (i) men outside the 2019 mailing target had not yet received vouchers during this period, and (ii) it ensures consistency with the online survey pre-

2021. In our discussion paper, we tested the effectiveness of this policy. See Kato, Sasaki and Ohtake (2022).

sented later. The antibody testing rate for the 2019 mailing target was 20.5%, compared to 1.6% for men without the 2019 mailing target.[8] In addition, the vaccination rate for the 2019 mailing target was 4.6%, compared to 0.3% for men outside the 2019 mailing target. A simple comparison between treatment statuses indicates that mailing vouchers in 2019 increased antibody testing and vaccination take-up by 18.9 and 4.3 percentage points, respectively.

B Empirical Strategy

Our identification strategy relies on RDD because simple mean differences are affected by confounding factors such as age differences between treatment and control groups. The estimated regression model is as follows:

$$Y_i = \tau_0 + \tau D_i + \beta_0(A_i - c) + \beta_1 D_i(A_i - c) + \mathbf{X}_i'\gamma, \tag{1}$$

where Y_i is the outcome variable indicating that individual i with age in days A_i has received antibody testing or been vaccinated as of March 17, 2020, and D_i is a dummy variable indicating that individual i is subject to the 2019 mailing target; that is, it takes 1 if $A_i < 17166$. Our parameter of interest is τ, which identifies the effect of mailing vaccination vouchers at the cut-off age ($c = 17166$).

We employed local linear regression with a triangular kernel, following Calonico, Cattaneo and Titiunik (2014). The bandwidth h was chosen to minimize the mean squared error of the treatment effect. We estimated Equation (1) using weighted least squares with kernel weights. Following Calonico, Cattaneo and Titiunik (2014), we also estimated bias-corrected confidence intervals to account for the asymptotic bias of the estimator. For statistical inference, we employed robust standard errors rather than clustered standard errors, as suggested by Lee and Card (2008), because Kolesár and Rothe (2018) found that clustered standard errors lead to lower coverage probabilities than heteroskedasticity-robust standard errors.

[8] The take-up of antibody testing for the 2019 mailing target was similar to the aggregated data provided by the MHLW. According to the MHLW, the number of antibody tests using vouchers up to January 2020 was 1.17 million. Dividing this value by the population of men subject to the 2019 mailing target (6.46 million) yields the take-up of antibody tests as 18%.

When using birthdays as a running variable, there might be significant seasonality or heaping in the distribution of birthdays, potentially leading to a bias in the estimated effects (Barreca, Lindo and Waddell, 2016). However, as illustrated in Figure B2 in Supplemental Appendix B, we found no clear evidence of seasonality or heaping in our local government data, suggesting that the bias from nonrandom heaping in the distribution is minimal.

To increase efficiency, we included the duration of residence and the number of medical institutions in the (zipcode-level) residential area where vaccination was carried out as covariates $\mathbf{X}_i$.[9] If the covariate is continuous at the threshold, then the parameter τ in equation (1) identifies the local average effect at the cutoff (Calonico et al., 2019). We estimated the RD effect on the covariate in Panel A of Table B1 in Supplemental Appendix B, indicating that the covariate was discontinuous at the threshold.

However, this discontinuity appears to be due to noise in the data near the threshold rather than systematic factors. Indeed, we also estimated the local average effect using the donut-hole RD (Barreca, Lindo and Waddell, 2016), which excludes samples from one month before and after the threshold. The results, presented in Panel B of Table B1 in Supplemental Appendix B, suggest the continuity of the covariate. Thus, as a robustness check, we also estimated the local average effect on outcomes using the donut-hole RD to verify the robustness of our main results.

C Effects on Antibody Testing and Vaccination Take-Up

Main Results. Figure 3 depicts the age profiles of antibody test take-up (left panel). We found a clear discontinuity in the antibody test take-up rates at the threshold. Table 1 shows that mailing vaccination vouchers increased the antibody test take-up rates by 19.3 percentage points, which was statistically significant ($p < 0.001$).

The effects on vaccination rates can be predicted from the effects on antibody test take-up rates. According to a survey by the NIID, the unconditional negative ratio (the

[9] The MHLW listed medical institutions where vaccination was carried out. This list included the names of medical institutions, zip codes, complete addresses, and phone numbers. We utilized this data (`https://www.mhlw.go.jp/stf/seisakunitsuite/bunya/kenkou_iryou/kenkou/kekkaku-kansenshou/rubella/index_00001.html` Accessed: October 13, 2022).

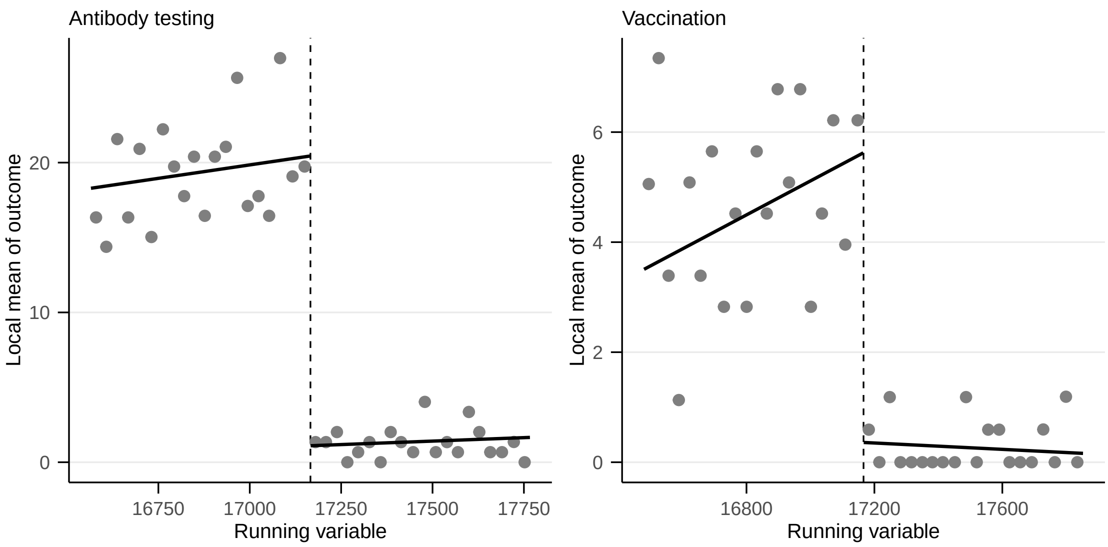

Figure 3: RD Effect of Mailing Vaccination Vouchers on Antibody Testing and Vaccination.
Note: The running variable is age in days. The dashed vertical line represents the threshold (17,166 days). The scatter plot shows the local sample mean. The regression line represents the prediction of WLS estimation in Equation (1) using the effective sample (samples within the bandwidth). Data source: Local administrative data.

proportion of individuals without antibodies, that is, those requiring vaccination) is approximately 22% around the cutoff (ages 46 and 47 in the NIID data shown in Figure 1). Here, two assumptions are introduced. First, it is assumed that mailing vaccination vouchers does not affect antibody test screening (this assumption will be examined in detail in the next subsection). Second, it is assumed that the negatives always get vaccinated. Under these assumptions, the effect on vaccination rates is predicted to be 22% of the effect on antibody test take-up rates, or $4.246 (= 19.3 \times 0.22)$ percentage points.

To validate this prediction, we visualize the effect on vaccination take-up in the right panel of Figure 3. Similar to the antibody test take-up, we found a clear discontinuity in vaccination take-up at the threshold. Table 1 shows that mailing vaccination vouchers increased vaccination take-up by 5.3 percentage points, which was statistically distinguishable from zero ($p < 0.001$). This point estimate was higher than the predicted value of 4.246, but the difference was not statistically significant ($z = 1.171$, $p = 0.242$).

Next, we examine the cumulative effects of vaccination voucher mailing over time. Figure 4 illustrates the RD effects on cumulative antibody test take-up rates and cumulative vaccination rates on various days. The effect on the antibody testing rate exhibited a

Table 1: Estimation Results of RD Effect on Antibody Testing and Vaccination

	Estimate	Bias-corrected CI	Bandwidth	N
A. Full Sample				
Antibody testing	19.3	[16.5, 23.1]	600.5	27745
	(1.7)			
Vaccination	5.3	[3.7, 7.2]	686.3	27745
	(0.9)			
B. Donut-hole				
Antibody testing	20.4	[16.9, 25.4]	496.1	27446
	(2.2)			
Vaccination	4.9	[3.3, 6.9]	654.6	27446
	(0.9)			

Note: Robust standard errors are in parentheses. The unit of RD effect is a percentage point. Bias-corrected CI represents the bias-corrected 95 percent confidence interval proposed by Calonico, Cattaneo and Titiunik (2014). Panel A uses the full sample. Panel B uses the donut-hole sample excluding 30 days before and after the threshold (17,166 age in days). Data source: Local administrative data.

concave relationship over time. By the first week of August (four months after mailing), the effect on the antibody testing rate reached 15 percentage points, accounting for 78% of the overall effect. Thus, the mailing of vaccination vouchers significantly boosted antibody testing shortly after mailing, but its effect diminished over time. In comparison, the effect on vaccination rates showed a relatively constant increase over time. By the first week of August, the effect on vaccination rates reached 2.8 percentage points, accounting for 53% of the overall effect.

