DISCUSSION PAPER SERIES

IZA DP No. 16053

Risk Compensation after COVID-19 Vaccination

Jisoo Hwang
Seung-sik Hwang
Hyuncheol Bryant Kim
Jungmin Lee
Junseok Lee

APRIL 2023

I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 16053

Risk Compensation after COVID-19 Vaccination

Jisoo Hwang
Seoul National University

Seung-sik Hwang
Seoul National University

Hyuncheol Bryant Kim
Hong Kong University of Science and Technology and IZA

Jungmin Lee
Seoul National University and IZA

Junseok Lee
UC Berkeley

APRIL 2023

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Risk Compensation after COVID-19 Vaccination*

This paper studies the causal impacts of vaccine eligibility on social distancing behaviors (risk compensation). We apply a regression discontinuity design around the birth date cutoff of vaccine eligibility using large, high-frequency data from credit card and airline companies as well as survey data. We find no evidence of risk compensation although vaccine take-up increases substantially with eligibility. We find some evidence of self-selection into vaccine take-up based on perception towards vaccine effectiveness and side effects, but we do not find that the treatment effects differ between compliers and never-takers.

JEL Classification: I12, I18

Keywords: COVID-19, vaccine take-up, selection, risk compensation, social distancing

Corresponding author:
Jungmin Lee
Department of Economics
Seoul National University
1 Gwanak-ro
Gwanak-gu
Seoul
South Korea
E-mail: jm-lee90@snu.ac.kr

* We thank Ajin Lee, Zhuan Pei, Cristian Pop-Eleches, and participants at 2021 Asia Impact Evaluation Conference, Korea International Economic Association 2021 Winter Conference, and Korean Association of Health Economics and Policy 2021 Fall Conference for helpful comments. We are also grateful to Jeju Air for providing the airline data. Credit card data is obtained via South Korean government's data voucher program. This research is financially supported by the Interdisciplinary Research Project Grant, Seoul National University, and the Ministry of Education of the Republic of Korea and the National Research Foundation of Korea (NRF-2020S1A3A2A02104190). This project received IRB approval from the Hong Kong University of Science and Technology (protocol ID:HREP-2021-0352).

1 Introduction

Boosting vaccine take-up is deemed crucial for public health during the COVID-19 pandemic. One concern of mass vaccination, however, is potential risk compensation. Risk compensation, or the Peltzman effect, is a behavioral response to a change in perceived risk (Peltzman 1975; Evans and Graham 1991; Kremer 1996). In contrast to clinical trials where people are unaware of their treatment status, individuals in real-life settings may reduce their social distancing efforts after vaccination because they perceive lower risks of COVID-19 infection. Risk compensation may thus offset the benefits of vaccination and lead to higher risks of infection in mass vaccination settings than in clinical trials.[1]

This paper studies the causal impacts of vaccine eligibility on social distancing behaviors. The national vaccination schedule in South Korea, where vaccination eligibility dates differed by birth date, provides us a unique opportunity to understand vaccine take-up decisions and find credible evidence on risk compensation. To be specific, we use a regression discontinuity design (RDD) by taking advantage on the difference in vaccination timing between those born just before and after December 31, 1961, using exact birth date information. Those born before the cutoff (treatment group) became eligible for the first dose from June 6, 2021, whereas those born after the cutoff (control group) became eligible later on July 26, 2021. We have a window of 45 days where we can observe behavioral differences between those who were born just before and after the cutoff date.[2]

[1]A simulation study shows that even with high vaccine efficacy, a 3.2-fold increase of exposure to virus would halve vaccination benefits (Ioannidis 2021). Moreover, the selection of subjects into a clinical trial could be different from a scale-up project that addresses a general population. If a scale-up project encourages specific groups of people to get vaccinated, the effects of vaccination on risky behaviors could differ from those in a clinical trial. In fact, Mukherjee et al. (2022) estimated COVID vaccine effectiveness taking advantage of age cutoff in a mass vaccination setting in India, and found the vaccine's impact on COVID-19 infection in the scale-up setting is different from that in medical trials in the U.K. (Nasreen et al. 2022) and Canada (Bernal et al. 2021), but it is not possible to distinguish between possible reasons including demographic factors, take-up rate, selection, and risk compensation.

[2]This also leads to a time gap in the start date of the second dose between the two groups (August 23, 2021 and September 6, 2021), which we exploit to study the behavioral response to the second

We use large, high-frequency, administrative data as well as survey data to measure vaccine take-up and social distancing behaviors. First, we obtain data on daily vaccination rates of the entire population born in 1961 and 1962 from the Korea Disease Control and Prevention Agency (KDCA) and estimate the first-stage effect of the vaccine rollout strategy on vaccine take-up of the target group.

Second, we examine the impacts on risk compensation using measures of social distancing from three different datasets—credit card, airline, and survey. The credit card company data contains daily transaction records by spending category and card holders' exact birth date for 394,930 individuals born in 1961 and 1962. The airline company data includes information on members' daily travel information and exact birth date for 33,613 individuals born in 1961 and 1962. We also collect data from a survey on 3,018 people born in 1961 and 1962 implemented right before vaccines became eligible for the control group. Because the survey contains information on social distancing behaviors as well as individual's birth month-year and vaccination status, we are able to estimate the intention-to-treat (ITT) effect and the local average treatment effect (LATE) on compliers.

Third, we test for selection heterogeneity and external validity of the treatment effect (Brinch et al. 2017, Kim and Lee 2017, Bertanha and Imbens 2020, Einav et al. 2020, Kowalski 2022). To investigate whether individuals who take vaccination are different from those who do not, we identify always-takers, treated compliers, untreated compliers, and never-takers, then compare their baseline characteristics around the eligibility cutoff. We also assess the external validity of our LATE estimate by comparing average treatment effects on compliers and those on never-takers and always-takers.

To summarize our findings, first, we find that vaccine take-up increases by 63.4% points with eligibility in the survey data, similar to 65.8% found in the KDCA administrative data. Second, even with substantial compliance to the vaccination pol-

dose (full inoculation) as well.

icy, we find little evidence of risk compensation in terms of credit card usage, air travel, or social distancing behaviors. Third, comparing characteristics of compliance groups around the cutoff, we find little difference between always-takers and treated compliers or between treated and untreated compliers. We find some significant differences between untreated compliers and never-takers, however: never-takers are less likely to believe in vaccine effectiveness, more likely to worry about side effects, and are also less likely to be college-educated or to have white-collar jobs. Despite selection into vaccine take-up, we find no evidence of treatment effect heterogeneity. Testing equality in the average treatment effect across compliance groups around the threshold (Bertanha and Imbens 2020), we find that the effects are not statistically different. That is, there is no evidence of risk compensation among not only compliers but also always-takers and never-takers.

Our paper contributes to understanding risk compensation after COVID-19 vaccination. Our paper is closely related to Agrawal et al. (2022) and Aslim et al. (2022) which also study the impacts of COVID-19 vaccination on risk-mitigating behaviors and the delay of medical care, respectively. Agrawal et al. (2022) find no evidence on risk-mitigating behaviors including mask wearing or avoiding crowds and restaurants. Aslim et al. (2022) find that receiving a COVID-19 vaccine reduces the likelihood of delaying medical care. Both papers have identification challenges due to lack of birth date information, however. For example, Agrawal et al. (2022) use birth year as the running variable and rely on self-reported survey data. Aslim et al. (2022) employ difference-in-differences and instrumental variable estimation also using birth year.[3] Our approach is a RDD with exact birth date.

[3]There are some prior studies which examine cross-sectional or before and after differences by vaccination status. For example, Goldszmidt et al. (2021) find that those who are fully vaccinated are less likely to practice social distancing based on representative samples of twelve countries. In addition, Yamamura et al. (2022), Bernal et al. (2021) and Hunter and Brainard (2021) observe a small but significant increase in infection rates during the first few weeks following the first dose take-up of vaccines in Japan, the U.K., and Israel, respectively. Researchers speculated that the results reflected lower compliance with protective behaviors (Rubin et al. 2021; Independent SAGE 2021; SPI-B 2021). On the other hand, Wright et al. (2021) found little evidence of such changes in the U.K. These studies, however, are based on case-control or simple before and after comparison, and

To our knowledge, this paper is also the first to conduct a complier analysis of COVID-19 vaccination to explore selection into vaccination. Previous studies have relied on surveys asking people's willingness to take vaccines (Alsan and Eichmeyer 2021, Bughin et al. 2020, Khan et al. 2021, Kreps and Kriner 2021, Neumann-Böhme et al. 2020, Schwarzinger et al. 2021, Thunstrom et al. 2021). However, once vaccines become eligible, actual take-up may well differ from intentions. Identifying compliers and never-takers can help sharpen the policy target group to promote vaccination.

2 Institutional Details

2.1 COVID-19 social distancing policy in South Korea

South Korea responded effectively to COVID-19 pandemic through non-pharmaceutical interventions during the earlier phase of the pandemic when vaccines were not available. Regional lockdowns have never been imposed even during the period with the highest incidence. People could freely travel across regions and also use public transportation. Restaurants, shops, and facilities have remained largely open, although some constraints regarding closing hours or maximum capacity have been imposed depending on the number of new cases per week. For example, for most of our study period (June to October 2021), private gatherings of more than six people were banned and restaurants had to close by 10pm. Less than 50% capacity was allowed for cultural and religious facilities. Higher-risk facilities such as fitness centers, public baths, and nightlife venues (bars and karaokes) were at times subject to additional restrictions.[4] Wearing masks both indoors and outdoors has been mandatory during study period, and public compliance to the mask mandate has

thus their estimates might be biased due to unobservable confounders.

