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Hiroki Kato, Osaka University

Shusaku Sasaki, Osaka University

Fumio Ohtake, Osaka University

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Hiroki Kato † [1], Shusaku Sasaki ‡ [2], and Fumio Ohtake § [1, 2]

[1]Graduate School of Economics, Osaka University, Japan
[2]Center for Infectious Disease Education and Research (CiDER), Osaka University, Japan

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Abstract

This study examines the effectiveness of mailing vouchers for the rubella immunization program against rubella in Japan using local administrative data and nationwide online survey data. To raise the rubella antibody prevalence among men in their 40–50s, and thus achieve herd immunity, Japanese government has launched the rubella immunization program intended for 40-to-57-year-old men and sent vouchers for free testing and vaccination through local governments since FY2019. Local governments have sent vouchers to eligible men over progressively over at least two years. During FY2019, local governments sent vouchers to men aged 40–46 years, while men aged 47–57 years had to apply to their local government to receive vouchers. We estimate the effect of mailing vouchers in a regression discontinuity design that took advantage of the fact that age determined the mailing target in FY2019. The results with both data show that mailing vouchers increased the take-up of antibody testing and vaccination by around 19 and 5 percentage points, respectively. Sending vouchers increases awareness of the rubella immunization program by 37.2 percentage points, suggesting that the mailing of vouchers improves awareness of this program and increase antibody testing and vaccination take-up.

JEL: D90, H75, I18
Keywords: Vouchers, Rubella, Antibody testing, Vaccination, Regression discontinuity design

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†Corresponding author. E-mail: h-kato@econ.osaka-u.ac.jp
‡E-mail: ssasaki.econ@cider.osaka-u.ac.jp
§E-mail: ohtake@econ.osaka-u.ac.jp

1 Introduction

Even if an individual is eligible to enroll in a social security program, such as public assistance, actual enrollment depends on the individual's decision. This means the take-up often does not reach 100%. Factors of incomplete take-up vary by country and the administrative system that has been implemented, but the literature does identify some common factors: transaction costs with application to the program, lack of information, and stigma (Currie, 2006; Ko and Moffitt, 2022).[1]

A similar problem arises with immunization programs—one of the most important public health policies. Vaccination not only reduces the risk of developing infectious diseases, but also has social benefits, such as unburdening the finite health care delivery system. Vaccines for prevention of infection, such as the rubella vaccine, provide nearly full immunity in the vaccinated, which, in turn, reduces the risk of infection in others, especially immunocompromised populations.

In standard economic theory, the market includes individuals who do not internalize the social benefit of vaccination, leading to suboptimal coverage. This situation suggests a need for government intervention to promote immunization.[2] Although many countries offer, in principle, a free immunization schedule at appropriate intervals of age, the immunization coverage, corresponding to the take-up in social security service, has not reached 100%, especially in developing countries (Muhoza et al., 2021). One possible reason for this *incomplete* immunization coverage is a lack of knowledge about routine immunizations and vaccines in developing countries (Favin et al., 2012), a problem that even exists in developed countries in administering lesser-known vaccination (Anderson, 2014).

There exist two types of policies to increase immunization rates further. The first is mandatory immunization (e.g., Brito et al., 1991; Geoffard and Philipson, 1997), but this approach has significant barriers to implementation, such as obtaining public consent in developed countries that prioritize ethics. Developing countries, on the other hand, may lack resources, including administrative systems. The second type of policy provides financial incentives that exceed the vaccination cost. The effectiveness of this policy has been recently studied in the context of the COVID-19 vaccination (e.g., Campos-Mercade et al., 2021; Brehm et al., 2022; Barber and West, 2022) and conditional cash transfers in developing countries (e.g., Barham and Maluccio, 2009; Banerjee et al., 2010; Chandir et al., 2022).

[1] Enrollment in social security programs is stigmatized because of its effect on self-esteem and reduced utility owing to others knowing that one is receiving such assistance. Ko and Moffitt (2022) identifies operational errors by service providers as one of the factors of incomplete take-up of social security services, especially in low-income countries.

[2] Individuals can obtain the social benefits of immunization through the vaccination behavior by others. Even if some individuals internalize the social benefits, we expect continued free-riding behavior to keep coverage below the socially optimal level (e.g., Ibuka et al., 2014).

As the latter policy depends on the decision of those eligible for immunization, financial incentive alone may be ineffective for routine immunization programs, especially when low awareness is a bottleneck. In this case, at the same time, policymakers should increase awareness of policies and the value of immunization by information interventions, such as informing eligible persons about their vaccination options through a letter (e.g., Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019). Sending vouchers can also be an effective option for increasing awareness of programs and the salience of financial incentives (Kacker et al., 2022).

This study examines the effect of mailing vouchers on vaccination behavior and awareness of immunization programs. We specifically base our analysis on Japan's rubella immunization program that began in FY2019. In Japan, before this program was launched, men born between April 2, 1962, and April 1, 1979 (40–57 years old as of 2019) had not received rubella vaccination through routine immunization. Their rubella antibody prevalence is thus lower than that of women of the same generation and that of other generations, and herd immunity against rubella has eluded the country (see section 2 for details). To acquire herd immunity against rubella, the Ministry of Health, Labor and Welfare (MHLW) launched the rubella immunization program for men aged 40–57 years. For this program, MHLW sent vouchers for free rubella antibody testing (about JPY 5,000, equivalent to USD 45) and vaccination (about JPY 10,000, equivalent to USD 90) to eligible men through local governments.[3]

There are several advantages to exploiting this program for examining the effectiveness of mailing vouchers. First, local governments sent vouchers progressively over at least two years, targeting men based on their age. In FY2019, the first year, the target for mailing was men aged 40–46 years. Men aged 47–57 years received the vouchers after FY2020 by default, but could receive vouchers in the first year by requesting their local government to issue them. By exploiting the fact that age determines the default timing of mailing vouchers, we can test the effect of mailing vouchers based on a regression discontinuity design (RDD). RDD assumes individuals cannot precisely manipulate the score that determines treatment assignment. Under this assumption, as all predetermined variables, including unobservables, are independent of treatment status around the assignment threshold, we can identify the average causal effects through the differences in the sample means around the threshold (Lee, 2008). This way, RDD allows us to estimate the causal effects as reliably as in a randomized experiment. We estimate the effect of mailing vouchers in FY2019 by comparing outcomes for men slightly below and slightly above 46 years of age.

The second advantage is low awareness of the rubella immunization program. Our online

[3] We assume USD 1 = JPY 111 (exchange rate on April 1, 2019).

survey shows that only 23.5% of men aged 47–57 years, who were outside the FY2019 mailing target, knew about this program. Hori et al. (2021) similarly found that only 27% of those who did not receive a voucher recognized that the government recommends rubella vaccination.[4] We can test whether sending out vouchers for less-aware immunization programs can improve attention.

Using local administrative data and a nationwide online survey, we test the effectiveness of mailing vouchers based on RDD. The local administrative data provide accurate information on antibody testing and vaccination behavior, allowing us to precisely estimate the effect of sending out coupons. To complement results with local administrative data, we test the data of the nationwide online survey to determine whether the results of the administrative data are not simply specific to the local government (geographical external validity) and we further examine the mechanism of the mailing vouchers.

The results of local administrative data show that sending out vouchers increases antibody testing and the vaccination take-up by 19.1 and 4.7 percentage points, respectively. As results with online survey data are quantitatively similar to the effect of mailing vouchers on antibody testing and vaccination estimated by local administrative data, the results with local administrative data have external validity geographically. Mailing vouchers increases the awareness of the rubella immunization program by 37.2 percentage points. This suggests that mailing vouchers improves incomplete information and increases the take-up of the rubella immunization program.

This study is an important contribution to the literature on information interventions to increase the take-up of social security services and vaccination programs. As noted earlier, a common factor for incomplete take-up is a lack of information about the programs. Studies have explored simple information provisions that convey the likelihood of eligibility and the benefits from enrollment to resolve this problem (e.g., Daponte et al., 1999; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Goldin et al., 2022). Finkelstein and Notowidigdo (2019) found that letters informing low-income individuals that they may be eligible for food stamps, a food cost assistance policy in the U.S., and supporting the application process increase take-up. Similarly, Bhargava and Manoli (2015) found that letters informing people of their eligibility for Earned Income Tax Credit, another U.S. program, increases its take-up. Kacker et al. (2022) found that an information voucher promoted free-of-cost eye exam appointments. Our study is part of this literature.

The remaining study is organized as follows. In section 2, we describe the background of the

[4]The awareness rate can be an upper bound for the vaccination rate. That is, the take-up of additional routine rubella vaccinations is at most 30% when local governments do not send vouchers. If people underestimate the vaccination's value and incur high transaction costs, the take-up would be lower than 30%. This value is low compared with the take-up rate of the social security program compiled by Ko and Moffitt (2022).

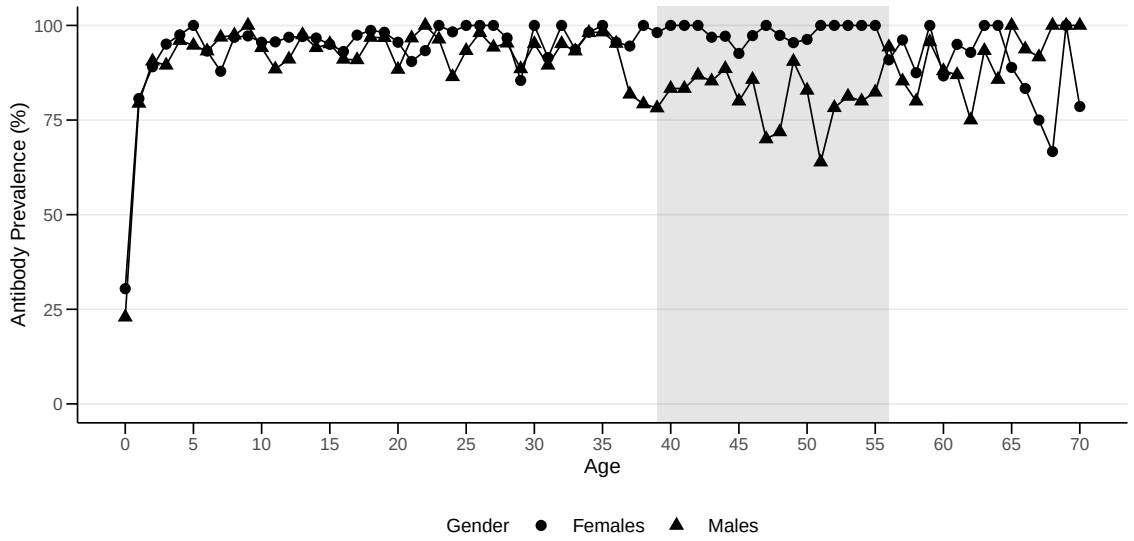

Figure 1: Prevalence of Rubella Antibodies by Age and Gender. Data: NIID "2018 National Epidemiological Surveillance of Vaccine-Preventable Diseases (NESVPD)".

rubella immunization program. In section 3, we present the results derived from local administrative data. In section 4, further present complementary results derived from nationwide online survey data. We conclude in section 5.

2 Background of Rubella Routine Immunization in Japan

Similar to the flu, rubella is a contagious disease transmitted by infected droplets. Its main symptoms are fever and rash. If a woman in the early stages of pregnancy is infected, she may give birth to a child with congenital rubella syndrome, which manifests defects of the eye and ear. Its potential severity has made the Japanese government set rubella as a disease requiring immunization to prevent its spread.

According to Kinoshita and Nishiura (2016), Japan could achieve herd immunity against rubella if the antibody prevalence for all generations were above 90%. However, the antibody prevalence among men in their 40–50s is still about 80% owing to fewer vaccination opportunities through routine immunization and the low likelihood of natural infection (see Figure 1).[5]

To raise the antibody prevalence among men in their 40–50s from 80% to 90%, and thus achieve herd immunity, the MHLW has offered them the opportunity for routine vaccination since

[5]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.

FY2019. This rubella immunization program is intended for men born between April 2, 1962, and April 1, 1979 (40–57 years old as of 2019). To ensure the administration is efficient, eligible men must first undergo antibody testing. Only those who test negative can receive the rubella vaccination. Following the Immunization Act of 1948, eligible men can receive antibody testing and vaccination for free.

Local governments have sent free vouchers for antibody tests and vaccines to eligible men progressively over at least two years as per the guidelines set by the MHLW, which determines the FY2019 mailing target by an eligible man's date of birth, that is:

1. April 2, 1962 – April 1, 1972: while not the primary targets for FY2019, they could receive vouchers in FY2019 by requesting their local government to issue them
2. April 2, 1972 – April 1, 1979: targets for FY2019

Each group above is subject to different processes for receiving antibody testing for the rubella immunization program for FY2019. Men in the former group had to find information about this program on their own, while men in the latter group could obtain the information without such an effort cost. Even if they had sufficient information about the rubella immunization program and were willing to be vaccinated, the men in the former group would still have had to request their local government to issue vouchers.[6] In contrast, men in the latter group received vouchers without such transaction costs. Thus, we consider that mailing vouchers eased the barriers (information acquisition and application procedures) to receiving antibody testing during FY2019. In the following sections, we test this hypothesis using RDD, as an identification strategy, which exploits the FY2019 mailing target determined by age.

3 Local Administrative Data

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

[6] Alternatively, men outside the FY2019 mailing target may stop requesting vouchers in anticipation of receiving them in the next year, and consequently, stop receiving antibody testing and vaccination in FY2019. However, awareness of the rubella immunization program among this group is low, which makes such strategic behavior unlikely (We discuss this point in detail in section 4.3).

3.1 Data

We received anonymized personal data from Tsukuba City on eligible men, excluding those who had already moved out when we received the data, in early June 2020 ($N = 30,936$). The personal data include the masking address, date of birth and date of move-in. For those who moved in before March 31, 2019, Tsukuba City identified the FY2019 mailing target based on the guideline set by the MHLW. Thus, we limit the analysis sample to eligible men who moved into Tsukuba City before March 31, 2019 ($N = 27,736$).

As the personal data contain a birth date, we calculate age in days as of April 1, 2019, and create a treatment variable indicating the FY2019 mailing target.[7] The treatment variable is a dummy variable taken as 1 if an individual was born on April 2, 1972–April 1, 1979 (age in days less than 17,166 days or age less than 47 years). Tsukuba City sent out vouchers to them 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. This data records the date of antibody testing and vaccination (April 1, 2019–February 28, 2022) for those who received at least antibody testing ($N = 8,043$).[8] Eligible men not in this data have not received both antibody testing and vaccination within the analysis period. We create dummy variables indicating eligible men received antibody testing or vaccination by a specific date to use them as outcomes.

Figure 2 splits the analysis sample into those who are in the FY2019 mailing list and those who are not and plots the transition in the sample average of the outcome variable (take-up rates) for each subsample. By default, men subject to the FY2019 mailing target received vouchers on April 12, 2019, while men outside the FY2019 mailing target received vouchers on April 1, 2020.[9] We find antibody testing and vaccination take-ups are concave for both groups after the default mailing date. In other words, antibody testing and vaccination are more frequent immediately after Tsukuba City mailed vouchers, but this momentum wanes over time.

We evaluate the effect of mailing vouchers on the outcome variable as of March 17, 2020, for consistency with the online survey presented later. The antibody testing rate for the FY2019 mailing target is 20%, compared with 1.5% for men without the FY2019 mailing target.[10] In

[7] We exclude samples with incomplete birth dates ($N = 1$).

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

[9] As its own policy, Tsukuba City resends vouchers on June 14, 2021, to those who have not taken an antibody test by March 31, 2021, or to the negatives who have not been vaccinated by March 31, 2021. We test the effectiveness of this policy in section 3.4.

[10] The take-up of antibody testing for the FY2019 mailing target is similar to the aggregated data provided by the MHLW. According to the MHLW, the number of antibody tests using vouchers up to January 2020 is 1.17 million. Dividing this value by the population of men subject to the FY2019 mailing target (6.46 million) yields the take-up of

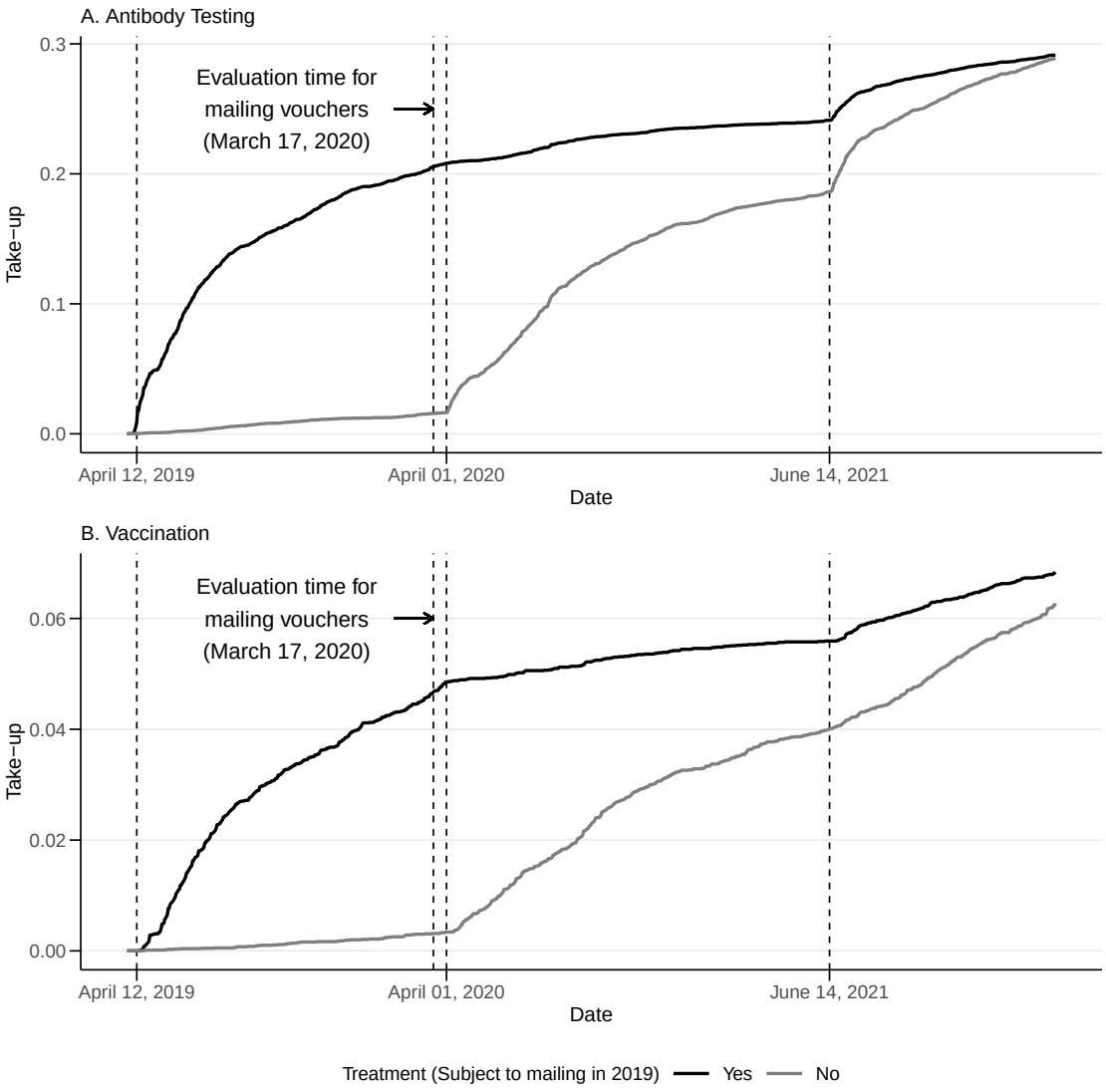

Figure 2: Cumulative Take-Up of Antibody Testing and Vaccination by Treatment Status

addition, the vaccination rate for the FY2019 mailing target is 4.6%, compared with 0.3% for men outside the FY2019 mailing target. As the negative ratio of antibody tests is 25% (Figure A.1 in Appendix A) and the vaccination rate among the negatives is 80% (Figure A.2 in Appendix A), regardless of treatment status, the lower take-up of vaccination than the take-up of antibody testing is mainly due to the lower negative ratio of antibody tests.[11] In summary, a simple comparison between the treatment status suggests that mailing vouchers in FY2019 increased antibody testing and vaccination take-up by 18.5 and 4.3 percentage points, respectively.

3.2 Empirical Strategy

Our RD model is as follows:

$$Y_{iad} = \tau_0 + \tau D_{ia} + f(a-c) + \lambda_d + \epsilon_{iad}, \tag{1}$$

where Y_{iad} is the outcome variable indicating that individual i with birth date d and age in days a has received antibody testing or been vaccinated as of March 17, 2020, and D_{ia} is a dummy variable indicating that individual i is subject to the FY2019 mailing target, that is, it takes 1 if $a < 17166$. Thus, our parameter of interest is τ, which identifies the effect of mailing vouchers at a cut-off value.

We assume that the function $f(a-c)$ of the score centered at the threshold $c = 17166$ that determines the treatment state has a shape that varies with the treatment state. We approximate this function by a polynomial function. As Gelman and Imbens (2019) does not recommend using higher-order polynomial functions when approximating globally, we use linear and quadratic functions. Thus, the score function is $f(a-c) = \sum_{\gamma=1}^{2}[\beta_{0\gamma}(a-c)^{\gamma} + \beta_{\gamma} D_{ia}(a-c)^{\gamma}]$.