Robustness. Panel B of Table 1 presents the results of a donut-hole RD, excluding samples from one month before and after the threshold, to mitigate discontinuity in covariates caused by data noise. Additionally, Figure B3 in Supplemental Appendix B examines the sensitivity of the estimated values to the bandwidth. As a result, the estimated RD effects were stable and statistically significant across different bandwidths and donut-hole samples, confirming that our main findings are robust and not driven by data noise or bandwidth selection.

Another concern is the relative age effect. Several studies have pointed out that the age at which school begins could influence health behaviors (Arnold and Depew, 2018; Johansen, 2021). In Japan, children who turn six years old by April 2, the school entry date, enter elementary school. Therefore, because the age at which school begins changes

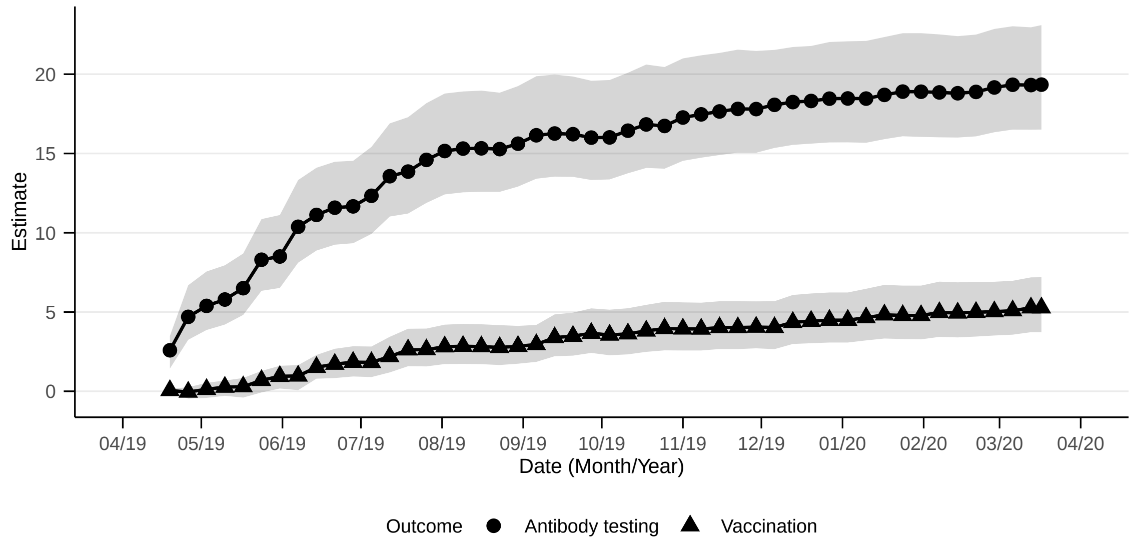

Figure 4: Cumulative Effect of Mailing Vaccination Vouchers on Antibody Testing and Vaccination.
Note: The gray area shows bias-corrected 95 percent confidence interval proposed by (Calonico, Cattaneo and Titiunik, 2014). Data source: Local administrative data.

significantly at the threshold of our RD design (Matsubayashi and Ueda, 2015), our main results may be confounded by the relative age effect on antibody testing and vaccination. If this confounding is substantial, antibody test take-up and vaccination should also show discontinuity on April 1 in other years. Figure B4 in Supplemental Appendix B illustrates the estimation results, suggesting that the relative age effect has minimal impact on our main findings.

D Targeting Efficiency and Its Dynamics

We now examine the targeting efficiency of mailing vaccination vouchers. Here, targeting efficiency refers to the share of antibody-negative individuals among those induced to test. For infectious diseases where herd immunity has been established to some extent in the population, effective vaccination policies are required. In particular, interventions that attract individuals without immunity are important not only from a cost-effectiveness perspective but for achieving the program's public health objective. If those induced to test mostly already possess antibodies, the mailing only adds testing costs without improving the antibody prevalence rate. Conversely, if they mostly lack antibodies, the antibody prevalence rate improves at low cost. Whether this share is high or low must be judged against the antibody-negative rate in the eligible population.

We examine this issue using a fuzzy RD framework. In the first stage, we estimate the effect τ_T on antibody test take-up T_i using a sharp RD design, where T_i is a dummy variable indicating antibody test take-up. In the second stage, we estimate the effect τ_{NT} on the product of antibody test take-up and negative results $N_i \times T_i$ using a sharp RD design, where N_i is a dummy variable indicating that the test result is negative (vaccination is needed), and $N_i \times T_i$ is a dummy variable indicating that an antibody test was taken and a negative result was received. Finally, we estimate the ratio τ_{NT}/τ_T which identifies the negative rate among those who responded to mailing vouchers. Thus, within the fuzzy RD framework, we can identify the type of individuals who responded to mailing vouchers in terms of antibody possession.

The parameter τ_T represents the proportion of individuals who responded to mailing vouchers. Let $T_i(d)$ be a dummy variable indicating antibody test take-up under treatment status $D_i = d$. Those who responded to mailing vouchers are either compliers who undergo antibody testing in response to the mailing ($T_i(1) > T_i(0)$) or defiers who refuse antibody testing in response to the mailing ($T_i(1) < T_i(0)$). The parameter τ_T represents the difference in proportions between compliers and defiers:[10]

$$\begin{aligned} \tau_T &= E(T_i(1) - T_i(0)|A_i = c) \\ &= \Pr(T_i(1) > T_i(0)|A_i = c) - \Pr(T_i(1) < T_i(0)|A_i = c) \end{aligned} \tag{2}$$

In the following analysis, we assume that there are no defiers around the cutoff. Under this assumption, the parameter τ_T represents the proportion of compliers who respond positively to mailing vouchers: $\tau_T = \Pr(T_i(1) > T_i(0)|A_i = c)$.

The parameter τ_{NT} represents the local RD effect on the joint outcome of negative N_i and antibody testing T_i. Mailing vouchers does not affect antibody possession N_i. Thus, the parameter τ_{NT} identifies the following parameter:

$$\tau_{NT} = E(N_i(T_i(1) - T_i(0))|A_i = c) \tag{3}$$

[10]In the case of always-takers ($T_i(1) = T_i(0) = 1$) or never-takers ($T_i(1) = T_i(0) = 0$), $T_i(1) - T_i(0) = 0$, so $E(N_i(T_i(1) - T_i(0))|T_i(1) - T_i(0) = 0, A_i = c) = 0$.

As discussed above, those who responded to mailing vouchers can be divided into two types: compliers and defiers. The parameter τ_{NT} is then the difference between the negative rates of compliers and defiers, each weighted by the corresponding type share.

$$\begin{aligned}\tau_{NT} =& E(N_i|T_i(1) > T_i(0), A_i = c)\Pr(T_i(1) > T_i(0)|A_i = c) \\ &- E(N_i|T_i(1) < T_i(0), A_i = c)\Pr(T_i(1) < T_i(0)|A_i = c)\end{aligned} \tag{4}$$

Under the assumption of $\Pr(T_i(1) < T_i(0)|A_i = c) = 0$, the parameter τ_{NT} represents the product of the proportion of compliers who respond positively to the mailing and their negative rate: $\tau_{NT} = E(N_i|T_i(1) > T_i(0), A_i = c)\Pr(T_i(1) > T_i(0)|A_i = c)$. Finally, the parameter τ_{NT}/τ_T identifies the negative rate among compliers:

$$\frac{\tau_{NT}}{\tau_T} = E(N_i|T_i(1) > T_i(0), A_i = c) \tag{5}$$

Using the estimate of this ratio, we test the null hypothesis that the negative result (N_i) is independent of potential outcomes of antibody testing $(T_i(0), T_i(1))$, conditional on age A_i. Under this null hypothesis, $\tau_{NT}/\tau_T = E(N_i|A_i = c)$, where $E(N_i|A_i = c)$ is the antibody non-possession rate at the cutoff age. According to the NIID data shown in Figure 1, the antibody possession rate around the cutoff (ages 46 and 47 in that NIID data) is 78%.[11] We can approximate $E(N_i|A_i = c) \approx 0.22$.[12] Thus, the null hypothesis is $\tau_{NT}/\tau_T = 0.22$. If the null hypothesis is rejected, it suggests that individuals decide to undergo antibody testing based on their negative status (or their belief thereof). Specifically, if $\tau_{NT}/\tau_T < 0.22$, unfavorable selection occurs because mailing vouchers attracts individuals who already possess antibodies. If $\tau_{NT}/\tau_T > 0.22$, negative individuals are more likely to undergo antibody testing by mailing vouchers, implying that favorable selection occurs.

