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Information to the Wife, Action by the Husband: Spousal exposure to a public health railway ad and men's preventive behavior*

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Abstract

While public health interventions have traditionally targeted decision-makers themselves, this paper shows that targeting their household members may be more effective in the context of Japan's rubella vaccination policy. Japan introduced a program which subsidizes preventive behaviors from antibody testing through vaccination targeting middle-aged men. We overcome a key limitation in measuring public health advertising effects by combining a natural experiment based on a Tokyo railway advertisement campaign with individual-level data linking exposure opportunity, viewing the advertisement, and offline preventive behavior. We find no statistically significant effect of eligible men's own use of the train lines displaying the campaign on antibody testing take-up. In contrast, their spouses' use increased antibody testing take-up by 1.4 percentage points in the short run and 4.54 percentage points in the long run. Instrumental variable estimates show that the direct effect of viewing the advertisement was not statistically significant, suggesting that spousal persuasion plays a key role. These results imply that expanding the targeting of interventions to third parties can be an effective policy option.

Keywords: Targeting, Intra-household decision making, Preventive behavior, Railway advertisement, Natural experiment
JEL classification: D13, D64, I12, I18, M37

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1 Introduction

In the design of public policies including public health, targeting strategy—who should receive an intervention—is a central challenge. Previous research has assumed that behavior is determined by the individual alone and has selected targets based on the decision-maker's characteristics (e.g., Athey et al., 2025b; Ida et al., 2026). However, other household members can plausibly influence the decision-making process (e.g., Fadlon and Nielsen, 2019). For example, they may persuade or negotiate with the decision-maker (e.g., Nyqvist et al., 2024). In such cases, potentially effective targets extend beyond the decision-maker to include other household members. This study shows that, in the context of vaccination, there are settings in which reaching other household members can be more effective.

Expanding targeting becomes practically necessary when direct interventions are no longer effective. For example, as time passes since an initial intervention, additional direct interventions may not sufficiently change behavior as individuals who are responsive to the intervention have already changed their behavior, while those who are less responsive to the intervention are left behind. One solution is to revise the content of subsequent interventions. Another is to expand targets beyond the decision-maker alone.

A series of papers on intra-household decision-making suggests conditions under which interventions aimed at someone other than the decision-maker can be effective. Previous research, predominantly in low- and middle-income countries, shows that when mothers rather than fathers control how resources are allocated, children's health and educational outcomes improve (e.g., Thomas, 1990; Lundberg et al., 1997; Duflo, 2003; Qian, 2008; Nyqvist and Jayachandran, 2017).[1] These patterns suggest gender differences in altruism toward future generations. Hence, in contexts where behavior raises the welfare of future generations, whom to target within the household can shape intervention effectiveness. Specifically, interventions directed toward the spouse may be more effective than those directed toward the husband. Even when the husband is the decision-maker, targeting the spouse may lead to changes in the husband's behavior if she has sufficient ability and opportunity to persuade or negotiate.

[1] The concentration of evidence in low- and middle-income countries reflects the availability of experiments and clean identification strategies, not a lack of relevance in high-income countries. In fact, Lundberg et al. (1997) offers rare evidence from a high-income country, using reforms to the UK's child benefit program.

We focus on rubella control measures in Japan. Japan has experienced several recent rubella outbreaks; one reason is that men in their 40s and 50s have relatively low antibody prevalence because of institutional reasons. During these outbreaks, women were reportedly infected during early pregnancy and gave birth to children with visual or hearing impairments (National Institute of Infectious Diseases, 2019). To address this problem, the Japanese government implemented a program from April 2019 to March 2025 that offered free rubella vaccination to men in their 40s and 50s, for whom the risk of severe illness from rubella is extremely small. That is, rubella vaccination of men in this age group primarily generates social benefits—especially for future generations—rather than private health gains. Therefore, knowing whom to target within the household is critical for this policy.

To allocate vaccines efficiently, the government, through its rubella vaccination program, introduced free antibody testing prior to vaccination and offered vaccination only to those whose test results were negative. As of March 2023, the nationwide antibody testing rate remained at around 28%, partly owing to the COVID-19 pandemic, and the slow growth in antibody testing has become a major policy concern.[2]

To solve this issue, our research team has collaborated with the Ministry of Health, Labour and Welfare (MHLW) to develop communication materials for the program (Kato et al., 2024). One such material is a Japanese-language video advertisement which communicates the potential adverse consequences of a rubella infection for future generations and explains how to use the program. In November 2023, we displayed it on in-car digital signage on select railway lines in the South Kanto region (Tokyo, Saitama, Chiba, and Kanagawa). We empirically evaluate the effectiveness of this railway advertisement.

We use the large railway network in the South Kanto region, especially in Tokyo, as a "natural laboratory." It is among the largest railway networks in the world and is heavily used for commuting to work and school. In this network, multiple railway companies

[2]The data are from the Ministry of Health, Labour and Welfare. The policy goal was to increase antibody prevalence among men in their 40s and 50s (the target group) from 80% to 90%, which roughly corresponds to achieving a 10% vaccination rate through the program. According to data from the National Institute of Infectious Diseases, antibody prevalence among men in their 40s and 50s is about 80%, implying that 20% of those who undergo antibody testing are eligible for vaccination (Figure 1). A back-of-the-envelope calculation suggests that an antibody testing rate of 50% is required to achieve a 10% vaccination rate through the program. As of March 2023, the vaccination rate was only 6% (21% of the antibody testing rate at that time). From the perspective of vaccination uptake, low utilization of the program continues to be a worrying concern.

operate lines, and even between the same origin and destination stations, commuters may choose among several routes. Moreover, many lines have in-car digital signage, but we displayed the video advertisement on only a subset of those lines operated by JR East. Accordingly, our basic research design compares users and non-users of campaign lines after controlling for observable attributes such as residential and workplace locations. In other words, this study interprets commuting on lines that carried the campaign as an intent-to-treat (ITT) measure of opportunities to view the advertisement and examines its effects on preventive behavior.

We surveyed approximately 10,000 men online; the respondents were men who were eligible for the rubella vaccination program and resided in the South Kanto region at the time of the advertisement campaign. We collected information on commuting routes for both the eligible men and their spouses, as well as on eligible men's viewing of the video advertisement and their preventive behaviors (antibody testing and vaccination). Our main outcome measures are preventive behaviors within the first month after the intervention. Specifically, we focus on antibody testing which is the first step of the rubella vaccination program. We also followed up on preventive behaviors over the first year following the intervention in order to examine the persistence of the advertisement effect.

We found that treatment effects reversed depending on who was exposed to the information. When eligible husbands commuted on railway lines that carried the video advertisement, antibody testing did not increase. By contrast, when spouses used those lines, husbands' antibody testing increased and remained higher over time. The short-run effect over the first month after the intervention was 1.40 percentage points; the long-run effect over the first year after the intervention was 4.54 percentage points. Both short- and long-run effects are statistically significant and robust to alternative covariate adjustments and statistical methods. These results are consistent with spouses sharing the advertisement information at home and encouraging their husbands to get tested. They suggest that simple information provision may be insufficient: within-household persuasion, reminders, and decision support likely mattered.

Our findings offer a new perspective on targeting policies for behavioral interventions. Recent advances in machine learning have facilitated the analysis of treatment-effect heterogeneity based on observable individual characteristics (Wager and Athey, 2018; Murakami et al., 2022). Following this trend, recent studies have addressed which types of decision-makers should be targeted (e.g., Athey et al., 2025a,b; Ida et al., 2026). Other studies have examined which types of individuals are more likely to change their behavior in response to interventions (e.g., Alatas et al., 2016; Finkelstein and Notowidigdo, 2019). Both strands of the literature investigate the characteristics of the decision-makers. We argue for expanding the target of intervention beyond decision-makers and including even their closely related counterparts.

This study also speaks to the literature on intra-household decision making (Jayachandran and Voena, 2026). As noted above, directing resources to mothers, rather than fathers, is known to improve children's welfare (e.g., Thomas, 1990; Lundberg et al., 1997; Duflo, 2003; Pitt et al., 2003; Qian, 2008; Nyqvist and Jayachandran, 2017; Armand et al., 2020; Dizon-Ross and Jayachandran, 2023; Nyqvist et al., 2024). Such evidence contributed to the design of programs that pay transfers directly to mothers—Mexico's PROGRESA is a leading example (e.g., Schultz, 2004). Our findings are consistent with a difference in altruism between spouses that previous research highlights. Husbands appear to prioritize private payoffs over benefits to future generations, which helps explain why the video advertisement alone did not change their behavior. By contrast, wives' stronger concern for future generations fits a story in which the advertisement supplied a hook for persuasion at home. Overall, the analysis suggests that insights from intra-household decision making can inform targeting of public-health interventions.[3]

Finally, we offer insights into the evaluation of advertising effectiveness. By linking exposure opportunities, advertisement viewing, and subsequent offline preventive behavior for the same individuals, this study addresses a key limitation in measuring the effects of public-health advertising. Randomized controlled trials (RCTs) are widely used to measure the effects of advertising (e.g., Hu et al., 2007; Blake et al., 2015; Sahni, 2016), typically by randomizing exposure opportunities. Comparisons between treatment and control groups in such settings identify the effect of exposure opportunities—an intention-to-treat (ITT) effect—which generally understates the true effect of advertising (Johnson et al., 2017).[4] In contrast, analysis of observational data, including viewing information, poses

[3]As in our study, Katagiri et al. (2025) show that reminders directed at employees' spouses rather than at employees themselves increase employees' willingness to undergo additional health screenings.

[4]The ITT effect incorporates both the true effect of advertising and effect of exposure opportunities on

an endogeneity problem because viewing is a choice correlated with unobservables (Assael et al., 2021). In Section 7, where we discuss the mechanisms behind our main results, we combine a natural experiment with observational survey data within an instrumental-variable framework to estimate the causal effect of actually viewing the advertisement. We exploit a large railway network in which exposure opportunities vary exogenously with predetermined commuting routes, and we collect data on viewing and outcomes through an online survey. We then use the intensity of exposure opportunities (daily use of treated railway lines) as an instrument for viewing the advertisement to estimate its causal effect on preventive behavior.

The remaining paper is organized as follows. Section 2 describes the institutional background of rubella policy in Japan and our railway advertisement campaign. Sections 3 and 4 outline our empirical strategy and data. Section 5 presents our main results on the effects of exposure opportunities to the campaign. Section 6 examines the long-run effects of the campaign. Section 7 discusses the mechanisms using instrumental-variable estimates of the effect of ad viewing. Section 8 concludes.

2 Background and Study Setting

2.1 Rubella Vaccination Program in Japan

In 2013 and 2018, Japan experienced major rubella outbreaks, driven by the introduction of viruses originating from Southeast Asia and concentrated among men in their 40s and 50s (National Institute of Infectious Diseases, 2019). The most serious concern in these outbreaks was not infection among middle-aged men per se but transmission via droplets from infected men to women in the early stages of pregnancy.

Men in their 40s and 50s carry an extremely low risk of severe illness from rubella infection. However, if a woman in early pregnancy becomes infected with rubella, she may give birth to a child with congenital rubella syndrome (CRS), which can involve visual and auditory impairments. Indeed, during the 2013 outbreak, 45 CRS cases were reported

viewing rates. Not everyone assigned an opportunity to be exposed to the ad will actually view it, and conversely, some individuals without such opportunities may still see the ad. In such cases, the difference in viewing rates between treatment and control groups is attenuated, and the ITT effect understates the true effect of the advertisement.