The score function f may not adequately address the seasonality and heaping of birthdays (Barreca et al., 2016). For example, suppose that we observe mass points at the beginning of each month, which can be explained by a variable correlated with the outcome variables. As, in the extreme, our RDD compares men born on April 1, 1972, to men born on April 2, 1972, non-random heaping may bias the estimated effect τ, prompting us to control the 365 birth date fixed effects λ_d.[12]

antibody tests of 18%.

[11] The negative ratio for antibody testing is higher than the unconditional negative ratio (about 20%), estimated from the NIID data presented in Figure 1. Thus, people who (believe that) they do not have antibodies may be more likely to receive antibody testing. Although there is a small sample problem owing to a small number of antibody tests, we find this selection in early 2019 quite strong for those who are ineligible for the FY2019 mailing (Figure A.1 in Appendix A).

[12] The local administrative data cannot clearly observe seasonality or heaping of the date-of-birth distribution (Figure

In addition, because our RD design uses April 1, 1972, the day before the start of an elementary school in Japan, as the threshold date, our estimates may be affected by a relative age effect (Matsubayashi and Ueda, 2015). Although birth date fixed effects may control for this effect, as an alternative approach, we estimate the RD effect in the case where the threshold is April 1 of other years. If there is a statistically significant effect at the artificial cut-offs, then the estimated effect at the true cut-off may reflect a relative age effect. We present our results in Figure E.1 in Appendix E and find no statistically significant RD effects outside of the true cut-off. Therefore, the mailing vouchers effect, which we show in the following subsection, is not likely to be affected by relative age effect.[13]

When making statistical inferences, Lee and Lemieux (2010) recommends using standard errors that are robust to heteroskedasticity. Also, when the running variable is discrete, such as age, many previous studies have used robust standard errors clustered at each point of the running variable as a robust inference against model misspecification (Lee and Card, 2008). However, as Kolesár and Rothe (2018) does not recommend their use, we use unclustered robust standard errors (Huber–White standard errors).

3.3 Effect of Mailing Vouchers

Figure 3 shows the age profiles of antibody test take-up (Panel A) and vaccination take-up (Panel B). The fitted lines use a model approximating the score function by a global linear function. The take-ups of antibody testing and vaccination for men subject to the FY2019 mailing are higher than for the remaining men. Antibody testing and vaccination take-up are discontinuous at the cut-off value (born on April 1, 1972, 47 years old as of April 1, 2019). Thus, mailing vouchers promotes antibody testing and vaccination. The RD effects of mailing vouchers on antibody testing and vaccination are 19.3 and 4.7 percentage points, respectively, statistically significant at the 1% level.

We perform a sensitivity analysis to check the validity of this estimate. Table C.1 in Appendix C estimates a model that controls for fixed effects of birth date (or fixed effects of birth month and birth year) and a quadratic approximation of the score function. Figure D.1 in Appendix D

B.1 in Appendix A). Thus, we expect that controlling birth date fixed effects would not significantly change the estimated effect of mailing vouchers. As there are some unobserved birth dates, we also estimate a model controlling for fixed effects of birth month and birth year.

[13] When using an RDD with age as the running variable, the problem is that all individuals will eventually receive the treatment. Thus, individuals currently in the control state may change their behavior strategically in anticipation of receiving treatment in the future (Lee and Lemieux, 2010). To address this, we sometimes estimate a so-called donut-hole RD, which excludes samples around the threshold and estimates the RD model (Shigeoka, 2014). Our RDD does not lead to this problem, because the treatment is implemented only once in FY2019.

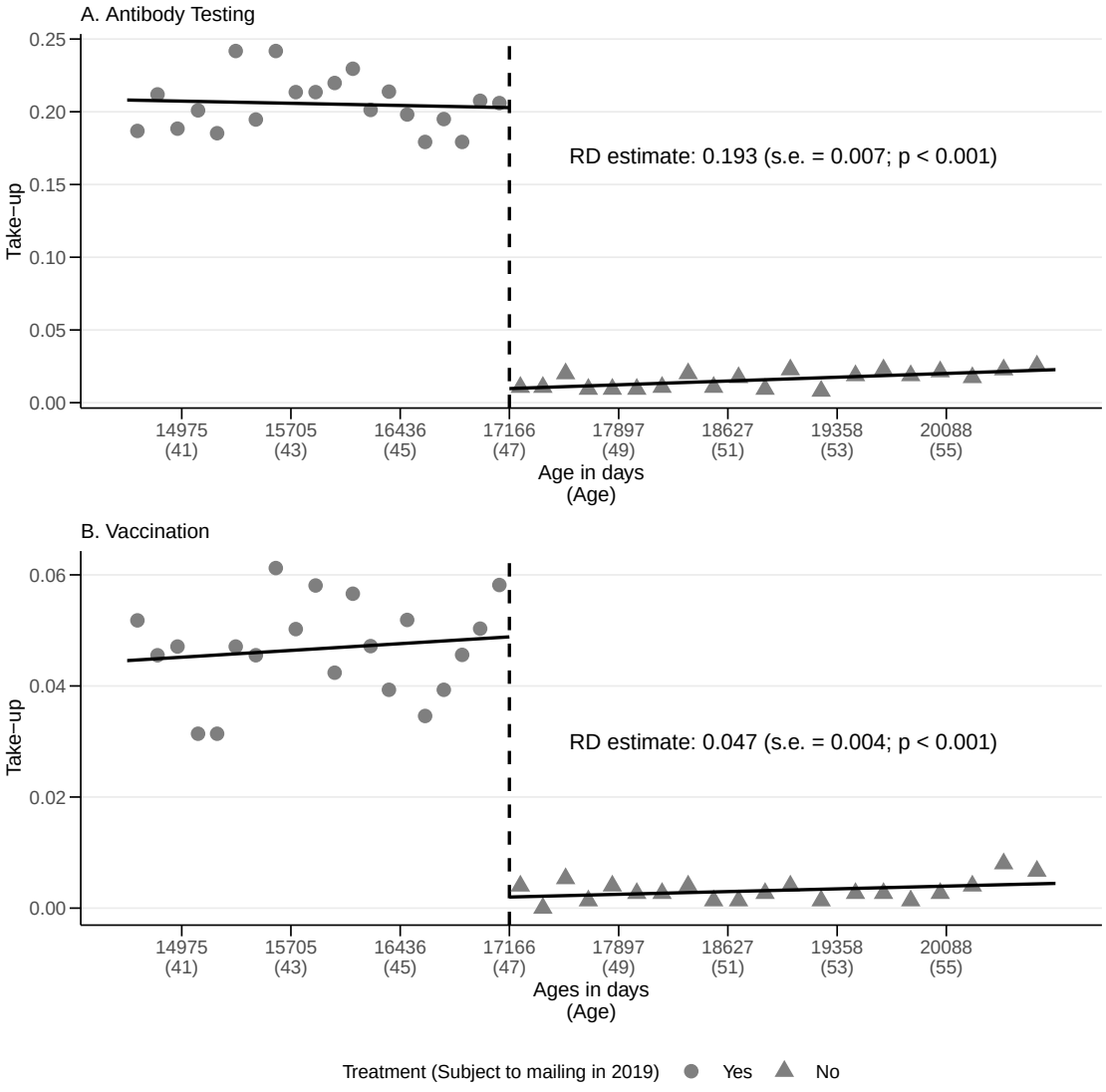

Figure 3: RD Effect of Mailing Vouchers on Antibody Testing and Vaccination. *Notes*: The scatterplot shows the local sample mean. The lines are the fitted lines from the regression model of the linear age profile estimated in each subsample split by the treatment status. The RD estimate is the difference between the estimated intercepts of the model in each subsample. We perform statistical tests using the z-score approach.

also performs nonparametric estimation by local linear regression. This method estimates our RD model (1) by least squares weighted by the kernel density computed under a given bandwidth. These results are quantitatively similar to those in Figure 3.

According to the NIID survey shown in Figure 1, the unconditional negative ratio (i.e., the ratio of those not to possess antibodies) is 20%. Therefore, if there is no selection indicating that eligible men who (believe that they) do not have antibodies are more likely to receive antibody testing regardless of mailing vouchers, and if the negatives are always vaccinated, then the RD effect on vaccination should be 20% of the RD effect on antibody testing, 3.86 percentage points ($= 0.193 \times 0.2$). This theoretical effect is slightly smaller than the estimated RD effect. Thus, although it may be a statistical error, mailing vouchers may strengthen the selection for antibody testing or increase vaccination take-up among the negatives.

To confirm this point, we estimate the RD effect on antibody test results conditional on antibody test takers and the RD effect on vaccination conditional on the negatives (Table C.2 in Appendix C). The results indicate that mailing vouchers may reduce the negative ratio of antibody tests, suggesting that sending vouchers promotes antibody testing among those who do not need to be vaccinated. Thus, mailing vouchers may weaken the selection for antibody testing.[14] In addition, mailing vouchers increases the vaccination take-up among the negatives, which would make the estimated RD effect on vaccination larger than the theoretical value. However, these results are not robust because the size and statistical significance of the effects vary across models.

3.4 Effect of Second Mailing of Vouchers

We found that mailing vouchers promotes antibody testing and vaccination. In this case, should policymakers send vouchers again to those who did not act after receiving the first voucher? To answer this question, we exploit the unique policy of Tsukuba City. Tsukuba City resent vouchers on June 14, 2021, to those who had not received antibody testing or the negatives who had not been vaccinated until March 31, 2021.[15]. We test the effectiveness of this policy using the single interrupted time series (SITS) analysis and RD analysis.

[14] As will be shown later, mailing vouchers improves awareness of the additional rubella routine immunization program. In other words, this intervention lowers the information cost. The standard economic theory argues that barriers to information acquisition and other barriers to application serve as a screening of applicants and increase the program's efficiency. The finding that mailing vouchers weakens the selection for antibody testing reinforces this argument.

[15] More precisely, Tsukuba City identified those who had not received antibody testing or the unvaccinated negatives based on data as of March 31, 2021. However, because individual behavior is reflected in the local government's data with a two-month delay, some individuals who had undergone antibody testing or received vaccination by March 31, 2021 were subject to second mailing of vouchers.

3.4.1 SITS Analysis

As those who did not receive the second mailing of vouchers are already vaccinated or have antibodies (no need to vaccinate), there is no comparable control group for estimating the second mailing of vouchers effect. Therefore, we perform a SITS analysis, which is widely used mainly in public health (Wagner et al., 2002; Linden, 2015; Karaivanov et al., 2022). This analysis uses pre-intervention time series data to construct a transition without the second mailing of vouchers (counterfactual) and compares it to the actual trend after the intervention (see Appendix F for technical details).

We create time series data since April 1, 2020, on the number of tests and vaccinations to focus solely on the second mailing intervention. As daily data will include many days without antibody testing or vaccination, we create weekly time series data. We analyze the sample separately by treatment status to test the extent to which the effect of the second mailing differs by whether to be subject to the FY2019 mailing. Figure F.1 in Appendix F plots constructed time-series data. In both groups, the number of antibody tests jumps up immediately after the second mailing, and then decays. In addition, the number of vaccinations increases after the second mailing for the FY2019 mailing target, while there is no clear increase in the number of vaccinations for the remaining men. We can observe the same fact in Figure 2.

Owing to the count time series data, we assume a Poisson distribution and estimate the following log-linear model for the distributional mean λ:

$$\ln \lambda_w = \alpha + \beta_1 T_w + \beta_2 D_w + \beta_3 T_w D_w + \epsilon_w, \tag{2}$$

where T_w is a linear time trend and D_w is a dummy variable taking one after the intervention. The model adds a cross term between T_w and D_w, which assumes different time trends before and after the intervention. The intervention effect is captured by the coefficients β_2 and β_3. The coefficient β_2 is the effect of the second mailing immediately after the intervention, while the coefficient β_3 captures changes in the intervention effect over time. If time is considered the running variable, this model is essentially the same as the RDD. The estimation results show that the second mailing of vouchers increases the number of antibody tests and vaccinations immediately after resending. The effect on antibody testing decays over time, but the change over time in the effect on vaccination varies with the first mailing date (model (5)–(8) in Table F.1 in Appendix F).[16]

[16] We estimate models assuming a negative binomial distribution. We also estimate models that account for autocorrelation in the data using the Liboschik et al. (2017) methodology. The results show that, in all models, the second mailing of vouchers increases the number of antibody tests and vaccinations immediately after the sending of vouch-

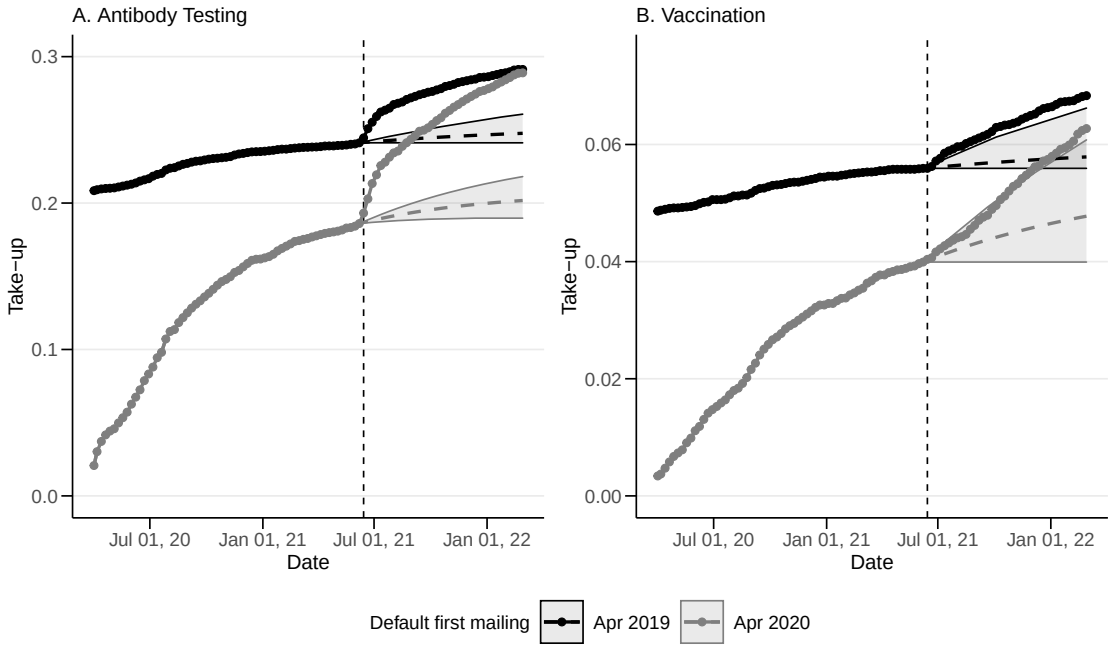

Figure 4: Time series of Observed and Counterfactual Cumulative Weekly Take-Up. *Notes*: The dotted solid line represents the transition in observed take-up. The dashed line is the transition of counterfactual take-up. The 95% prediction interval is the shaded area. The counterfactual is the ratio of the cumulative sum of the predicted values of the mean of the Poisson distribution. See Appendix F for details.

Table 1: Observed and Counterfactual Take-Up Using the Poisson Model

		Antibody Testing (%)			Vaccination (%)		
Default First Mailing	N	Observed	Counterfactual	Difference	Observed	Counterfactual	Difference
Apr 2020	15004	28.9	20.2	8.7	6.3	4.8	1.5
			[19.0, 21.8]	[7.1, 9.9]		[4.0, 6.1]	[0.2, 2.3]
Apr 2019	12731	29.1	24.8	4.4	6.8	5.8	1.0
			[24.1, 26.1]	[3.1, 5.0]		[5.6, 6.6]	[0.2, 1.2]
Difference in treatments				4.4			0.5
				[2.1, 6.9]			[−1.0, 2.1]

Note: We show the take-up of antibody testing and vaccination as of February 28, 2022, the last day of our study period. The counterfactual is the take-up under the situation where there is no second mailing of vouchers. We constructed the 95% prediction interval for the counterfactual at the 2.5 and 97.5 percentiles of the Poisson distribution with the mean replaced by the predictions of the SITS model, while accounting for estimation uncertainty. See the Appendix F for technical details. The difference between the observed and counterfactual take-up for each group indicates the effect of the second mailing. The difference in the effect of the second mailing for each group indicates the effect of shortening the time to sending by 1 year.

In Figure 4, the dashed line represents the counterfactual take-up using the cumulative total of the predicted value of λ_w when $D_w = 0$. The dotted solid lines in this figure are the observed antibody testing and vaccination take-up. For antibody testing, we find that the second mailing of vouchers increases the take-up of antibody testing, regardless of whether to be subject to the FY2019 mailing. As of February 28, 2022, the last day of our analysis period, the observed antibody testing take-up for the FY2019 mailing target is 29.1%, while its counterfactual is 24.8%. Thus, the effect of the second mailing of vouchers on antibody testing for this group is 4.4 percentage points (second row of Table 1). Similarly, the observed and counterfactual take-up of antibody testing for men outside the FY2019 mailing target is 28.9% and 20.2%, respectively, and hence, the effect of the second mailing of vouchers on antibody testing for this group is 8.7 percentage points (first row of Table 1). This effect is 4.4 percentage points (twice as high) as the effect in the FY2019 mailing target group (third row of Table 1).

For vaccination, sending vouchers again, whether to be subject to the 2019 mailing, also slightly increases vaccination take-up. Up to February 28, 2022, the observed and counterfactual vaccination take-up for the FY2019 mailing target group is 6.8% and 5.8%, respectively, and hence, the effect of the second mailing on vaccination for this group is 1 percentage points (second row of Table 4). Similarly, the observed and counterfactual vaccination take-up for men outside the FY2019 mailing target is 6.3% and 4.8%, respectively, and hence, the effect of the second mailing on antibody testing in this group is 1.5 percentage points (first row of Table 4). This effect is 0.5 percentage points higher than the effect in the FY2019 mailing target group (third row of Table 4). However, as the 95% prediction interval includes 0, there may be no difference in the effect of the second mailing on vaccination between two groups.

3.4.2 RD Analysis

The difference in the second mailing effect by whether to be included in the FY2019 mailing list represents the effect of shortening the interval between the first and second mailing. Among men for the FY2019 mailing, unvaccinated negatives and those who had not received antibody testing received the coupons again two years later. Men outside the FY2019 mailing target received the voucher in April 2020 by default, but could obtain it in FY2019 through the issuance request. Thus, unvaccinated negatives and those who had not received antibody testing in this group received vouchers again as early as 1 year later. If we limit the analysis sample to those eligible for

ers. The direction, magnitude, and statistical significance of the effect change over time differ depending on the model specification. However, results are quantitatively unchanged. See Appendix F in details

resending, the interval between two mailings is determined solely by age. Thus, we can estimate the effect of the shorter interval between the first and second mailing by using the RDD as an identification strategy.

The outcome variable is a dummy variable indicating receiving antibody testing (or vaccination) up to February 28, 2022. The treatment variable takes a value of 1 for not being included in the FY2019 mailing target, that is, $D_{ia} = 1[a \geq 17166]$. Note that this is inverted from the treatment variable in Figure 3.

Figure 5 shows the age profiles of antibody test take-up (Panel A) and vaccination take-up (Panel B). Evidently, the shorter interval increases the antibody test and vaccination take-up of unvaccinated negatives (and those who had not received antibody testing) by 4.1 and 1 percentage points, respectively. These results are statistically significant and quantitatively consistent with counterfactual analysis using SITS.

To test the validity of the RD estimates, we perform a sensitivity analysis. Table C.3 in Appendix C estimates models controlling 365 birthday fixed effects and models with a quadratic approximation of the score function. Figure D.2 in Appendix D is a nonparametric estimation with local linear regression. These results are quantitatively similar to those in Figure 5, but the effect on vaccination is statistically insignificant. To confirm the influence of the relative age effect, Figure E.2 in Appendix E estimates the RD effect at the placebo cut-off. As a result, the RD effect at most placebo cut-offs is statistically insignificant, and hence, the influence of the relative age effect is not very severe.

This analysis includes behavior before the second mailing (April 1, 2021–June 14, 2021). If behavior in pre-treatment period is discontinuous at the cut-off, the RD effect in Figure 5 would have captured the effect of the first mailing, rather than the effect of the shorter interval between the two mailings. We estimate the RD effect, where the outcome is a dummy variable that takes a value of 1 if one receives antibody testing (or vaccination) by June 14, 2022 (Table C.4 in Appendix C). The effect on antibody testing is statistically insignificant. However, when we fit a model with a linear approximation of the score function, the effect on vaccination is statistically significant. Thus, we can interpret the effect on antibody testing in Figure 5 as the effect of the shorter interval between the two mailings, while the effect on vaccination in Figure 5 does not completely eliminate the effect of the first mailing.