Table 2 shows the results of the fuzzy RD estimation. The first-stage estimate of τ_T is approximately 19%. As with Table 1, this estimate can be interpreted as the RD effect

[11] Since τ_{NT}/τ_T identifies the negative rate among compliers at the cutoff age, we use the antibody possession rate around the cutoff (ages 46 and 47 in the NIID data) as the population benchmark. This value is slightly lower than the 81% antibody possession rate among eligible men (ages 39–56 in the NIID data).

[12] As previously mentioned, in addition to this null hypothesis, imposing the assumption that negatives always receive vaccination, the effect on vaccination becomes 22% of the effect on antibody testing.

Table 2: Targeting Efficiency of Mailing Vaccination Vouchers

	Full sample	Donut-hole sample
	(1)	(2)
A. RD estimates		
First-stage estimates (Testing)	19.3	18.9
	(1.8)	(1.4)
Wald estimates	28.7	28.7
	(5.0)	(3.7)
Bandwidth	511.5	980.7
N	27745	27446
B. Statistical test for targeting efficiency		
Wald estimates under H0	22	22
Bias-corrected CI of Wald estimates	[18.8, 38.5]	[21.5, 35.9]
Z-test for Wald estimates	$p = 0.185$	$p = 0.070$

Note: Robust standard errors are in parentheses. The unit of RD effect is a percentage point. The null hypothesis of the sorting test is that, regardless of the treatment, there is no sorting of antibody testing based on antibody possession. In the fuzzy RD, the null hypothesis posits that the wald estimate is equal to 22%. The bias-corrected CI represents the bias-corrected 95 percent confidence intervals (Calonico, Cattaneo and Titiunik, 2014) for the Wald estimates. The donut-hole sample excludes 30 days before and after the threshold from full sample. Unlike Table 1 (sharp RD), this table uses the fuzzy RD framework which could differ bandwidths and first-stage estimates. Data source: Local administrative data.

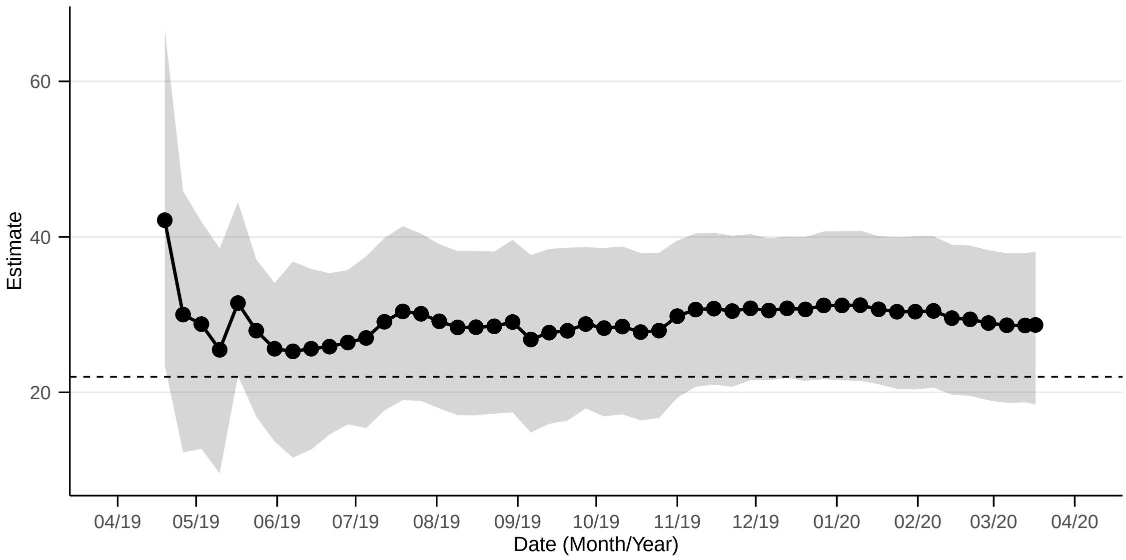

Figure 5: Cumulative Negative Rates of Compliers.
Note: The gray area shows bias-corrected 95 percent confidence interval (Calonico, Cattaneo and Titiunik, 2014). The horizontal line shows estimates under the null hypothesis (negative rate of 22 percent). The dashed line shows the negative rate of individuals who undergo antibody testing in the control group. Data source: Local administrative data.

on antibody test take-up. Although the bandwidth differs, this result is quantitatively similar to Table 1. The Wald estimate of τ_{NT}/τ_T shows that the negative rate among compliers who undergo antibody testing in response to mailing vouchers is approximately 29%. This estimate is statistically distinguishable from zero ($p < 0.001$) and higher than the null hypothesis of 22%. Compliers are thus disproportionately likely to receive negative test results relative to random selection with respect to antibody possession at the cutoff. The bias-corrected 95% confidence interval includes the null hypothesis of 22%. Furthermore, the Z-test cannot reject the null hypothesis. Overall, these results indicate that mailing vouchers raises take-up while maintaining targeting efficiency at the population benchmark.

We next examine how targeting efficiency evolved over time. Whereas Table 2 uses the full period, Figure 5 shows the fuzzy RD estimates based on cumulative antibody test uptake and test results up to each evaluation date. Each point therefore shows the implied negative rate among compliers induced to test by mailing vouchers up to that date. Immediately after mailing (the first point), the negative rate among compliers is high at 40%, and we can reject the null hypothesis of 22% at the 5% level (95% confidence interval:

[23.5%, 66.8%]). This suggests that those who needed vaccination were proactive in participating in the vaccination program through mailing vouchers. Over time, the estimated negative rate among compliers declines as additional test takers are more likely to possess antibodies. In later periods, we can no longer reject the null hypothesis of 22%. Taken together, favorable selection is concentrated in the early post-mailing period, while in every period targeting efficiency remains at least as high as the population benchmark.

III External Validity and the Awareness Mechanism

In this section, we present the results of a nationwide online survey to complement the results from local administrative data. We examine the geographic external validity and the mechanism of the effect of mailing vaccination vouchers.

A Survey Data and Empirical Strategy

We commissioned the internet research firm MyVoice to conduct the online survey. The data were collected from two surveys, including a follow-up survey. The first survey, conducted from February 15 to 17, 2020, included 4,200 men aged 39–59 years. In addition to personal demographics, the first survey asked about awareness of the rubella immunization program. The follow-up survey was conducted from March 17 to 25, 2020, among the respondents of the first survey. We received responses from 3,963 individuals (attrition rate = 5.6%). The follow-up survey asked whether participants had undergone antibody testing or vaccination. We included men aged 40–56 years who participated in both surveys ($N = 3,444$).

Our identification strategy is essentially the same as that used in the local administrative data analysis. The online survey contained only the birth year and birth month. Therefore, the running variable was age in months as of April 2019.[13] Following the guidelines set by the MHLW, the treatment variable indicated that men aged 480–563 months (younger than 47 years as of April 2019) received vaccination vouchers by default ($N = 1,418$, 41% of the analysis sample).[14] Due to the small number of observations in

[13]We assume that no one born in April had a birthday by April 2019.

[14]According to interviews conducted by the MHLW, 96% of local governments sent vaccination vouchers

the online survey data, local linear regression lacks estimation precision. Therefore, as our main specification, we fitted Equation (1) globally.

We examined the validity of the RDD using online surveys. First, Figure B5 in Supplemental Appendix B illustrates the distribution of the running variables. We found no clear evidence of seasonality or nonrandom heaping. Additionally, the age distribution in the online survey resembled that of the 2015 census, suggesting that the online survey provides a nationally representative sample. Second, Table B2 in Supplemental Appendix B estimates the discontinuity of the covariates at the threshold. We confirmed that all covariates were continuous at the threshold. Taken together, the RDD using online surveys appears to be valid.