[4]During periods when the number of new cases were high, from July 12 to October 17, 2021 in Seoul, for instance, 1 person per $8m^2$ was allowed in public baths, karaoke bars, and indoor sports facilities. Customers could not use indoor sports centers for more than two hours or use shower rooms.

been relatively high (Lee and You 2020).

While these social distancing rules did not alter fundamentally with vaccine roll-out, some waivers were introduced for fully vaccinated individuals during the study period. Notably, restriction on the maximum number of people who can attend private gatherings was partly relaxed for fully vaccinated individuals. Those fully vaccinated were also exempt from the mandatory two-week quarantine after foreign travel or after contact with a COVID-19 patient, as long as they were tested negative on PCR tests. Most other social distancing rules, including the mask mandate, continued to apply during our study period regardless of vaccination status.[5]

From a global perspective, the South Korean government's response to COVID-19 was less restrictive than countries imposing lockdowns, but stricter than others without major social distancing policies. According to the Oxford COVID-19 Government Response Tracker which constructs a "COVID-19 stringency index" based on policies including stay-at-home orders, school closures, restrictions on gatherings, and face covering requirements, South Korea is considered to have below-average stringency among 184 countries (Hale et al. 2021). During our study period of July 2021, South Korea's stringency index was lower than China and Australia but higher than Israel and New Zealand; it was relatively similar with the U.S. and France. People could freely travel and visit various facilities during our study period, so there was enough room to potentially engage in risk compensation behaviors.

[5]The government announced in late May 2021 that people who receive at least one shot of the vaccine would be exempt from wearing masks outdoors from July 2021, but the decision was reversed the first week of July 2021. A more widespread relaxation of social distancing rules did not occur in Korea until November 2021, when the government declared a new stage of "living with COVID-19." Restrictions on the maximum number of people who can gather and business operation hours were significantly relaxed or lifted altogether. For example, up to ten (or twelve, depending on region) individuals could gather and restaurants could open 24 hours. This new scheme did not last long, however, due to the spread of the Omicron variant in December 2021.

2.2 COVID-19 vaccination in South Korea

The KDCA prioritized immunizing the elderly, and vaccine eligibility was determined by date of birth (Table 1). The time lag between age groups was mainly caused by the shortage of vaccine supply. Exceptions to the age criteria applied to priority groups and those who managed to reserve "leftover vaccines" that have become available after someone canceled or did not show up to their appointment.[6]

Figure 1 presents the take-up rate of the first and second doses among those born in 1961 and 1962, according to KDCA data. Vaccination rate increases sharply after each cohort becomes eligible (start dates marked with vertical lines). In fact, it takes less than two weeks from the start date for the take-up rate to reach about 80 percent for both the first and second doses in each cohort.

We divide our study period from June to October, 2021 into six time intervals (labeled T1–T6 in Figure 1) according to the national vaccine rollout schedule.[7] The first dose vaccination rates in T1, T2, and T3 differ significantly between the treatment and control groups. We argue that we can capture the cleanest estimate of risk compensation during T2. If some of the treatment group experience side effects shortly following their vaccination in T1, we would underestimate the true effect. If some of the control group experience side effects shortly following their vaccination in T3, we would overestimate the treatment effect. Similarly, we could underestimate and overestimate the treatment effect of the second dose in T4 and T5, respectively. The magnitude of bias could be smaller in T4 and T5 (compared to T1 and T3), however, as those who experienced side effects after the first dose are less likely to take the second dose (compared to those who did not experience side effects). Lastly in T6, we do not expect to see difference between the treatment and

[6]Priority groups include hospital staff, nursing home staff, inpatients and employees of nursing homes and long-term care facilities, teachers, flight attendants, police, firefighters, and military personnel. Leftover vaccines could be reserved through real-time booking system provided by major internet portals.

[7]The periods are as follows: T1 (June 7–June 30), T2 (July 1–July 25), T3 (July 26–Aug 25), T4 (Aug 26–Sept. 4), T5 (Sept. 5–Sept. 26), and T6 (Sept. 27–Oct. 7).

control groups' behaviors unless there are dynamic effects in risk compensation.

3 Data and Study Sample

3.1 Data

We use data from four different sources. First, we use administrative data from the KDCA, which contain daily vaccine take-up rates of the entire population of those born in 1961 and 1962. Using the data, we confirm whether the staggered introduction of vaccine eligibility led to a time gap in vaccination rates between the two cohorts.

Second, we use data on credit card usage obtained from Shinhan Card, the company with the largest market share (21.5%) in South Korea. The data record credit card usage by category in about ten-day intervals from January 2020 to September 2021 (from T1 to T6), aggregated by card holders' birth date.[8] We use data of 394,930 individuals born in 1961 and 1962 aggregated at the birth date level. The data are useful for the purpose of our study in that many risk compensation behaviors can be detected by an increase in spending in categories involving face-to-face interactions, such as restaurants and offline shopping.

Third, we use airline company data obtained from Jeju Air, whose market share (17.2%) is ranked top in the Korean domestic flight market.[9] We use individual-level daily data, which contain 30,874 airline members born in 1961 and 1962 with their exact birth date and travel history from January 2020 to September 2021.

Finally, we use data from a telephone survey conducted by Gallup Korea. A total of 3,018 people born in 1961 and 1962 were recruited from a broadly representative

[8] The nine spending categories are as follows: food and beverage, sports and entertainment, miscellaneous services (beauty salons, education, fuel), lodging, offline retail (supermarkets, department stores, cars), clothing (clothes, accessories, cosmetics), home appliances (furniture, electronics), medical expenditure (hospitals, pharmacies), and online retail.

[9] The company specializes in a domestic route, between Seoul (Gimpo) and Jeju Island, the most popular vacation venue in South Korea. Seoul–Jeju is the busiest airline route in the world.

panel of the survey company. The sampling design involves stratification by sex, age, and region. The survey was conducted during the week of July 14–23, 2021, about a month after the treatment group became eligible for vaccines but a few days before the control group became eligible (corresponding to period T2 in Figure 1).

The survey data includes a rich set of information on individuals' demographic characteristics and social distancing behaviors. Particularly, information on individual's vaccination status allows us to distinguish between different compliance groups. Although self-reported and subject to reporting errors, the survey includes questions on their social distancing behaviors, vaccination status, risk perceptions, and birth month-year. Questions on social distancing include whether the respondent avoided meeting with others and also his/her participation in specific types of activities such as travel and eating out at restaurants.[10] The questionnaire is attached in Appendix D.

3.2 Outcome variables

Our main outcome variables besides vaccination rates are 1) the average per-day per-user number of offline credit card transactions, 2) the average per-day per-member number of air travel, and 3) social distancing behaviors measured in the survey. First, using the credit card data, we compute the average daily number of transactions divided by the number of card holders in each birth date.[11] We focus on offline transactions to measure the degree to which a person gets involved in a situation with risk of infection. We exclude online shopping and medical expenditure because they do not necessarily involve risk compensation. We report the results for not only total offline transactions but also for each spending category separately.

[10] Activities include travel, meeting relatives and friends, visits to church/temple and hospital during the last month, as well as visits to restaurant/cafe, gym, bar/karaoke, hair salon/spa and cultural facility (museum, concert hall, movie theater), grocery shopping, and public transportation use (bus, train, subway) during the past week.

[11] We divide the total number of credit card transactions by the number of credit card users during the pre-treatment period (January 1, 2021–June 5, 2021). We get similar results when we use the number of credit card users in 2020 as the denominator (not reported).

Second, using the airline data, we construct the average per-day per-member number of air travel. Similar to the way we construct the outcome variable for card spending, for each time interval from T1 to T6, we compute the average daily number of trips made by members of each birth date and divide it by the total number of airline members in each birth date. Using detailed flight information, we are also able to construct the variable for each route separately and check whether there are any differences across routes, which may correspond to different purposes of trips.

Finally, from the survey data, we construct three main indexes of social distancing behavior: "engaged in social activities," which is the average of indicators for the ten types of social activities (listed in footnote 10), "ever engaged in social activities without mask," which equals one if the respondent has ever engaged in any of these activities without a mask, and "avoided contact with others" based on a general question. Again, we do not include hospital visit as a social activity because it is not necessarily risk compensation behavior. For example, individuals may have visited hospitals in order to get vaccinated, to receive treatment for vaccine side effects, or to catch up on delayed medical care (Aslim et al. 2022).

3.3 Summary statistics

Panels A, B, and C of Table 2 present the summary statistics of the study sample in the credit card, airline, and survey data, respectively. Panel A shows that the average age is 59.5 and about 20% reside in Seoul. The sample size is 730 (= 365 $*$ 2) because the credit card data are aggregated by birth date. There are 394,930 individuals born in 1961 and 1962 in our credit card data (on average 541 in each birth date) which comprises about 22.6% of the population.[12] The average number of offline transactions during the pre-treatment period (from January 1, 2021 to June 5, 2021)

[12]There are 888,491 and 858,519 residents age 60 and 61 (born in 1961 and 1962), respectively, in South Korea as of December 2022 (Ministry of Interior and Safety).

is about 0.51 transactions per day per card holder. Panel B summarizes the data on Jeju Air members born in 1961 and 1962. Among the 33,613 members, 56% are male, and the average membership duration is about 3.6 years (1,316 days). During the pre-treatment period, the average number of trips per day per member is 0.0017.