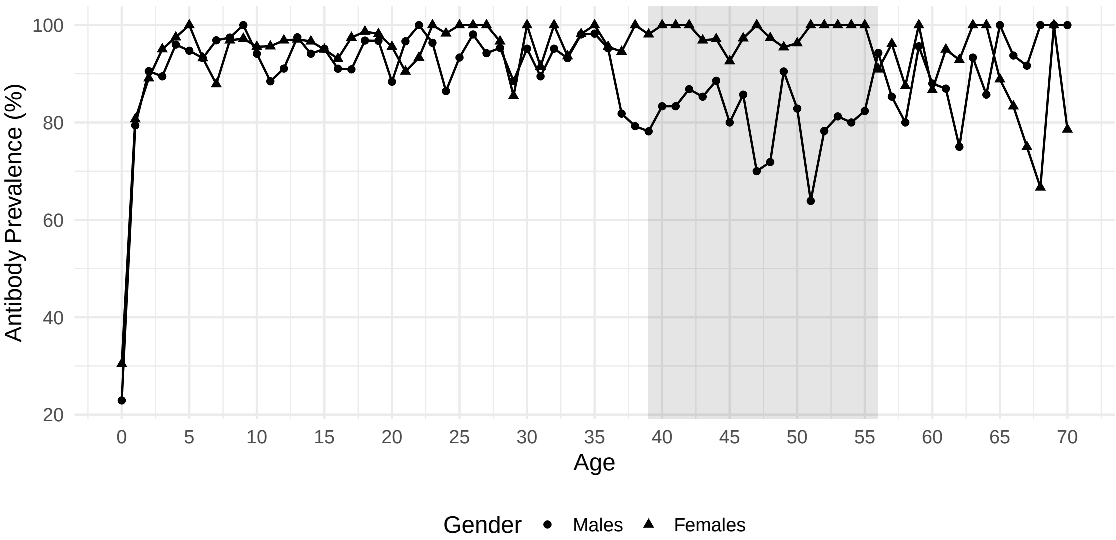

Figure 1. Antibody Prevalence of Rubella by Age and Gender in Japan.
Data source: 2018 National Epidemiological Surveillance of Vaccine-Preventable Diseases, National Institute of Infectious Diseases.

(National Institute of Infectious Diseases, 2019). To prevent such serious outcomes, it was essential for Japan to establish herd immunity against rubella.

The outbreaks were rooted in a pitfall of antibody prevalence owing to institutional reasons. To reach herd immunity against rubella, all age cohorts in Japan must have at least 90% antibody prevalence (Kinoshita and Nishiura, 2016). However, according to the data from the National Institute of Infectious Diseases (NIID), antibody prevalence among men in their 40s and 50s is only about 80%, noticeably lower than that of other cohorts (Figure 1). The main reason is historical: women in these cohorts were eligible for routine vaccination against rubella, whereas men were not. For men in their 40s and 50s to acquire antibodies, they essentially needed to contract rubella naturally (Nishiura et al., 2015).

Since the introduction of routine vaccination for girls has reduced rubella circulation over time (Kinoshita and Nishiura, 2016), men in their 40s and 50s are less likely to acquire antibodies through natural infection. Therefore, raising antibody prevalence among men in their 40s and 50s from 80% to 90% has become a concrete policy target for achieving herd immunity against rubella.[5]

Accordingly, the government introduced a nationwide program that offered free anti-

[5] In September 2025, the WHO declared that rubella was no more endemic in Japan (WHO, 2025).

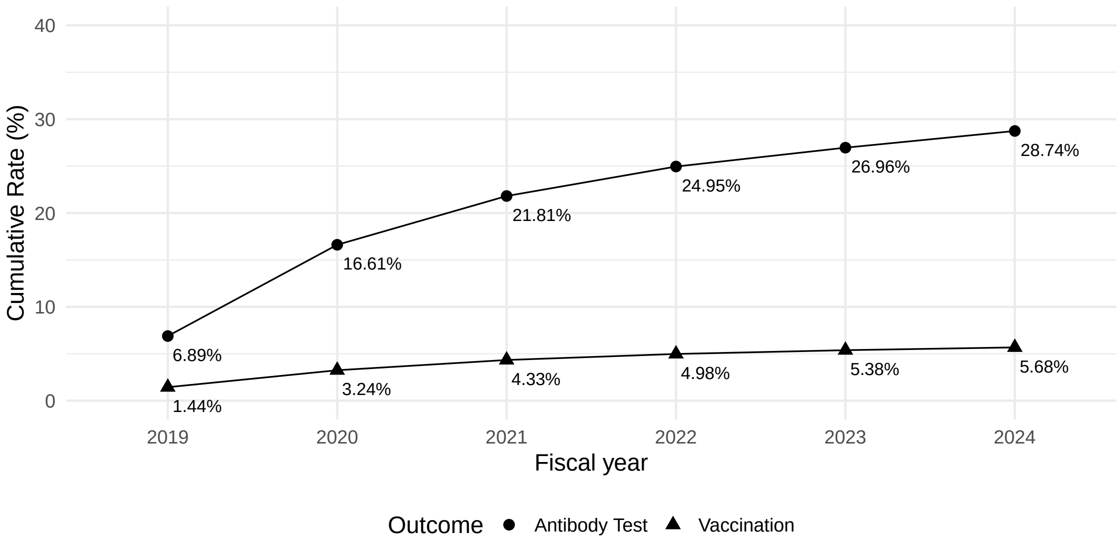

Figure 2. Cumulative Rate of Antibody Test and Vaccination in the South Kanto Region. *Data source*: Ministry of Health, Labour and Welfare.

body testing and vaccination for men in their 40s and 50s from April 2019 to March 2025. The program involved two steps: First, all eligible men were to undergo antibody testing, which would identify the approximately 20% of men who were negative. Second, only those whose test results were negative would receive vaccination. Thus, antibody testing played a crucial screening role in order to enable efficient allocation of vaccines.

To examine how the program was implemented, we focus on the South Kanto region, comprising Tokyo, Saitama, Chiba, and Kanagawa. Figure 2 shows the cumulative antibody testing and vaccination rates in this region. The antibody testing rate increased from 7% in 2019 to 29% in 2024, and the vaccination rate increased from 1.4% to 5.7% over the same period. Under the program, roughly 20% of tested men are expected to be negative and, therefore, eligible for vaccination. Accordingly, the vaccination rate should be about 20% of the antibody testing rate. In fact, 20% of the 7% testing rate in 2019 is 1.4%, which is close to the observed vaccination rate. Similarly, 20% of the 29% testing rate in 2024 is 5.8%, which is very close to the actual vaccination rate. The low vaccination rate is clearly driven by the low antibody testing rate. Despite the absence of financial costs to individuals, the main challenge of has been the insufficient uptake of antibody testing.

Figure 3. Railway Advertisement Display

2.2 Railway Advertisement Campaign in the South Kanto Region

To address the low uptake of antibody testing, our research team collaborated with the MHLW from the early stages of the program and developed communication strategies. Our objective was to increase antibody testing among the eligible men. Given that sufficient financial incentives were already in place, we designed behavioral interventions, such as information provision, and then empirically evaluated their effectiveness.

To design the communication strategy, we first obtained key insights from earlier work. The central concern is not infection among men in their 40s and 50s but their risk of transmitting rubella to women in early pregnancy. For these men, rubella vaccination primarily yields social benefits rather than private health gains. Many eligible men do not fully recognize these social benefits, and messaging emphasizing altruistic motives, namely, protecting future generations, is particularly effective (Kato et al., 2024).

Building on these insights, in 2023 we produced a Japanese-language video advertisement.[6] The video conveyed three main messages: (i) many men may mistakenly believe that they contracted rubella in the past and are, therefore, immune; (ii) rubella infection can have serious adverse consequences for future generations, especially when women are infected during early pregnancy; and (iii) the procedures for using the government program for antibody testing and vaccination. The video was broadcast on select railway

[6] The Japanese version of the video advertisement is available on YouTube (`https://youtu.be/kmQNEnlYKp4`).

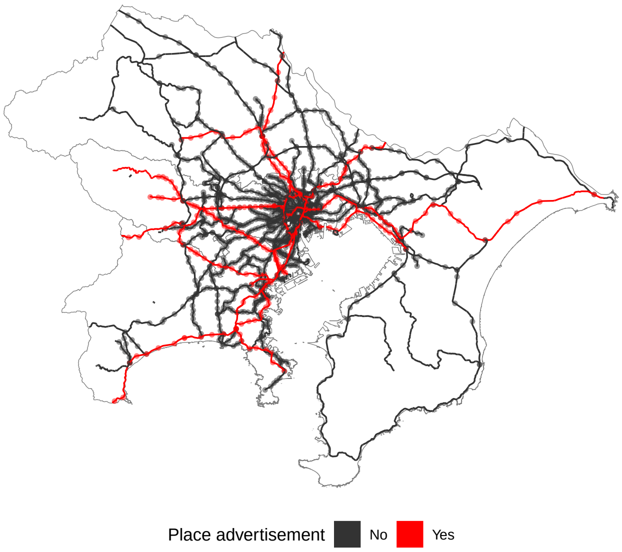

Figure 4. Map of the Railway Advertisement Campaign.
Notes: The dot represents the station where respondents of our survey used in our experiment period.

lines in the South Kanto region and via online platforms such as YouTube. On trains, it was shown on in-car digital signage displays (Figure 3).

Our identification strategy exploited variation in railway lines in the South Kanto region. As shown in Figure 4, the region, particularly Tokyo, is served by a dense railway network. The campaign was implemented only on a subset of lines. Individuals differ in which lines they routinely use, and multiple commuting routes may be available between the same origin and destination stations. Consequently, whether a person regularly used a campaign line would vary across individuals.

Figure 4 visually distinguishes campaign and non-campaign lines: the red lines indicate those on which the advertisement was placed, and the black lines indicate untreated lines. Representative lines equipped with in-car digital signage capable of displaying video advertisements are operated by JR East. Some lines operated by other railway companies also have in-car digital signage. These lines provide commuters with alternative routes. Among them, we displayed the video advertisement only on lines operated by JR East. Campaign lines were chosen solely based on technical feasibility, specifically the availability of in-car digital signage, and were thus unrelated to individuals' behaviors or characteristics.

The campaign ran from October 16 to November 15, 2023. As shown in Figure 2, the antibody testing rate in the South Kanto region increased only modestly from 25% in 2022 to 27% in 2023, a rise of just 2 percentage points. Thus, the campaign was implemented at a time when antibody testing had already increased to some extent but further growth had slowed. The remaining eligible individuals were likely those most resistant to behavioral change. Accordingly, the impact of the railway advertisement campaign may differ from the effects documented by Kato et al. (2024).

3 Empirical Strategy

Our main analysis is based on an ITT framework, in which treatment is defined by whether individuals commute on campaign lines. The ITT estimand captures the effect of commuting on campaign lines (i.e., exposure opportunity) on preventive behaviors (antibody testing and vaccination). The empirical analysis consists of three studies. Although the outcome variables and treatment definitions differ across studies, the empirical methods are common.

3.1 Empirical Methods

Our ITT analysis estimates the effect of commuting on a campaign line on preventive behavior. There are many railway lines in the South Kanto region, and individuals vary in which lines they routinely use; we exploit this variation for identification. Because the program targets men in their 40s and 50s, we define individuals' usual railway use in terms of the lines they use for commuting. Those who commute on campaign lines should have more opportunities to be exposed to the advertisement than those who do not. Accordingly, we compare preventive behaviors between commuters who use campaign lines and those who do not, estimating the following short regression—or equivalently, a difference-in-means:

$$Y_i = \alpha + \beta_{ITT}\text{RailwayLine}_i + \epsilon_i, \tag{1}$$

where Y_i is the outcome variable (preventive behavior) and RailwayLine_i is a dummy variable indicating whether individual i uses a campaign line for commuting. The coefficient β_{ITT} captures the effect of commuting on a campaign line on preventive behavior and is our primary parameter of interest.