B Geographic External Validity

To verify whether the results from the local government data apply nationally, we estimate the RD effects of mailing vaccination vouchers on antibody test take-up and vaccination. The outcome variables are dummy variables indicating whether the respondents had undergone antibody testing (or vaccination).

Main Results. Figure 6 displays the age profiles of antibody test take-up and vaccination. We found clear discontinuities in both antibody test take-up and vaccination at the threshold. Panel A of Table 3 indicates that mailing vaccination vouchers increased antibody test take-up and vaccination rates by 18.2 and 4.2 percentage points, respectively. The effect on vaccination rates was higher than the predicted value based on the effect on antibody testing (4 $(= 18.2 \times 0.22)$ percentage points), but the difference was not statistically significant ($z = 0.878$, $p = 0.380$). These results were quantitatively similar to those from the local government data, suggesting the geographic external validity of the results from the local government data.

Robustness. As alternative specifications, we fitted a global quadratic function of the running variable and a nonparametric function (estimated by local linear regression) in

by October 2019. Thus, few in the treatment group had not yet received vaccination vouchers. Some local governments might have deviated from the guidelines set by the MHLW and set their own rules. For example, a local government sent vaccination vouchers in FY2019 to all eligible individuals, regardless of their dates of births. In this case, the control group includes those who automatically received the vaccination vouchers. While we cannot quantify how many of these local governments exist, we believe that they are few in number.

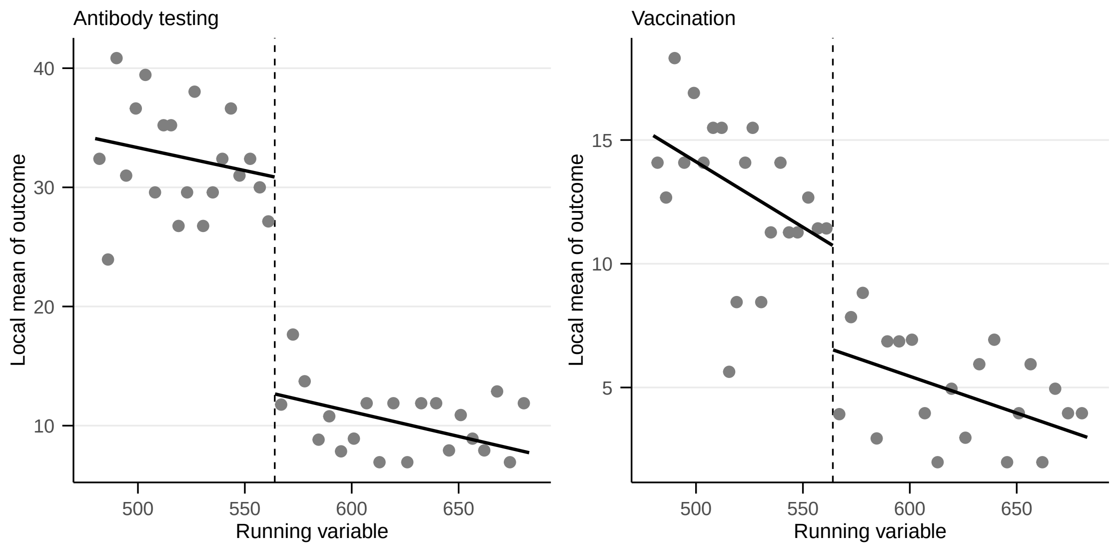

Figure 6: Geographic External Validity of RD Effect of Mailing Vaccination Vouchers on Antibody Testing and Vaccination.
Note: The running variable is age in months. The dashed vertical line represents the threshold (564 months). The scatter plot shows the local sample mean. The regression line represents the prediction of estimation in Equation (1). Data source: Nationwide online survey data.

Panels B and C of Table 3, respectively. We found positive RD effects on antibody test take-up, as in Panel A. However, the estimated effects quantitatively differ from those in Panel A. The RD effects on vaccination rates were quantitatively similar to those in Panel A, though not statistically significant. Thus, our main results are robust to alternative functional forms, while estimated magnitudes are quantitatively unstable across specifications.

Next, we examine the influence of relative age effects. We estimated placebo RD effects using April in other years as alternative thresholds (Figure B6 in Supplemental Appendix B). Even in the absence of relative age effects, placebo RD effects may still appear statistically significant. This is because we fitted a global linear function of the running variable, and the estimation range of the running variable includes the true threshold. As a result, the global linear approximation can be affected by the discontinuity at the true threshold. Therefore, we assess the influence of relative age effects by comparing the magnitudes of placebo RD effects with the RD effect at the true threshold. If relative age effects are absent, placebo RD effects should be smaller than the true RD effect. Our estimates show that the RD effects on antibody testing and vaccination are largest at the

Table 3: Estimation Results of RD Effect on Antibody Testing and Vaccination (Online Survey Data)

	Estimate	SE	Bias-corrected CI	Bandwidth	N
A. Global fitting (First-order)					
Antibody testing	18.2	(2.8)			3444
Vaccination	4.2	(2.0)			3444
B. Global fitting (Second-order)					
Antibody testing	13.5	(4.2)			3444
Vaccination	4.3	(3.0)			3444
C. Local linear regression					
Antibody testing	10.5	(6.0)	[-2.2, 21.3]	22.4	3444
Vaccination	4.6	(4.1)	[-3.8, 12.1]	24.9	3444

Note: Robust standard errors are in parentheses. The unit of RD effect is a percentage point. In Panel A, we fit Equation (1) globally. In Panel B, we globally fit the following model: $Y_i = \tau_0 + \tau D_i + \beta_0(A_i - c) + \beta_1(A_i - c)^2 + \beta_2 D_i(A_i - c) + \beta_3 D_i(A_i - c)^2 + X_i'\gamma$. In Panel C, we employed local linear regression to estimate Equation (1). The bias-corrected CI represents the bias-corrected 95 percent confidence intervals (Calonico, Cattaneo and Titiunik, 2014). Data source: Nationwide online survey data.

true threshold, suggesting that the influence of relative age effects is limited.

Remarks on Interpretation. There are two concerns in interpreting the RD effects. The first concern is potential bias in the outcomes due to recall bias, experimenter demand effects, and social desirability bias. The outcomes are based on self-reports of past behavior. Individuals who received vaccination vouchers may recall their past behavior more carefully than those who did not and may provide responses that align with social norms or perceived experimenter expectations. Due to data limitations, we cannot quantitatively assess the influence of recall bias or experimenter demand effects. However, we can quantitatively assess the potential influence of social desirability bias. We find that variables related to compliance with social norms (e.g., taking on moral behavior, accepting immoral behavior in anonymous conditions) are not discontinuous at the threshold (Table B2 in Supplemental Appendix B). This result suggests that the influence of social desirability bias is limited.

The second concern is the use of other programs. In Japan, in addition to the rubella vaccination program we focus on, there is a public health policy that provides free antibody testing to pregnant couples. The online survey does not collect information on how respondents received antibody testing, so the outcomes may include behaviors outside the

Table 4: Estimation Results of RD Effect on Program Awareness

	Estimate	SE	Bias-corrected CI	Bandwidth	N
A. Global fitting (First-order)					
Program Awareness	37.4	(3.1)			3444
B. Global fitting (Second-order)					
Program Awareness	40.7	(4.7)			3444
C. Local linear regression					
Program Awareness	41.2	(5.6)	[29.8, 51.9]	35.5	3444

Note: Robust standard errors are in parentheses. The unit of RD effect is a percentage point. In Panel A, we fit Equation (1) globally. In Panel B, we globally fit the following model: $Y_i = \tau_0 + \tau D_i + \beta_0(A_i - c) + \beta_1(A_i - c)^2 + \beta_2 D_i(A_i - c) + \beta_3 D_i(A_i - c)^2 + X_i'\gamma$. In Panel C, we employed local linear regression to estimate Equation (1). The bias-corrected CI represents the bias-corrected 95 percent confidence intervals (Calonico, Cattaneo and Titiunik, 2014). Data source: Nationwide online survey data.

rubella vaccination program of interest. Because the marriage rate is related to eligibility for the program targeting pregnant couples, a discontinuity in the marriage rate at the threshold would suggest that RD effects are contaminated by the use of other programs. However, we find that the marriage rate is not discontinuous at the threshold (Table B2 in Supplemental Appendix B). Therefore, at least the influence of other programs through changes in marital composition is limited.

C The Awareness Mechanism

Mailing vaccination vouchers increased antibody testing and vaccination take-up for the vaccination program. However, whether mailing vaccination vouchers removed barriers to information acquisition is an empirical question because individuals targeted for mailing may not be aware of the program if they do not check their mail. This subsection aims to estimate the effect of mailing vaccination vouchers on program awareness and thereby examine whether it removed barriers to information acquisition.