Regardless of voucher mailing, if the decision to receive antibody testing is unrelated to (a belief of) negative antibodies and if the negatives always vaccinate, the RD effect on vaccination is 0.82 percentage points ($= 0.041 \times 0.2$)—marginally smaller than the estimated RD effect. Thus,

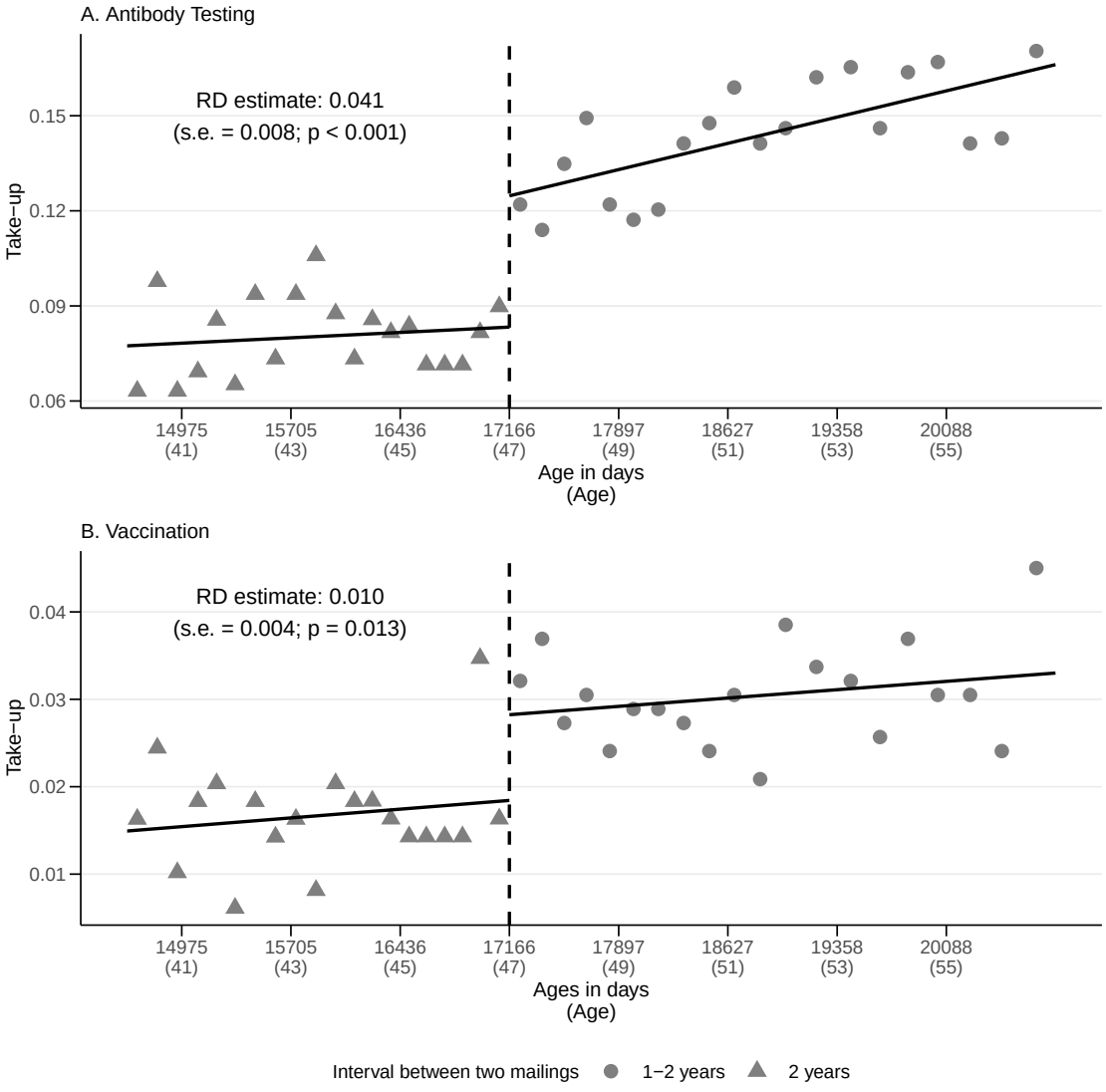

Figure 5: RD Effect of Mailing Vouchers Again on Antibody Testing and Vaccination. *Notes*: The scatterplot shows the local sample mean. The lines are the fitted lines from the regression model of the linear age profile estimated in each subsample split by the treatment status. The RD estimate is the difference between the estimated intercepts of the model in each subsample. We perform statistical tests using the z-score approach.

the shorter sending interval may strengthen the selection for antibody testing owing to negative beliefs or increase vaccination take-up of the negatives.

To confirm this point, we estimate the RD effect on the test results conditional on antibody test takers and the RD effect on vaccination take-up conditional on the negatives (Table C.5 in Appendix C). The results show that the shorter interval of mailings decreases the negative ratio of antibody tests, suggesting that the selection of antibody testing owing to negative beliefs weakens. In other words, shortening the interval between two mailings promotes those who do not need to be vaccinated to receive antibody testing. Although many models show statistically insignificant effects, the shorter interval of mailings also increases vaccination take-up of the negatives, which may make the estimated effect on vaccination larger than the theoretical one.

3.5 Summary of Local Administrative Data Analysis

This section uses local administrative data to examine the effect of mailing vouchers on antibody testing and vaccination. The results show that sending vouchers increases antibody testing take-up by 19.3 percentage points and vaccination take-up by 4.7 percentage points. As we will see in the next section, these results have geographically external validity (not local government-specific results), as the results of the online survey quantitatively have similar results. The online survey shows that the main mechanism of these results is increasing awareness of the rubella immunization program by mailing vouchers.

We also examine the effect of resending vouchers, a local government's unique policy, finding that the second mailing of vouchers promotes antibody testing and vaccination take-up. The shorter the interval between two mailings is, the stronger these effects are. The SITS analysis shows that, for men outside the FY2019 mailing target (received coupons after one year at the earliest), the second mailing of vouchers increases antibody testing take-up by 8.7 percentage points and vaccination take-up by 1.5 percentage points. The SITS analysis and the RD analysis show that the effect on antibody testing for this group is twice as large as the effect among eligible men for the FY2019 mailing. The effect on vaccination for this group is 1.5 times the effect for the FY2019 mailing target group, but the result is not statistically robust. We cannot empirically examine the mechanisms of these results, but propose our hypothesis in Section 5.

4 Nationwide Online Survey Data

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

4.1 Data and Empirical Strategy

We commissioned Internet research firm MyVoiceCom Co. Ltd. to conduct an online survey at the end of FY2019. The data consisted of two surveys, including a follow-up survey. The first survey was conducted between February 15, 2020, and February 17, 2020, for 4,200 men aged 40–59 years. In addition to personal demographics, the first survey asked about previous antibody testing and vaccination, awareness of the additional routine rubella immunization program, and knowledge about rubella. The follow-up survey was conducted from March 17, 2022, to March 25, 2022, among respondents of the first survey. We received responses from 3,963 individuals (attrition rate = 5.6%). The follow-up survey asked whether they had undergone antibody testing or vaccination since the first survey. We use men aged 40–56 years who participated in both surveys ($N = 3,444$).

Our identification strategy is the same as in the local administrative data analysis. However, as the online survey only contains the birth year and the birth month, we need to re-establish the running variables and the thresholds at which the treatment determines. Therefore, the running variable is the age in months as of April 2019.[17] Following the guidelines set by the MHLW, the treatment variable indicates that men with an age of 523–563 months (less than 47 years old) received the vouchers by default ($N = 1,418$, 41% of the analysis sample).[18]

As in the local administrative data analysis, we estimate the model controlling birth month fixed effects to accommodate for seasonality and non-random mass points of the age distribution.[19] To check the influence of the relative age effect, we estimate the RD effect at placebo thresholds in April of other years (Appendix E). Unlike the local administrative data analysis, we can demonstrate the validity of the RDD by testing for discontinuities in demographic characteristics and

[17] Assume that no one born in April has a birthday.

[18] According to interviews conducted by the MHLW, 96% of local governments sent vouchers by October 2019. Thus, few in the treatment group have not yet received vouchers. Some local governments might deviate from the guidelines set by the MHLW and set their own rules. For example, a local government sent vouchers in FY2019 to all eligible individuals, regardless of their date of births. In this case, the control group includes those who automatically received the vouchers. While we cannot quantify how many of these local governments exist, we believe that they are few in number.

[19] Figure B.2 in Appendix B shows the age distribution of the online survey. We find more clearly the seasonality and heaping of the distribution than the local administrative data. We also test whether our online survey data ensure sample representativeness by comparing it with the 2015 census. The distributions of the two data are somewhat similar.

health outcomes. If some covariates are discontinuous at the cut-off, then the effects identified by our RDD may include the influence of covariates. We estimate their discontinuity of annual income, education level, marital status, and health habits such as exercise and flu vaccination (Figure B.3 in Appendix B). The results show that most of the discontinuities in the covariates are statistically insignificant, but only flu vaccination habits are statistically significantly discontinuous at the 10% level. We, therefore, estimate a model that controls for this covariate.

4.2 External Validity

To begin, we estimate the RD effects of mailing vouchers on antibody testing and vaccination to test the geographical external validity of local administrative data analysis. We use follow-up survey responses to create a binary outcome variable indicating having undergone antibody testing (or having been vaccinated) in the past.

However, we reserve some caution in interpretation. First, as we rely on self-reports, which are prone to misremembrance and incorrect responses. Thus, the estimated effect may be biased. Second, we cannot rule out antibody testing and vaccination behavior before and without the rubella immunization program. In particular, married men may receive antibody testing outside the additional routine immunization program because of a public health policy allowing married couples with pregnancy to receive rubella antibody testing for free in Japan. The outcome variable used here includes voluntary behavior that is not influenced by sending vouchers. However, as we cannot empirically show that these problems are not serious, we assume that self-reported bias and behavior unrelated to the rubella immunization program are random around the thresholds.

Figure 6 shows the age profiles of antibody testing take-up (Panel A) and vaccination take-up (Panel B). The results show that mailing vouchers in FY2019 increases antibody testing and vaccination take-up by 18.7 and 4.4 percentage points, respectively. These effects are statistically significant.

To test the validity of these estimates, we perform a sensitivity analysis. Table C.6 in Appendix C estimates the model controlling birth month fixed effects and covariates. Figure D.3 in Appendix D shows a nonparametric estimation with local linear regression. These results are quantitatively similar to those in Figure 6, but the statistical significance for the effect on vaccination differs. Figure E.3 in Appendix E also estimates the RD effect at the placebo cut-off. As a result, the RD effects at most placebo cut-offs are statistically insignificant. Thus, the relative age effect is not severe.

The analysis with the online survey is very close quantitatively to the results of the local ad-

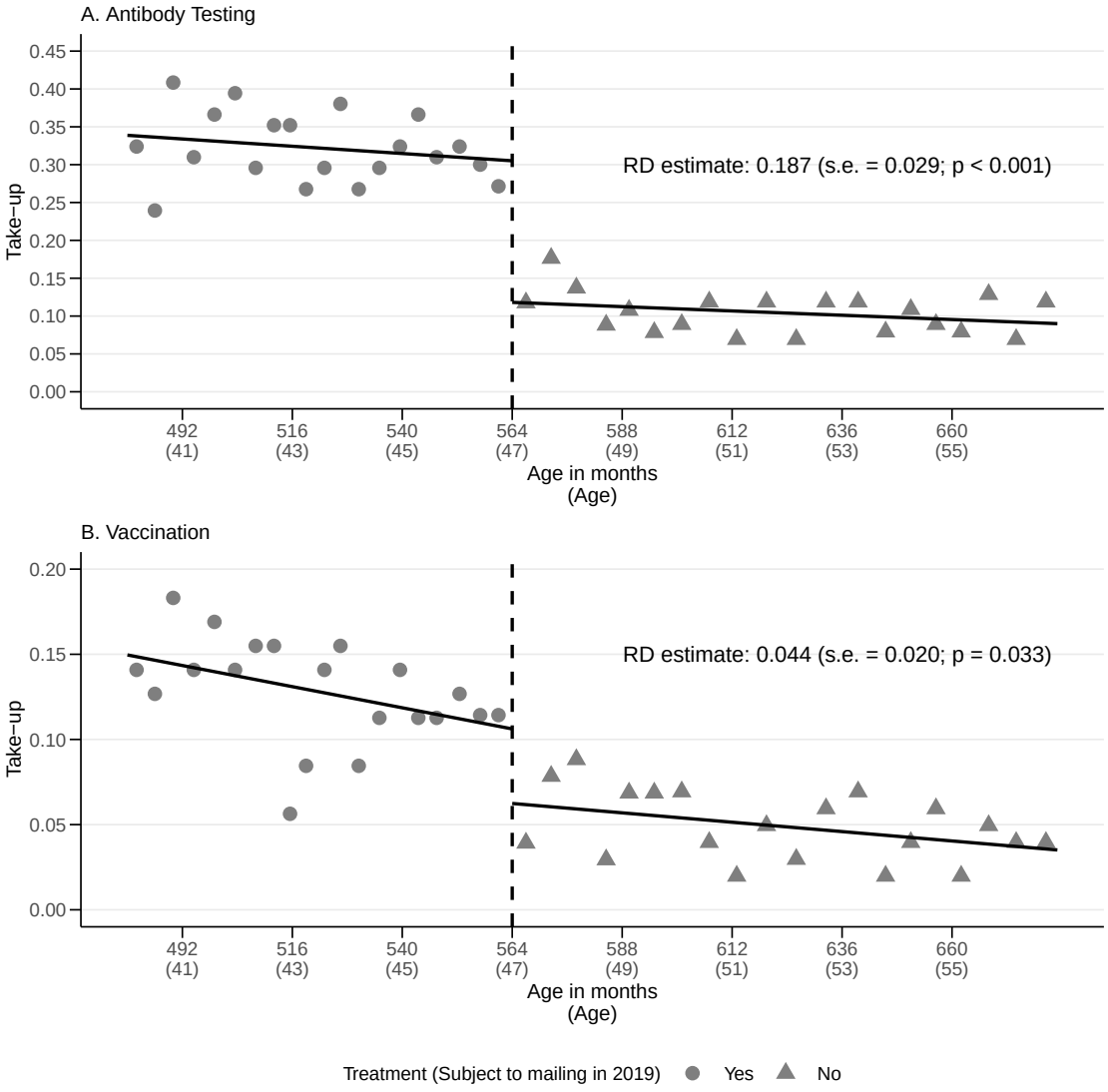

Figure 6: RD Effect of Mailing Vouchers on Antibody Testing and Vaccination Using Nationwide Online Survey Data. *Notes*: The scatterplot shows the local sample mean. The lines are the fitted lines from the regression model of the linear age profile estimated in each subsample split by the treatment status. The RD estimate is the difference between the estimated intercepts of the model in each subsample. We perform statistical tests using the z-score approach.

ministrative data. Therefore, the results of the local administrative data are not specific to the local government. That is, the results of the local government data show geographically external validity.

4.3 Mechanism: Improving Lack of Information

The mailing of vouchers has increased antibody testing and vaccination take-up for the rubella immunization program. Two possible mechanisms explain this result in the point of awareness of the immunization program. The first is that eligible men are unaware of this program, unless they have received vouchers. If so, mailing vouchers would improve the incomplete information about the rubella immunization program and promote antibody testing and vaccination. This mechanism is our hypothesis presented in section 1. Second, eligible men are aware of this immunization program regardless of whether or not to receive vouchers. If so, those who are not subject to the FY2019 mailing target should be unwilling to obtain vouchers in FY2019 with a costly application, discouraging antibody testing and vaccination. In this case, the RD effect of mailing vouchers in FY2019 captures the strategic behavior of eligible men in the control group, who received vouchers in FY2020 by default, rather than improving the lack of information. This subsection examines which possibility occurred.

Using the responses to the first survey, we create a dummy variable indicating that respondents know that the MHLW has implemented the rubella immunization program since FY2019. Estimating our RD model, where this dummy variable is an outcome, we examine the degree to which mailing vouchers increases awareness of the additional routine immunization program.

Figure 7 shows the age profile of awareness. The results show that sending vouchers increases awareness of the additional routine rubella vaccination by 37.2 percentage points, which is statistically significant at the 1% level. As a sensitivity analysis of this estimate, models (1)–(2) in Table C.7 in Appendix C estimate the RD models controlling birth month fixed effects and covariates. Figure D.4 in Appendix D shows a nonparametric estimation with local linear regression. Figure E.4 in Appendix E estimates RD effects on the placebo cut-off. These results suggest that the results in Figure 7 are robust.

The results provide support for the first mechanism: the mailing of vouchers improves awareness of the additional routine vaccinations and increase antibody testing and vaccination take-up. In addition, 24% of men outside the FY2019 mailing target recognized the rubella immunization program. The COVID-19 outbreak since January 2020 is one of reasons why awareness of the program is low. Thus, at most 24% of men in the control group may have taken the strategic behavior

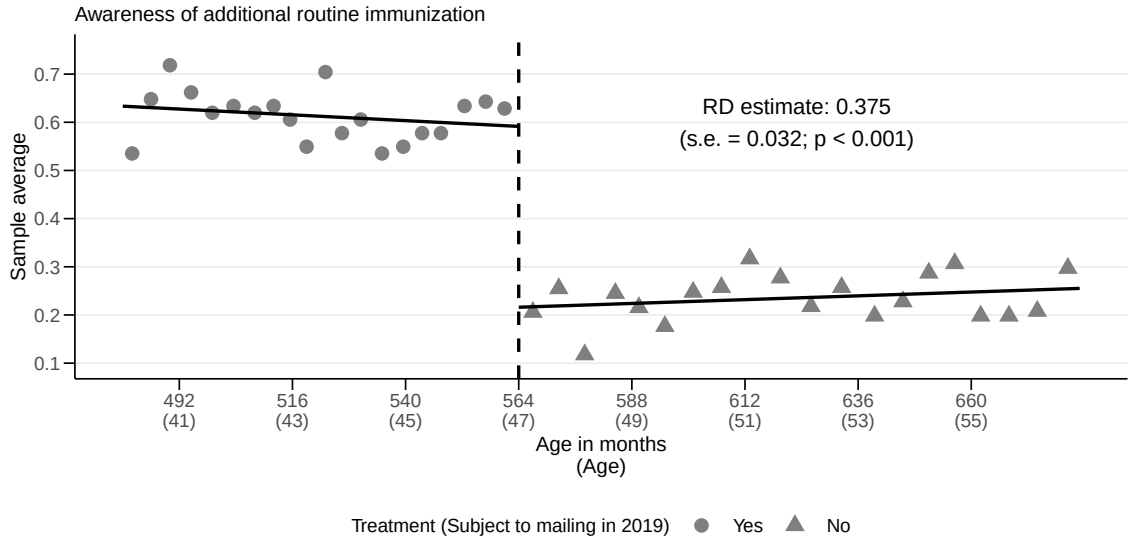

Figure 7: RD Effect of Mailing Vouchers on Awareness of Additional Routine Immunization. *Notes*: The scatterplot shows the local sample mean. The lines are the fitted lines from the regression model of the linear age profile estimated in each subsample split by the treatment status. The RD estimate is the difference between the estimated intercepts of the model in each subsample. We perform statistical tests using the z-score approach.

described earlier. However, this influence is relatively smaller than the first mechanism. Therefore, the improvement of lack of information is the main driver for the effect of mailing vouchers for antibody testing and vaccination.

4.4 Mechanism: Vaccination Value

Besides awareness of the program, mailing vouchers may also increase the value of possessing antibodies, thus promoting antibody testing and vaccination. To test this possibility, we use the responses to the first survey on rubella knowledge and subjective probability of infection to examine how mailing vouchers affects these factors.[20]

Panels A and B in Figure 8 show the age profiles of the recognition of the risk of congenital rubella syndrome and the severity risk, respectively. The results show that mailing vouchers has no statistically significant effect on two risk awareness. We can obtain these results in models controlling birth month fixed effects and covariates (columns (3)–(6) of Table C.7 in Appendix C) and nonparametric estimation (Panels A and B in Figure D.5 in Appendix D).

Panels C and D of Figure 8 show the age profiles of the own infection probability, and the infection probability among men in their 40–50s, respectively. The results show that mailing vouchers lowers the two subjective infection probabilities; particularly, the effect on own infection proba-

[20] Relative age effects do not affect the RD estimates in this subsection (See Figure E.5 in Appendix E).