One concern is that commuting routes reflect both individuals' circumstances and their choices. Commuting on campaign lines may systematically differ by observable characteristics (specifically, where people live and work). We therefore adjust for these observables and estimate the effect of commuting on a campaign line using the following linear regression model:

$$Y_i = \alpha + \beta_{ITT}^X \text{RailwayLine}_i + \delta X_i' + \epsilon_i, \tag{2}$$

where X_i is a vector of individual characteristics including residential and workplace locations. The coefficient β_{ITT}^X is the ITT parameter of interest after adjusting for observables.

To assess the robustness of our findings, we estimate the ITT effect using two alternative methods. The first method is inverse probability weighting (IPW). In this approach, we first estimate the propensity score $\hat{P}_i = P(\text{RailwayLine}_i = 1|X_i)$ using a logit model. Then, we construct the following weights, $W_i = 1/(\text{RailwayLine}_i\hat{P}_i + (1 - \text{RailwayLine}_i)(1 - \hat{P}_i))$, and estimate the short regression model (1) by weighted least squares with weights W_i. The second method is augmented inverse probability weighting (AIPW). Using the propensity score and the predicted outcome values, this method can yield a consistent estimator of the ITT effect under weaker conditions than IPW. See Appendix A for details.

After controlling for observable characteristics, commuting on campaign lines is plausibly as good as random. Importantly, campaign lines were selected based on technical feasibility (the availability of in-car digital signage), rather than on individuals' characteristics. Moreover, given the short duration of the campaign, it is unlikely that people with unobserved preferences (e.g., stronger health motivation) would switch commuting routes simply to see the advertisement. As a supplementary check, we conduct the sensitivity analysis proposed by Oster (2019), which quantifies how the ITT estimate would change under selection on unobservables (see Appendix A for implementation details). If the resulting estimate is similar to β_{ITT}^X, it suggests that unobserved confounding is unlikely to drive our results.

In Section 7, we use an instrumental-variables (IV) approach to examine whether the effect of exposure opportunities on railways (the ITT effect) can be explained solely by the effect operating through ad viewing. The IV approach estimates the causal effect of viewing the advertisement at campaign lines on preventive behavior. Because viewing is a choice, a naive OLS comparison between viewers and non-viewers may be biased. We therefore use whether the respondent commutes on a campaign line as an instrument for ad viewing. Since campaign lines were selected based on technical feasibility as described above, this instrument is likely to satisfy the exclusion restriction in the sense that it affects preventive behavior only through ad viewing.

3.2 Three Studies

All three studies share the same ITT design but vary the treatment definition and the timing of outcome measurement. Together, they examine (i) the effect of eligible men's exposure opportunities to the campaign (measured by commuting on campaign lines), (ii) the effect of spouses' exposure opportunities to the campaign, and (iii) longer-run impacts and potential substitution across vaccination channels. Study 1 was approved by the institutional review board (IRB) of the Center for Infectious Disease Education and Research, The University of Osaka (approval number: 2023CRER0925), and Studies 2 and 3 were approved by the IRB of the Graduate School of Economics, The University of Osaka (approval number: R70120-2).

Study 1 examines the short-run effect of eligible men's commuting on campaign lines on their own preventive behavior within one month after the intervention. The treatment variable RailwayLine_i is a dummy indicating whether respondent i uses campaign lines for commuting. The outcome Y_i measures respondent i's preventive behavior (antibody testing and vaccination) over the first month after the railway advertisement campaign.

Study 2 investigates the short-run effect of spouses' commuting on campaign lines on eligible men's preventive behavior within one month after the intervention. The treatment variable RailwayLine_i is a dummy indicating whether the spouse of respondent i commutes on campaign lines. Because this treatment measure is based on retrospective reporting in the follow-up survey, it may be subject to recall bias and confounding related to household characteristics. We therefore control for observable characteristics, including the eligible man's own campaign-line commuting, residential and workplace locations, and household composition. The outcome Y_i measures the husband's preventive behavior (antibody testing and vaccination) over the first month after the railway advertisement campaign.

Finally, Study 3 evaluates the long-run effects of the advertisement and potential substitution across vaccination channels. The rubella vaccination program we focus on imposes opportunity costs of time. Test results are typically available only after about a week, and vaccination is available only to those with negative test results. Thus, those with high opportunity costs may substitute away from the routine program to alternative channels (e.g., local government subsidies or out-of-pocket). Since the ultimate policy goal is antibody acquisition (i.e., establishing herd immunity), such substitution may not be detrimental from a policy perspective.

In Study 3, we examine the effects of husbands' and spouses' commuting on campaign lines on husbands' preventive behavior over the first year after the intervention. Depending on the specification, the treatment variable RailwayLine_i is a dummy indicating whether the husband or the spouse uses campaign lines for commuting. The outcome Y_i measures the husband's preventive behavior (antibody testing and vaccination) over the first year after the railway advertisement campaign. For vaccination outcomes, we construct mutually exclusive measures by vaccination channel (the MHLW program, out-of-pocket, local government subsidies).

4 Online Survey Data

We commissioned an online survey via MyVoice.com. The survey consisted of three main waves: screening (September 2023), main survey (December 2023), and follow-up survey (January 2025). Figure 5 shows the survey timeline.

Before implementing the railway advertisement campaign, we invited men aged 44–62 years (eligible for the vaccination program) living in the South Kanto region (Tokyo, Saitama, Chiba, and Kanagawa) to participate in the screening survey. During the screening survey, information on commuting routes was collected. We asked whether respondents used railways for commuting and, for those who did, collected their routes (boarding and alighting stations for each railway line). Using these responses, we created a dummy variable, RailwayLine_i, indicating whether respondents used campaign lines for Studies 1 and 3. Information on antibody testing status at the time of the screening survey was

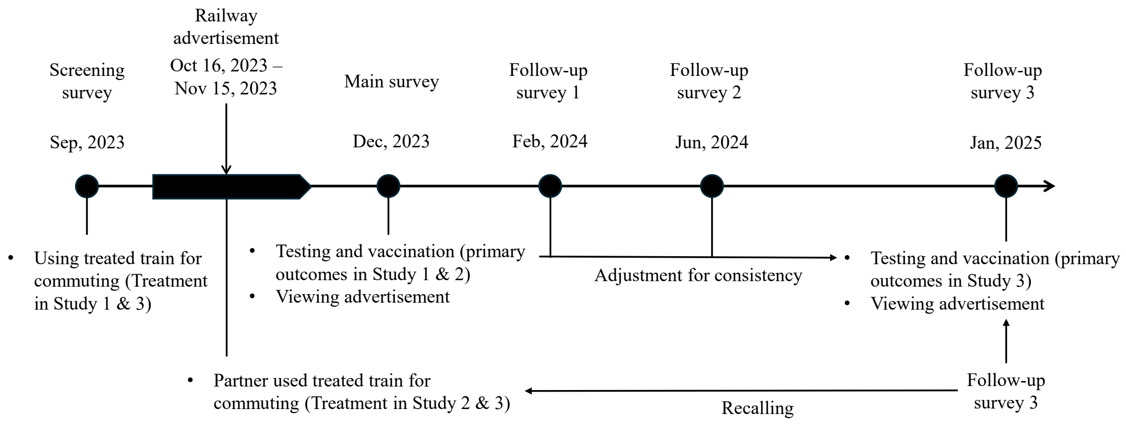

Figure 5. Timeline of the Survey and Railway Advertisement Campaign

also collected. Based on these responses, we invited those who used railways for commuting and had not yet undergone antibody testing to participate in the main and follow-up surveys.

The main survey was conducted in December 2023, one month after the railway advertisement campaign finished. We asked whether the respondents had undergone antibody testing within one month after the campaign ended. Those who had undergone antibody testing were asked about their test results. Those who reported negative results were asked whether they had received the vaccination. These preventive behavior variables were used as outcome variables in Studies 1 and 2.

In the same survey, we showed all of the main survey respondents the advertisement video and asked them whether they had seen the advertisement. Those who had seen the advertisement were asked about the media through which they viewed it. Using these responses, we created a dummy variable indicating whether respondents had seen the advertisement video on the railways. We used this variable in the IV analysis to discuss mechanisms in Section 7.

The follow-up survey was conducted in January 2025, approximately one year after the main survey. This survey collected information on family members' railway use and preventive behaviors. We asked whether any family member had used a campaign line during the railway advertisement campaign. If a family member had used the line, we asked which member had used it. Using these responses, we created a dummy variable, RailwayLine_i, indicating whether the spouse of the respondent had used a campaign line for Study 2.

In the same follow-up survey, we asked whether respondents had undergone antibody testing and vaccination within the first year after the campaign ended. We also asked about the vaccination channels respondents used (routine vaccination program provided by the MHLW, out-of-pocket, or local government subsidies). In addition, we conducted follow-up surveys on preventive behaviors in February and June 2024; accordingly, we adjusted the responses to ensure consistency. These responses were used as the outcome variables in Study 3.

The sample size for each study was as follows. In Study 1 (short-term effect of direct exposure opportunities), we used respondents who participated in the main survey and worked at the time of the screening survey ($N = 9,580$). When analyzing the long-term effects of direct exposure opportunities in Study 3, we used respondents in Study 1 who participated in both the main survey and follow-up survey ($N = 5,444$). For Study 2 (short-term effect of spousal exposure opportunities), we used respondents who were married at the time of the main survey and participated in both the main and follow-up surveys ($N = 3,685$). When analyzing the long-term effects of spousal exposure opportunities in Study 3, we used the same sample as Study 2.

5 Main Results: Comparison of the Effects of Exposure Opportunities Between Husbands and Their Spouses

Study 1 examines how eligible men's use of campaign lines for commuting affects their preventive behaviors (antibody testing and vaccination). Study 2 examines how their spouses' use of campaign lines for commuting affects preventive behaviors of eligible men.

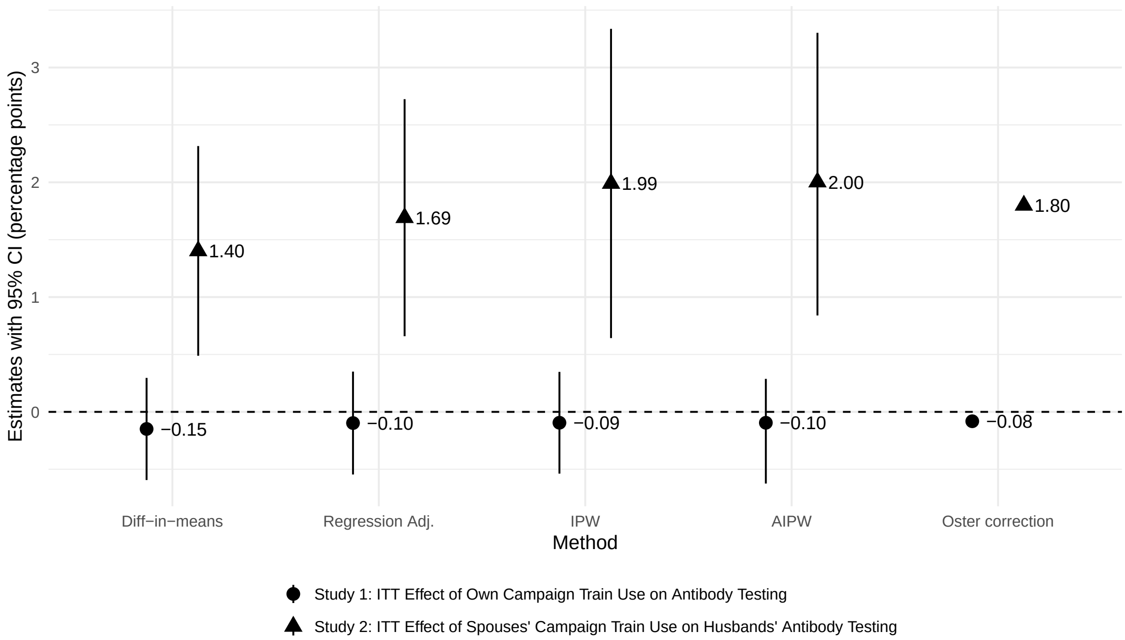

Figure 6. ITT Effects on Antibody Testing.
Notes: Circles show the ITT effect of eligible men's use of campaign lines on take-up rates of antibody testing (Study 1). Triangles show the ITT effect of their spouses' use of campaign lines on take-up rates of antibody testing of husbands (Study 2). N = 9,580 for Study 1 and N = 3,685 for Study 2. Difference-in-means, regression adjustment, and IPW use robust standard errors. Statistical inference for AIPW is based on 100 bootstrap samples. Error bars indicate 95% confidence intervals. The confidence interval for AIPW is constructed from the 2.5th and 97.5th percentiles of the bootstrap distribution. Oster-adjusted estimates represent ITT effects if unobserved factors could be accounted for.