Using the responses to the first survey, we created a dummy variable indicating that respondents knew that the MHLW had implemented the vaccination program since April 2019. By estimating our RD model, in which this dummy variable was the outcome, we examined the degree to which mailing vaccination vouchers increased awareness of the vaccination program.

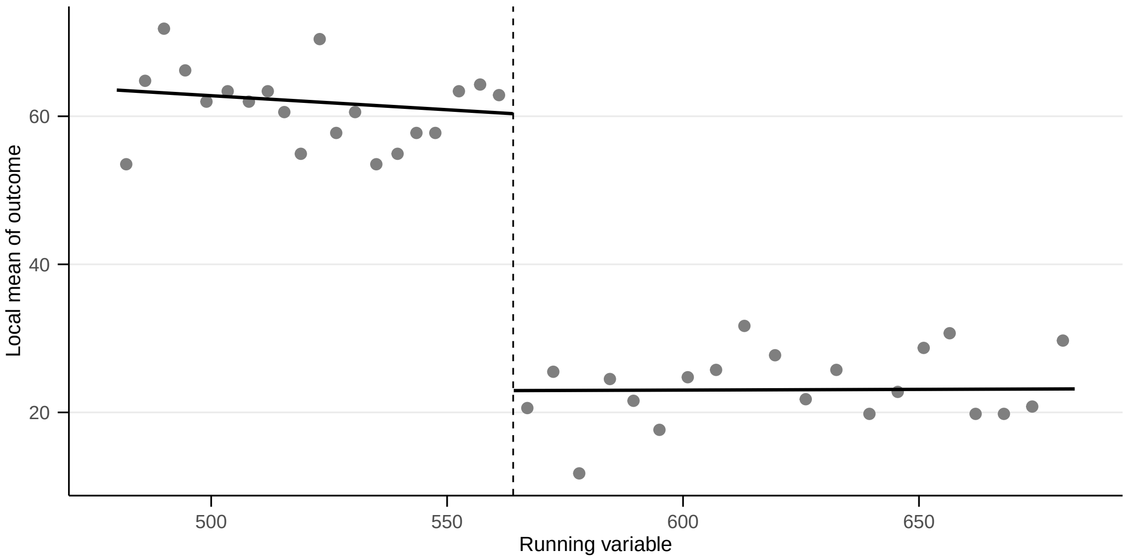

Figure 7: RD Effect of Mailing Vaccination Vouchers on Program Awareness.
Note: The running variable is age in months. The dashed vertical line represents the threshold (564 months). The scatter plot shows the local sample mean. The regression line represents the prediction of estimation in Equation (1). Data source: Nationwide online survey data.

Main Results. Figure 7 illustrates the age profile of program awareness, showing a clear discontinuity at the threshold. Panel A of Table 4 indicates that mailing vaccination vouchers increased the proportion of individuals aware of the program by 37.4 percentage points, which was statistically significant ($p < 0.001$). Even considering that the awareness rate among those not targeted by the mailing in 2019 was 23.5%, this result indicates that mailing vaccination vouchers effectively addressed the information barrier.

Using a fuzzy RD design, we examined the effects of improved program awareness on antibody testing and vaccination uptake. In this analysis, the treatment variable was program awareness rather than the mailing of vaccination vouchers. Therefore, we quantitatively investigated how program awareness affects antibody testing and vaccination uptake. However, as shown in Figure 7, program awareness did not change sharply (from 0% to 100%) at the age cutoff for the mailing of the vaccination vouchers. To address this, we employed the fuzzy RD approach. Specifically, we calculated the Wald estimate, which is the ratio of the effect on the outcome variables (shown in Table 3) to the effect on program awareness (shown in Table 4). The resulting estimates can be interpreted as the effect of awareness on antibody testing and vaccination for those who first learned about the vaccination program due to mailing (at the cutoff).

Table B3 in Supplemental Appendix B presents estimation results. For those who learned about the program due to mailing, program awareness increased the antibody testing take-up and vaccination rates by 48.7 and 11.3 percentage points, respectively. These increases were statistically significant. These results indicate that removing the information barrier increased program participation.

Robustness. First, we confirmed that alternative specifications (a global quadratic specification and a nonparametric specification) yielded results similar to those reported in Panel A of Table 4 (See Panels B and C of Table 4). Second, given that relative age could influence health behavior (Johansen, 2021), it is natural to consider that relative age also affects program awareness. Therefore, we verified that the RD effect of mailing vaccination vouchers on program awareness is not influenced by relative age effects. Figure B7 in Supplemental Appendix B presents the discontinuity in program awareness at the placebo cutoff in April for other years. The results showed that program awareness was discontinuous even at the placebo cutoff. However, because the effect size was the greatest at the true threshold (564 months), the effect on program awareness should be interpreted as an effect of mailing vaccination vouchers rather than as a relative age effect.

IV Interpretation and Policy Implications

Our analysis shows that voucher mailing increased program participation while maintaining targeting efficiency at the 22 percent population benchmark. More precisely, immediately after mailing, the intervention disproportionately drew in individuals who actually needed vaccination and thereby raised targeting efficiency; over time, however, this advantage attenuated (Figure 5). In this section, we offer an interpretation of these dynamics and draw out the policy implications. One plausible account is that voucher mailing raised efficiency primarily by alleviating information frictions and, possibly, by prompting individuals to form or update beliefs about their own antibody status, with stronger or more salient beliefs translating into earlier take-up. The mailing also reduced application costs, but the rest of this section focuses on the informational channel.

A defining feature of our setting is that baseline awareness of the rubella immunization program was strikingly low: only 23.5% of men outside the FY2019 mailing target reported being aware of the program. This contrasts sharply with means-tested programs studied previously, most notably SNAP, in which roughly 82% of eligible individuals participate (Schanzenbach, 2023) and awareness is correspondingly high (Finkelstein and Notowidigdo, 2019). In such a low-awareness environment, individuals may have little reason to consider whether they need vaccination in the first place; put differently, limited awareness may leave many without occasion to reflect on their own immune status. From this perspective, voucher mailing may have mattered by supplying that occasion.

On this interpretation, the mailing can be viewed as a low-cost informational shock that prompted individuals to revisit their past infection or vaccination history and to form, or update, beliefs about whether they currently possess rubella antibodies. Because adult rubella infection is rare and childhood vaccination records are imperfectly remembered, these beliefs would naturally differ across individuals: those without clear memories of past infection or vaccination may infer a relatively high probability of being antibody-negative, whereas those with vivid recollections may infer a lower probability. We do not directly observe these subjective beliefs, so this mechanism should be read as an interpretive account rather than a directly tested one. Still, it is consistent with the dynamic patterns documented below.

This interpretation can be organized in a simple framework: whether and when an individual takes up testing depends jointly on the level and the confidence of these subjective beliefs. Individuals who hold a high and confident belief of being antibody-negative would perceive larger expected benefits from vaccination and less reason to delay. Individuals with lower or less confident beliefs would perceive smaller expected benefits and thus be less inclined to act immediately. This account is consistent with the temporal pattern in Figure 5. Immediately after mailing, the negative rate among compliers is high, suggesting that early respondents disproportionately lack antibodies. Over time, this rate gradually declines and the efficiency gain from mailing fades.

Two policy implications follow. First, in low-awareness environments, information provision is a precondition for effective self-selection. Alatas et al. (2016) and the broader self-selection literature (Heckman and Vytlacil, 2005; Heckman, 2010) show that delegating the participation decision to individuals can improve targeting when private information about returns is meaningful. Our results sharpen this lesson: when baseline knowledge of the program is scarce, individuals cannot translate their private information into appropriate participation decisions, and a low-cost informational intervention such as voucher mailing can unlock self-selection's targeting potential. This efficiency gain, however, is concentrated in the early period after the intervention.

Second, our results speak to the need for and timing of alternative targeting interventions beyond those that rely solely on self-selection. Because lower-return individuals progressively enter the program over time, an informational intervention that relies on voluntary take-up is unlikely to sustain targeting efficiency in the long run. The mixed evidence in the previous literature, in which reducing frictions draws in lower-return individuals and sometimes higher-return ones (Alatas et al., 2016; Finkelstein and Notowidigdo, 2019; Deshpande and Li, 2019; Domurat, Menashe and Yin, 2021; Castell et al., 2025), is consistent with this view: outcomes may depend on where in this temporal cycle a given program is evaluated. Our findings suggest that the efficiency-enhancing window for such outreach may be narrower than is commonly assumed, a possibility that aligns with the SNAP evidence reported by Finkelstein and Notowidigdo (2019). Once that window closes, sustaining efficiency requires alternative tools such as government-directed targeting that leverages administrative records on early participants to predict which remaining individuals are most likely to benefit (Kitagawa and Tetenov, 2018; Athey and Wager, 2021) and automatically enrolls those predicted to benefit most.