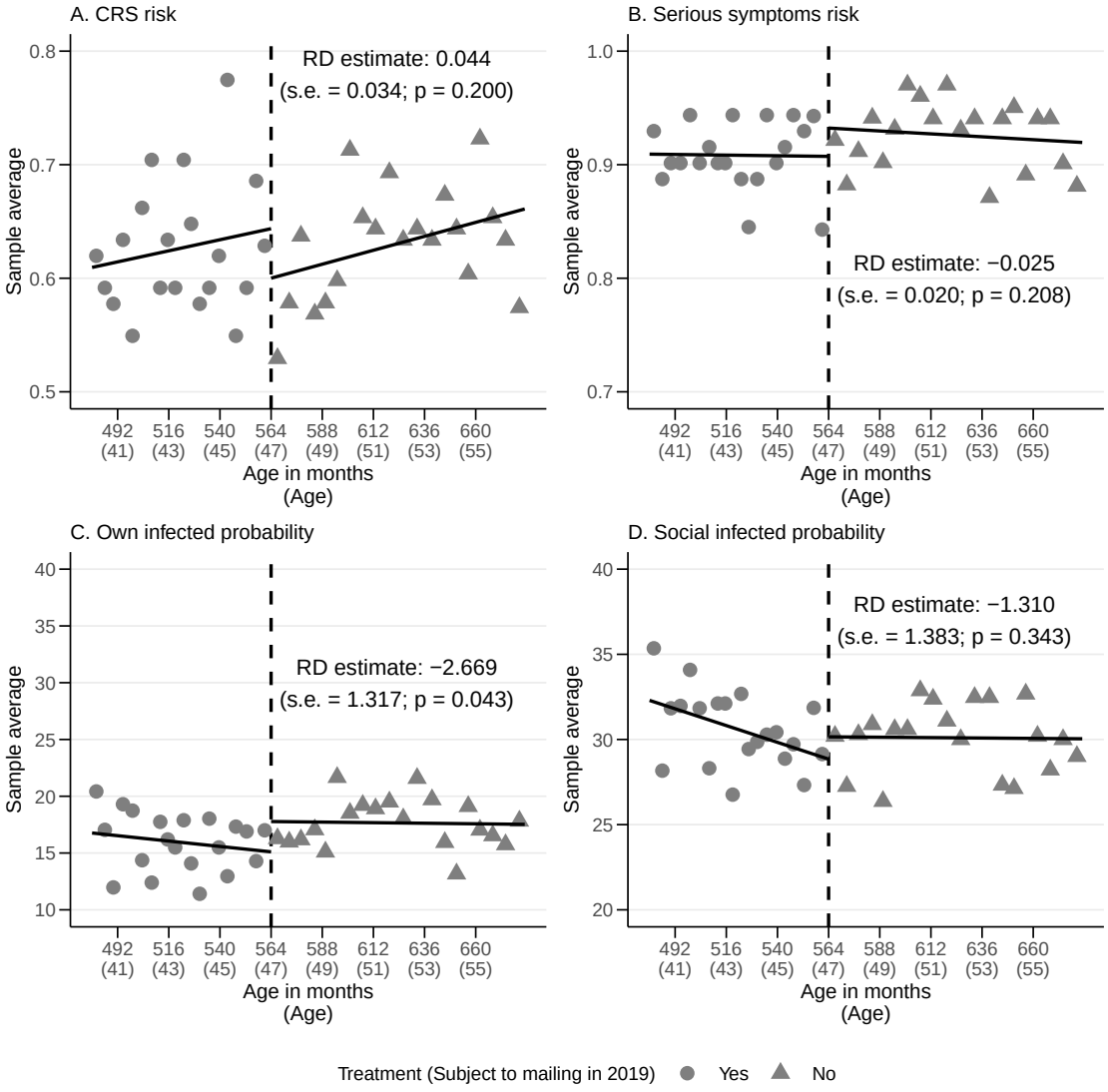

Figure 8: RD Effect of Mailing Vouchers on Vaccination Value. *Notes*: The scatterplot shows the local sample mean. The lines are the fitted lines from the regression model of the linear age profile estimated in each subsample split by the treatment status. The RD estimate is the difference between the estimated intercepts of the model in each subsample. We perform statistical tests using the z-score approach.

bility is statistically significant at the 5% level. Thus, sending vouchers may reduce the value of vaccination. However, this result is not robust, as the models entailing a quadratic approximation of the score function (model (8) in Table C.7 of Appendix C) or nonparametric estimation (Panels C in Figure D.5 of Appendix D) are statistically insignificant. In sum, mailing vouchers does not increase the value of possessing antibodies enough to promote antibody testing and vaccination.

5 Discussions and Conclusions

This study provides empirical evidence to support the success of mailing vouchers for Japan's rubella immunization program for improving the awareness of the program and the take-up of antibody testing and vaccination. We estimated the effect of mailing vouchers using the RDD as an identification strategy. First, sending vouchers increased antibody testing and vaccination take-up by 19.1 and 4.7 percentage points, respectively, based on an analysis of local government administrative data that accurately records antibody testing and vaccination behavior.

Based on a nationwide online survey, we examined the geographical external validity of the results with the local administrative data and the mechanism of mailing vouchers. Sending vouchers increased antibody testing and the vaccination take-up by 18.7 and 4.4 percentage points, respectively, suggesting the geographical external validity of the results with the local administrative data. In addition, mailing vouchers increased the awareness of the additional immunization program by 37.2 percentage points, implying that sending vouchers improves awareness and the take-up of the targeted program.

We also tested the effect of the second mailing of vouchers by exploiting the cooperating local government's unique policy. SITS analysis revealed that resending vouchers increased antibody testing and vaccination take-up. However, this kind of analysis imposes strong assumptions to identify the treatment effect owing to a before-and-after comparison (Baicker and Svoronos, 2019). We suggest constructing a comparable control group to examine the effect of the second mailing of vouchers by relaxing the identification assumptions. Moreover, reducing the interval between two mailings by about 1 year approximately doubles the resending effect on antibody testing and the resending effect on vaccination by about 1.5 times, although the later result was not statistically robust. This is the result obtained through SITS and RDD analyses.

As the first mailing of vouchers significantly improved awareness of the rubella immunization program, resending vouchers may serve as a reminder than improving incomplete information. Reminders solve incomplete take-up that is attributed to an eligible person's procrastination and

limited attention (Karlan et al., 2016; Ericson, 2017). Reminders have been proven to be effective in the Earned Income Tax Credit (e.g., Guyton et al., 2017) and vaccination (e.g., Dai et al., 2021) in the literature as well. Kacker et al. (2022) called such vouchers a tangible reminder. Additionally, it may be better to send reminders frequently, as a shorter interval between two mailings increased the effectiveness of resending vouchers in our study. These results may be further analyzed in future works.

Recent studies in behavioral economics show that "nudges," such as adding text messages that have been shown to increase the value of vaccination to reminders (Milkman et al., 2021; Dai et al., 2021) and ensuring blank space to write appointments (Milkman et al., 2011), make reminders more effective. The MHLW regulates the design of the vouchers, and we found no elements that could serve as nudges. As vouchers alone cannot increase the value of vaccination, we suggest determining and testing various designs for better effectiveness.

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Appendix A Additional Data Summaries of Local Administrative Data

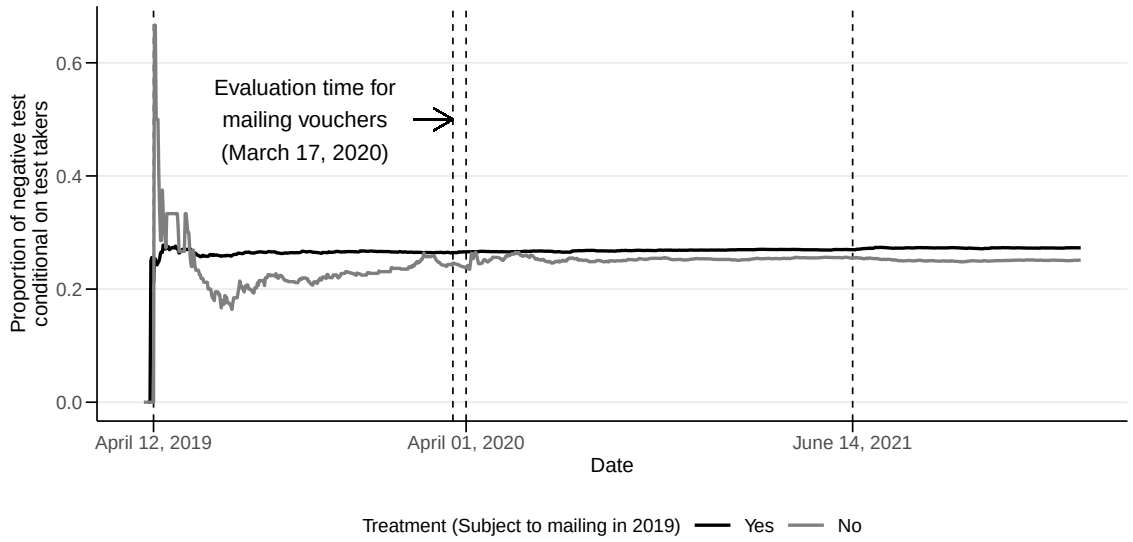

Figure A.1: Cumulative Proportion of Negative Tests Conditional on Test-takers by Treatment Status

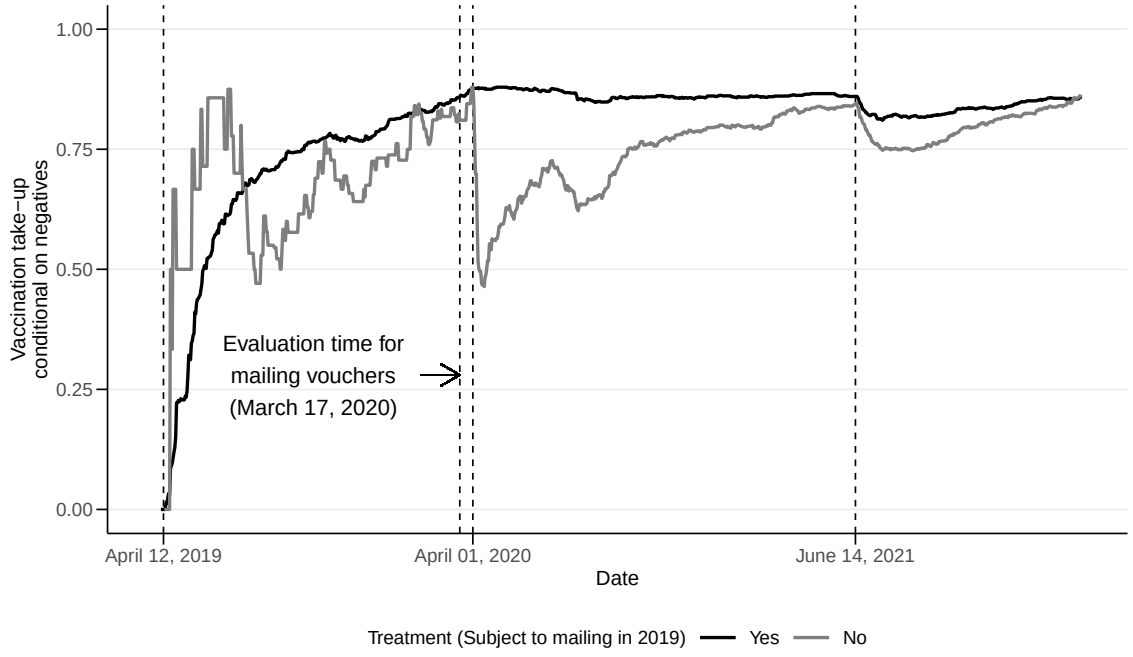

Figure A.2: Cumulative Vaccination Rates Conditional on the Negatives by Treatment Status

Appendix B Validity of Our RD Strategy

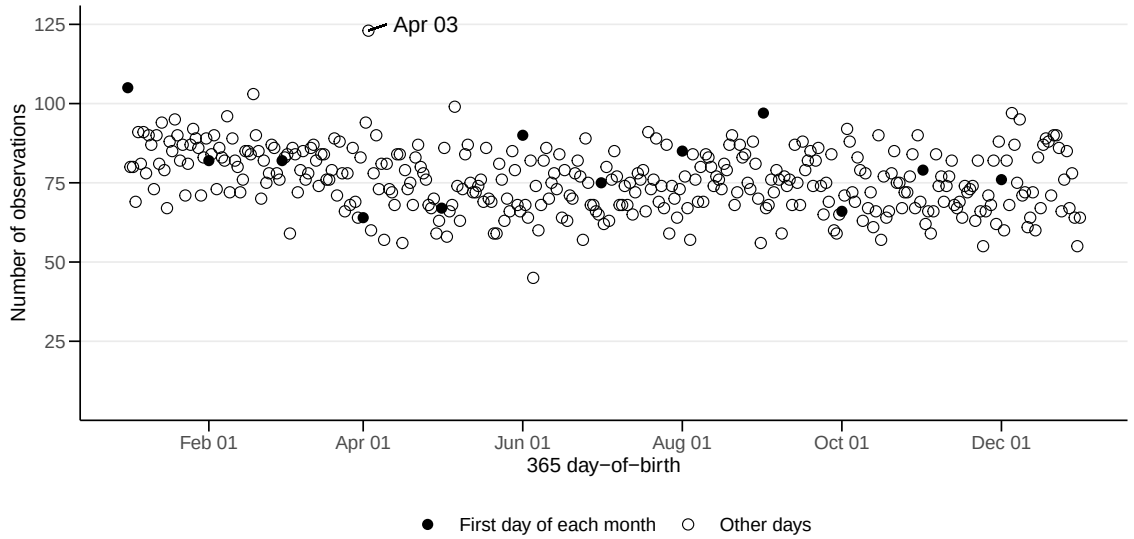

Figure B.1: Distribution of 365 Days-of-Birth (Local Administrative Data)

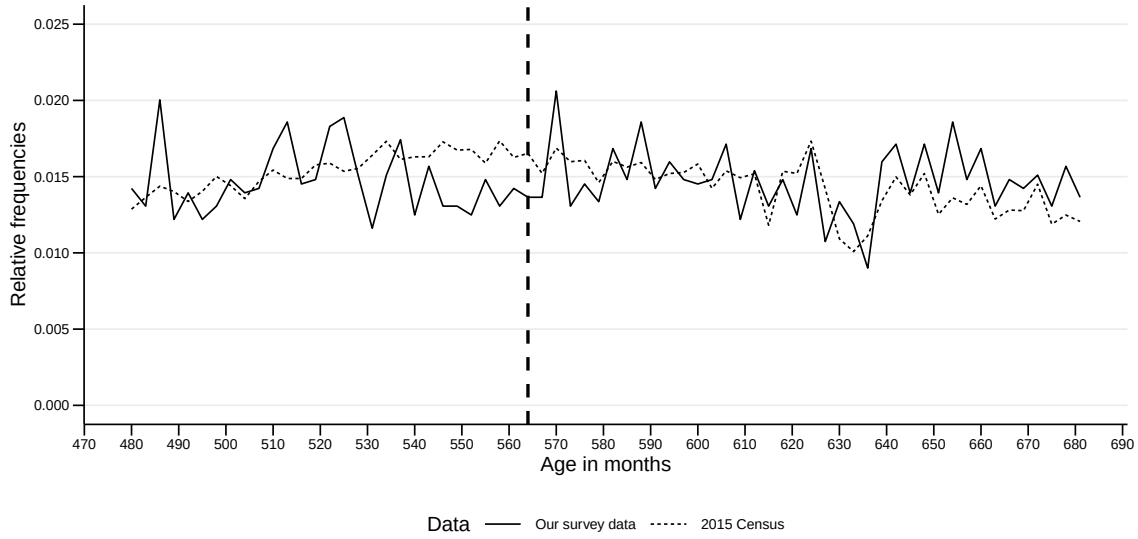

Figure B.2: Age Distributions of Online Survey Data and 2015 Census. *Notes*: As the census provides a population aggregated on age in quarterlies, we also collapse data quarterly. The relative frequency is calculated so that the population aged 40–56 years is 1.

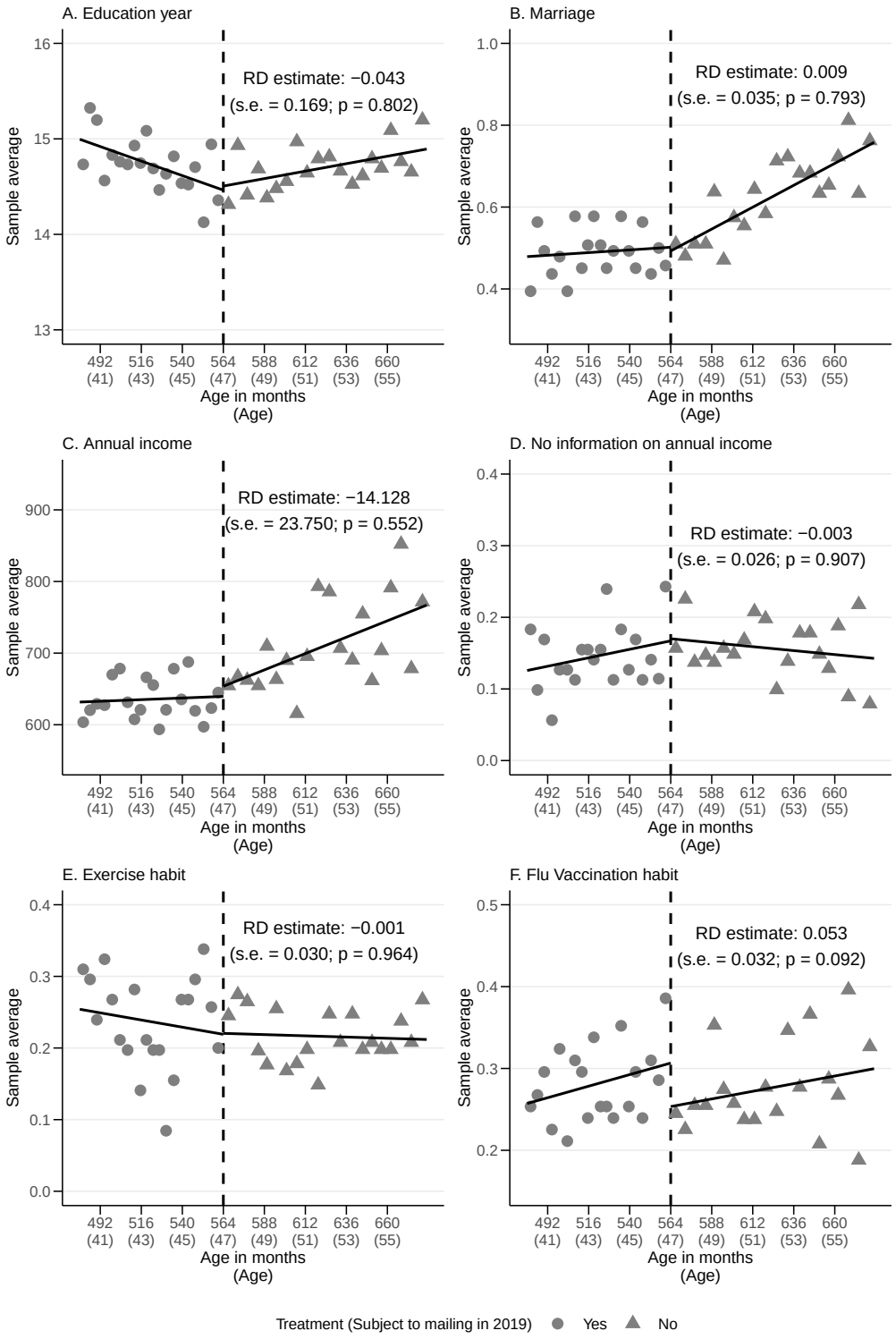

Figure B.3: RD Estimates on Covariates Using Nationwide Online Survey Data. *Notes*: The scatterplot shows the local sample mean. The lines are the fitted lines from the regression model of the linear age profile estimated in each subsample split by the treatment status. The RD estimate is the difference between the estimated intercepts of the model in each subsample. We perform statistical tests using the z-score approach.

Appendix C Regression Analysis

Table C.1: Parametric RD Estimates on Antibody Testing and Vaccination

	Antibody testing				Vaccination			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sent in April 2019	0.191***	0.193***	0.168***	0.185***	0.047***	0.053***	0.044***	0.054***
	(0.007)	(0.014)	(0.011)	(0.016)	(0.004)	(0.008)	(0.006)	(0.009)
Num.Obs.	27 735	27 735	27 735	27 735	27 735	27 735	27 735	27 735
R2 Adj.	0.097	0.098	0.098	0.098	0.021	0.021	0.021	0.021
FE: Birth year		X		X		X		X
FE: Birth month		X		X		X		X
FE: 365 day-of-birth	X		X		X		X	
Polynomial order	1	1	2	2	1	1	2	2

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We use robust standard errors (Huber–White standard errors). We control 365 day-of-birth fixed effects, birth month fixed effects, birth year fixed effects, and linear time trend for duration of residence as of 31 March 2019.

Table C.2: Parametric RD Estimates on Negative Tests and Vaccination Conditional on the Negatives

	Negative tests				Vaccination among negatives			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sent in April 2019	0.018	−0.407**	−0.166	−0.393**	0.093	0.334	0.307*	0.379*
	(0.070)	(0.165)	(0.107)	(0.168)	(0.115)	(0.222)	(0.163)	(0.227)
Num.Obs.	2851	2851	2851	2851	750	750	750	750
R2 Adj.	0.001	0.013	0.004	0.013	0.083	−0.002	0.091	−0.003
FE: Birth year		X		X		X		X
FE: Birth month		X		X		X		X
FE: 365 day-of-birth	X		X		X		X	
Polynomial order	1	1	2	2	1	1	2	2

Sample: Those who received antibody testing (model (1)–(4)); those whose antibody test is negative (model (5)–(8)). Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We use robust standard errors (Huber–White standard errors). We control 365 day-of-birth fixed effects, birth month fixed effects, birth year fixed effects, and linear time trend for duration of residence as of 31 March 2019.

Table C.3: Parametric RD Estimates of Shorter Interval between Two Mailings on Antibody Testing and Vaccination

	Antibody testing				Vaccination			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Resent one year later	0.041***	0.051***	0.043***	0.041**	0.010**	0.009	0.010	0.005
	(0.008)	(0.020)	(0.012)	(0.021)	(0.004)	(0.010)	(0.006)	(0.011)
Num.Obs.	22 271	22 271	22 271	22 271	22 271	22 271	22 271	22 271
R2 Adj.	0.013	0.011	0.013	0.011	0.003	0.003	0.003	0.003
FE: Birth year		X		X		X		X
FE: Birth month		X		X		X		X
FE: 365 day-of-birth	X		X		X		X	
Polynomial order	1	1	2	2	1	1	2	2

Sample: Those who (i) moved in by 31 March 2019 and (ii) have not received antibody testing or have not been vaccinated despite negative results as of 31 March 2021. Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We use robust standard errors (Huber–White standard errors). We control 365 day-of-birth fixed effects, birth month fixed effects, birth year fixed effects, and linear time trend for duration of residence as of 31 March 2019.