5.1 ITT Effects on Antibody Testing

Eligible men's commuting on campaign lines did not increase their antibody testing, whereas their spouses' commuting did. Figure 6 shows the ITT effects on antibody testing for Study 1 (circles) and Study 2 (triangles). In a simple difference-in-means, eligible men's commuting on campaign lines reduced the antibody testing rates by 0.15 percentage points (equivalent to 0.01 standard deviations).[7] Because the 95% confidence interval includes zero, this difference is not statistically significant at the 5% level. By contrast, spouses' commuting on campaign lines increased husbands' antibody testing rates by 1.40 percentage points (equivalent to 0.11 standard deviations).[8] Because the 95% confidence interval excludes zero, this difference is statistically significant at the 5% level. Overall, our main results suggest that providing eligible men's spouses with opportunities to view the advertisement is more effective at promoting husbands' antibody testing than providing the same opportunities to husbands themselves.

These results remain unchanged after accounting for both observed and unobserved confounding. Simple differences-in-means may be biased. This is because whether eligible men used campaign lines for commuting showed some imbalance with respect to respondents' attributes, particularly in the combination of residential and workplace locations (Figure B1 in Appendix B). Likewise, spouses' commuting on campaign lines was also imbalanced in observable characteristics, particularly husbands' commuting on campaign lines and family size (Figure B2 in Appendix B).[9] We confirmed that IPW eliminates these imbalances. Moreover, the main results were unchanged under regression adjustment and IPW. The results are also robust to AIPW; if anything, controlling for observables slightly strengthens the estimated effect of spousal commuting. Finally, following Oster (2019), we compute Oster-adjusted ITT estimates, which suggest that our

[7]The effect may be larger for men who have more contact with women of childbearing age, given the higher public-health risk when rubella infects women in early pregnancy. We therefore tested for heterogeneity in the ITT effect of eligible men's commuting on campaign lines by the female share in the workplace (a proxy for exposure to younger women) and found no evidence of effect heterogeneity.

[8]We conducted analogous analyses using daughters' and mothers' commuting on campaign lines, but the ITT effects on antibody testing were not statistically significant.

[9]Given the imbalance in eligible men's commuting on campaign lines, we should also adjust for their spouses' use of campaign lines for commuting in Study 1. However, the results presented in this section do not include this adjustment. Using the same data as Study 2, we examined the effect of providing exposure opportunities to eligible men while adjusting for their spouses' use of campaign lines for commuting (see Table C5 in Appendix C). The ITT effect on antibody testing was negative and not statistically significant at the 5% level, consistent with the result in Figure 6.

conclusions would be similar even if unobserved confounding were present. Taken together, the contrast between the limited ITT effect in Study 1 and the positive ITT effect in Study 2 is robust to covariate adjustment and alternative statistical methods.

Failure of exposure to eligible men cannot explain why the ITT effect of eligible men's commuting on campaign lines is limited. The more frequently eligible men used campaign lines, the more likely they were to see the advertisement. Indeed, those who used campaign lines for commuting had higher rates of viewing the advertisement than those who did not; the difference was 2.22 percentage points, equivalent to 0.12 standard deviations (Figure B3 in Appendix B). The difference is statistically significant at the 5% level and remains unchanged, even after accounting for both observed covariates and unobserved factors (Table C1 in Appendix C). Although those who used campaign lines for commuting were more likely to see the advertisement, they did not undergo antibody testing. Thus, the ITT analysis indicates that showing railway advertisements to eligible men does not lead to antibody testing.

In addition, eligible men are more likely to view the advertisement when their spouses commute on campaign lines. Indeed, those whose spouses commute on campaign lines had higher rates of viewing the advertisement than those whose spouses did not; the difference was 3.52 percentage points, equivalent to 0.18 standard deviations (Figure B4 in Appendix B). The difference is statistically significant at the 5% level and remains unchanged, even after accounting for both observed covariates and unobserved factors (Table C2 in Appendix C). We discuss the mechanism of these results in Section 7.

5.2 ITT Effects on Vaccination Rates

We expect the ITT effect on vaccination to be mechanically small because vaccination is available only to those with negative antibody test results. As shown in Figure 1, the NIID data indicate that approximately 20% of eligible individuals would be negative. Therefore, the ITT effect on vaccination is predicted to be 20% of the ITT effect on antibody testing. This back-of-the-envelope prediction relies on two implicit assumptions: (i) the antibody status does not affect the decision to undergo antibody testing, and (ii) all negative individuals get vaccinated. Using the results from the previous subsection, eligible men's commuting on campaign lines is predicted to reduce vaccination rates by 0.015 to 0.03

percentage points. Their spouses' commuting on campaign lines is predicted to increase vaccination rates by 0.28 to 0.38 percentage points. We examine whether our estimated ITT effects on vaccination are consistent with these predictions.

The estimated ITT effects on vaccination are consistent with the prediction in Study 1 but are somewhat larger than the prediction in Study 2. In Study 1, eligible men's commuting on campaign lines reduced vaccination by 0.012–0.014 percentage points (Table C3 in Appendix C), which falls within the predicted range (a decrease of 0.015–0.03 percentage points). In Study 2, spouses' commuting on campaign lines increased vaccination by 0.21–0.65 percentage points (Table C4 in Appendix C); among these estimates, the specification with covariate controls exceeds the predicted range of 0.28–0.38 percentage points. However, none of the estimates is statistically significant at the 5% level; perhaps because our sample is not large enough to detect ITT effects on vaccination.

6 Persistence of ITT Effects

Study 3 examines the long-run effects of the intervention. Our long-run outcomes are preventive behaviors (antibody testing and vaccination) for roughly one year from the railway advertisement campaign to the final follow-up survey conducted in January 2025. We then compare the long-run effects with the short-run effects from Studies 1 and 2. If the long-run effects are larger than the short-run effects, this would suggest persistent effects of the intervention.

The long-run outcomes are defined as stock variables. After the main survey, we conducted three follow-up surveys (including the final follow-up). We constructed the long-run outcome variables to be consistent with the responses from these surveys. Specifically, if a respondent reported having undergone antibody testing in any survey wave (including the main survey), the long-run indicator for antibody testing took a value of 1 regardless of their response in the final follow-up survey. This definition reduces reliance on responses from a single time point and yields more reliable measures of long-run outcomes.

As in the short-run analysis, respondents' commuting on campaign lines does not affect long-run antibody testing. In Figure 7, the circles show the ITT effect of respondents' use of campaign lines for commuting on the cumulative take-up rates of antibody-testing over

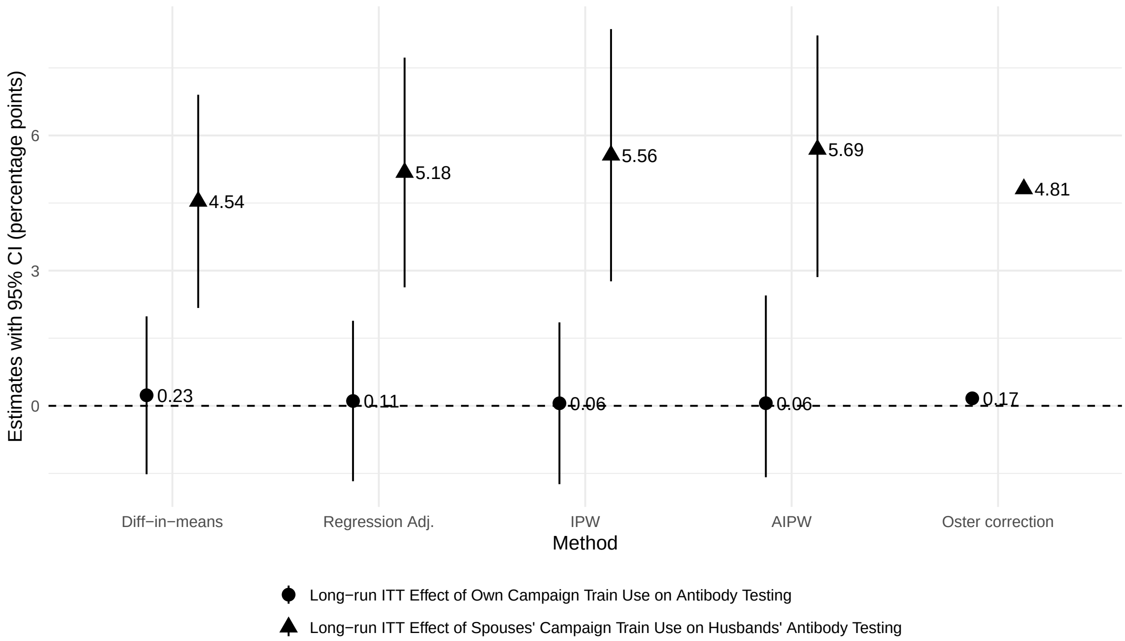

Figure 7. ITT Effects on The Cumulative Antibody Testing.
Notes: Circles show the ITT effect of eligible men's use of campaign lines on the cumulative take-up rates of antibody testing (N = 5,444). Triangles show the ITT effect of the spouses' use of campaign lines on the cumulative take-up rates of husbands' antibody testing (N = 3,685). Robust standard errors are used for the difference in means, regression adjustment, and IPW. Statistical inference for AIPW is based on 100 bootstrap replications. Error bars indicate 95% confidence intervals for each estimate. For AIPW, confidence intervals are constructed from the 2.5th and 97.5th percentiles of the bootstrap distribution. The Oster-adjusted estimates represent ITT effects if unobserved factors could be accounted for.

the year following the railway advertising campaign. Even when we extend the observation window to one year, the ITT effect remains statistically insignificant. This conclusion is unchanged when adjusting for observed covariates or accounting for unobserved confounding.

In contrast, spouses' commuting on campaign lines has a persistent effect on husbands' antibody testing. In Figure 7, the triangles show the ITT effect of spouses' use of campaign lines for commuting on the cumulative take-up rates of antibody testing over the year following the railway advertising campaign. The simple difference-in-means implies a long-run ITT effect of 4.54 percentage points, statistically significant at the 5% level. The long-run ITT effect is roughly three times as large as the short-run effect (1.40 percentage points in Figure 6), suggesting that the effect of spousal commuting persists over time.[10] This result is robust to analyses that adjust for observed covariates and account for unobserved confounding.

We examine the long-run effects of spouses' commuting on campaign lines on husbands' vaccination. The estimated long-run ITT effect on vaccination is smaller than the predicted magnitude, defined as the product of the long-run ITT effect on antibody testing and the share of those who would test negative (as discussed in Section 5.2). The NIID data suggest that a benchmark share of those who would be negative is 20% (Figure 1). Using the simple difference-in-means, the long-run ITT effect on antibody testing is 4.54 percentage points, implying a predicted long-run ITT effect on vaccination of about 0.91 percentage points. However, the estimated long-run ITT effect on vaccination is 0.38–0.78 percentage points, smaller than the prediction (Table C6 in Appendix C). Below, we examine two potential explanations for this gap.