Taken together, these implications point to a potential role for a sequenced policy design: in the early phase of a program, governments may benefit from pairing self-selection with low-cost outreach that lowers information frictions; in the later phase, they may increasingly rely on government-directed targeting that can build on information revealed by early participants rather than relying solely on the continued voluntary take-up of self-aware high-return individuals. Ida et al. (2026) show that optimally combining targeting by observables with self-selection outperforms either approach alone. Our results complement this finding by suggesting that the optimal mix may itself evolve over the life of a program: self-selection is more valuable in the early phase when high-return individuals act first, while observable-based targeting becomes increasingly important as this favorable selection attenuates.

Two scope conditions deserve emphasis. First, our mechanism rests on a low-awareness environment in which informational shocks have meaningful bite; in settings with already high awareness, the marginal value of further outreach is likely smaller. Second, our argument presumes that policymakers can, over time, build the data infrastructure required for credible mandatory targeting. Determining when to switch from self-selection-based outreach to government-directed targeting is therefore a central, and still largely open, question for the design of social programs.

V Conclusion

This study shows that mailing vouchers, along with program notification letters, improved program awareness and promoted antibody testing and vaccination uptake while maintaining targeting efficiency at the population benchmark. Importantly, however, targeting efficiency changed over time: it was high immediately after mailing but gradually declined as lower-return individuals entered the program. These findings suggest a potential role for sequenced policy design, in which governments initially leverage self-selection through low-cost informational outreach and increasingly complement it with government-directed targeting as favorable selection attenuates. Our results thus highlight the need for future research on when governments should transition from informational outreach to government-directed targeting.

One limitation of this study is that the two channels through which voucher mailing operates could not be fully separated. The mailing reduced both information acquisition costs and application costs, yet we were unable to estimate their effects independently; doing so would require a treatment arm providing information alone, without distributing vouchers. Even with this limitation, our findings suggest that the informational channel is the more important driver of the take-up and targeting patterns we document, given the strikingly low baseline awareness among those not targeted for mailing.

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Supplemental Appendix: "Mailing Vouchers, Take-Up, and Targeting Efficiency: Evidence from Rubella Immunization Programs"

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Contents

Appendix

A Complete Data of Antibody Testing and Vaccination (Data of Ministry of Health, Labour and Welfare)

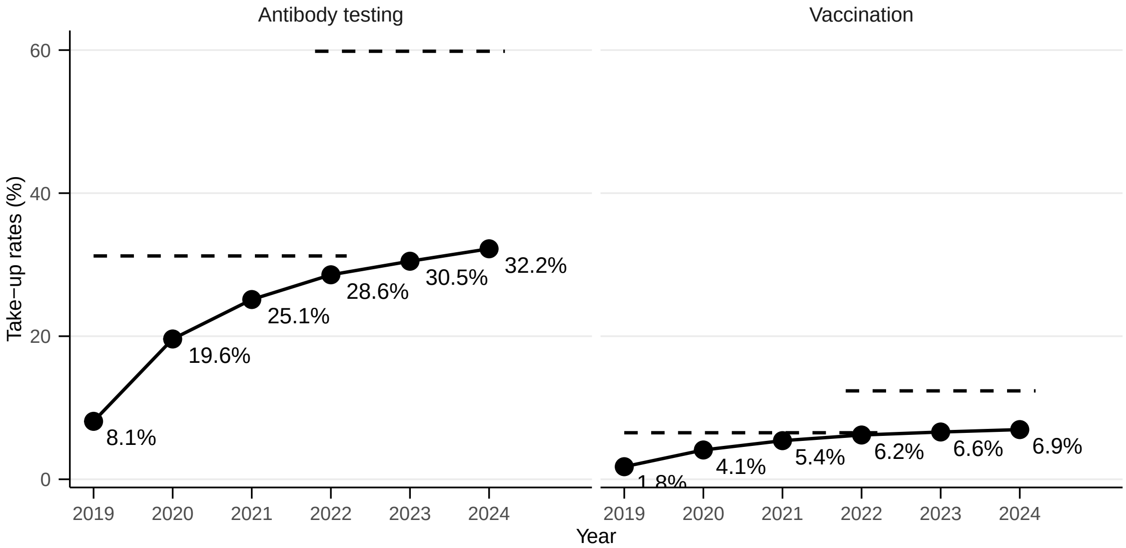

Figure A1: Cumulative Take-up Rates of Antibody Testing and Vaccination.
Note: Horizontal dashed lines indicate goals set by the Ministry of Health, Labour and Welfare. Data source: Complete data from Ministry of Health, Labour and Welfare.

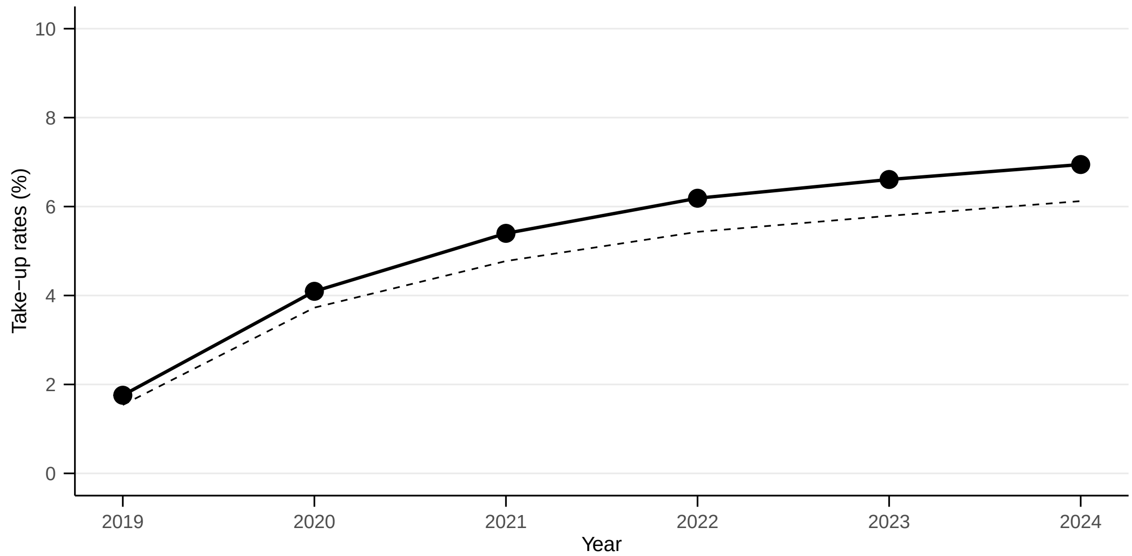

Figure A2: Realized and Predicted Vaccination Rates.
Note: The solid line shows the realized vaccination rate. The dashed line shows the predicted vaccination rate if antibody testing take-up were independent of antibody status and those whose test results were negative are vaccinated. Under these assumptions, the predicted vaccination rate can be calculated as 19% of take-up rate of antibody testing where 19% is the average antibody-negative rate among eligible men (ages 40–56 as of April 1, 2019). Data source: Complete data from Ministry of Health, Labour and Welfare.