Table C.4: Parametric RD Estimates of Shorter Interval between Two Mailings on Antibody Testing and Vaccination before Resending

	Antibody testing				Vaccination			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Resent one year later	0.004	0.009	0.006	0.011	0.004***	0.002	0.006***	0.002
	(0.003)	(0.009)	(0.005)	(0.009)	(0.001)	(0.003)	(0.002)	(0.003)
Num.Obs.	22 271	22 271	22 271	22 271	22 271	22 271	22 271	22 271
R2 Adj.	0.001	0.000	0.001	0.000	0.001	0.002	0.002	0.002
FE: Birth year		X		X		X		X
FE: Birth month		X		X		X		X
FE: 365 day-of-birth	X		X		X		X	
Polynomial order	1	1	2	2	1	1	2	2

Sample: Those who (i) moved in by 31 March 2019 and (ii) have not received antibody testing or have not been vaccinated despite negative results as of 31 March 2021. Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We use robust standard errors (Huber–White standard errors). In columns (1) to (4), the outcome variable is a dummy variable that is 1 if the sample received antibody testing until 14 June 2021 when the local government resent the free vouchers. In columns (5) to (8), the outcome variable is a dummy variable that is 1 if the sample was vaccinated until 14 June 2021. We control 365 day-of-birth fixed effects, birth month fixed effects, birth year fixed effects, and linear time trend for duration of residence as of 31 March 2019.

Table C.5: Parametric RD Estimates of Shorter Interval between Two Mailings on Negative Tests and Vaccination Conditional on the Negatives

	Negative tests				Vaccination among negatives			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Resent one year later	−0.151***	−0.179**	−0.183***	−0.187*	0.260***	0.209	0.215	0.202
	(0.043)	(0.087)	(0.065)	(0.097)	(0.092)	(0.139)	(0.136)	(0.157)
Num.Obs.	2579	2579	2579	2579	836	836	836	836
R2 Adj.	0.017	0.022	0.018	0.021	0.027	0.038	0.036	0.037
FE: Birth year		X		X		X		X
FE: Birth month		X		X		X		X
FE: 365 day-of-birth	X		X		X		X	
Polynomial order	1	1	2	2	1	1	2	2

Sample: Those who (i) moved in by 31 March 2019 and (ii) have not received antibody testing or have not been vaccinated despite negative results as of 31 March 2021. Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We use robust standard errors (Huber–White standard errors). We use those who received antibody testing in model (1) to (4), and those whose antibody test is negative in model (5) and (8). We control 365 day-of-birth fixed effects, birth month fixed effects, birth year fixed effects, and linear time trend for duration of residence as of 31 March 2019.

Table C.6: Parametric RD Estimates on Antibody Testing and Vaccination Using Nationwide Online Survey

	Antibody Testing		Vaccination	
	(1)	(2)	(3)	(4)
Sent in 2019	0.177***	0.129***	0.044**	0.045
	(0.029)	(0.044)	(0.021)	(0.031)
Num.Obs.	3444	3444	3444	3444
R2 Adj.	0.098	0.099	0.028	0.028
Polynomial order	1	2	1	2

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We use robust standard errors (Huber–White standard errors). We control a dummy making flu vaccination a habit and birth month fixed effects.

Table C.7: Parametric RD Estimates on Awareness and Vaccination Value Using Nationwide Online Survey

	Awareness		CRS risk		Serious symptoms risk		Own infected probability		Social infected probability	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sent in 2019	0.366***	0.399***	0.052	0.095*	−0.019	0.012	−3.170**	0.602	−1.495	0.918
	(0.032)	(0.048)	(0.034)	(0.052)	(0.020)	(0.032)	(1.321)	(1.977)	(1.394)	(2.110)
Num.Obs.	3444	3444	3444	3444	3444	3444	3444	3444	3444	3444
R2 Adj.	0.167	0.166	0.005	0.006	0.007	0.008	0.009	0.010	0.011	0.011
Polynomial order	1	2	1	2	1	2	1	2	1	2

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We use robust standard errors (Huber–White standard errors). “Awareness” is a dummy variable indicating that the respondent knows the additional routine immunization of rubella. “CRS risk” is a dummy variable indicating that the respondent knows that an infected woman in her early pregnancy may have a child with congenital rubella syndrome (CRS). “Serious symptoms” risk is a dummy variable indicating that the respondent knows serious illness owing to infection. “Own infected probability” is the subjective probability of being infected with rubella. “Social infected probability” is the subjective probability a man in 40s or 50s is infected with rubella. We control flu vaccination a habit and birth month fixed effects.

Appendix D Estimating RD Effects by Local Linear Regression

To examine the sensitivity of the RD estimates in the study, we estimate the RD effects using a nonparametric method. For estimation, we consider the following RD model:

$$Y_{ia} = \tau_0 + \tau D_{ia} + \beta_{01}(a - c) + \beta_1 D_{ia}(a - c) + \epsilon_{ia}, \tag{3}$$

where Y_{ia} is an outcome variable of individual i with a age in days (or months) and D_{ia} is a treatment variable that is determined by the value of running variable a and the cut-off value c. Thus, our parameter of interest is τ, which indicates the treatment effect.

We estimate the model (3) by the local linear regression. First, given the bandwidth h, we compute the kernel density weight $K((a - c)/h)$, where the kernel density function $K(x)$ is uniform, that is, $K(x) = 1[|x| \leq 1]/2$. The local linear regression allows us to obtain estimates by the least squares weighted with kernel density weight:

$$\min \sum_{i=1}^{N} \left(Y_{ia} - \tau_0 - \tau D_{ia} - \beta_{01}(a - c) + \beta_1 D_{ia}(a - c)\right)^2 K\left(\frac{a - c}{h}\right). \tag{4}$$

Using the local administrative data, given a bandwidth, Figure D.1 plots the estimated effect of sending vouchers in FY2019. Figure D.2 plots the estimated effect of the shorter interval between two mailings. The bandwidth is $h \in \{30, 31, 32, \ldots, 364, 365\}$. The horizontal dashed line shows the treatment effect with the RD model (3) estimated by ordinary least squares.

Figures D.3–D.5 summarize the nonparametric analyses using the online survey. Figure D.3 plots the estimated effect of sending vouchers in FY2019. Figure D.4 plots the effect of mailing vouchers on the awareness of the additional routine rubella vaccinations. Finally, Figure D.5 shows the effect of mailing vouchers on the vaccination value. The bandwidth is $h \in \{4, 5, 6, \ldots, 89, 90\}$. The horizontal dashed line shows the treatment effect with the RD model (3) estimated by ordinary least squares.

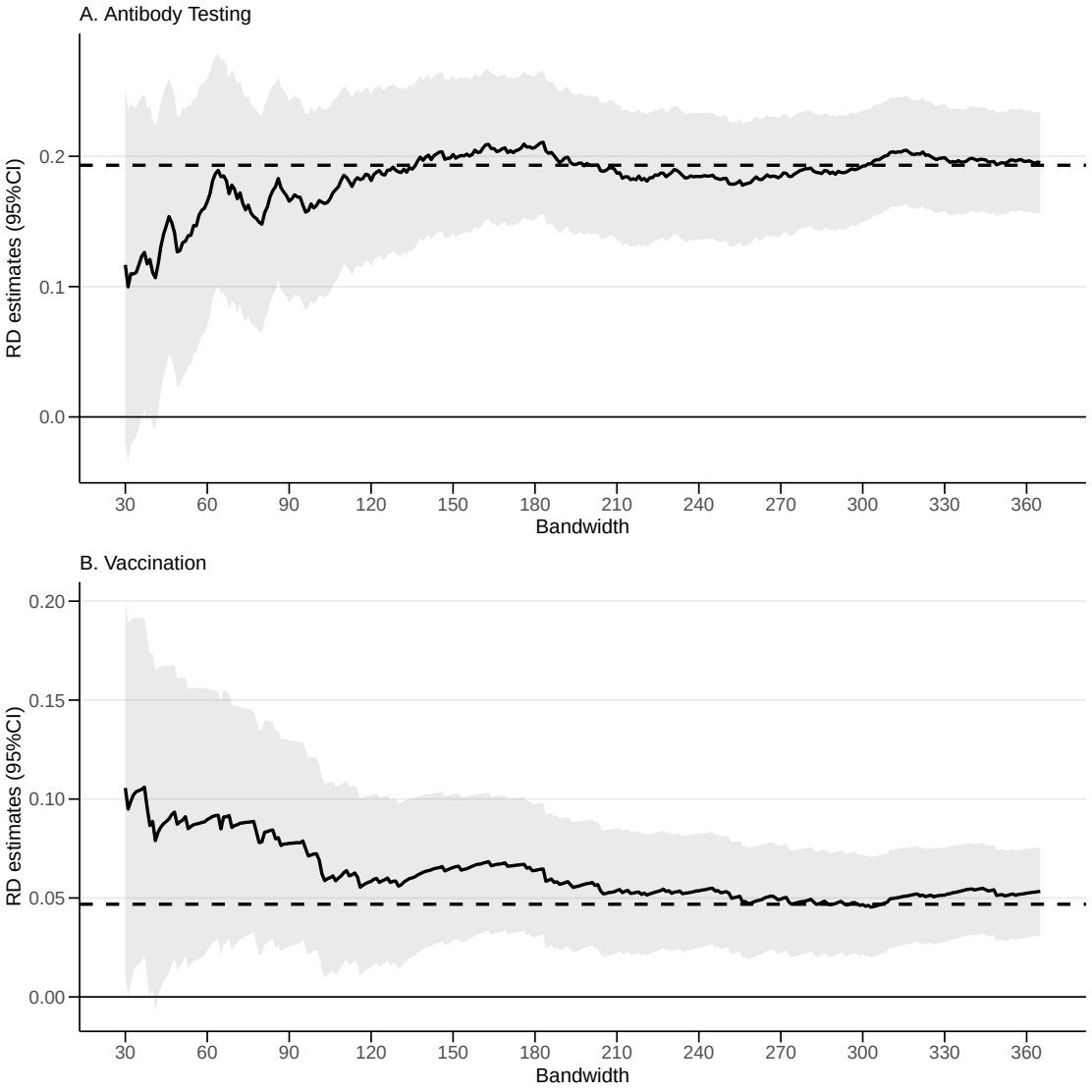

Figure D.1: Nonparametric RD Effect of the First Mailing of Vouchers by Bandwidth (Local Administrative Data). *Notes*: The horizontal dashed line represents the treatment effect with a model that assumes a linear approximation of age profile, fully interacted with a treatment variable, estimated by ordinary least squares. The shaded area is a 95% confidential interval of the treatment effect estimated by local linear regression with uniform kernel density weight.

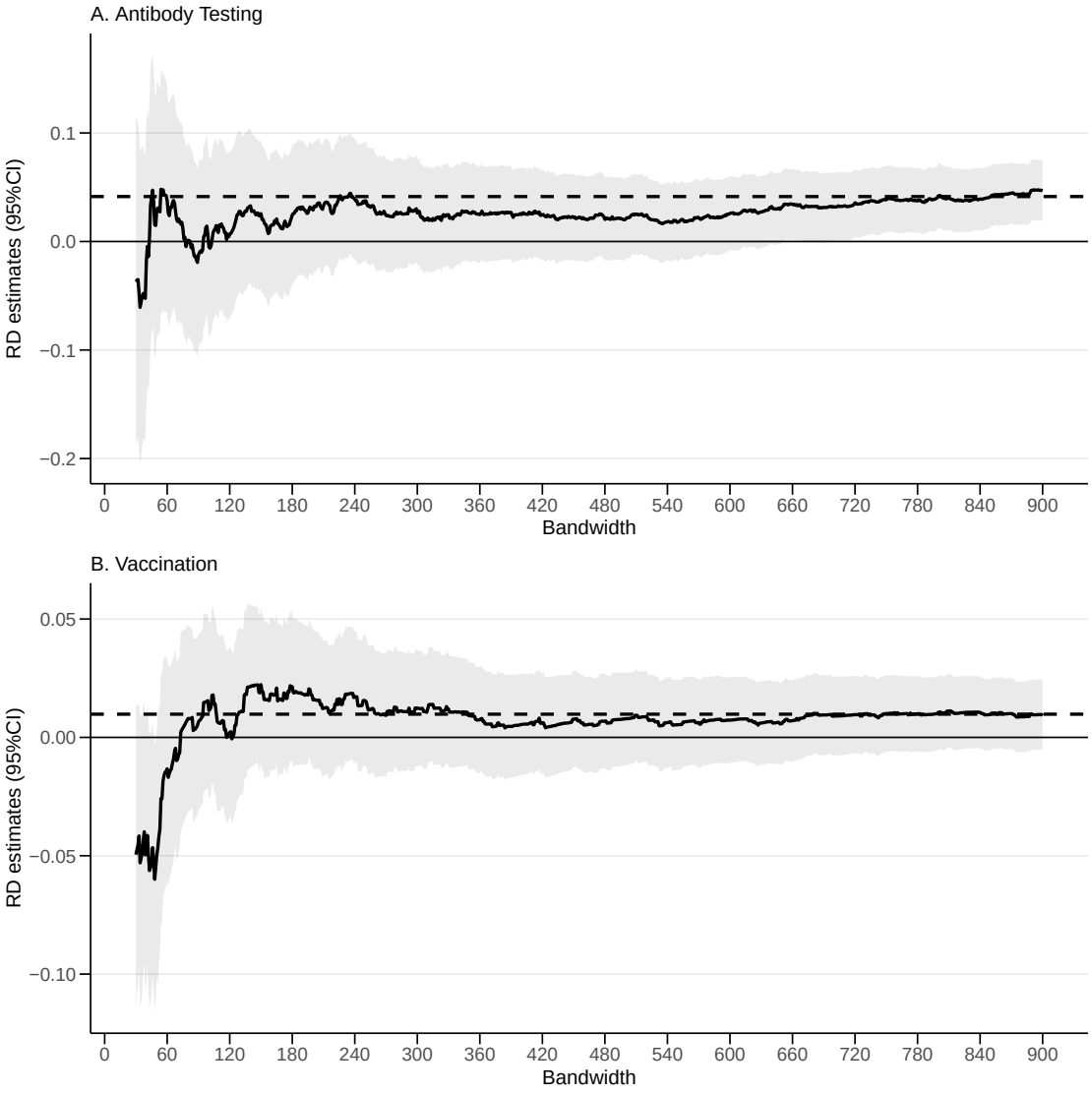

Figure D.2: Nonparametric RD Effect of the Second Mailing of Vouchers by Bandwidth (Local Administrative Data). *Notes*: The horizontal dashed line represents the treatment effect with a model that assumes a linear approximation of age profile, fully interacted with a treatment variable, estimated by ordinary least squares. The shaded area is a 95% confidential interval of the treatment effect estimated by local linear regression with uniform kernel density weight.

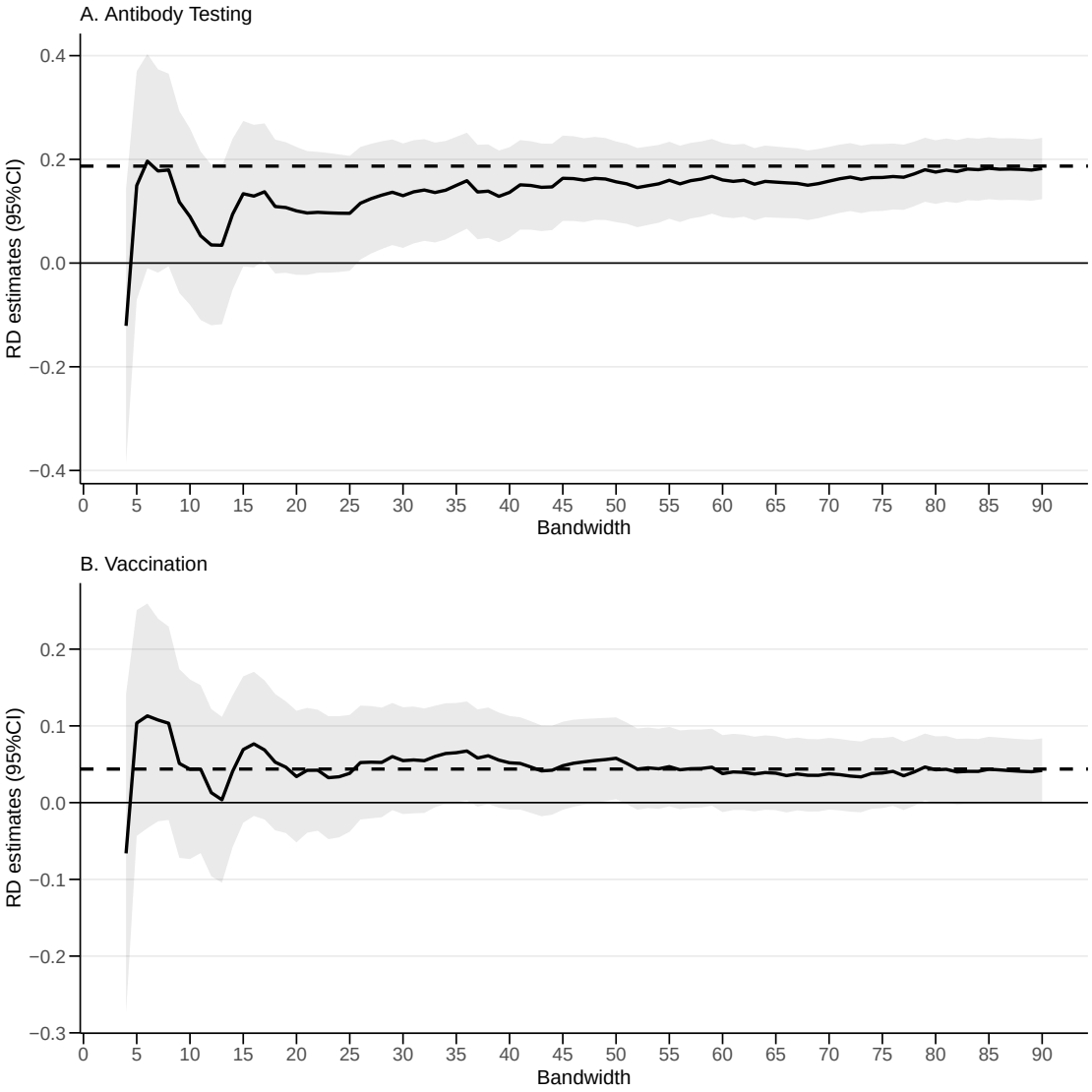

Figure D.3: Nonparametric RD Effect of the Mailing of Vouchers by Bandwidth (Online Survey Data). *Notes*: The horizontal dashed line represents the treatment effect with a model that assumes a linear approximation of age profile, fully interacted with a treatment variable, estimated by ordinary least squares. The shaded area is a 95% confidential interval of the treatment effect estimated by local linear regression with uniform kernel density weight.

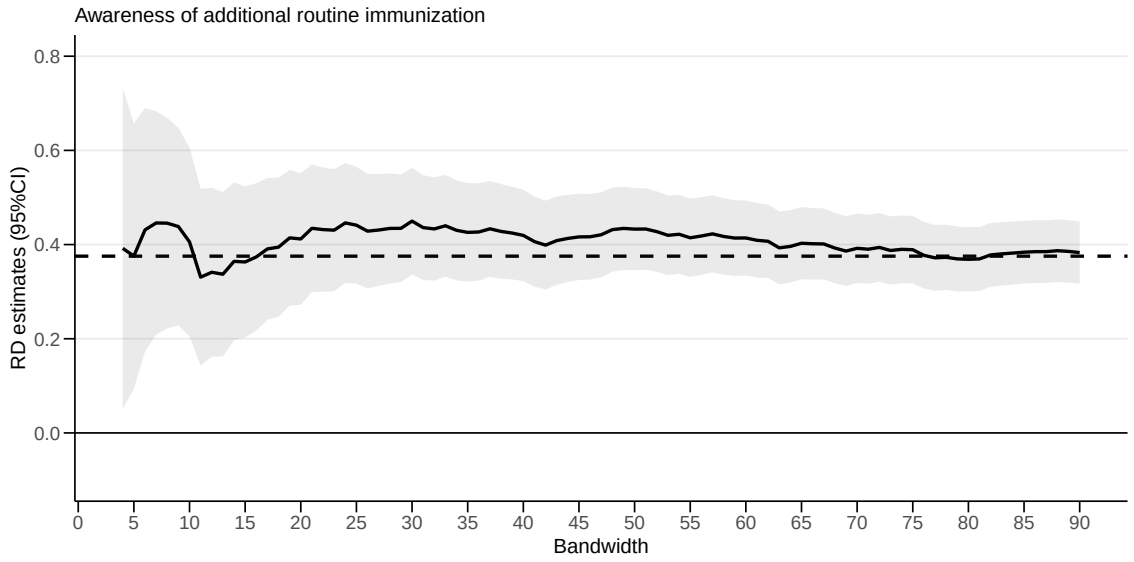

Figure D.4: Nonparametric RD Effect of the Mailing of Vouchers on Awareness of Additional Routine Immunization by Bandwidth (Online Survey Data). *Notes*: The horizontal dashed line represents the treatment effect with a model that assumes a linear approximation of age profile, fully interacted with a treatment variable, estimated by ordinary least squares. The shaded area is a 95% confidential interval of the treatment effect estimated by local linear regression with uniform kernel density weight.