Compared to the administrative data, the survey data include a richer set of socioeconomic background variables. The average age is 59.5, about 51% of respondents are male, 83% are married, about 17% reside in Seoul, 29% are self-employed, and about 30% do not currently work for pay. The survey data also inform us of respondents' preferences and attitudes, such as political orientation, risk attitudes, and concern about COVID-19 vaccine side effects.

4 Empirical Framework

This section explains our empirical framework. Our first approach in subsection 4.1 is the conventional RDD. Combining the effects of vaccine eligibility on vaccine take-up and social distancing behaviors, we find the LATE on compliers. To explore the generalizability of the LATE, we study selection heterogeneity and external validity in subsection 4.2.

4.1 Setup of the empirical analysis

We implement a regression discontinuity design by taking advantage of the fact that COVID-19 vaccination eligibility varies discontinuously over date of birth. For each outcome variable, we estimate the following model:

$$Y_i = \beta \cdot \mathbb{I}(Z_i \geq \tau) + g(Z_i) + e_i \qquad (1)$$

where Y_i is the outcome variable such as vaccine take-up or social distancing behavior of individual i, Z_i is date of birth of individual i and $g(\cdot)$ is a continuous function which capture the link between date of birth and the outcome variable. τ is the eligibility cutoff date of birth, December 31, 1961, which is the main cutoff in our analysis.

When Y_i is an indicator for vaccine take-up, β becomes the first-stage effect of vaccine eligibility on actual vaccine take-up. When Y_i is social distancing behavior, β is the ITT effect of vaccine eligibility on social distancing behavior. The LATE can be identified by dividing the latter by the former, capturing the effect of vaccination on social distancing behaviors, i.e., the risk compensation effect.

We use a non-parametric approach to estimate the function $g(\cdot)$ by approximating it through a polynomial function of Z_i over a narrow range of data. In the main analysis, we use a local linear regression with uniform kernel. Our preferred bandwidth is 365 days because we believe these are narrow enough to compare observations below and above the cutoff and wide enough to be precise. We report a series of robustness checks with different polynomial degrees and bandwidths to show how robust our results are to alternative specifications.

A critical assumption to our identification strategy is that individuals who were born just before and after the cutoff date are comparable in terms of observable and unobservable determinants of risk compensation behavior. The assumption seems reasonable since we have no *a priori* reason why those born just before and after the cutoff would behave differently. To support this assumption, we test the smoothness of observable baseline characteristics around the cutoff, including 1) average daily transactions by spending category during the pre-treatment period and residential area in the credit card data (panel A of Table A1 and Figure A1), 2) sex, average days of membership, and average daily trips by route during the pre-treatment period in the airline data (panel B of Table A1 and Figure A2), 3) sex, marital status, residential area, education level, job, political orientation, and attitude and belief about COVID-19 vaccines in the survey data (Table A2 and Figure A3).

Indeed, we do not find any significant differences between individuals just below and above the cutoff in most observable characteristics. Only three of 32 discontinuity estimates turn out to be statistically significant. We also find that the results are robust to alternative bandwidths and polynomial degrees as well as to the inclusion of baseline controls (Figure A4, Figure A5, and Figure A6).[13]

4.2 Selection heterogeneity and external validity

The effect of vaccine eligibility estimated in equation (1) is identified based on compliers. The average effect on compliers may not be generalizable to the general population, and the treatment effects of compliers and never-takers could be different.

We first check selection heterogeneity by comparing observable characteristics of compliers, always-takers, and never-takers, similar in spirit to Kim and Lee (2017) and Einav et al. (2020). To do so, we restrict the sample to those who took vaccination regardless of their eligibility and apply the same RD framework as our main model to detect any discrete changes at the cutoff in the characteristic under investigation. Because everyone took vaccination in this restricted sample (all vaccinated sample), any significant difference around the cutoff should be due to the compositional change of vaccine-takers around the cutoff; those born just before the cutoff consist of always-takers and compliers, while those born just after consist of only always-takers. Thus, estimating equation (1) with the restricted sample allows us to

[13] We also conduct the McCrary test examining whether the density function of the running variable is smooth around the cutoff (McCrary 2008; Lee and Lemieux 2010). Panels A, B, and C of Table A3 present the t-statsitics for the McCrary test around the treatment and placebo cutoffs, using several alternative bandwidths for the survey, credit card, and airline company data, respectively. While some estimates turn out to be statistically significant in panel A, they are not disproportionately larger than those at the placebo cutoffs. Also, most t-statistics are significant in panels B and C as administrative data give precise estimates, but the t-statistics around the cutoff are not particularly larger than those at the placebo cutoffs, neither. Figure A7 illustrates the density of the running variable in the three datasets that we use in this study. Around the time when people in our study sample were born, the number of births in January is larger than that in December. This phenomenon is known to be related to the specific age-reckoning method in Korea, where one's age is counted using the number of calendar years one has lived. Given this culture, parents-to-be might select January to make their children enjoy a relative-age advantage within their birth cohort (Kim 2021). It is difficult, however, to predict how parents' birth month selection could bias our estimates for the effect of vaccination on risk compensation. It is unlikely that the birth month selection has a long-term effect after 60 years.

compare the characteristics of treated compliers and always-takers. Similarly, if we restrict the sample to those who did not take vaccination (all unvaccinated sample), we can compare untreated compliers with never-takers. Therefore, we can characterize four distinct groups, treated compliers, untreated compliers, always-takers, and never-takers, each of which can be of particular importance to policy makers. Appendix A provides details of the estimation method.

We also assess the external validity of the LATE estimates to never-takers and always-takers. The question regarding never-takers is particularly important considering how governments tried to address vaccine hesitancy in order to boost vaccine take-up. Following Bertanha and Imbens (2020), we jointly test for discontinuity in social distancing behavior at the cutoff in the two samples we defined above: all vaccinated and all unvaccinated samples. Rejecting the null of no difference at the threshold between treated compliers and always-takers and between untreated compliers and never-takers would cast doubt on the external validity of our LATE estimates.[14]

5 Empirical Findings

5.1 Impacts of vaccination on social distancing behaviors

As shown in Figure 1, vaccine take-up rates among those born in 1961 and 1962 increased in tandem with the staggered introduction of vaccine eligibility. In Table 3 panel A, we present the differences in vaccine take-up rates between the 1961 cohort and 1962 cohort using the KDCA data, separately for each period from T1 to T6. The differences in vaccine take-up during periods T1, T2, and T3 are driven by the first dose of vaccination, while the differences from the second dose are largely

[14] As Bertanha and Imbens (2020) explain, however, the failure to reject the null of no difference does not necessarily support external validity but suggests a lack of significant treatment effect heterogeneity (Brinch et al. 2017). Also note that the test for all unvaccinated sample here is a RD version of the untreated outcome test in Kowalski (2022) for RCT.

ignorable. For example, in T2, the difference in the first dose vaccination is 65.8% while that in the second dose is 0.2%. On the other hand, the differences in vaccination rates during periods T4 and T5 are mainly driven by the second dose. Lastly, the differences in take-up rates almost disappear in T6. Given that some people experience side effects shortly after vaccination and that it typically takes about two weeks for vaccines to become highly effective at protecting against COVID-19 infections, we expect to observe risk compensation particularly at T2 (for first dose) and T5 (for second dose).[15]

Figure 2 presents the results of credit card usage (panel A) and domestic air travel (panel B) in each period from T1 to T6. The horizontal axis represents the birth date centered around December 31, 1961 (solid vertical line). The left-hand side of the cutoff are those born in 1962 and the right-hand side are those born in 1961. We divide people based on their birth date into 72 bins. Each dot in the figure indicates the mean of the outcome variable for those born in each birth date bin, and the size of the dot reflects the sample size of the bin. The vertical difference between the two trends at the cutoff is a graphical analog of β in equation (1).

The graphs show no clear evidence of risk compensation in both credit card usage and domestic air travel throughout the periods. We observe no discernible gaps at the cutoff. Because air travel is a relatively rare event, the results in panel B are noisier than in panel A.

The corresponding regression results are shown in panels B and C of Table 3. For instance, the standard errors in panel B are around 0.004, meaning that we are able to capture any change greater than 0.00784 (= 0.004∗1.96) transactions per day in the credit card data. As about 0.5 daily transactions were made per user in each period, this corresponds to a 1.57% (= 0.00784/0.5) increase in credit card usage.

[15]Note that at T2 the control group did not receive the first dose yet, whereas at T5 the control group also starts to receive the second dose. Considering that some may experience side effects and decrease their social activities shortly after vaccination, our estimates of risk compensation at T5 could be biased upwards. We nevertheless find no evidence of an increase in social activities in the treatment group.

Therefore, the results in Table 3 mean that, if any, the risk compensation effect is no larger than 1.57%.[16]

We conduct a similar analysis using the survey data. Note that the survey was conducted on July 14–23 (period T2), right before the control group became eligible to receive the first dose. Figure 3 presents the results where observations are aggregated at birth month-year level. Panel A of Figure 3 shows that the take-up rate of the first dose increased significantly at the eligibility cutoff, consistent with what we observe from the administrative data in panel A of Table 3. The regression results presented in Table 4 column (1) confirm the graphical finding. Eligibility for first dose increases the take-up rate by 63.4% points. It is a 352% increase (=63.4/18.0) as the take-up rate for the control group is 18.0%.