B Figures and Tables

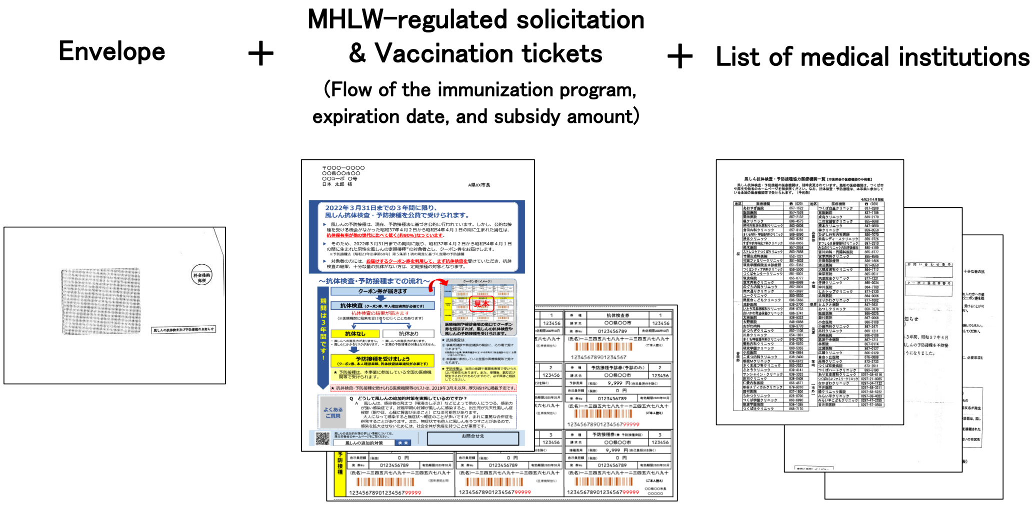

Figure B1: Mailing Contents of Tsukuba City

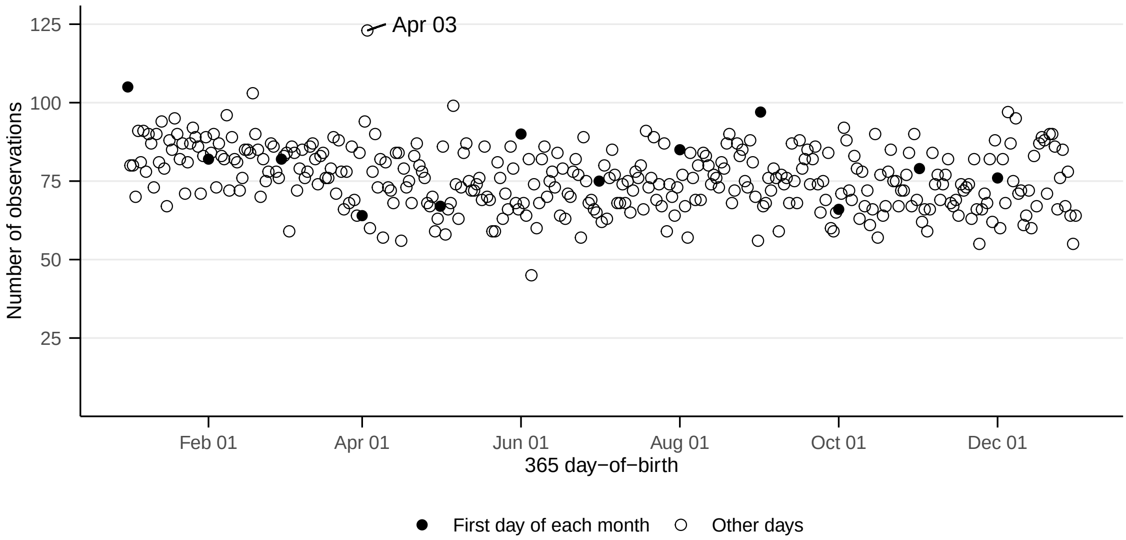

Figure B2: Distribution of 365 Days-of-Birth in A Year
Note: Data source: Local administrative data.

Table B1: Continuity Test of Covariates (Administrative Data)

	Estimate	Bias-corrected CI	Bandwidth	N
A. Full Sample				
Duration of residence	-474.7	[-1058.0, -23.6]	779.13	27745
	(263.9)			
Number of listed hospitals in same zip	0.2	[0.1, 0.3]	752.82	27745
	(0.1)			
B. Donut-hole				
Duration of residence	-385.2	[-985.2, 61.2]	870.09	27446
	(266.9)			
Number of listed hospitals in same zip	0.1	[-0.0, 0.3]	730.79	27446
	(0.1)			

Note: Robust standard errors are in parentheses. Bias-corrected CI represents the bias-corrected 95 percent confidence interval proposed by Calonico, Cattaneo and Titiunik (2014). Panel A uses the full sample. Panel B uses the donut-hole sample excluding 30 days before and after the threshold (17,166 days). Data source: Local administrative data.

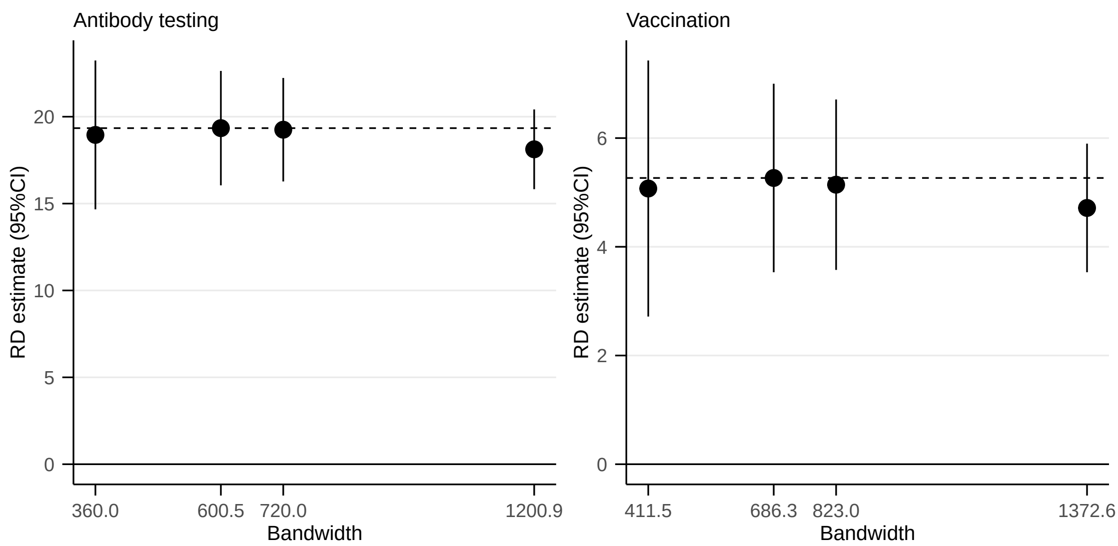

Figure B3: RD Effect of Mailing Vouchers by Bandwidth (Administrative Data).
Note: We use the optimal bandwidth for the mean squared error, convergence error rate, and their doubling. The vertical lines represent conventional 95 percent confidence intervals. The dashed horizontal lines indicate the estimates in Panel A of Table 1 in the main manuscript. Data source: Local administrative data.

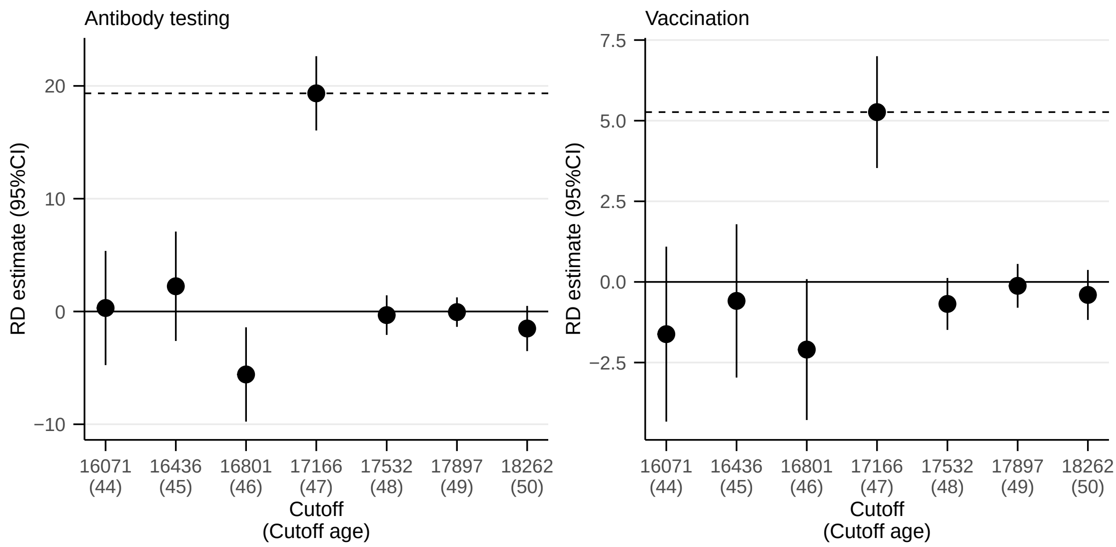

Figure B4: Continuity Test of Antibody Testing and Vaccination at Placebo Cutoffs.
Note: True cutoff is 17,166 days (47 years). The vertical lines represent conventional 95 percent confidence intervals. The dashed horizontal lines indicate the estimates in Panel A of Table 1 in the main manuscript. Data source: Local administrative data.