The first possibility is that the share who tested negative is lower than the benchmark (20%), perhaps because more participants tested positive. In this case, the long-run ITT effect on vaccination would be less than 20% of the long-run ITT effect on antibody testing. However, among those who underwent antibody testing within one year after the railway advertising campaign, 22.3% tested negative. Therefore, this first explanation

[10] The long-run ITT effect of spouses' commuting on campaign lines is also economically meaningful. From 2022 to 2024, the antibody testing rates in the South Kanto region increased from 24.95% to 28.74%, an increase of 3.79 percentage points (Figure 2). The long-run ITT effect is approximately 1.2 times as large as this increase.

cannot explain the gap.

Second, vaccination may have occurred through channels outside the MHLW program on which we focus. The MHLW program entails time costs: it takes approximately one week to obtain antibody test results, and vaccination is available only if the test is negative. Individuals with high opportunity costs for time may therefore have substituted into other programs—for example, subsidies offered by many local governments to couples planning pregnancy—or may have been vaccinated out of pocket. We examine whether spouses' commuting on campaign lines affects vaccination through these alternative channels. Table C7 in Appendix C reports ITT effects on vaccination by channel (MHLW program, out-of-pocket payment, and local government subsidies). Although not robust, spouses' commuting on campaign lines may increase vaccination through local government subsidies. The sum of the effects across channels can be interpreted as the effect on overall vaccination because the channel-specific outcomes are mutually exclusive.[11] The resulting long-run ITT effect on overall vaccination is 0.79–1.29 percentage points, similar to the predicted value. Overall, these results suggest that, in the long run, vaccination may have occurred through multiple programs.

7 Mechanisms

7.1 Information versus Persuasion

We examine two hypotheses for why exposure through spouses is effective. The first is an *information effect*: when spouses view the advertisement, the information reaches eligible men and draws their attention to the campaign, increasing eligible men's ad viewing and thereby promoting preventive behaviors (especially antibody testing). The second is a *persuasion effect*: when spouses view the advertisement, they encourage eligible men to take preventive actions. The key distinction between these hypotheses is whether the proximate trigger for preventive behavior is the advertisement itself or spouses' persuasion.

First, consistent with the first part of the information-effect channel, we found that

[11] In the survey, respondents who reported being vaccinated were asked to select only one vaccination channel.

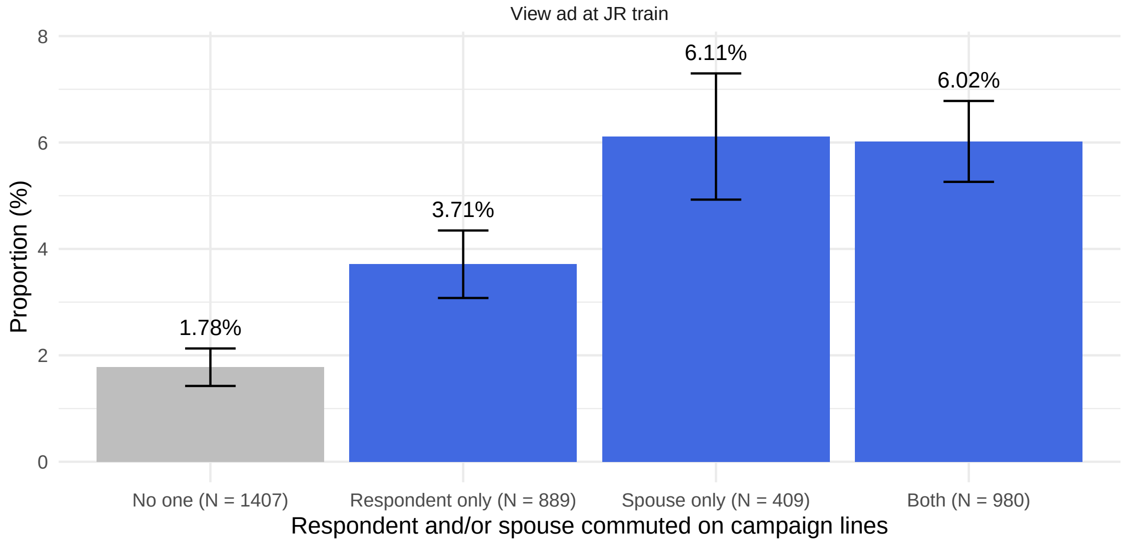

Figure 8. Advertisement Viewing Rates by Whether The Respondents and/or Their Spouses Commutes on Campaign Lines.
Notes: We use the Study 2 sample (N = 3,685). See Table C8 in Appendix C for statistical significance.

spouses' exposure opportunities increased eligible men's advertisement viewing. Figure 8 reports advertisement viewing rates by whether respondents and their spouses commute on campaign lines. When neither commuted on campaign lines (No one group), the viewing rate was 1.78%. When only the spouse commuted on campaign lines (Partner only group), the viewing rate rose to 6.11%, a statistically significant difference of 4.33 percentage points (Table C8 in Appendix C). Notably, in both groups respondents did not commute on campaign lines, so any exposure to the advertisement must come from non-commuting settings. Nevertheless, spousal commuting increases respondents' viewing. By contrast, when only respondents commuted on campaign lines (Respondent only), the viewing rate was 3.71%, which is 1.93 percentage points higher than the No one group. This effect is about 45% of the effect of the Partner only group, and the difference between the two effects is statistically significant at the 10% level (linear hypothesis test 1 in Table C8 in Appendix C). Moreover, when the spouses commuted on campaign lines, the additional effect of the respondent's commuting is negligible (linear hypothesis test 2). Overall, these results are consistent with a household information-transmission channel operating through spouses.

However, we found no evidence that supports the second part of the information-effect

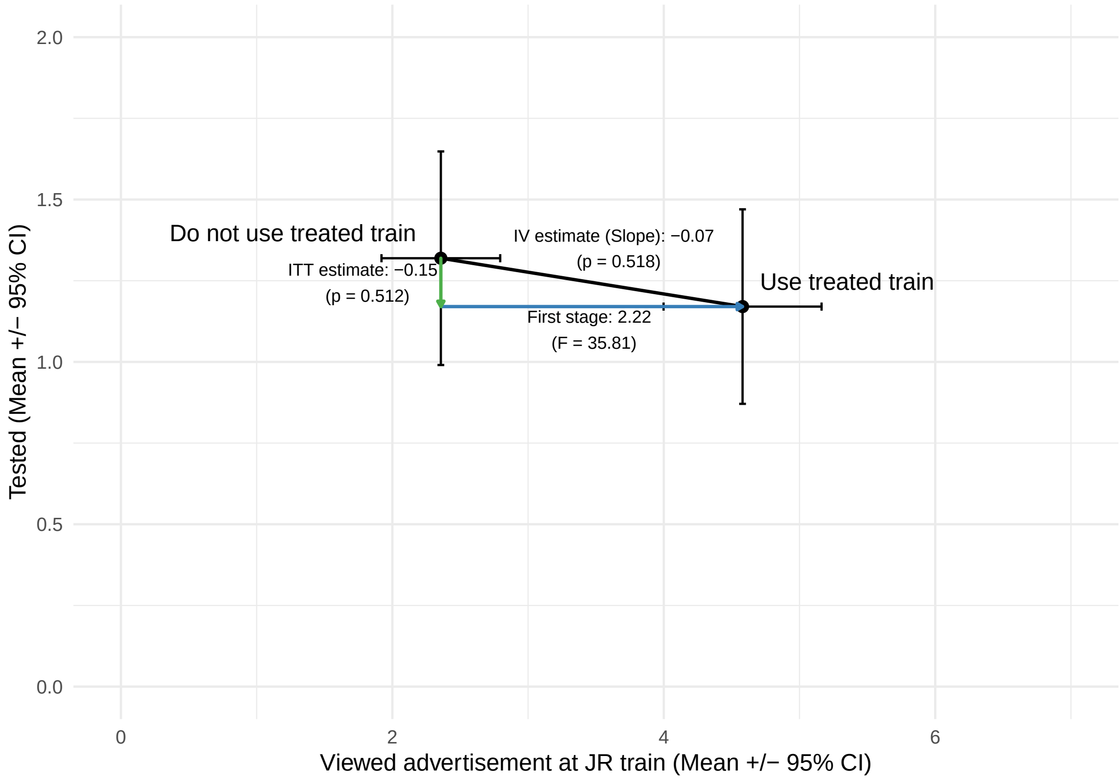

Figure 9. Visualizing the IV Estimate of The Effect of Advertisement Viewing.
Notes: The instrument is whether the respondent commutes on campaign lines. Each point shows the sample mean antibody-testing rate (y-axis) and advertisement-viewing rate (x-axis) for each instrument group. Error bars indicate 95% confidence intervals for each mean. The solid line connects the two group means; its slope is the Wald estimate. The Wald estimate is the ITT estimate (the effect of commuting on campaign lines on antibody testing) divided by the first-stage estimate (the effect of commuting on campaign lines on advertisement viewing).

hypothesis—advertisement viewing increases preventive behaviors. Figure 9 shows that the IV estimate of the effect of advertisement viewing on antibody testing was negative and statistically insignificant. This conclusion is unchanged when controlling for covariates (Table C9 in Appendix C). Because OLS estimates may suffer from endogeneity (e.g., individuals with high health consciousness may be more attentive to advertisements), we use respondents' commuting on campaign lines as an instrument for advertisement viewing. The exclusion restriction plausibly holds because campaign lines were selected based on technical feasibility of running in-car digital signage.[12] In addition, the instrument is not weak (the first-stage F-statistic exceeds 30). The direction of the IV estimates is consistent with the negative ITT effect shown in Figure 6. Since the ITT effect is the product of the first-stage effect of campaign-line commuting on advertisement viewing (positive effect shown in Table C1 in Appendix C) and the second-stage effect of advertisement viewing on preventive behavior, a negative ITT effect suggests a negative second-stage effect. Moreover, even when extending the time window to one year, the IV results still showed no effect of advertisement viewing on preventive behaviors (Table C10 in Appendix C). Overall, the IV evidence suggests that the information channel does not operate through advertisement viewing leading to preventive behaviors.

Taken together, the evidence suggests that the ITT effect of spouses' commuting on campaign lines is more likely driven by persuasion than by information. The persistence of the spousal-commuting ITT effect further supports this interpretation: the video advertisement ran only for one month, making continued information exposure difficult, whereas spousal persuasion can plausibly be repeated within the household. Moreover, among respondents who did not view the advertisement, spouses' commuting on campaign lines was positively associated with antibody testing (Figure B5 in Appendix B). This association is statistically significant (Table C11 in Appendix C). This suggests that, even without direct advertisement viewing, respondents may have taken preventive actions due to persuasion by spouses who viewed the advertisement. However, because this analysis restricts the sample based on an endogenous variable (whether the advertisement was viewed), there remains selection bias. Thus, these results are suggestive of persuasion

[12] It is difficult to use spouses' commuting on campaign lines as an instrument. Spousal persuasion induced by advertisement viewing could directly affect preventive behaviors without operating through respondents' advertisement viewing, violating the exclusion restriction required for IV.

effects.