Panels B to D of Figure 3 and columns (2)–(4) of Table 4 present the results on three indexes of social distancing behaviors based on self-reported responses. There is no clear discontinuity in any of these outcomes near the cutoff.[17] None of the coefficients are statistically significant and the estimates are relatively precise. For example, we are able to capture any change in the average probability of engaging in social activities greater than 2.4 ($= 0.012 * 1.96$) percentage points.

Table A4, Table A5, and Table A6 (and Figure A8, Figure A9, and Figure A10) present impacts on credit card utilization, airline trips, and self-reported social distancing behaviors by category, respectively. Overall, we do not find discrete jumps at the cutoff across categories and periods. Although there is concern of multiple hypothesis testing, we find an interesting pattern that medical expenditure (panel I of Table A4) and hospital visits (column (12) of Table A6) increased. We do not

[16] An exception is observed in panel B column (1), where we find a small and significant *decrease* in credit card usage during T1, shortly after the first dose eligibility date. This is opposite to risk compensation, and could reflect a temporary drop in offline spending due to mild complications after receiving the first shot. But the estimates and their significance vary depending on specifications using different polynomial degrees and alternative bandwidths as well as including baseline control (panels A and B of Figure A11).

[17] We conduct robustness checks using alternative bandwidths, polynomial degrees, and including baseline controls, and find largely similar results (see Figure A12).

wish to strongly claim that vaccination increased these two outcomes, however, because these results are not robust to alternative specifications (panels C and D of Figure A11 and panel A of Figure A13) and medical expenditure is also one of the few variables that are not balanced in the pre-treatment period (Table A1). If any, an increase in medical expenditure might reflect an increase in healthcare-seeking behavior upon being vaccinated as in Aslim et al. (2022) or due to vaccine complications.

Lastly, we explore possible risk compensation at other age groups shown in Table 1. Difference in the timing of vaccine eligibility is 49 days between 1961 and 1962 cohorts; there are also some differences in timing (10 to 57 days) between 1971–1972, 1966–1967, 1956–1957, and 1946–1947 cohorts. Figure A14 presents the number of credit card transactions (panel A) and domestic air trips (panel B) in the first 10–12 days after vaccination for each cohort as well as in the stacked-up sample. We confirm no clear evidence of risk compensation in both credit card usage and domestic air travel.

In sum, we conclude that there is no evidence of risk compensation after COVID-19 vaccination whether we use measures from administrative data of credit card and airline companies or self-reported survey data.[18]

5.2 Selection into vaccination and external validity beyond compliers

In this section, we analyze the characteristics of (treated and untreated) compliers, always-takers, and never-takers, which allows us to understand differences between those who respond and not respond to vaccine eligibility. Note that we can only use the survey data for this exercise because we need information on both eligibility (birth date) and actual vaccine take-up status.

[18] We also find similar results when we employ event-study difference-in-differences instead of RD. See Appendix B and Figure A15.

First, as we explain in Appendix A, the proportions of always-takers, compliers, and never-takers around the cutoff can be identified using equations (A2)–(A4) as 17.2%, 63.4%, and 19.4%, respectively. We then present the means of covariates around the cutoff for each of these compliance groups in columns (1)–(4) of Table 5.

Next, we test for selection heterogeneity by comparing the means of individual characteristics between always-takers and treated compliers in column (5) and between untreated compliers and never-takers in column (6) of Table 5. Figure A16 and Figure A17 present corresponding results graphically.[19] The most salient difference between compliers and never-takers is their perception towards vaccine effectiveness and side effects (column (6) of Table 5).[20] Never-takers are significantly less likely to believe that vaccines are effective and more likely to worry about side effects, which apparently explain why they refuse to get vaccination. We also find that never-takers tend to be less-educated and less likely to be white-collar workers than compliers. We do not find substantial differences in other characteristics, however, including political or risk preferences.[21] The findings suggest that perceptions about vaccine effectiveness and side effects could be important to increase vaccine take-up.

We assess the external validity of the LATE to other compliance groups, especially because there are some differences in individual baseline characteristics between compliers and never-takers. Specifically, we look for discontinuities in the two sets of conditional mean graphs, outcomes between treated compliers and always-takers as well as between untreated compliers and never-takers (Bertanha and Imbens 2020). We do not observe significant jumps around the cutoff in social distancing behaviors (Figure 4).

To test this formally, we jointly test equations (A8) and (A9) in Appendix A.

[19]More details are explained in subsection 4.2 and Appendix A.

[20]No selection heterogeneity is observed between always-takers and treated compliers (column (5)) as well as treated and untreated compliers (not reported).

[21]This is different from findings of some surveys on vaccine willingness in the U.S that vaccine take-up is correlated with political preference (Reiter et al. 2020, Kreps and Kriner 2021).

Specifically, columns (5) and (6) of Table 6 test equality between the average outcome of always-takers and treated compliers and never-takers and untreated compliers, respectively. The joint F-test results for the pair are shown in column (7). We do not find evidence of treatment effect heterogeneity, implying that selection in vaccine take-up does not necessarily translate into different treatment effects.

5.3 Discussion

There can be several explanations for the lack of risk compensation following COVID-19 vaccination. We present our preferred explanation first and then alternative explanations that we consider less plausible. Our preferred explanation is the high compliance with social distancing policies as well as limited vaccine-related incentives during the study period. According to data from the COVID-19 behavior tracker, most people in South Korea complied with social distancing policies such as wearing face masks outside the home (Belot et al. 2020; Jones 2020). As mentioned in Section 2.1, substantial incentives for the vaccinated such as the "vaccine pass" were not introduced during the study period. In addition, social distancing rules (the maximum number of people who can gather, business closing hours, maximum capacity at facilities) remained in place. The official rules were not strict from a global perspective, but social pressure to comply with them was strong (Task Force for Tackling COVID-19, Ministry of Foreign Affairs 2020). All these factors may have contributed to preventing the vaccinated from engaging in risk compensation behaviors.

The second explanation is that the vaccinated might have been concerned about potential risks they could impose on others, and thus were reluctant to engage in risky behaviors. For example, if a vaccinated individual in the treatment group engages in social activities, this might tempt his/her younger, unvaccinated family members to also participate in such activities. Because this kind of spillover effect is most likely to occur between spouses, we explore this possibility by checking whether our results differ by spouse's vaccine eligibility in Figure A18 and Table A7. We do not detect risk compensation among those with eligible spouses.

Third, our study sample might have had limited social activities even before the pandemic, and hence the scope for risk compensation small. However, as shown in Table 2, more than 70 percent of respondents in our survey sample are currently working. According to the 2019 Korean Time Use Survey, people in their 60s spend 12 percent of their daily time at work, not much lower than 18 percent among those in their 40s and 50s.[22] We find that people in their 60s spend more time than those in their 50s on social activities such as sports, face-to-face socializing, offline shopping, and religious activities. These activities would have been depressed during the pandemic, and thus could be resumed once vaccinated. Furthermore, exploiting the eligibility schedule, we replicate our analysis at other birth date cutoffs and find similar effects regardless of age group (Figure A14).

Fourth, risk compensation might be limited if people do not believe that vaccines significantly reduce COVID-19 related risks. In our setting, the concern could be particularly valid if there is lower credibility for AstraZeneca than Pfizer/Moderna, because the treatment and control groups were assigned to different vaccine types due to supply issues (Table 1). However, in Figure A14, we find similar results at other cutoffs in which the treatment and control groups were subject to the same vaccine type—birth date date cutoffs of 1971, 1966, and 1956.

Fifth, the potential for risk compensation could be limited due to a reduction in household income during the COVID-19 pandemic. Many businesses and households experienced financial difficulties during the pandemic as revenues and income fell. Lee and Yang (2021) showed that by April 2020, the COVID-19 outbreak eliminated about 4 percent of total non-farm employment nationwide in South Korea. We think that the income effect is unlikely to explain the lack of risk compensation, however, because we consistently find no evidence on risk compensation across

[22] Based on authors' calculations.

different educational attainment and occupation groups (Figure A18 and Table A7).

Sixth, it is worth noting that behavioral changes after COVID-19 vaccination can be driven not only by a change in the perceived risk of infection but also by a change in the cost of engaging in social activities. For example, if proof of vaccination is required to enter certain facilities such as restaurants or workplaces, it would be relatively less costly for vaccinated individuals to visit these places. Given that there were at least a few waivers for the vaccinated during the study period, our findings on the (lack of) behavioral changes after vaccination are in fact an upper bound of risk compensation.

Lastly, the lack of risk compensation might be due to the short time horizon of our study. A priori, it is unclear whether risk compensation would be more prominent in the short or long run. Risk compensation could be suppressed during relatively early stages following vaccination if it takes longer than a few months for those vaccinated to modify their risk perception. On the other hand, with pandemic fatigue, those vaccinated may engage in risky behaviors soon after vaccination. We are not able to test the long-run effect on risk compensation due to the study design.

6 Conclusion

This paper addresses risk compensation after COVID-19 vaccination and the role of selection into vaccine take-up by taking advantage of a unique vaccination rollout scheme in South Korea. Those born in 1961 became eligible for the first dose from June 6, 2021 while those born in 1962 became eligible only after July 26, 2021. We compare social distancing behaviors of those born just before and after the birth date cutoff of December 31, 1961 with large, high-frequency, administrative data from credit card and airline companies as well as survey data.