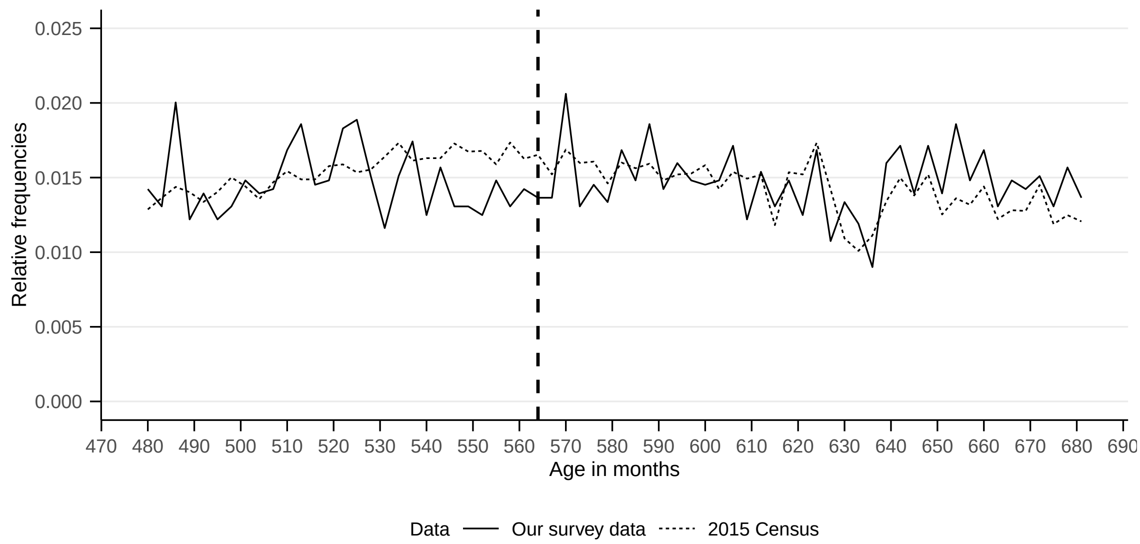

Figure B5: Age Distributions of Online Survey Data and 2015 Census.
Note: As the census provides a population aggregated on age in quarterlies, we also collapse data quarterly. The relative frequency is calculated so that the population aged 40–56 years is one. Data source: Nationwide Online Survey Data (solid line) and 2015 Population Census in Japan published by Ministry of Internal Affairs and Communications (dashed line).

Table B2: Continuity Test of Covariates (Online Survey Data)

	Estimate	SE	N
Education year	-0.043	(0.169)	3444
1 = Married	0.009	(0.035)	3444
Annual income	-14.128	(23.764)	3444
1 = No self-report of income	-0.003	(0.026)	3444
1 = Household with child	-0.012	(0.033)	3444
1 = Working	0.003	(0.021)	3444
1 = Working from home	-0.010	(0.017)	3444
1 = Use public transportation	-0.001	(0.034)	3444
1 = Large firm size	-0.025	(0.034)	3444
1 = Habit of Flu shot	0.053	(0.032)	3444
Taking on moral behavior (5 point scale)	-0.018	(0.067)	3444
Accepting immoral behavior in anonymous condition (5 point scale)	0.108	(0.069)	3444

Note: Robust standard errors are in parentheses. We globally fit the following model: $Y_i = \tau_0 + \tau D_i + \beta_0(A_i - c) + \beta_1 D_i(A_i - c) + \mathbf{X}_i'\gamma$. Data source: Nationwide Online Survey Data.

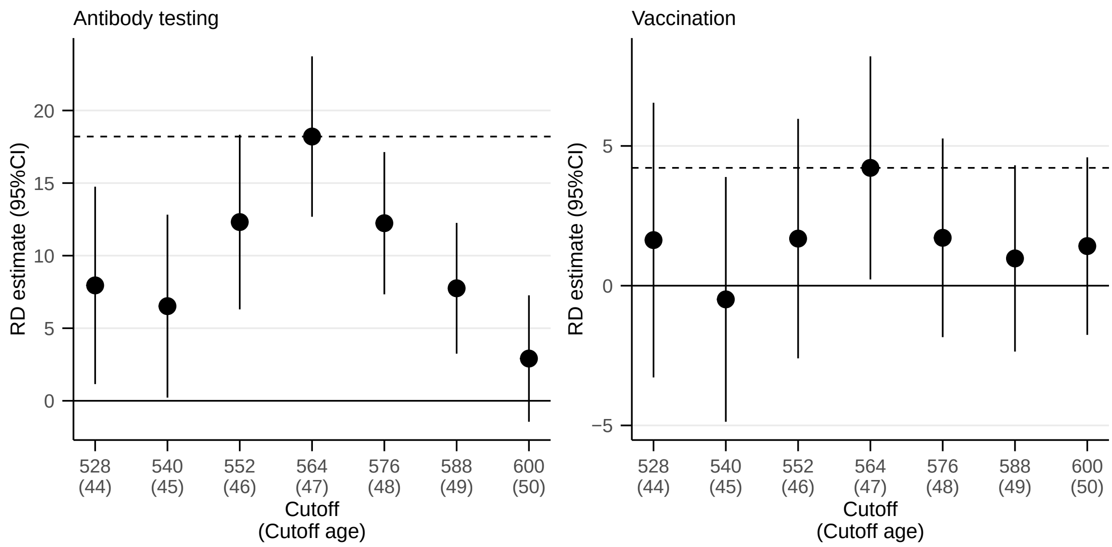

Figure B6: Continuity Test of Antibody Testing and Vaccination at Placebo Cutoffs (Nationwide Online Survey Data).
Note: True cutoff is 564 months (47 years). The vertical lines represent conventional 95 percent confidence intervals. The dashed horizontal lines indicate the estimates in Panel A of Table 3 in the main manuscript. Data source: Nationwide online survey data.

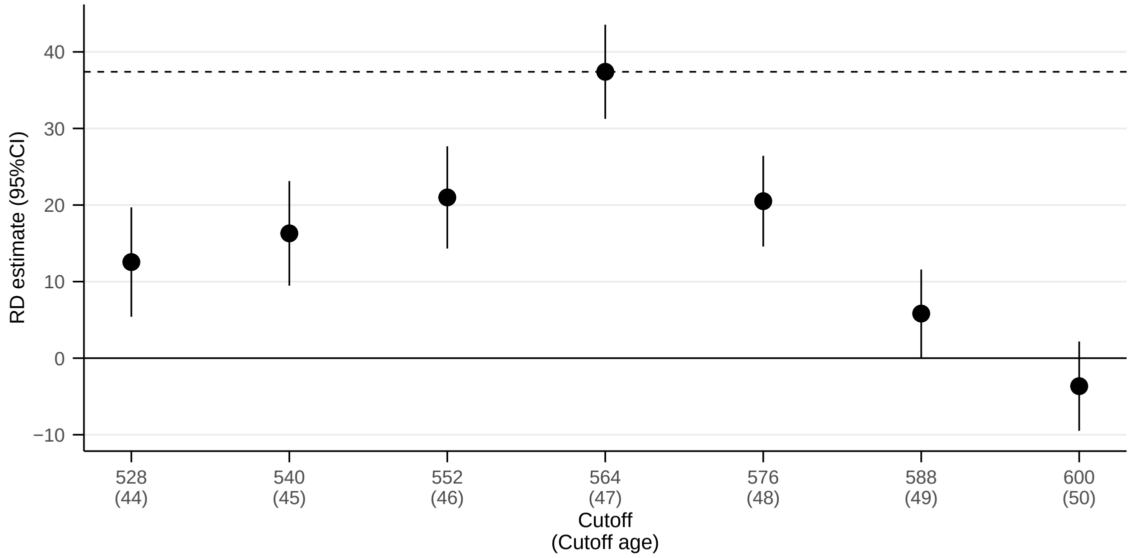

Figure B7: Continuity Test of Program Awareness at Placebo Cutoffs.
Note: True cutoff is 564 months (47 years). The vertical lines represent conventional 95 percent confidence intervals. The dashed horizontal lines indicate the estimates in Panel A of Table 4 in the main manuscript. Data source: Nationwide online survey data.

Table B3: Fuzzy RD Analysis for Antibody Testing and Vaccination (Online Survey Data)

	Estimate	SE	N
Antibody testing	48.7	(7.3)	3444
Vaccination	11.3	(5.4)	3444

Note: Robust standard errors are in parentheses. The unit of the RD effect is percentage points. The estimates reported here represent the ratio of the estimates in Panel A of Table 3 in the main manuscript and Panel A of Table 4 in the main manuscript (Wald estimates). These estimates capture the average effect for those who became aware of the vaccination program due to the mailing of the vaccination voucher, which differs from the population targeted in the estimates reported in Panel A of Table 3 in the main manuscript. Data source: Nationwide online survey data.

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