7.2 Economic Model of Persuasion

To understand why spousal persuasion can change husbands' behavior, we summarize husbands' decision making in a simple economic model. We specify the condition under which an eligible husband takes preventive behavior as follows:

$$\alpha_H H(\theta_c) - C_1(k, w_h) \geq -C_0(m), \tag{3}$$

where α_H is the husband's altruism (constant), and $H(\theta_c)$ is his perception of benefits to future generations, which depends on knowledge θ_c.[13] The husband incurs two costs. $C_1(k, w_h)$ is the cost of taking preventive action; it depends on direct cost k (including program participation fees and psychological costs) and the opportunity cost of time w_h (the wage rate).[14] $C_0(m)$ is the cost of not taking preventive action; it is increased by spousal persuasion m. C_0 includes guilt from disappointing the spouse's expectations and damage to the husband's social image shaped by her. The husband takes preventive action depending on whether the benefit $\alpha_H H(\theta_c)$ outweighs the net cost $\Delta C(k, w_h, m) \equiv C_1(k, w_h) - C_0(m)$. Thus, even with sufficient altruism, limited knowledge (small $H(\theta_c)$) or high costs (large $\Delta C(k, w_h, m)$) can prevent preventive behavior.

In our simple model, information provision through the advertisement raises the perceived benefit of preventive behavior by updating knowledge θ_c (increasing $H(\theta_c)$), whereas spousal persuasion lowers the net cost $\Delta C(k, w_h, m)$. The IV estimates indicate that directly providing information to husbands does not translate into preventive behavior. The model suggests that the result may arise either because knowledge θ_c is not updated sufficiently, or because α_H is not large enough for the increase in benefits to exceed the net cost. Either way, our empirical results suggest that reductions in the net cost induced by spousal persuasion outweighed the increases in benefits induced by information provision.

[13]For simplicity, we abstract from private benefits of preventive behavior. Because severe outcomes from rubella are rare, the benefit of acquiring antibodies is small. The video advertisement also places little emphasis on private benefits.

[14]In the rubella vaccination program we focus on, direct cost k is likely sufficiently low. Under this program, antibody testing and vaccination are free. Hence, k includes psychological costs associated with testing or vaccination as well as travel costs.

For information provision targeted at spouses to matter, spouses must ultimately engage in persuasion. The benefit to the spouse from persuading depends on the perceived benefit to future generations, $\alpha_W H(\theta_c)$, where α_W is the spouse's altruism (constant).[15] If altruism differs by gender with $\alpha_W > \alpha_H$, the same information provision can encourage spousal persuasion rather than changing the husband's behavior directly. Our analysis suggests that spouses' viewing the advertisement strengthens persuasion motives and thereby promotes husbands' preventive behaviors. This pattern is consistent with $\alpha_W > \alpha_H$ and with prior research on intra-household decision making (Thomas, 1990; Duflo, 2003; Dizon-Ross and Jayachandran, 2023; Nyqvist et al., 2024).

This economic framework suggests two conditions under which expanding the intervention target from the decision maker to someone close to that person may be valuable. First, the objective of behavioral change aligns with the related person's motivations rather than with the decision maker's motivations. In our setting, husbands' preventive behavior against rubella is oriented toward protecting future generations, while spouses likely exhibit greater altruism than husbands. Given this premise, the same information is more readily expected to encourage spousal persuasion than to induce the husbands' behavioral change directly. Second, persuasion can sufficiently reduce the decision maker's net cost $\Delta C(k, w_h, m)$. The reduction in net costs should depend on the related person's abilities and circumstances. For example, in our context, sizable reductions may arise because spouses are comparatively well educated and especially close interpersonal partners for the decision maker. In sum, expanding targeting can be effective when both conditions hold.

8 Conclusion

Even when direct outreach to vaccination-eligible individuals themselves is ineffective, targeting their spouses can still improve eligible individuals' behavior. Using a railway advertising campaign for rubella vaccination on a large commuter network in the South Kanto region, we illustrate this possibility. Eligible male respondents' commuting on

[15] More precisely, the benefit of persuasion reflects expectations of benefits to future generations that depend on the probability that the husband takes preventive behavior. That probability depends on persuasion m.

campaign lines did not increase antibody testing, whereas their spouses' commuting on campaign lines increased husbands' antibody testing. Using simple difference-in-means, we estimate short-run impacts of 1.40 percentage points on antibody testing (within one month after the intervention). This effect persists over time: estimated long-run effects over one year after the intervention are 4.54 percentage points, roughly three times the short-run effect. Our findings are robust to adjusting for observable characteristics and to alternative estimation methods.

We infer that the positive effect of spouses' commuting on campaign lines reflects spousal persuasion rather than the advertisement itself. Commuting on campaign lines by either husbands or spouses raised husbands' advertisement viewing. However, husbands' viewing the advertisement did not increase antibody testing. Treating the railway advertising campaign as a natural experiment, we use instrumental variables to identify the causal effect of advertisement viewing. The estimated effect of viewing is not statistically significant. It is therefore plausible that the railway advertisements mainly created opportunities for spouses to persuade their husbands. This interpretation aligns with research on intra-household decision making in which spouses are relatively more altruistic than husbands (e.g., Thomas, 1990; Duflo, 2003; Dizon-Ross and Jayachandran, 2023; Nyqvist et al., 2024). We do not, however, directly test the persuasion mechanism.

Although these limitations are important topics for future work, our findings carry meaningful implications for targeting policy. Recent studies have tackled which decision makers policymakers should prioritize for intervention (e.g., Athey et al., 2025a; Ida et al., 2026). We push this idea one step further: broadening targeting from decision makers alone to individuals close to them may improve intervention effectiveness. When only relatively unresponsive decision makers remain as targets for direct outreach, expanding targeting in this direction can be one solution.

As discussed above, expanding the intervention target is effective when two conditions hold: (i) the objective of behavioral change aligns more closely with a related person's motivations than with the decision maker's, and (ii) persuasion can sufficiently reduce the decision maker's net costs. Another health domain in which these two conditions may also hold is smoking cessation. Smoking generates externalities—for example, harms to children's health—that parallel aspects of the rubella context, and smokers often face large net costs of quitting, including withdrawal symptoms. When the smoker is the husband, interventions that encourage a relatively more altruistic spouse to persuade him may therefore be valuable. The same idea may extend to parent–child relationships or to finance. For instance, Athey et al. (2025b) studies which students should receive reminders to renew scholarship support. If parents pay education costs (and thus have stronger incentives to save money than students) and sustained parental persuasion lowers students' net costs, sending reminders to parents rather than to students may be more effective. Pinning down the conditions and limits of expanding intervention targets through applications in other domains is an important topic for future research.

A Appendix: Doubly Robustness Estimation (AIPW) and Oster Correction

Augmented Inverse Probability Weighting (AIPW). AIPW can identify the ITT effect under weaker conditions than regression adjustment or inverse probability weighting (Glynn and Quinn, 2010). Specifically, AIPW yields a consistent estimator of the ITT effect if either the propensity score model or the main outcome regression model is correctly specified (but not necessarily both). We first estimate the propensity score $\hat{P}_i = P(\text{RailwayLine}_i = 1|X_i)$ using a logit model. We then obtain predicted outcome values $\hat{\mu}_1(X_i)$ and $\hat{\mu}_0(X_i)$, which are the predicted outcomes under treatment (commuting on campaign lines) and control (not commuting on campaign lines), respectively. We estimate the linear outcome regression $\mu_1(X)$ using observations with $\text{RailwayLine}_i = 1$ and $\mu_0(X)$ using observations with $\text{RailwayLine}_i = 0$. The AIPW estimator of the ITT effect is:

$$\begin{aligned}\hat{\beta}_{ITT}^{AIPW} =& N^{-1}\sum_{i=1}^{N}\left(\frac{\text{RailwayLine}_i(Y_i - \hat{\mu}_1(X_i))}{\hat{P}_i} + \hat{\mu}_1(X_i)\right) \\ & - N^{-1}\sum_{i=1}^{N}\left(\frac{(1-\text{RailwayLine}_i)(Y_i - \hat{\mu}_0(X_i))}{1-\hat{P}_i} + \hat{\mu}_0(X_i)\right)\end{aligned} \tag{A1}$$

We construct confidence intervals for this estimator using bootstrap resampling.

Oster Correction. Oster (2019) proposes a sensitivity analysis that quantifies how the estimated effect would change under selection on unobservables. The approach uses the two linear regression models presented in the main text. We first estimate the short regression without covariates:

$$Y_i = \alpha + \beta_{ITT}\text{RailwayLine}_i + \epsilon_i \tag{A2}$$

Let R denote the R^2 from this regression.

We then estimate the long regression with covariates:

$$Y_i = \alpha + \beta_{ITT}^{X}\text{RailwayLine}_i + \delta X_i' + \epsilon_i \tag{A3}$$

where X_i is a vector of individual characteristics. Let R^X denote the R^2 from this regression.

The estimate of the effect if unobservables could be controlled for, β^*_{ITT}, can be approximated as:

$$\beta^*_{ITT} \approx \beta^X_{ITT} - \delta \left(\beta_{ITT} - \beta^X_{ITT}\right) \frac{\hat{R} - R^X}{R^X - R} \tag{A4}$$

Here, δ is the relative importance of selection on unobservables compared with selection on observables. For example, $\delta = 2$ implies that unobservables are twice as important as observables in driving selection bias. The term $\hat{R}$ is the hypothetical R^2 that would be obtained if unobservables could be controlled for. Both δ and $\hat{R}$ are chosen by the researcher. Following Oster (2019), we set $\delta = 1$ and $\hat{R} = 1.3 * R^X$.

B Appendix: Figures

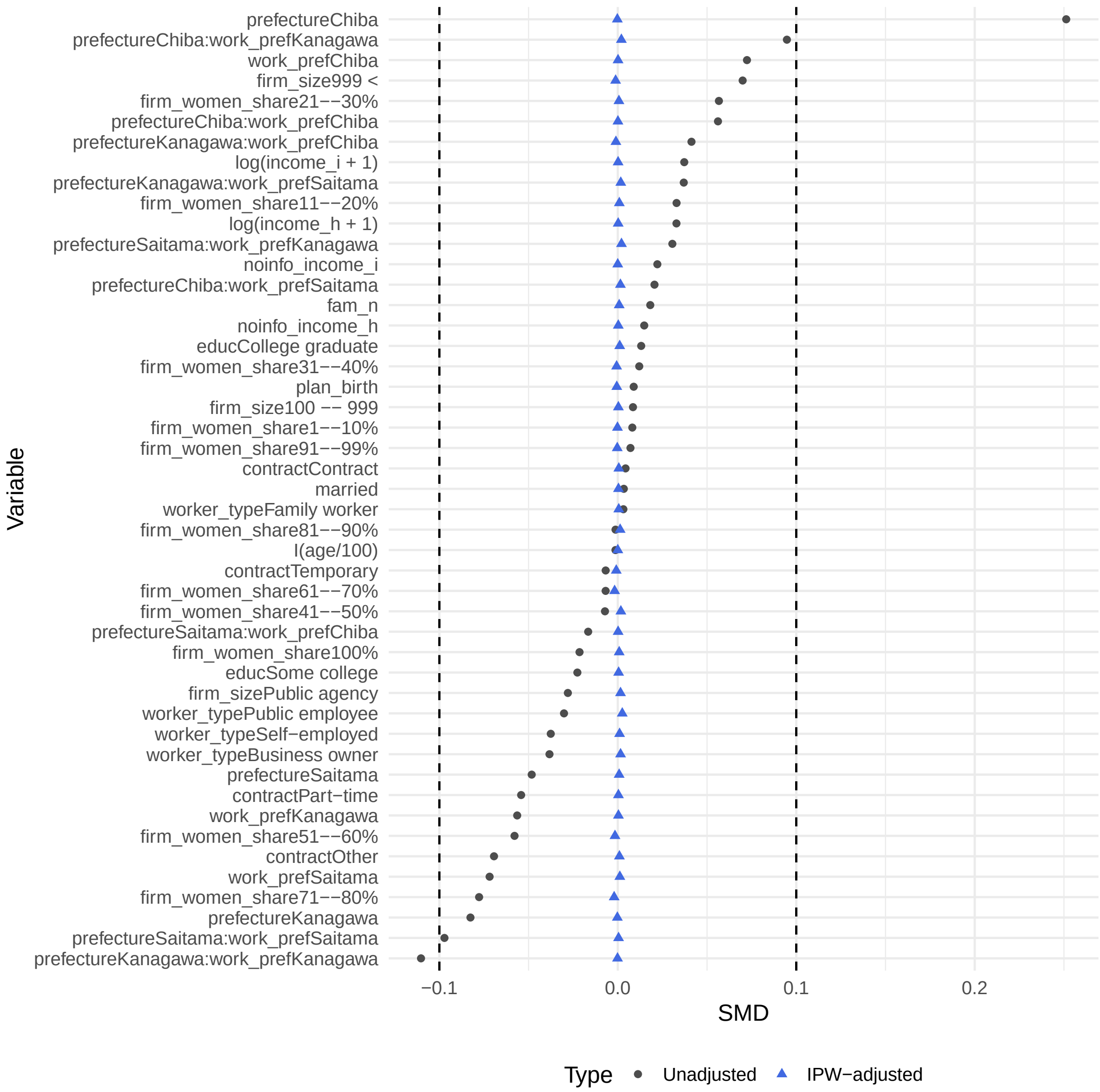

Figure B1. Covariate Balance by Whether the Respondents Commuted on Campaign Lines (Data: Study 1).