We find that vaccine eligibility significantly increased vaccine take-up, but that there is no evidence of risk compensation. That is, there is no significant increase in offline credit card spending, air travel, or self-reported social distancing efforts due to vaccination. The results are consistent across different subgroups and are also robust to alternative specifications. We also investigate the role of selection into vaccine take-up by comparing the characteristics and outcomes of compliers, always-takers, and never-takers. The key difference between compliers and never-takers is concern about vaccine side effects and their belief about vaccine effectiveness. Despite selection, we do not find evidence that social distancing behaviors differ across compliance groups.

Exploring potential mechanisms, we believe that our findings of no risk compensation is most likely the result of the public's high compliance with government's social distancing policies and the absence of substantial vaccine incentives like the vaccine pass during the study period. Now that COVID-19 vaccination may become routine, studying risk compensation and selection into take-up in such contexts would be an interesting avenue for future research.

References

Abadie, Alberto. 2003. "Semiparametric instrumental variable estimation of treatment response models." *Journal of Econometrics* 113 (2): 231–263.

Agrawal, Virat, Neeraj Sood, and Christopher M Whaley. 2022. "The ex-ante moral hazard effects of COVID-19 vaccines."Technical report, National Bureau of Economic Research.

Alsan, Marcella, and Sarah Eichmeyer. 2021. *Experimental Evidence on the Effectiveness of Non-Experts for Improving Vaccine Demand*. National Bureau of Economic Research.

Aslim, Erkmen G, Wei Fu, Chia-Lun Liu, and Erdal Tekin. 2022. "Vaccination Policy, Delayed Care, and Health Expenditures."Technical report, National Bureau of Economic Research.

Belot, Michèle, Syngjoo Choi, Julian C Jamison, Nicholas W Papageorge, Egon Tripodi, and Eline Van den Broek-Altenburg. 2020. "Six-country survey on COVID-19."

Bernal, Jamie Lopez, Nick Andrews, Charlotte Gower et al. 2021. "Effectiveness of the Pfizer-BioNTech and Oxford-AstraZeneca vaccines on covid-19 related symptoms, hospital admissions, and mortality in older adults in England: test negative case-control study." *British Medical Journal* 373.

Bertanha, Marinho, and Guido W Imbens. 2020. "External validity in fuzzy regression discontinuity designs." *Journal of Business & Economic Statistics* 38 (3): 593–612.

Brinch, Christian N, Magne Mogstad, and Matthew Wiswall. 2017. "Beyond LATE with a discrete instrument." *Journal of Political Economy* 125 (4): 985–1039.

Bughin, Jacques R, Michele Cincera, Kelly Peters, Dorota Reykowska, Marcin Żyszkiewicz, and Rafal Ohme. 2020. "Make it or break it: vaccination intent at the time of Covid-19." *Available at SSRN 3757618*.

Einav, Liran, Amy Finkelstein, Tamar Oostrom, Abigail Ostriker, and Heidi Williams. 2020. "Screening and selection: The case of mammograms." *American Economic Review* 110 (12): 3836–3870.

Evans, William N, and John D Graham. 1991. "Risk reduction or risk compensation? The case of mandatory safety-belt use laws." *Journal of Risk and Uncertainty* 4 (1): 61–73.

Goldszmidt, Rafael, Anna Petherick, Eduardo B Andrade, Thomas Hale, Rodrigo Furst, Toby Phillips, and Sarah Jones. 2021. "Protective Behaviors Against COVID-19 by Individual Vaccination Status in 12 Countries During the Pandemic." *JAMA network open* 4 (10): e2131137–e2131137.

Hale, Thomas, Noam Angrist, Rafael Goldszmidt et al. 2021. "A global panel database of pandemic policies (Oxford COVID-19 Government Response Tracker)." *Nature Human Behaviour* 5 (4): 529–538.

Hunter, Paul R, and Julii Suzanne Brainard. 2021. "Estimating the effectiveness of the Pfizer COVID-19 BNT162b2 vaccine after a single dose. A reanalysis of a study of 'real-world' vaccination outcomes from Israel." *Medrxiv*.

Imbens, Guido W, and Joshua D Angrist. 1994. "Identification and estimation of local average treatment effects." *Econometrica: journal of the Econometric Society* 467–475.

Independent SAGE. 2021. "Maintaining adherence to protective behaviours during vaccination rollout: Summary and recommendations." *The Independent SAGE Report* 30 9.

Ioannidis, John. 2021. "Benefit of COVID-19 Vaccination Accounting for Potential Risk Compensation." *Available at SSRN 3773950*.

Jones, Sarah P. 2020. "Imperial college london big data analytical unit and yougov plc. 2020." *Imperial College London YouGov Covid Data Hub, v1. 0, YouGov Plc*.

Khan, Hibah, Ms Era Dabla-Norris, Frederico Lima, and Alexandre Sollaci. 2021. *Who Doesnt Want to be Vaccinated? Determinants of Vaccine Hesitancy During COVID-19*. International Monetary Fund.

Kim, Hyuncheol Bryant, and Sun-mi Lee. 2017. "When public health intervention is not successful: Cost sharing, crowd-out, and selection in Korea's National Cancer Screening Program." *Journal of Health Economics* 53 100–116.

Kim, Taehoon. 2021. "Age culture, school-entry cutoff, and the choices of birth month and school-entry timing in South Korea." *Journal of Demographic Economics* 87 (1): 33–65.

Kowalski, Amanda E. 2022. "Behavior within a Clinical Trial and Implications for Mammography Guidelines."

Kremer, Michael. 1996. "Integrating behavioral choice into epidemiological models of AIDS." *The Quarterly Journal of Economics* 111 (2): 549–573.

Kreps, SE, and DL Kriner. 2021. "Factors influencing Covid-19 vaccine acceptance across subgroups in the United States: Evidence from a conjoint experiment." *Vaccine* 39 (24): 3250–3258.

Lee, David S, and Thomas Lemieux. 2010. "Regression discontinuity designs in economics." *Journal of Economic Literature* 48 (2): 281–355.

Lee, Jongkwan, and Hee-Seung Yang. 2021. "Pandemic and employment: Evidence from COVID-19 in South Korea." *Working Paper at http://www.kapf.or. kr.*

Lee, Minjung, and Myoungsoon You. 2020. "Psychological and behavioral responses in South Korea during the early stages of coronavirus disease 2019 (COVID-19)." *International Journal of Environmental Research and Public Health* 17 (9): 2977.

McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics* 142 (2): 698–714.

Mukherjee, Abhiroop, George Panayotov, Rik Sen, Harsha Dutta, and Pulak Ghosh. 2022. "Measuring vaccine effectiveness from limited public health datasets: Framework and estimates from Indias second COVID wave." *Science advances* 8 (18): eabn4274.

Nasreen, Sharifa, Hannah Chung, Siyi He et al. 2022. "Effectiveness of COVID-19 vaccines against symptomatic SARS-CoV-2 infection and severe outcomes with variants of concern in Ontario." *Nature Microbiology* 7 (3): 379–385.

Neumann-Böhme, Sebastian, Nirosha Elsem Varghese, Iryna Sabat, Pedro Pita Barros, Werner Brouwer, Job van Exel, Jonas Schreyögg, and Tom Stargardt. 2020. "Once we have it, will we use it? A European survey on willingness to be vaccinated against COVID-19."

Peltzman, Sam. 1975. "The effects of automobile safety regulation." *Journal of Political Economy* 83 (4): 677–725.

Reiter, Paul L, Michael L Pennell, and Mira L Katz. 2020. "Acceptability of a COVID-19 vaccine among adults in the United States: How many people would get vaccinated?" *Vaccine* 38 (42): 6500–6507.

Rubin, G James, Julii Brainard, Paul Hunter, and Susan Michie. 2021. "Are people letting down their guard too soon after covid-19 vaccination?" *British Medical Journal*.

Schwarzinger, Michaël, Verity Watson, Pierre Arwidson, François Alla, and Stéphane Luchini. 2021. "COVID-19 vaccine hesitancy in a representative working-age population in France: a survey experiment based on vaccine characteristics." *The Lancet Public Health* 6 (4): e210–e221.

SPI-B. 2021. "SPI-B: Possible impact of the COVID-19 vaccination programme on adherence to rules and guidance about personal protective behaviours aimed at preventing spread of the virus."