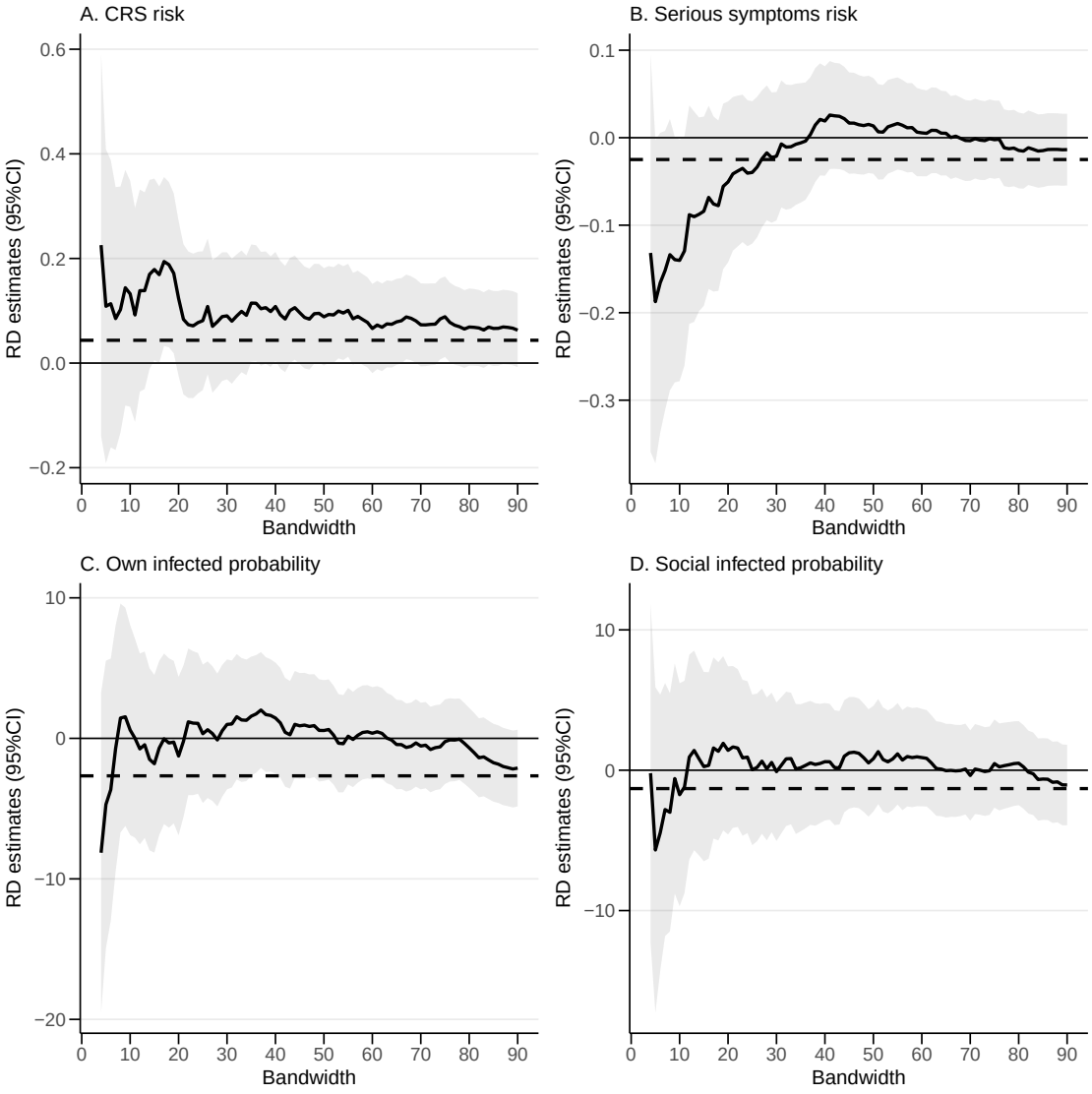

Figure D.5: Nonparametric RD Effect of the Mailing of Vouchers on Vaccination Value by Bandwidth (Online Survey Data). *Notes*: The horizontal dashed line represents the treatment effect with a model that assumes a linear approximation of age profile, fully interacted with a treatment variable, estimated by ordinary least squares. The shaded area is a 95% confidential interval of the treatment effect estimated by local linear regression with uniform kernel density weight.

Appendix E Estimating RD Effect at Placebo Cutoffs

To test the possibility of relative age effects or other factors affecting the estimated RD effect in the study, we estimate the RD effect at an artificial threshold. If there are relative age effects, we expect the RD effect at the placebo cut-off to be statistically significant. We manually set the cut-off value c that determines the treatment status and estimate the model presented in Appendix D with local linear regression (equation (4)).

Figures E.1 and E.2 summarize the estimation results with the local administrative data. The cut-off is April 1, 1964–1976 (the true one is April 1, 1972). Figure E.1 shows the estimated RD effect of mailing vouchers in FY2019 at a given cut-off. The bandwidth is assumed to be 90 days around the threshold ($h = 90$). Figure E.2 shows the estimated RD effect of a shorter interval between two mailings at a given cut-off. The bandwidth is assumed to be 660 days around the threshold ($h = 660$).

Figures E.3–E.5 summarize the estimation results using the online survey. The cut-off is April 1968–1976 (the true one is April 1972). The bandwidth is assumed to be 7 months around the threshold ($h = 7$) in all figures. Figure E.3 shows the estimated RD effects of mailing vouchers in FY2019 at the given cut-off. Figure E.4 shows the estimated RD effect of sending vouchers on the awareness of the additional rubella routine vaccinations at the given cut-off. Figure E.5 shows the estimated RD effect of mailing vouchers on the vaccination value at the given cut-off.

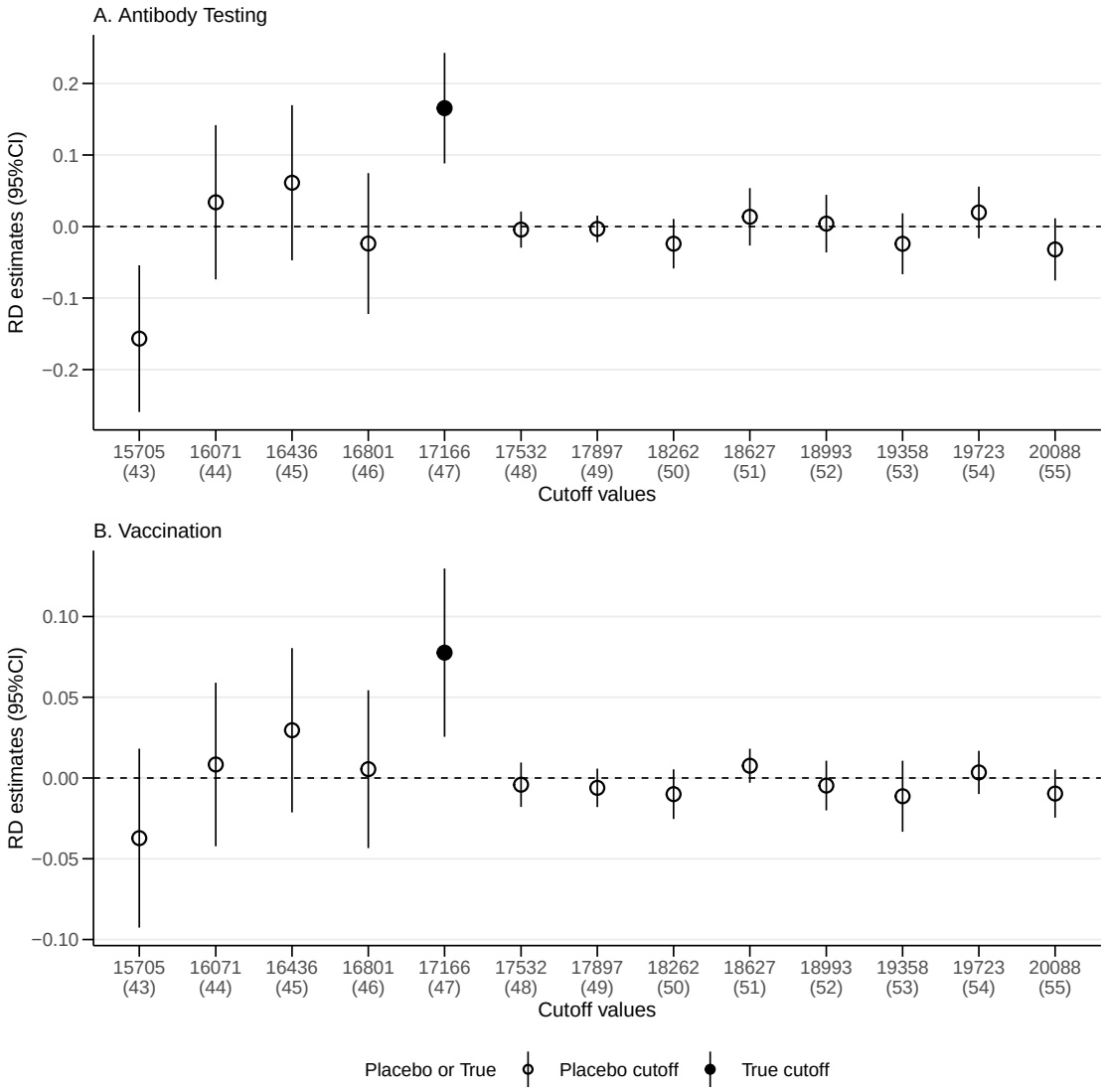

Figure E.1: RD Effect of the First Mailing of Vouchers at Placebo Cut-offs (Local Administrative Data). *Notes*: The unfilled circle represents the RD effect at the artificial threshold. The filled circle also shows the RD effect at the true cut-off. The vertical lines are 95% confidence intervals for the RD effect.

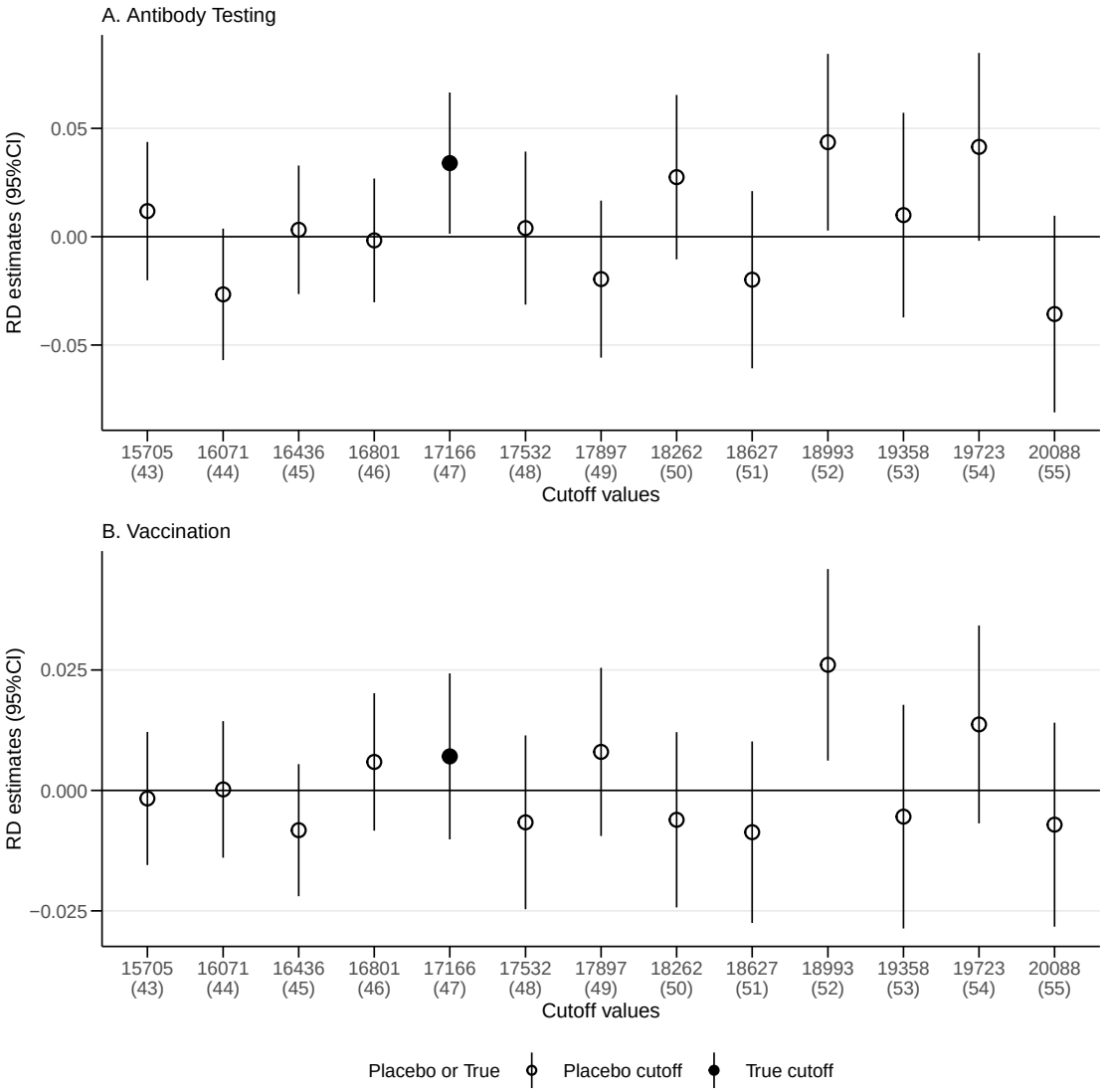

Figure E.2: RD Effect of the Second Mailing of Vouchers at Placebo Cut-offs (Local Administrative Data). *Notes*: The unfilled circle represents the RD effect at the artificial threshold. The filled circle also shows the RD effect at the true cut-off. The vertical lines are 95% confidence intervals for the RD effect.

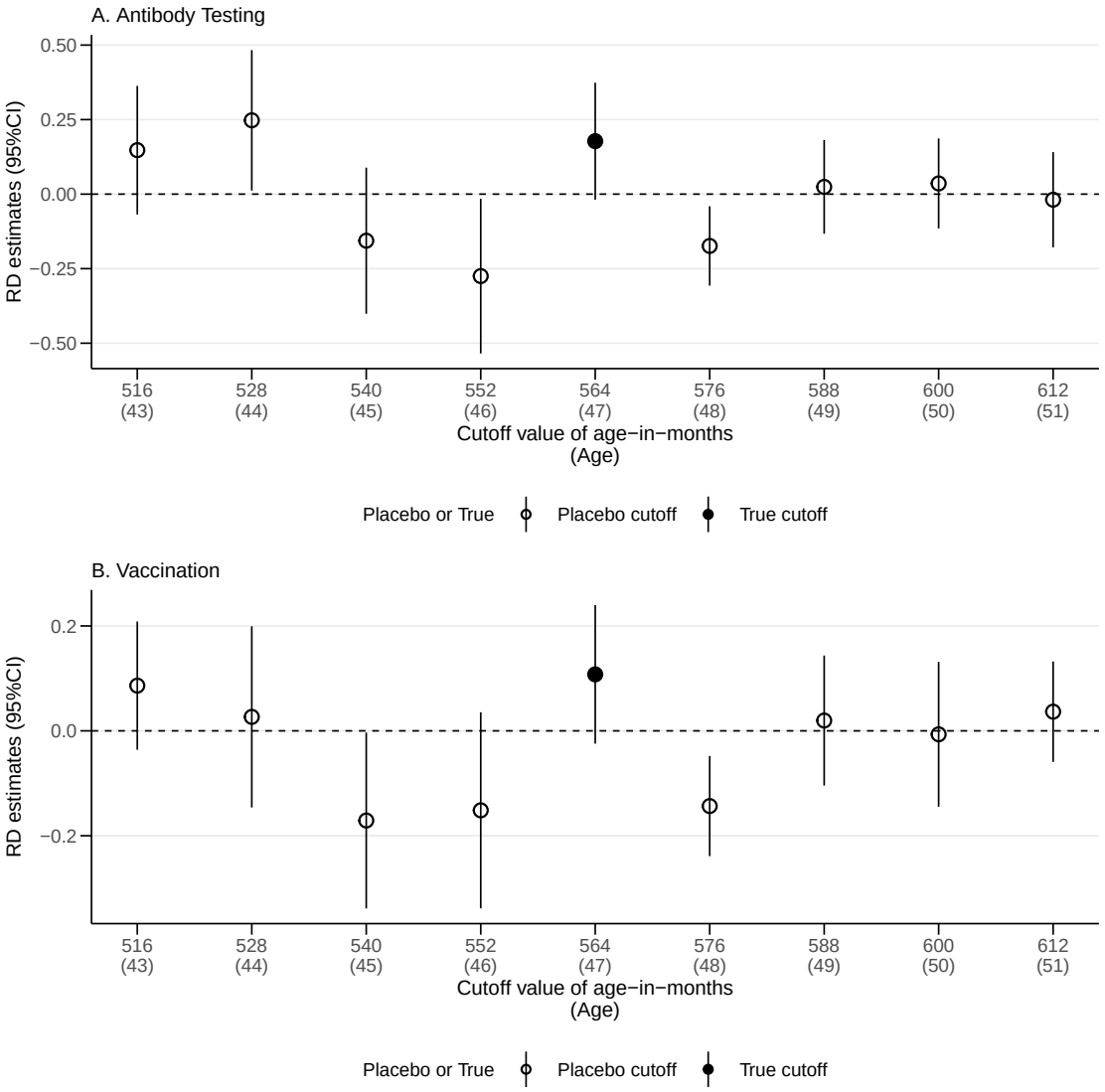

Figure E.3: RD Effect of the Mailing of Vouchers on Antibody Testing and Vaccination at Placebo Cut-offs (Online Survey Data). *Notes*: The unfilled circle represents the RD effect at the artificial threshold. The filled circle also shows the RD effect at the true cut-off. The vertical lines are 95% confidence intervals for the RD effect.

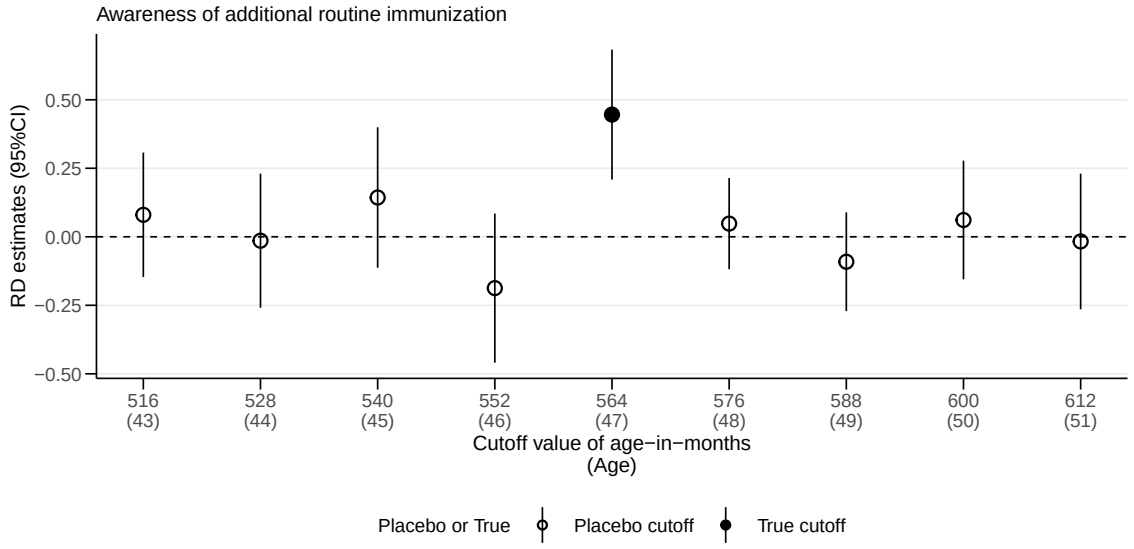

Figure E.4: RD Effect of Mailing Vouchers on Awareness of Additional Routine Immunization at Placebo Cut-offs (Online Survey Data). *Notes*: The unfilled circle represents the RD effect at the artificial threshold. The filled circle also shows the RD effect at the true cut-off. The vertical lines are 95% confidence intervals for the RD effect.