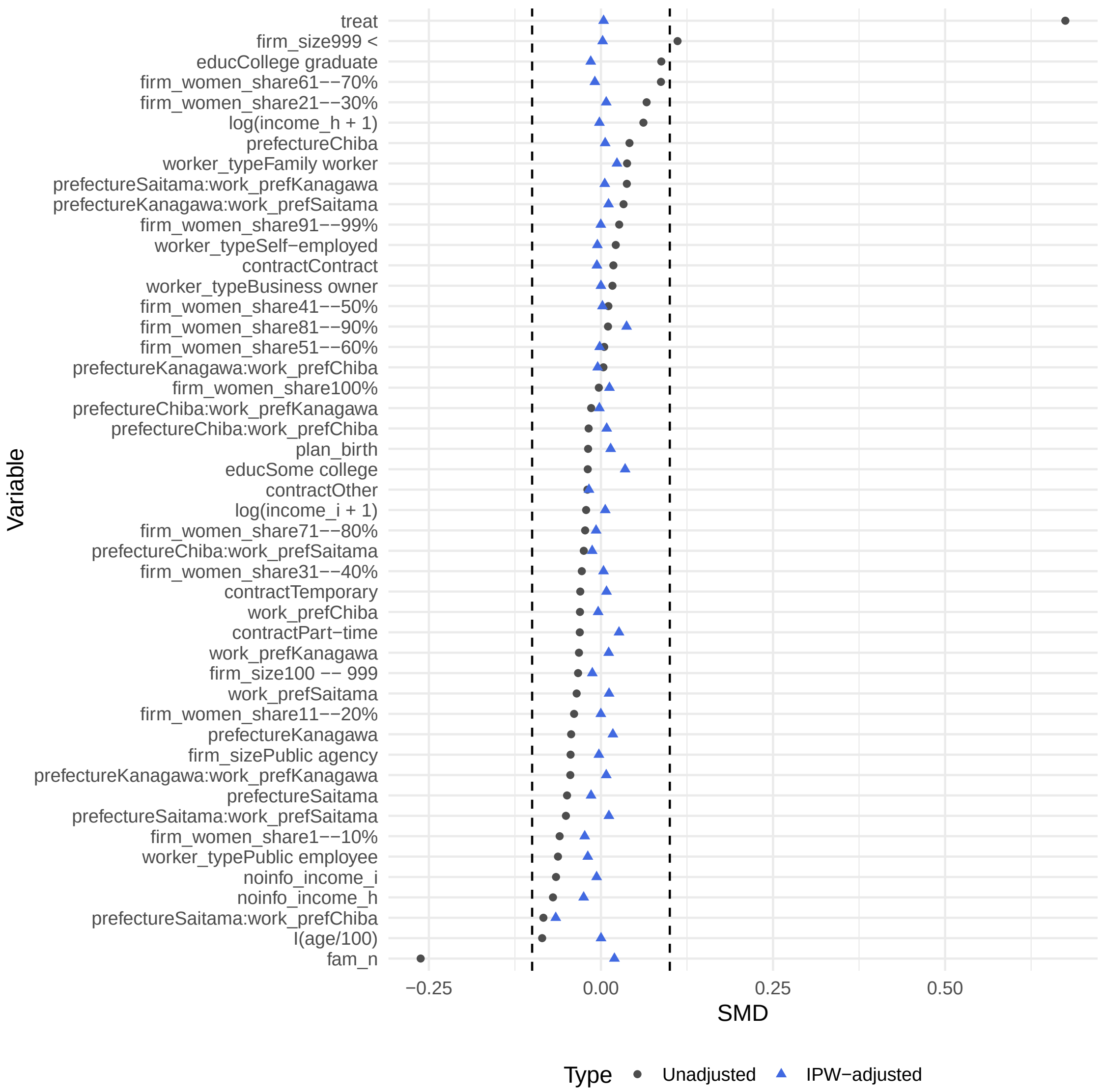

Figure B2. Covariate Balance by Whether the Spouses Commuted on Campaign Lines (Data: Study 2).

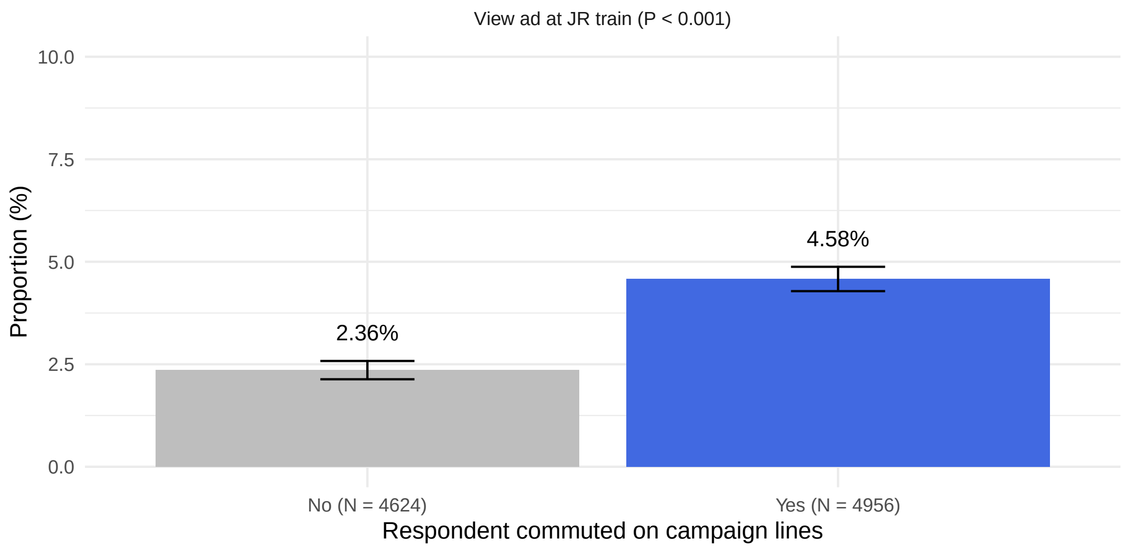

Figure B3. Advertisement Viewing Rate by Whether the Respondents Commuted on Campaign Lines (Data: Study 1).

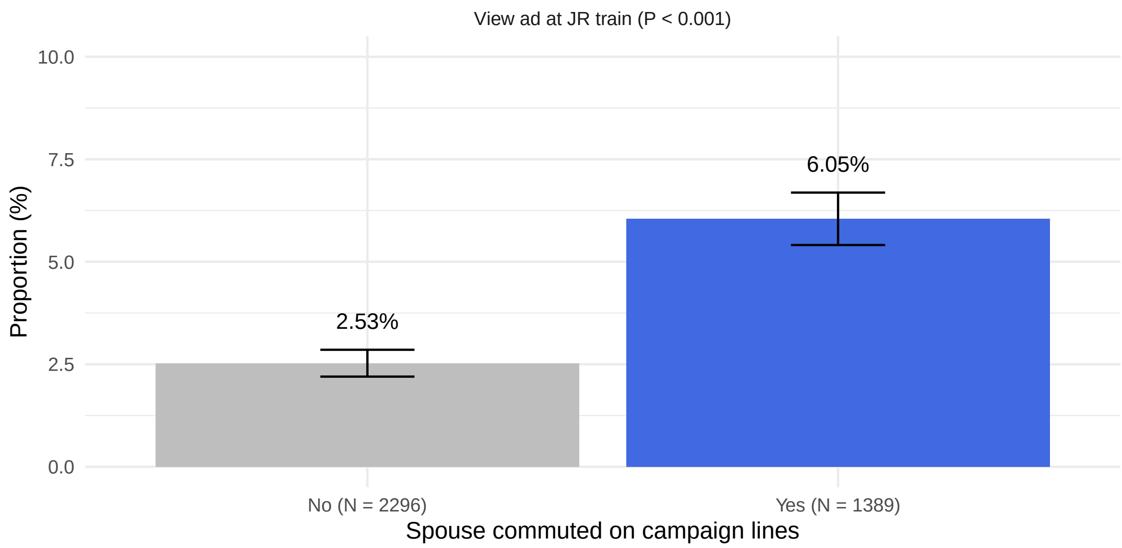

Figure B4. Respondents' Advertisement Viewing Rate by Whether Their Spouses Commuted on Campaign Lines (Data: Study 2).

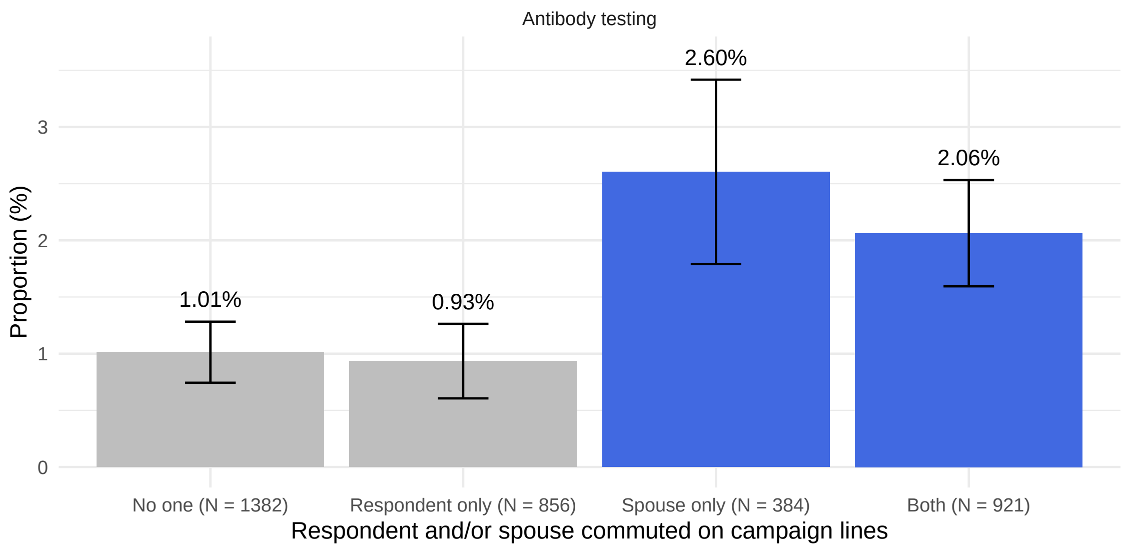

Figure B5. Antibody Testing Rate by Whether the Respondents and/or Their Spouses Commuted on Campaign Lines (Data: Study 2; respondents who did not view the JR advertisement).