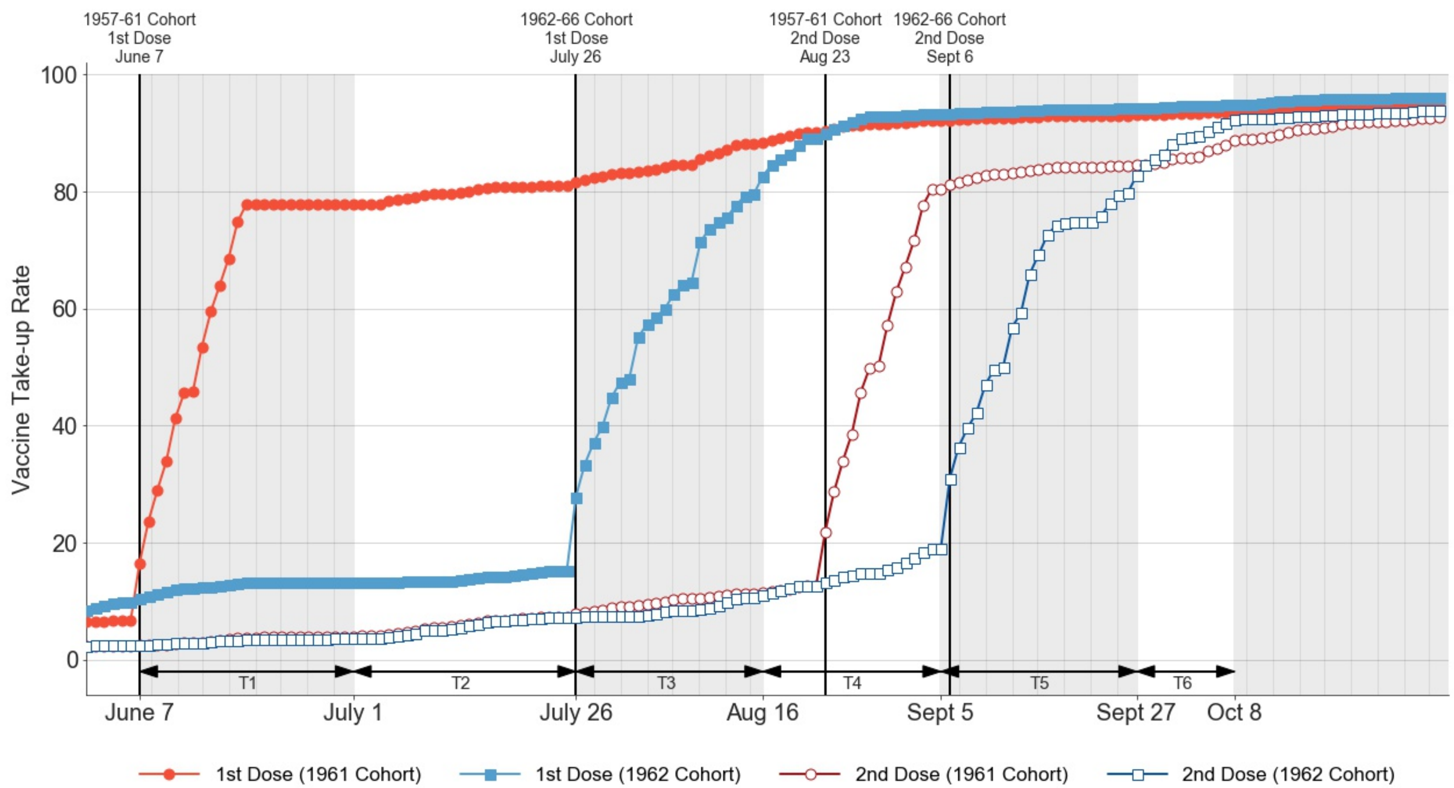
Task Force for Tackling COVID-19, Ministry of Foreign Affairs. 2020. "All about Korea's Response to COVID-19." https://www.mofa.go.kr/eng/brd/m_22591/view.do?seq=35&srchFr=&amp%3BsrchTo=&amp%3BsrchTp=&amp%3Bmulti_itm_seq=0&amp%3Bitm_seq_1=0&amp%3Bitm_seq_2=0&amp%3Bcompany_cd=&amp%3Bcompany_nm=&page=1&titleNm=.

Thunstrom, L, M Ashworth, D Finnoff, and S Newbold. 2021. "Hesitancy towards a COVID-19 vaccine and prospects for herd immunity. 2020." *Available at SSRN 3593098*.

Wright, Liam, Andrew Steptoe, Hei Wan Mak, and Daisy Fancourt. 2021. "Do people reduce compliance with COVID-19 guidelines following vaccination? A longitudinal analysis of matched UK adults." *MedRxiv*.

Yamamura, Eiji, Youki Kosaka, Yoshiro Tsutsui, and Fumio Ohtake. 2022. "Gender differences of the effect of vaccination on perceptions of COVID-19 and mental health in Japan." *arXiv preprint arXiv:2203.07663*.

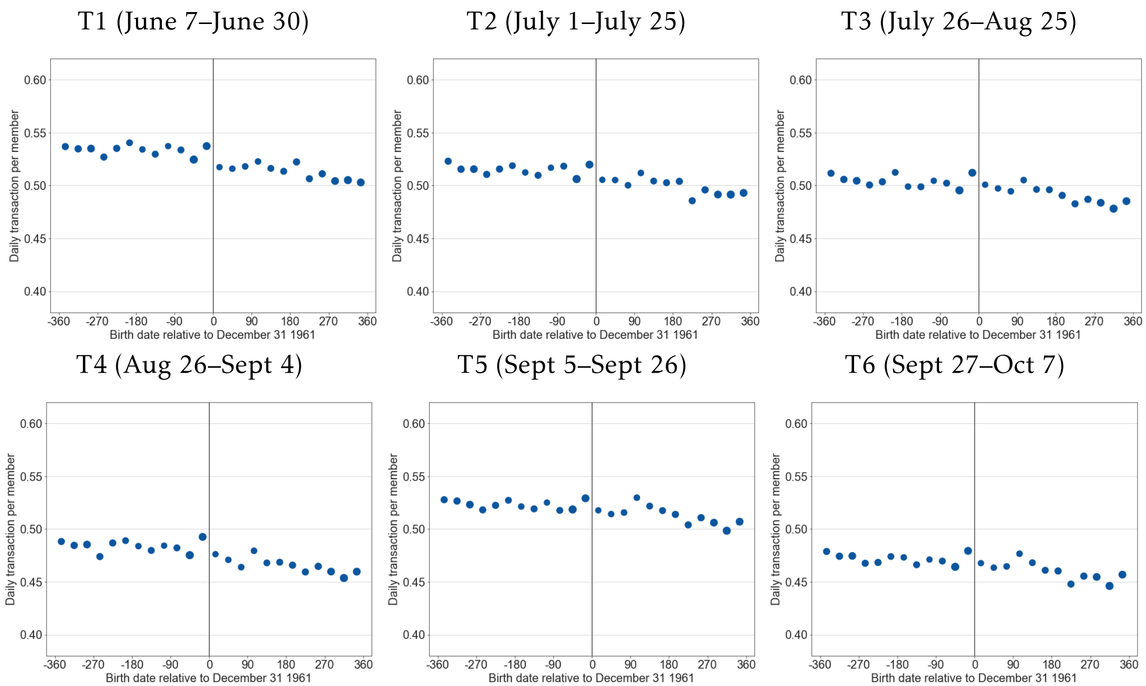
Figure 1: Vaccine take-up rates among 1961 and 1962 cohorts in 2021, South Korea



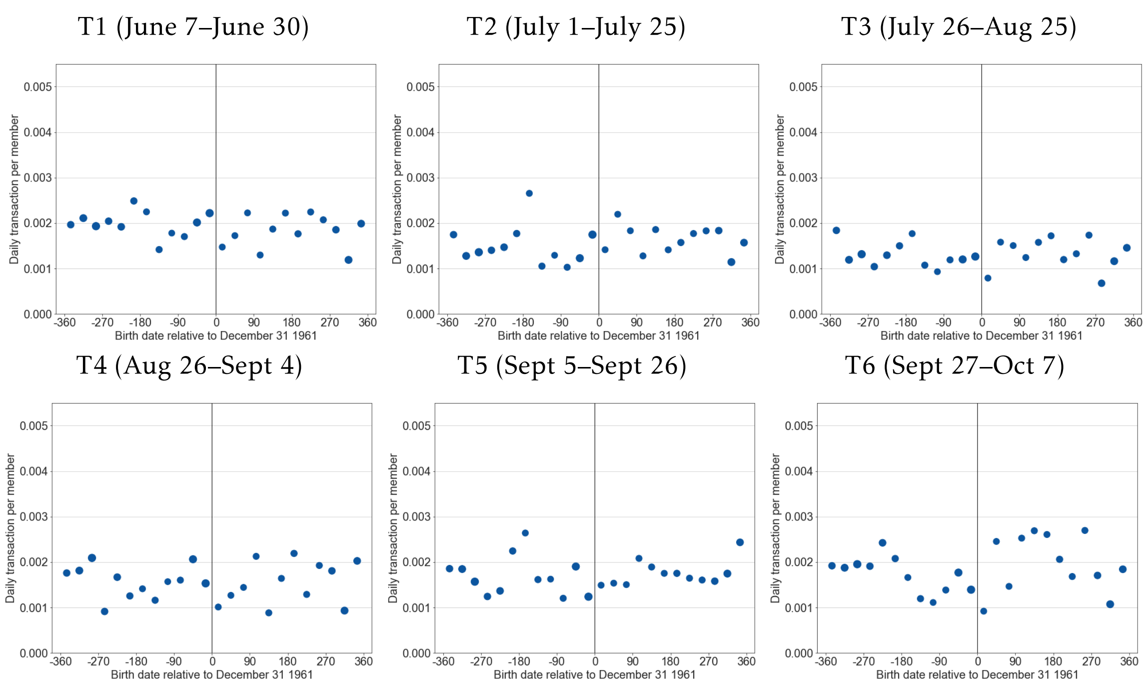
Notes: Data from KDCA. Vertical lines represent the date when each dose became available to each cohort group (Table 1). The periods are as follows: T1 (June 7–June 30), T2 (July 1–July 25), T3 (July 26–August 25), T4 (August 26–September 4), T5 (September 5–September 26), T6 (September 27–October 7).