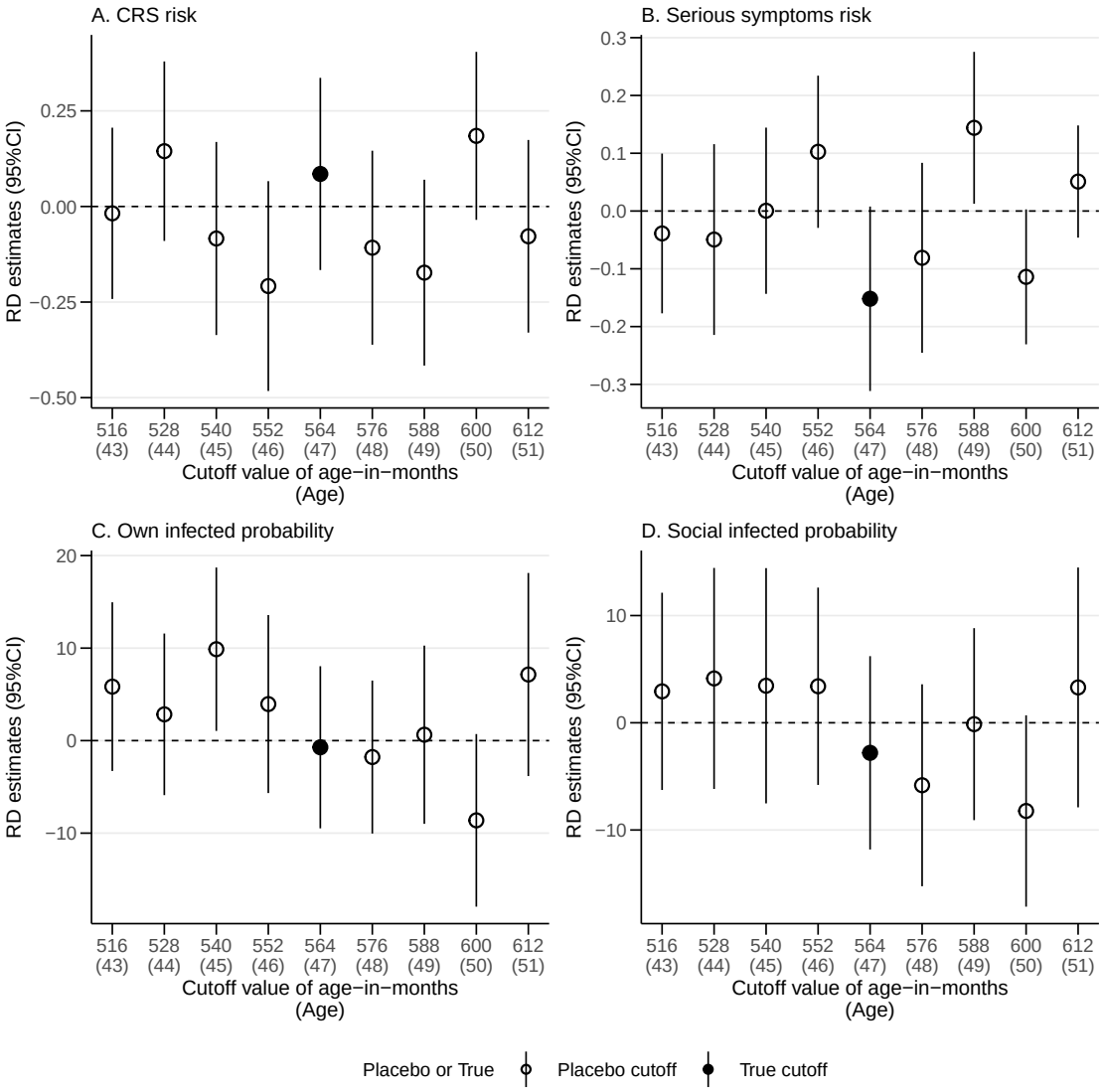

Figure E.5: RD Effect of the Mailing of Vouchers on Vaccination Value at Placebo Cut-offs (Online Survey Data). *Notes*: The unfilled circle represents the RD effect at the artificial threshold. The filled circle also shows the RD effect at the true cut-off. The vertical lines are 95% confidence intervals for the RD effect.

Appendix F Single Interrupted Time-Series Analysis

This section provides an overview of single interrupted time series (SITS) analysis. Using this analysis, we examine the effect of the second mailing of vouchers, a unique policy of the cooperating local government. As those who did not receive the second mailing vouchers are already vaccinated or have antibodies, there is no comparable control group for estimating the second mailing vouchers effect. Thus, SITS analysis uses the pre-intervention time series data to construct a transition without second sending vouchers (counterfactual) and compares it to the actual trend after the intervention.

We create time series data since April 1, 2021, on the number of tests and vaccinations in order to focus solely on the second mailing intervention. As daily data will include many days without antibody testing or vaccination, we create weekly time series data. We analyze the sample separately by treatment status to test the extent to which the effect of the second mailing differs depending on whether a person is eligible for the FY2019 mailing. Figure F.1 shows the constructed time-series data. In both groups, the number of antibody tests jumps immediately after the second mailing, and then decays. Additionally, the number of vaccinations increases after the second mailing for eligible men for the FY2019 mailing, while there is no clear increase in the number of vaccinations for the remaining men.

As time series data are count data, we use a Poisson regression model, which assumes that the observed values follow a Poisson distribution with mean and variance λ. We then construct a model that relates the parameter λ to the explanatory variables. The distribution assumed by this regression model is

$$\Pr(Y = y_w | x_w) = \frac{e^{-\lambda_w} \lambda_w^{y_w}}{y_w!}, \tag{5}$$

where y_w is the number of antibody tests and vaccinations, and x_w is a vector of explanatory variables. We formulate the model of parameter λ by a log-linear model. The SITS analysis uses the following log-linear model:

$$\ln \lambda_w = \alpha + \beta_1 T_w + \beta_2 D_w + \beta_3 T_w D_w, \tag{6}$$

where T_w is a linear time trend and D_w is a dummy variable taking one after the intervention. The model adds a cross term between T_w and D_w, which assumes different time trends before and after the intervention. The intervention effect is captured by the coefficients β_2 and β_3. The coefficient

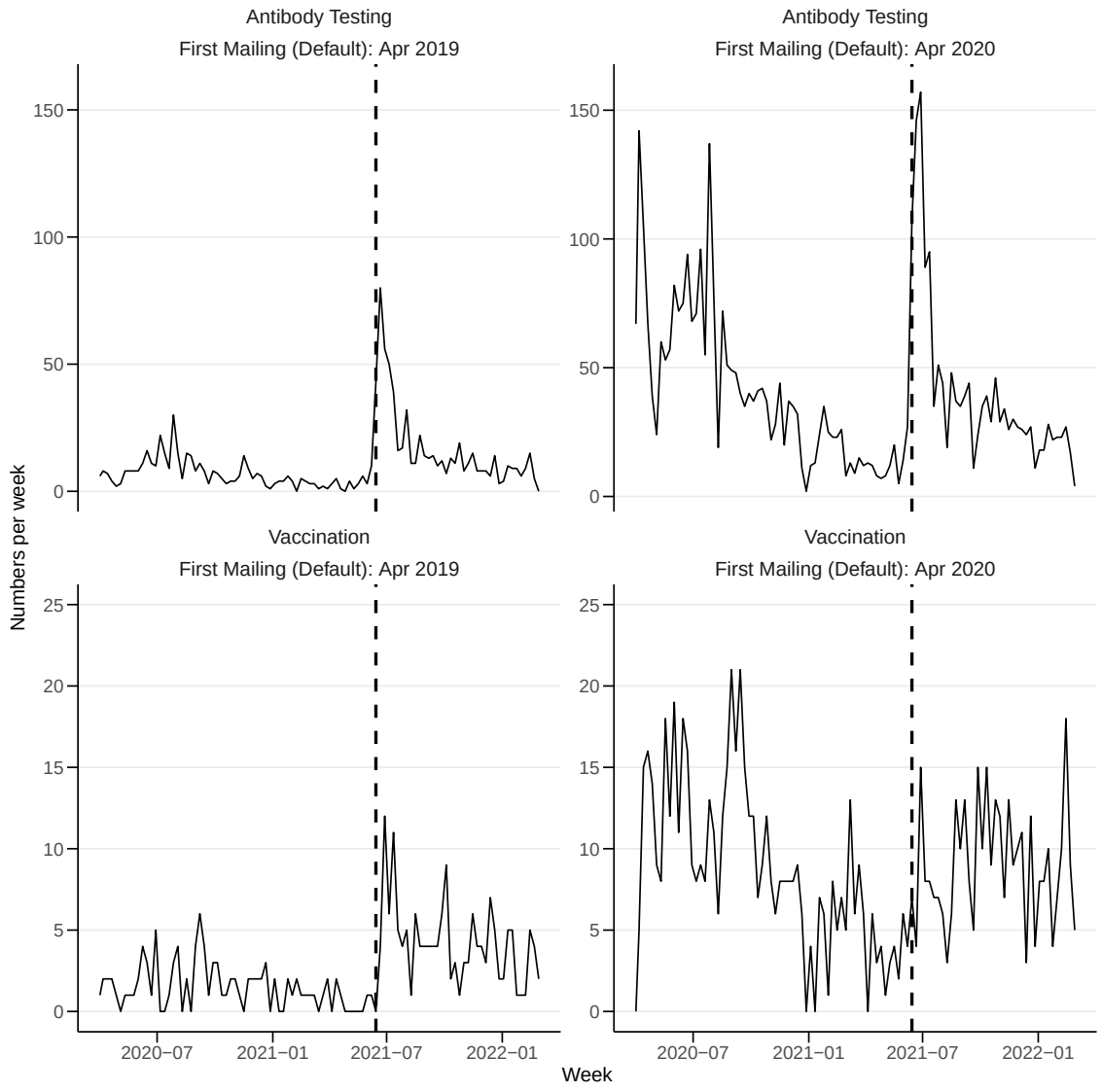

Figure F.1: Number of Weekly Antibody Tests Taken and Number of Vaccinations by Timing of First Mailing

Table F.1: Regression Results of the Segmented Regression Model

Model	Negative binomial				Poisson			
First mailing	Apr 2019		Apr 2020		Apr 2019		Apr 2020	
Outcome	Test	Vaccination	Test	Vaccination	Test	Vaccination	Test	Vaccination
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Intercept)	2.573***	0.879***	4.662***	2.777***	2.511***	0.866***	4.618***	2.730***
	(0.146)	(0.201)	(0.107)	(0.115)	(0.086)	(0.187)	(0.033)	(0.076)
T_w	−0.024***	−0.016***	−0.036***	−0.021***	−0.021***	−0.016***	−0.035***	−0.020***
	(0.004)	(0.006)	(0.003)	(0.003)	(0.003)	(0.006)	(0.001)	(0.002)
D_w	2.628***	1.924***	2.130***	0.710***	2.710***	1.907***	2.189***	0.656***
	(0.237)	(0.303)	(0.180)	(0.207)	(0.133)	(0.278)	(0.069)	(0.150)
$D_w T_w$	−0.034***	−0.003	−0.012*	0.024***	−0.046***	−0.003	−0.022***	0.022***
	(0.009)	(0.011)	(0.007)	(0.008)	(0.005)	(0.009)	(0.003)	(0.006)
Num.Obs.	101	101	101	101	101	101	101	101
AIC	599.3	371.0	828.9	578.4	707.3	371.4	1268.9	604.1

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We estimate a log-linear model with the logarithm of the mean of the distribution as the explained variable by the maximum likelihood method. Models (1)–(4) assume a negative binomial distribution, while models (5)–(8) assume a Poisson distribution. For interpretation of estimates, we need to exponentiate them. For example, the exponentiated coefficient on the Treated period in model (1) is $\exp(2.628) = 13.846$. This means that the average number of antibody tests during the treated period was 13.8 times higher than before.

β_2 is the effect of the second mailing immediately after the intervention, while the coefficient β_3 captures changes in the intervention effect over time. If time is considered the running variable, this model is essentially the same as the RDD. We estimate the Poisson model by a maximum likelihood method.

Models (5)–(8) in Table F.1 show the estimation results. Owing to the property of the Poisson distribution, namely, $E[y_w|x_w] = V[y_w|x_w] = \lambda_w$, it is necessary to exponentiate the coefficient values for interpretation. For example, model (5) uses time series data on the number of antibody tests, which restricts the sample to the FY2019 mailing target. The coefficient value β_2 in this model is 2.7. Thus, in this subsample, the second mailing of vouchers increases the number of antibody tests by 14.9 fold increase ($= \exp(2.7)$) immediately after the second mailing.

The counterfactual presented in our study uses the predicted value of λ_w. We obtain the 95% prediction interval for λ_w as follows. First, the 95% confidence interval for λ_w is derived. Next, the lower bound of the 95% prediction interval is the 2.5 percentile of the Poisson distribution, assuming the mean is the lower bound value of the confidence interval of λ_w. Finally, the upper bound of the 95% prediction interval is the 97.5 percentile of the Poisson distribution, assuming the mean is the upper bound value of the confidence interval of λ_w. The figure shown in the our study represents the cumulative sum of these values converted to take-up.

Generally, count data do not satisfy the property of equal expectation and variance that is implicitly assumed by the Poisson distribution (overdispersion). Therefore, we estimate a model assuming a negative binomial distribution that allows the expected value and variance to differ.

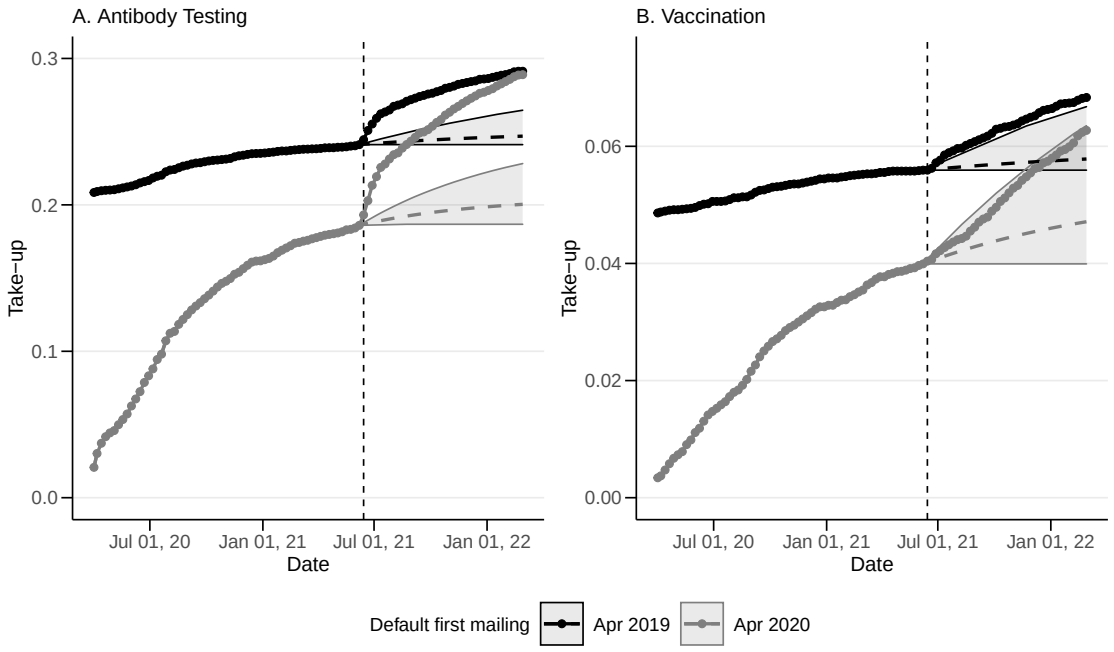

Figure F.2: Time-Series of Observed and Counterfactual Take-Up Using Negative Binomial Model. *Notes*: The dotted solid line represents the transition in observed take-up. The dashed line is the transition of counterfactual take-up. The 95% prediction interval is the shaded area. The counterfactual is the ratio of the cumulative sum of the predicted values of the mean of the negative binomial distribution.

Table F.2: Observed and Counterfactual Take-Up Using the Negative Binomial Model

		Antibody Testing (%)			Vaccination (%)		
Default First Mailing	N	Observed	Counterfactual	Difference	Observed	Counterfactual	Difference
Apr 2020	15004	28.9	20.0	8.9	6.3	4.7	1.6
			[18.7, 22.8]	[6.1, 10.2]		[4.0, 6.4]	[−0.1, 2.3]
Apr 2019	12731	29.1	24.7	4.4	6.8	5.8	1.1
			[24.1, 26.5]	[2.7, 5.0]		[5.6, 6.7]	[0.2, 1.2]
Difference in treatments				4.4			0.5
				[1.1, 7.6]			[−1.3, 2.1]

Note: We show the take-up of antibody testing and vaccination as of February 28, 2022, the last day of our study period. The counterfactual is the take-up under the situation where there is no second mailing of vouchers. We constructed the 95% prediction interval for the counterfactual at the 2.5 and 97.5 percentiles of the negative binomial distribution with the mean replaced by the predictions of the SITS model, while accounting for estimation uncertainty. We obtain the dispersion parameter for the variance of the distribution from the SITS model estimate, but we do not consider the uncertainty in that estimate. The difference between the observed and counterfactual take-up for each group indicates the effect of the second mailing. The difference in the effect of the second mailing for each group indicates the effect of shortening the time to sending by 1 year.

Table F.3: Regression Results of the INGARCH Model

Model	Negative binomial				Poisson			
First mailing	Apr 2019		Apr 2020		Apr 2019		Apr 2020	
Outcome	Test	Vaccination	Test	Vaccination	Test	Vaccination	Test	Vaccination
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Intercept)	1.554***	0.426***	4.593***	1.040	1.708***	0.541***	5.000***	1.040**
	(0.451)	(0.143)	(0.713)	(0.646)	(0.234)	(0.147)	(0.271)	(0.479)
T_w	−0.015**	−0.009**	−0.035***	−0.008	−0.017***	−0.010***	−0.038***	−0.008**
	(0.007)	(0.004)	(0.006)	(0.005)	(0.004)	(0.004)	(0.003)	(0.004)
D_w	1.658***	0.959***	1.963***	0.295	1.739***	1.079***	2.150***	0.295**
	(0.442)	(0.285)	(0.325)	(0.181)	(0.231)	(0.219)	(0.123)	(0.135)
$D_w T_w$	−0.022*	0.000	−0.011	0.007	−0.020***	0.003	−0.013***	0.007
	(0.012)	(0.005)	(0.009)	(0.007)	(0.007)	(0.006)	(0.004)	(0.005)
Model order								
p	3	1	1	1	4	1	4	1
q	3	2	3	1	3	3	3	1
Num.Obs.	101	101	101	101	101	101	101	101
AIC	578.7	366.0	818.1	576.3	599.3	363.2	1095.3	594.7

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. We estimate a log-linear model with the logarithm of the mean of the distribution as the explained variable by the maximum likelihood method. Models (1)–(4) assume a negative binomial distribution, while models (5)–(8) assume a Poisson distribution. For interpretation of estimates, we need to exponentiate them. For example, the exponentiated coefficient on the Treated period in model (1) is $\exp(1.658) = 5.249$. This means that the average number of antibody tests during the treated period was 5.25 times higher than before. We choose model orders p and q, which account for autocorrelation in the observed data, to minimize the Akaike information criterion (AIC). The model order p assumes that the mean of the distribution depends on the observations up to p periods ago. Model order q assumes that the mean of the distribution depends on the mean of the distribution up to q periods ago. The coefficients on the lag of the observed values and the lag of the conditional mean are removed from the table.

Models (1)–(4) in Table F.1 show the estimation results. Figure F.2 shows the counterfactual constructed with this model. Table F.2 summarizes the counterfactual analysis. These results are quantitatively similar to results assuming a Poisson distribution.

The standard regression models discussed above assume no autocorrelation of the error terms. However, in all the models presented in Table F.1, the residuals are autocorrelated (Figures F.3 and F.4). Thus, we estimate an integer-valued GARCH (INGARCH) model, allowing the parameter λ_w to depend on the past observations Y_{w-kl} and the past parameters λ_{w-l} (Liboschik et al., 2017). The SITS analysis uses the following INGARCH model:

$$\ln \lambda_w = \alpha + \beta_1 T_w + \beta_2 D_w + \beta_3 T_w D_w + \sum_{k=1}^{p} \delta_k \ln(y_{w-k} + 1) + \sum_{l=1}^{q} \gamma_l \ln \lambda_{w-l} \qquad (7)$$

Table F.3 shows the estimation results for the INGARCH model. The model orders p and q are chosen based on the AIC. To begin, we estimate the equation (7) with all combinations of $p \in \{1, 2, 3, 4\}$ and $q \in \{1, 2, 3\}$. Then, we compute AIC for each model and choose the model with the smallest AIC. The estimated coefficient values differ significantly from those in Table F.1, but the direction of the coefficients is similar. We also observe little autocorrelation in the residuals of the INGARCH model (Figures F.5 and F.6). Figures F.7 and F.8 compare the counterfactual

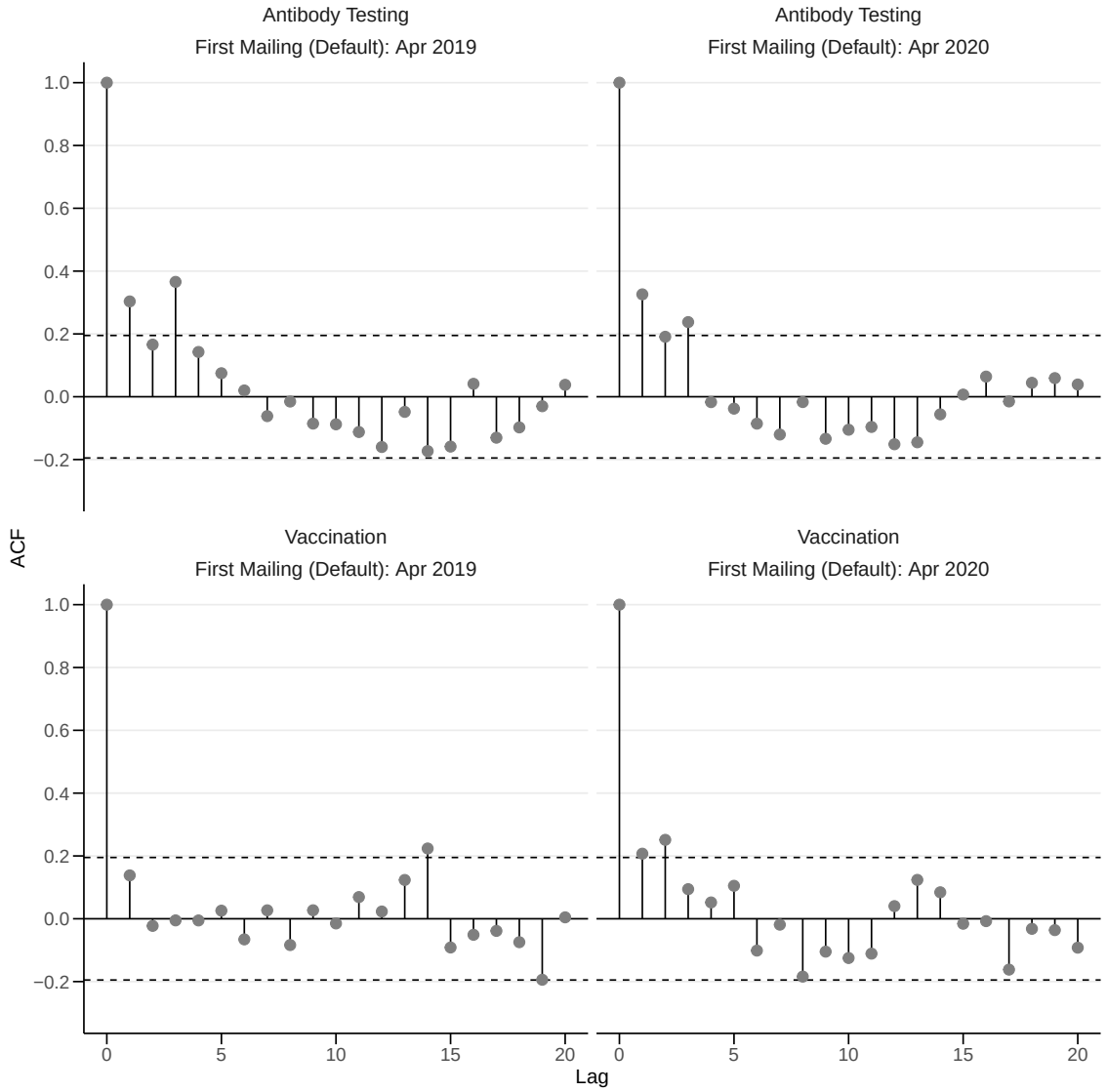

Figure F.3: Autocorrelation Function of Residuals of the Poisson Model. *Notes*: The two dashed horizontal lines represent the 95% confidential interval of the autocorrelation coefficient, assuming no correlation. If an autocorrelation coefficient crosses the dashed line, then it is statistically significant.