C Appendix: Tables

Table C1. Effects of Respondent's Commuting on Campaign Lines on Advertisement Viewing

	View ad at campaign train	
Method	Estimate	R-squared
(1) Regression without covariates	2.22	0.004
	(0.37)	
	[1.49, 2.95]	
(2) Regression with covariates	2.21	0.012
	(0.38)	
	[1.47, 2.95]	
(3) IPW	2.19	
	(0.38)	
	[1.45, 2.93]	
(4) AIPW (Doubly robust estimator)	2.19	
	(0.35)	
	[1.52, 2.75]	
(5) Oster (2019) estimate	2.21	

Note: N = 9,580. Brackets report 95% confidence intervals. The OLS rows (1)–(2) and IPW use robust standard errors. AIPW inference is based on 100 bootstrap samples; confidence intervals use the 2.5th and 97.5th percentiles of the bootstrap distribution. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B1 in Appendix B).

Table C2. Effects of Spouse's Commuting on Campaign Lines on Advertisement Viewing

	View ad at campaign train	
Method	Estimate	R-squared
(1) Regression without covariates	3.52	0.008
	(0.72)	
	[2.11, 4.93]	
(2) Regression with covariates	3.55	0.020
	(0.74)	
	[2.10, 5.00]	
(3) IPW	3.49	
	(0.75)	
	[2.03, 4.95]	
(4) AIPW (Doubly robust estimator)	3.55	
	(0.72)	
	[2.02, 4.71]	
(5) Oster (2019) estimate	3.56	

Note: N = 3,685. Brackets report 95% confidence intervals. The OLS rows (1)–(2) and IPW use robust standard errors. AIPW inference is based on 100 bootstrap samples; confidence intervals use the 2.5th and 97.5th percentiles of the bootstrap distribution. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B2 in Appendix B).

Table C3. ITT Effects of Respondent's Commuting on Campaign Lines on Preventive Behaviors

	Tested		Vaccinated	
Method	Estimate	R-squared	Estimate	R-squared
(1) Regression without covariates	-0.15	0.000	-0.14	0.000
	(0.23)		(0.11)	
	[-0.59, 0.30]		[-0.35, 0.07]	
(2) Regression with covariates	-0.10	0.005	-0.12	0.007
	(0.23)		(0.10)	
	[-0.55, 0.35]		[-0.33, 0.08]	
(3) IPW	-0.09		-0.12	
	(0.23)		(0.10)	
	[-0.54, 0.35]		[-0.32, 0.08]	
(4) AIPW (Doubly robust estimator)	-0.10		-0.12	
	(0.25)		(0.10)	
	[-0.62, 0.29]		[-0.30, 0.08]	
(5) Oster (2019) estimate	-0.08		-0.12	

Note: N = 9,580. Brackets report 95% confidence intervals. The OLS rows (1)–(2) and IPW use robust standard errors. AIPW inference is based on 100 bootstrap samples; confidence intervals use the 2.5th and 97.5th percentiles of the bootstrap distribution. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B1 in Appendix B).

Table C4. ITT Effects of Spouse's Commuting on Campaign Lines on Respondent's Preventive Behaviors

	Tested		Vaccinated	
Method	Estimate	R-squared	Estimate	R-squared
(1) Regression without covariates	1.40	0.003	0.21	0.000
	(0.47)		(0.20)	
	[0.49, 2.32]		[-0.18, 0.61]	
(2) Regression with covariates	1.69	0.015	0.44	0.021
	(0.53)		(0.24)	
	[0.66, 2.72]		[-0.04, 0.91]	
(3) IPW	1.99		0.65	
	(0.69)		(0.46)	
	[0.64, 3.34]		[-0.24, 1.54]	
(4) AIPW (Doubly robust estimator)	2.00		0.68	
	(0.68)		(0.49)	
	[0.84, 3.30]		[-0.00, 1.97]	
(5) Oster (2019) estimate	1.80		0.43	

Note: N = 3,685. Brackets report 95% confidence intervals. The OLS rows (1)–(2) and IPW use robust standard errors. AIPW inference is based on 100 bootstrap samples; confidence intervals use the 2.5th and 97.5th percentiles of the bootstrap distribution. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B2 in Appendix B).

Table C5. ITT Effects of Respondent's Commuting on Campaign Lines on Preventive Behaviors (Study 2 Sample)

	Tested		Vaccinated	
Method	Estimate	R-squared	Estimate	R-squared
(1) Regression without covariates	0.06	0.000	-0.39	0.001
	(0.41)		(0.18)	
	[-0.74, 0.87]		[-0.74, -0.03]	
(2) Regression with covariates	-0.37	0.015	-0.49	0.021
	(0.45)		(0.21)	
	[-1.25, 0.52]		[-0.90, -0.09]	
(3) IPW	-0.28		-0.49	
	(0.47)		(0.21)	
	[-1.20, 0.64]		[-0.90, -0.07]	
(4) AIPW (Doubly robust estimator)	-0.29		-0.47	
	(0.47)		(0.23)	
	[-1.11, 0.50]		[-0.88, -0.10]	
(5) Oster (2019) estimate	-0.50		-0.49	

Note: N = 3,685. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B2 in Appendix B); we also include an indicator for whether the respondent's spouse commuted on campaign lines. Brackets report 95% confidence intervals. The OLS rows (1)–(2) and IPW use robust standard errors. AIPW inference is based on 100 bootstrap samples; confidence intervals use the 2.5th and 97.5th percentiles of the bootstrap distribution.

Table C6. ITT Effects of Spouse's Commuting on Campaign Lines on Respondent's Long-Run Preventive Behaviors

	Tested (one year after campaign)		Vaccinated (one year after campaign)	
Method	Estimate	R-squared	Estimate	R-squared
(1) Regression without covariates	4.54	0.004	0.38	0.000
	(1.21)		(0.36)	
	[2.17, 6.90]		[-0.33, 1.09]	
(2) Regression with covariates	5.18	0.032	0.44	0.012
	(1.30)		(0.43)	
	[2.63, 7.73]		[-0.39, 1.28]	
(3) IPW	5.56		0.75	
	(1.43)		(0.57)	
	[2.76, 8.36]		[-0.36, 1.86]	
(4) AIPW (Doubly robust estimator)	5.69		0.81	
	(1.45)		(0.57)	
	[2.86, 8.22]		[-0.24, 2.03]	
(5) Oster (2019) estimate	4.81		0.46	

Note: N = 3,685. Brackets report 95% confidence intervals. The OLS rows (1)–(2) and IPW use robust standard errors. AIPW inference is based on 100 bootstrap samples; confidence intervals use the 2.5th and 97.5th percentiles of the bootstrap distribution. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B2 in Appendix B).

Table C7. ITT Effects of Spouse's Commuting on Campaign Lines on Long-Run Vaccination, by Channels

	Vaccinated (MHLW program)		Vaccinated (Out-of-pocket)		Vaccinated (Local government subsidy)	
Method	Estimate	R-squared	Estimate	R-squared	Estimate	R-squared
(1) Regression without covariates	0.38	0.000	0.01	0.000	0.40	0.001
	(0.36)		(0.13)		(0.22)	
	[-0.33, 1.09]		[-0.24, 0.26]		[-0.03, 0.83]	
(2) Regression with covariates	0.44	0.012	-0.06	0.007	0.55	0.009
	(0.43)		(0.12)		(0.26)	
	[-0.39, 1.28]		[-0.30, 0.18]		[0.04, 1.06]	
(3) IPW	0.75		-0.06		0.56	
	(0.57)		(0.10)		(0.29)	
	[-0.36, 1.86]		[-0.26, 0.15]		[-0.01, 1.14]	
(4) AIPW (Doubly robust estimator)	0.81		-0.06		0.59	
	(0.57)		(0.11)		(0.32)	
	[-0.24, 2.03]		[-0.31, 0.15]		[0.05, 1.18]	
(5) Oster (2019) estimate	0.42		-0.08		0.55	

Note: N = 3,685. Brackets report 95% confidence intervals. The OLS rows (1)–(2) and IPW use robust standard errors. AIPW inference is based on 100 bootstrap samples; confidence intervals use the 2.5th and 97.5th percentiles of the bootstrap distribution. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B2 in Appendix B).

Table C8. Effects of Respondent's and Spouse's Commuting on Campaign Lines on Respondent's Advertisement Viewing

	View ad at campaign train		
	(1)	(2)	(3)
Respondent commuted on campaign lines (A)	2.169***		1.682**
	(0.631)		(0.726)
Spouse commuted on campaign lines (B)		3.555***	4.285***
		(0.739)	(1.256)
(A) × (B)			−1.797
			(1.588)
Covariates	X	X	X
P-value of linear Hypothesis Test 1: (A) + (A) × (B) = 0			0.936
P-value of linear Hypothesis Test 2: (A) = (B)			0.059
Num.Obs.	3685	3685	3685
R2	0.003	0.019	0.020
R2 Adj.	0.003	0.008	0.008

Note: $*\, p < 0.1$; $**\, p < 0.05$; $***\, p < 0.01$. Robust standard errors are reported in parentheses. Effects are expressed in percentage points. We use Study 2 data. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B2 in Appendix B).

Table C9. Instrumental-Variables Effects of Advertisement Viewing on Campaign Lines on Preventive Behaviors (Instrument: Respondent's Commuting on Campaign Lines)

	Tested		Vaccinated	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
View advertisement on campaign lines	4.42***	−4.40	2.11**	−5.54
	(1.27)	(10.41)	(0.83)	(4.86)
F-statistic of Instrument		34.25		34.25
Num.Obs.	9580	9580	9580	9580
R2	0.011	−0.011	0.012	−0.061
R2 Adj.	0.006	−0.016	0.007	−0.066

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parentheses. We use Study 1 data. Outcomes are indicators for antibody testing or vaccination within one month after the advertisement campaign launched. Treatment indicates advertisement viewing on campaign lines; the excluded instrument indicates whether the respondent commuted on campaign lines. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B1 in Appendix B).

Table C10. Instrumental-Variables Effects of Advertisement Viewing on Campaign Lines on Long-Run Preventive Behaviors (Instrument: Respondent's Commuting on Campaign Lines)

	Tested (1 year after)		Vaccinated (1 year after)	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
View advertisement on campaign lines	11.16***	5.07	2.43*	−14.51
	(3.16)	(42.91)	(1.35)	(14.57)
F-statistic of Instrument		17.37		17.37
Num.Obs.	5444	5444	5444	5444
R2	0.027	0.026	0.011	−0.074
R2 Adj.	0.018	0.017	0.002	−0.084

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parentheses. We use Study 3 data. Outcomes are indicators for antibody testing or vaccination within one year after the advertisement campaign launched. Treatment indicates advertisement viewing on campaign lines; the excluded instrument indicates whether the respondent commuted on campaign lines. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B1 in Appendix B).

Table C11. Effects of Respondent's and Spouse's Commuting on Campaign Lines on Antibody Testing (Study 2; Respondents Who Did Not View the JR Advertisement)

	Antibody testing		
	(1)	(2)	(3)
Respondent commuted on campaign lines (A)	0.160		−0.110
	(0.400)		(0.449)
Spouse commuted on campaign lines (B)		1.451***	1.817**
		(0.479)	(0.878)
(A) × (B)			−0.467
			(1.041)
Covariates	X	X	X
P-value of linear Hypothesis Test: (B) + (A) × (B) = 0			0.024
Num.Obs.	3543	3543	3543
R2	0.000	0.014	0.014
R2 Adj.	0.000	0.002	0.001

Note: $* p < 0.1$; $** p < 0.05$; $* * * p < 0.01$. Robust standard errors are reported in parentheses. Effects are expressed in percentage points. Covariates include the respondent's age, education, income, place of residence, employment status, and household composition (see Figure B2 in Appendix B).

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