Figure 2: Effects of vaccine eligibility on social distancing behaviors (credit card data and airline data)

Panel A. Average daily offline transactions

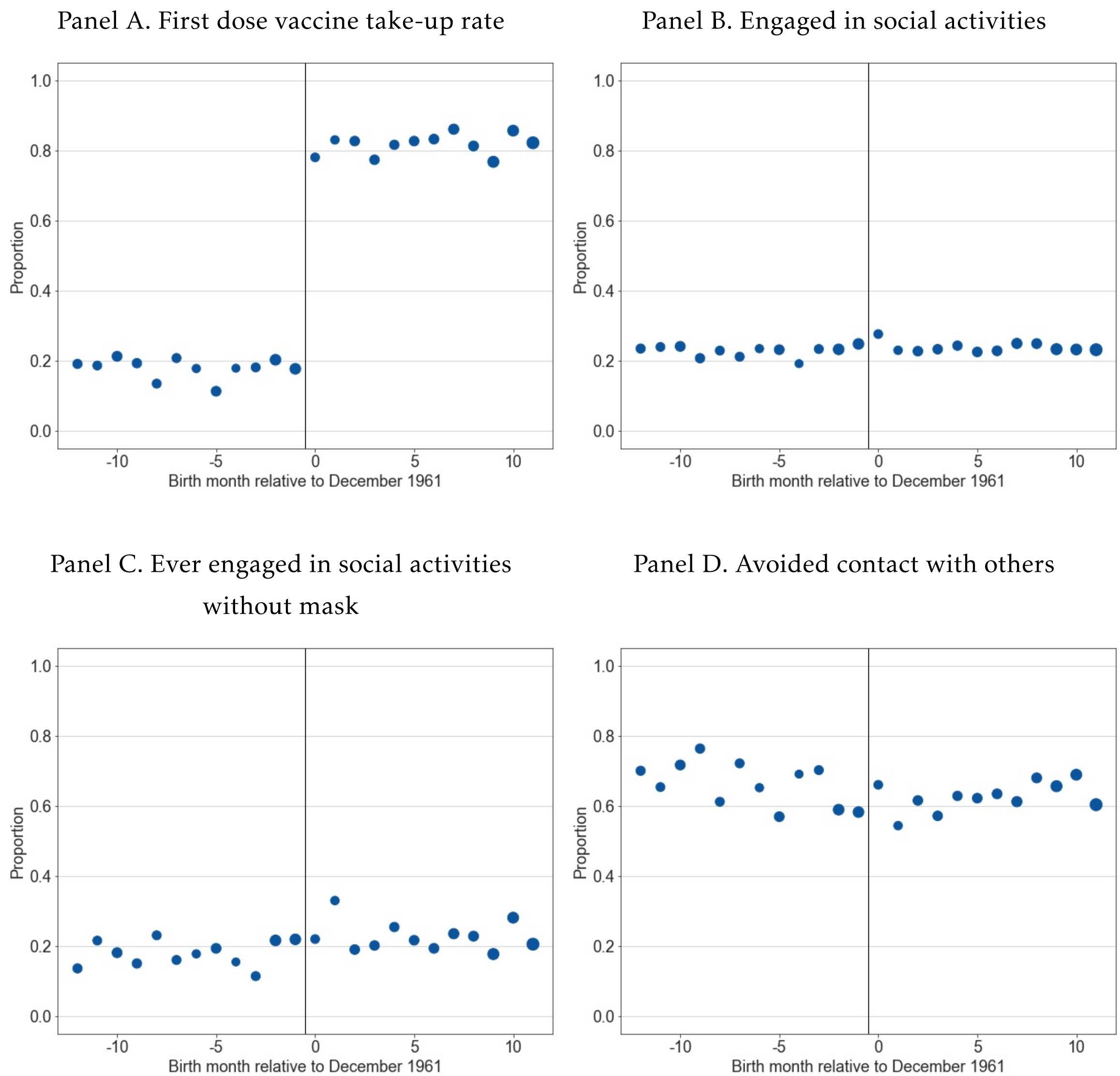


Panel B. Average daily domestic trips



Notes: These figures show reduced-form effects of the outcome variables in credit card data (panel A) and airline data (panel B) around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin.

Figure 3: Effects of vaccine eligibility on vaccine take-up and social distancing behaviors (survey data)

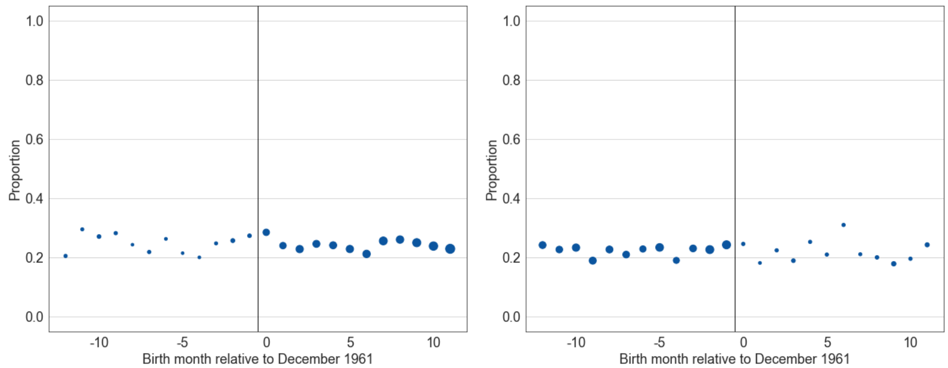


Notes: These figures show reduced-form effects of the outcome variables in survey data around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin.

Figure 4: External validity test

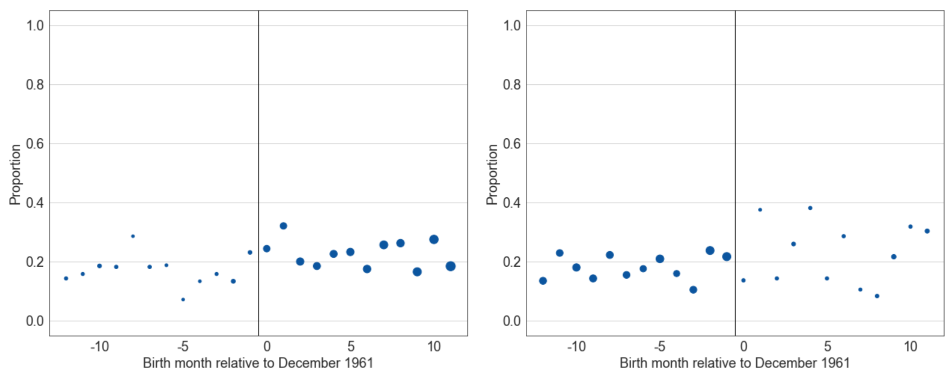
Panel A. Engaged in social activities

Always-takers vs Compliers | Never-takers vs Compliers



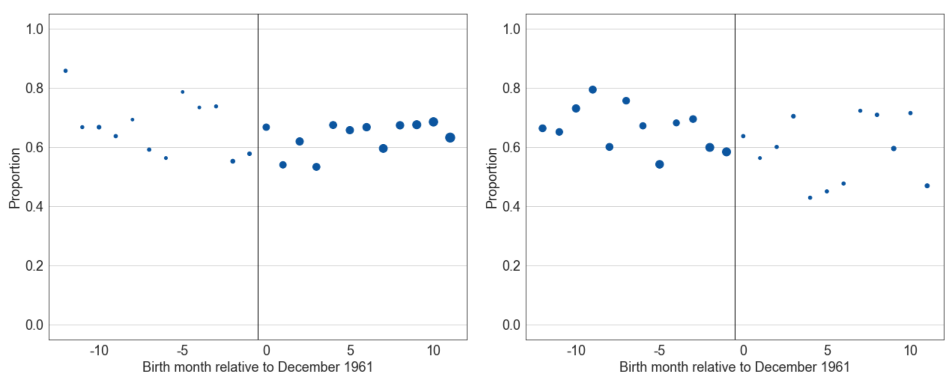
Panel B. Ever engaged in social activities without mask

Always-takers vs Compliers | Never-takers vs Compliers



Panel C. Avoided contact with others

Always-takers vs compliers | Never-takers vs Compliers



Notes: These figures show the difference in potential outcomes between compliance groups around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin.