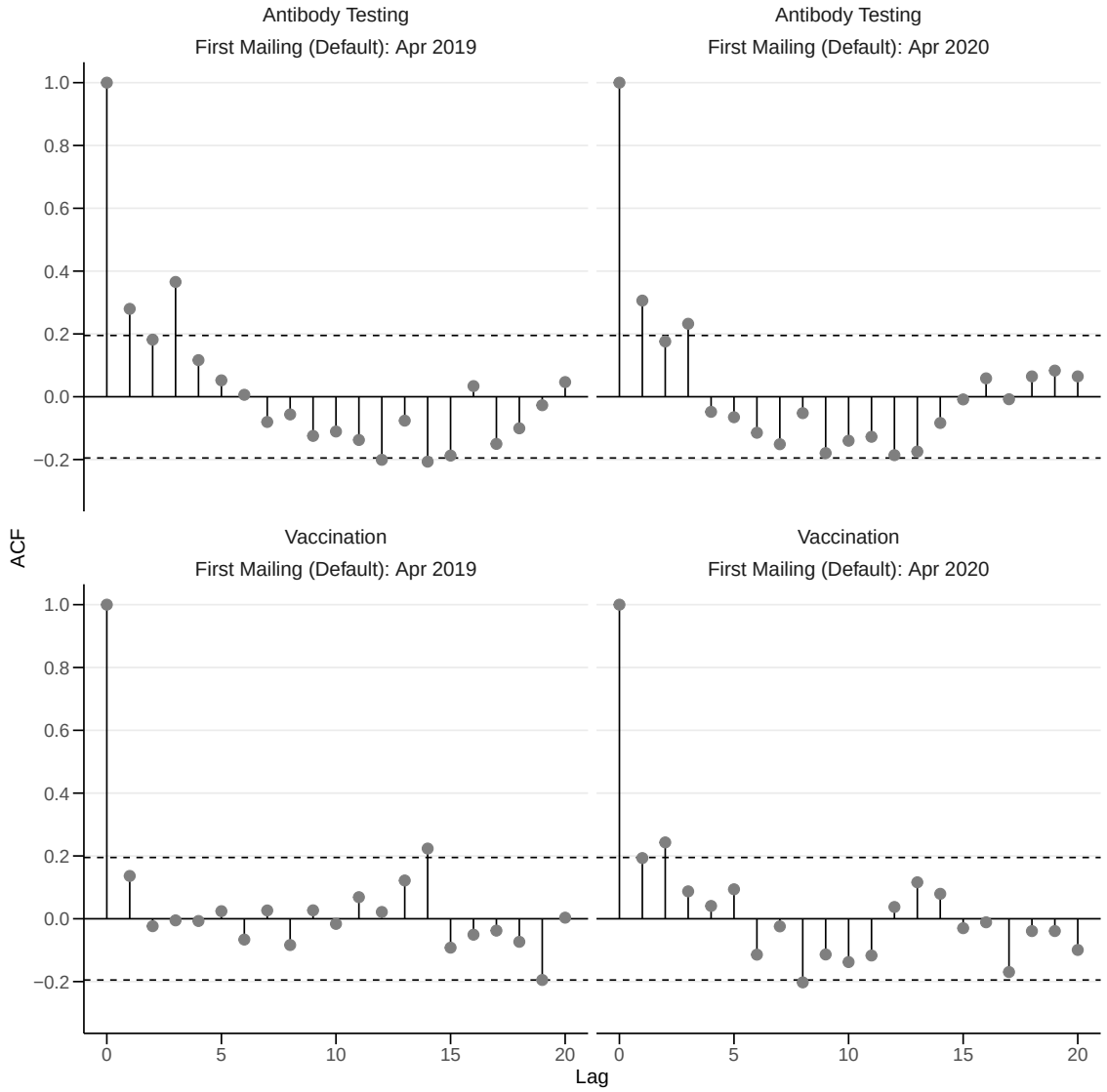

Figure F.4: Autocorrelation Function of Residuals of the Negative Binomial Model. *Notes*: The two dashed horizontal lines represent the 95% confidential interval of the autocorrelation coefficient, assuming no correlation. If an autocorrelation coefficient crosses the dashed line, then it is statistically significant.

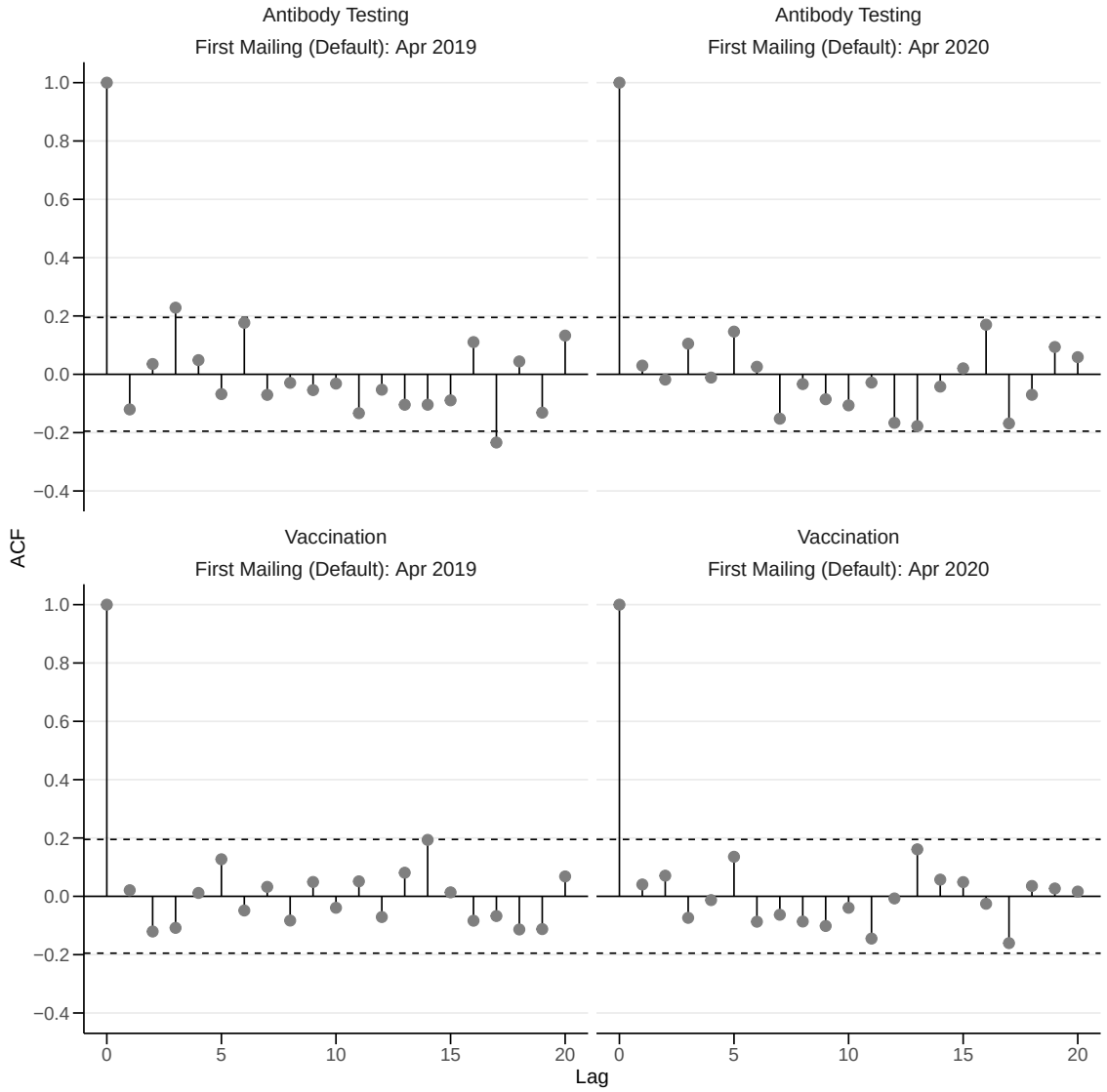

Figure F.5: Autocorrelation Function of Residuals of the INGARCH Model with Poisson Distribution. *Notes*: The two dashed horizontal lines represent the 95% confidential interval of the autocorrelation coefficient, assuming no correlation. If an autocorrelation coefficient crosses the dashed line, then it is statistically significant.

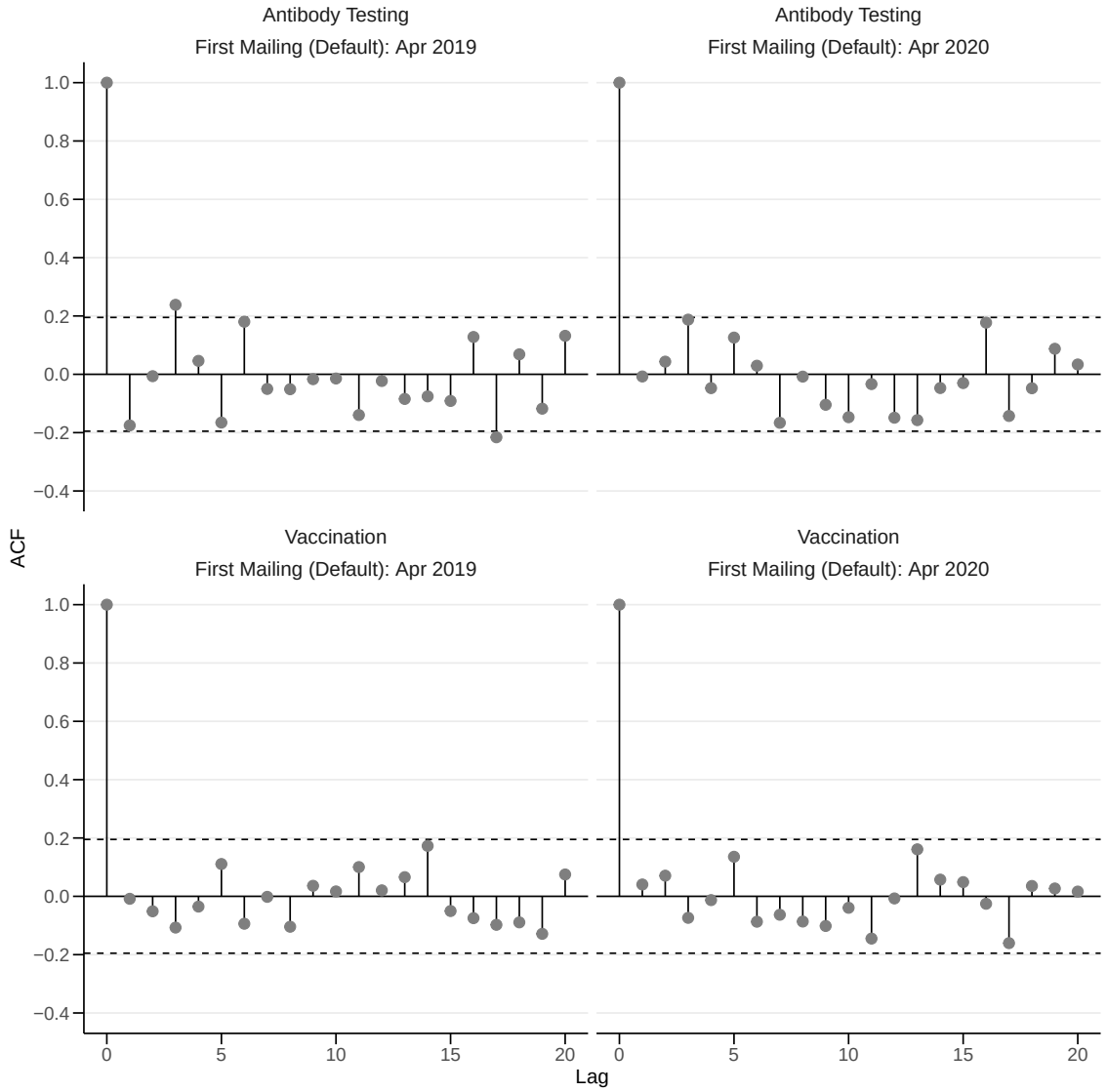

Figure F.6: Autocorrelation Function of Residuals of the INGARCH Model with Negative Binomial Distribution. *Notes*: The two dashed horizontal lines represent the 95% confidential interval of the autocorrelation coefficient, assuming no correlation. If an autocorrelation coefficient crosses the dashed line, then it is statistically significant.

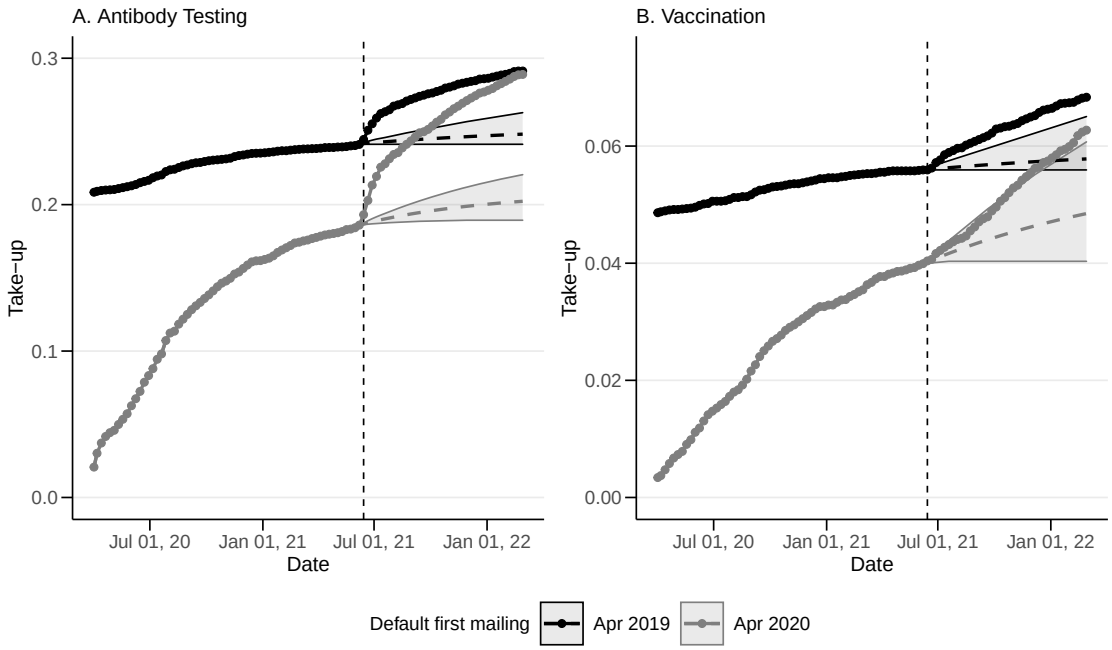

Figure F.7: Time-Series of Observed and Counterfactual Take-Up Using the INGARCH Model with Poisson Distribution. *Notes*: The dotted solid line represents the transition in the observed take-up. The dashed line is the transition of the counterfactual take-up. The 95% prediction interval is the shaded area. The counterfactual is the ratio of the cumulative sum of the predicted values of the mean of the Poisson distribution.

Table F.4: Observed and Counterfactual Take-Up Using the INGARCH Model

		Antibody Testing (%)			Vaccination (%)		
Default First Mailing	N	Observed	Counterfactual	Difference	Observed	Counterfactual	Difference
A. Poisson Distribution							
Apr 2020	15004	28.9	20.2	8.7	6.3	4.8	1.4
			[18.9, 22.0]	[6.8, 10.0]		[4.0, 6.1]	[0.2, 2.2]
Apr 2019	12731	29.1	24.8	4.3	6.8	5.8	1.1
			[24.1, 26.3]	[2.8, 5.0]		[5.6, 6.5]	[0.3, 1.2]
Difference in treatments				4.4			0.4
				[1.8, 7.1]			[−1.0, 1.9]
B. Negative Binomial Distribution							
Apr 2020	15004	28.9	20.2	8.7	6.3	4.8	1.4
			[18.7, 23.4]	[5.5, 10.2]		[4.0, 6.4]	[−0.1, 2.3]
Apr 2019	12731	29.1	24.9	4.2	6.8	5.8	1.1
			[24.1, 27.2]	[1.9, 5.0]		[5.6, 6.5]	[0.4, 1.2]
Difference in treatments				4.5			0.4
				[0.5, 8.2]			[−1.3, 1.9]

Note: We show the take-up of antibody testing and vaccination as of February 28, 2022, the last day of our study period. The counterfactual is the take-up under the situation where there is no second mailing of vouchers. We constructed the 95% prediction interval for the counterfactual at the 2.5th and 97.5th percentiles of the Poisson (Panel A) or negative binomial (Panel B) distribution with the mean replaced by the predictions of the SITS model. When using a negative binomial distribution, we obtain the dispersion parameter for the variance of the distribution from the SITS model estimate, but we do not consider the uncertainty in that estimate. The difference between the observed and counterfactual take-up for each group indicates the effect of the second mailing. The difference in the effect of the second mailing for each group indicates the effect of shortening the time to sending by 1 year.

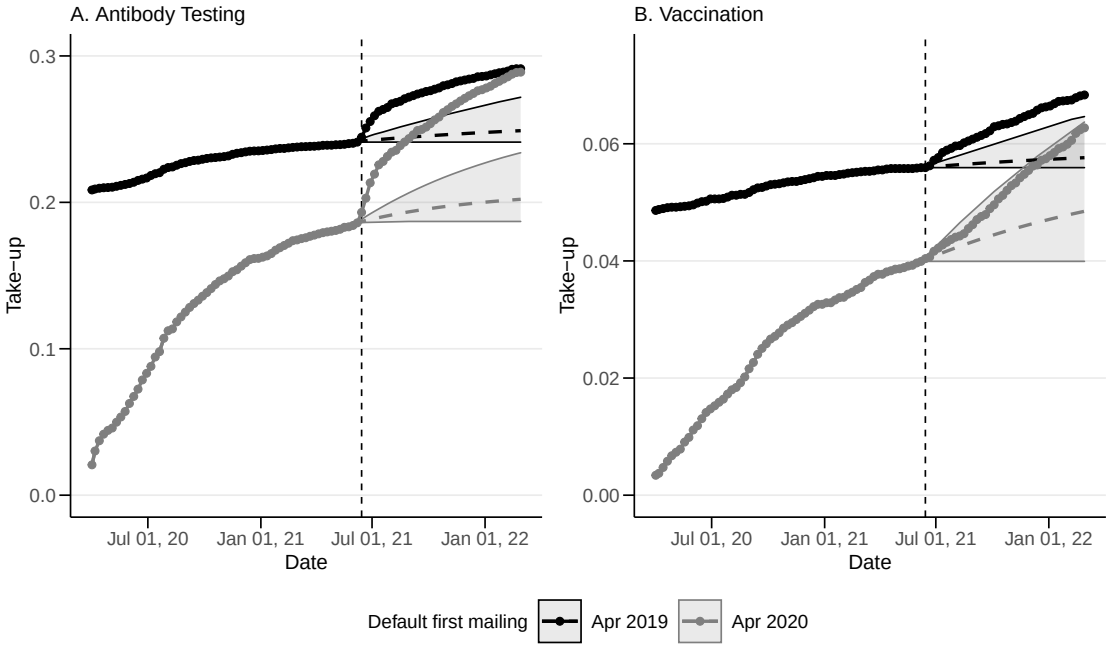

Figure F.8: Time-Series of Observed and Counterfactual Take-Up Using the INGARCH Model with Negative Binomial Distribution. *Notes*: The dotted solid line represents the transition in the observed take-up. The dashed line is the transition of the counterfactual take-up. The 95% prediction interval is the shaded area. The counterfactual is the ratio of the cumulative sum of the predicted values of the mean of the negative binomial distribution.

with the actual trend. We summarize the counterfactual analysis in Table F.4.