Table 1: Vaccine eligibility schedule in 2021, South Korea

Year of birth	1st dose	2nd dose	Vaccine type
–1946	April 1	April 22	Pfizer
1947–1956	May 27	August 12	AstraZeneca
1957–1961	June 7	August 23	AstraZeneca
1962–1966	July 26	September 6	Pfizer/Moderna
1967–1971	August 16	September 27	Pfizer/Moderna
1971–	August 26	October 7	Pfizer/Moderna

Table 2: Baseline statistics of estimation sample

Variable	Mean	SD	N
Panel A. Credit card data			
Age	59.5	0.50	730
Residence: Seoul	0.203	0.017	730
Average daily transactions by category			
Offline sector transactions	0.471	0.021	730
Food & beverage	0.111	0.008	730
Sports & entertainment	0.007	0.002	730
Miscellaneous services	0.107	0.006	730
Lodging	0.009	0.002	730
Offline retail	0.220	0.012	730
Clothing	0.008	0.001	730
Home appliances	0.008	0.001	730
Online retail	0.045	0.007	730
Medical expenditure	0.048	0.003	730
Panel B. Airline data			
Age	59.5	0.50	33,613
Male	0.564	0.496	33,613
Days of membership	1315.61	1125.68	33,613
Average daily trips by route	0.0017	0.0083	33,613
Mainland → Jeju	0.0007	0.0035	33,613
Jeju → Mainland	0.0007	0.0035	33,613
Mainland → Mainland	0.0004	0.0047	33,613

Variable	Mean	SD	N
Panel C. Survey data			
Age	59.5	0.50	2,916
Male	0.506	0.500	2,916
Married	0.834	0.372	2,913
Residence: Seoul	0.167	0.373	2,916
Education			
Middle school or less	0.127	0.333	2,854
High school	0.414	0.493	2,854
College or more	0.459	0.498	2,854
Occupation			
Self-employed	0.288	0.453	2,902
Blue-collar	0.178	0.383	2,902
White-collar	0.236	0.425	2,902
Non-employed	0.298	0.457	2,902
Preference			
Conservative (0, 1)	0.39	0.49	2,501
Risk preference (0–10)	4.28	2.88	2,853
Belief about vaccine effectiveness (0–10)	7.65	2.20	2,619
Worry about vaccine side effects (0–10)	4.35	3.30	2,902
Worry about COVID-19 infection (0–10)	6.01	3.26	2,891

Notes: This table presents the mean and standard deviation (SD) of baseline characteristics in each data. The period used for calculating average daily transactions and trips in panels A and B is January 1, 2021–June 5, 2021. The sample size is 730 (= $365 * 2$) in panel A because the credit card data are aggregated by birth date. The spending categories are as follows: food and beverage, sports and entertainment, miscellaneous services (beauty salons, education, fuel), lodging, offline retail (supermarkets, department stores, cars), clothing (clothes, accessories, cosmetics), home appliances (furniture, electronics), medical expenditure (hospitals, pharmacies), and online retail. The first seven types are "offline sector transactions," which necessarily involve face-to-face encounters. "Self-employed" includes farmer. "Non-employed" includes jobless, retired, and housewife. "Conservative" is an indicator of "strongly conservative" or "weakly conservative". "Risk preference": 0 - strongly risk-averse, 10 - strongly risk-taking. "Belief about vaccine effectiveness": 0 - expect no effects, 10 - expect strong effects. "Worry about vaccine side effects": 0 - not worried at all, 10 - very worried. "Worry about COVID-19 infection": 0 - not worried at all, 10 - very worried.

Table 3: Effects of vaccine eligibility on social distancing behaviors (credit card data and airline data)

	(1)	(2)	(3)	(4)	(5)	(6)
	June 7–June 30 T1	July 1–July 25 T2	July 26–Aug 25 T3	Aug 26–Sept 4 T4	Sept 5–Sept 26 T5	Sept 27–Oct 7 T6
	Panel A. Difference in vaccination rate (1961 cohort – 1962 cohort)					
Difference in the 1st vaccination rate	49.5	65.8	18.2	−1.2	−1.20	−1.3
Difference in the 2nd vaccination rate	0.2	0.2	2.3	44.0	23.0	−2.3
	Panel B. Average daily offline transactions					
RD estimates (β)	−0.008**	−0.003	0.002	−0.005	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Mean of dep var. in [-365 days, 0)	0.534	0.515	0.504	0.484	0.523	0.472
Observations	730	730	730	730	730	730
	Panel C. Average daily domestic trips					
RD estimates (β)	−0.0002	0.0002	0.0001	−0.0003	−0.0001	0.0007*
	(0.0003)	(0.0003)	(0.0002)	(0.0003)	(0.0003)	(0.0004)
Mean of dep var. in [-365 days, 0)	0.0020	0.0015	0.0013	0.0016	0.0017	0.0017
Observations	33,613	33,613	33,613	33,613	33,613	33,613

Notes: Panel A reports the difference in vaccination rates between the 1961 and 1962 cohorts in each period according to the KDCA data. Panels B and C represent the RD estimates of equation (1). We use a local linear regression with a uniform kernel and a 365-days bandwidth. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table 4: Effects of vaccine eligibility on vaccine take-up and social distancing behaviors (survey data)

	(1) First dose vaccine take-up rate	(2) Engaged in social activities	(3) Ever engaged in social activities without mask	(4) Avoided contact with others
RD estimates (β)	0.634***	0.012	0.039	0.006
	(0.029)	(0.010)	(0.031)	(0.037)
Mean of dep var. in [-12 months,0)	0.180	0.229	0.182	0.658
Observations	2,916	2,910	2,910	2,888

Notes: This table represents the RD estimates of equation (1). We use a local linear regression with a uniform kernel and a 12-months bandwidth. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table 5: Selection heterogeneity test (survey data)

	(1) Always -takers	(2) Treated compliers	(3) Untreated compliers	(4) Never -takers	(5)	(6)
Proportion	0.172	0.317	0.317	0.194		
	(0.022)	(0.014)	(0.014)	(0.020)		
Variable	Means at the cutoff				Difference in means	
					(1) – (2)	(3) – (4)
Male	0.483	0.514	0.588	0.480	−0.031	0.108
	(0.066)	(0.043)	(0.077)	(0.027)	(0.093)	(0.089)
Married	0.881	0.839	0.850	0.848	0.042	0.002
	(0.048)	(0.024)	(0.057)	(0.021)	(0.062)	(0.065)
Residence: Seoul	0.213	0.163	0.196	0.099	0.050	0.097
	(0.050)	(0.033)	(0.052)	(0.023)	(0.071)	(0.061)
Middle school or less	0.121	0.124	0.121	0.267	−0.003	−0.146**
	(0.037)	(0.026)	(0.060)	(0.020)	(0.054)	(0.066)
High school	0.351	0.433	0.400	0.444	−0.083	−0.043
	(0.061)	(0.041)	(0.073)	(0.032)	(0.088)	(0.086)
College or more	0.485	0.442	0.453	0.261	0.043	0.192**
	(0.060)	(0.037)	(0.079)	(0.032)	(0.081)	(0.093)
Self-employed	0.233	0.307	0.288	0.320	−0.074	−0.032
	(0.047)	(0.037)	(0.079)	(0.030)	(0.068)	(0.086)
Blue-collar	0.284	0.172	0.166	0.213	0.112	−0.048
	(0.056)	(0.031)	(0.054)	(0.020)	(0.075)	(0.062)
White-collar	0.336	0.241	0.279	0.071	0.094	0.207**
	(0.063)	(0.037)	(0.059)	(0.024)	(0.088)	(0.070)
Non-employed	0.146	0.275	0.265	0.381	−0.129	−0.115
	(0.053)	(0.038)	(0.069)	(0.029)	(0.077)	(0.080)
Conservative	0.363	0.252	0.309	0.424	0.111	−0.115
	(0.063)	(0.040)	(0.071)	(0.024)	(0.088)	(0.082)
Risk Preference	4.471	4.159	4.279	4.155	0.312	0.125
	(0.380)	(0.236)	(0.461)	(0.174)	(0.531)	(0.500)
Belief about vaccine effectiveness	6.848	7.065	7.256	5.651	−0.217	1.605**
	(0.383)	(0.224)	(0.448)	(0.170)	(0.517)	(0.482)
Worry about vaccine side effects	3.756	3.920	4.393	6.519	−0.164	−2.126**
	(0.409)	(0.263)	(0.521)	(0.178)	(0.596)	(0.566)
Worry about COVID-19 infection	5.891	5.904	5.820	6.330	−0.013	−0.510
	(0.461)	(0.271)	(0.558)	(0.173)	(0.626)	(0.616)

Notes: This table presents the mean characteristics at the cutoff for each compliance group—always-takers in column (1), treated compliers in column (2), untreated compliers in column (3), and never-takers in column (4)—estimated from equations (A5) and (A6). Columns (5)–(6) present the difference in the mean characteristics between compliance groups. We use a local linear regression with a uniform kernel and a 12-months bandwidth. Bootstrapped standard errors are in parentheses. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table 6: External validity of LATE (survey data)

Variable	Means at the cutoff (1) Always -takers	(2) Treated compliers	(3) Untreated compliers	(4) Never -takers	Difference in means (5) (1) – (2)	(6) (3) – (4)	Joint F-Test (7) (1) – (2) = 0 & (3) – (4) = 0
Engaged in social activities	0.250	0.248	0.227	0.223	0.002	0.004	0.039
	(0.019)	(0.012)	(0.021)	(0.008)	(0.026)	(0.023)	[0.981]
Ever engaged in social activities without a mask	0.170	0.260	0.202	0.215	−0.090	−0.013	1.689
	(0.048)	(0.035)	(0.066)	(0.025)	(0.070)	(0.073)	[0.430]
Avoided contact with others	0.585	0.607	0.596	0.596	−0.022	0.000	0.066
	(0.060)	(0.040)	(0.077)	(0.029)	(0.085)	(0.087)	[0.967]

Notes: This table shows our assessment of the external validity of the LATE using equations (A8) and (A9). Columns (1)–(4) show the mean of potential outcomes of each compliance group around the cutoff: (1) $E[Y(1)|G_i = A]$, (2) $E[Y(1)|G_i = C]$, (3) $E[Y(0)|G_i = C]$, (4) $E[Y(0)|G_i = N]$. Columns (5)–(6) show the difference in the mean of potential outcomes between compliance groups. Column (7) shows the joint test of $E[Y(1)|G_i = A] - E[Y(1)|G_i = C] = 0$ and $E[Y(0)|G_i = C] - E[Y(0)|G_i = N] = 0$ around the cutoff following the external validity test in Bertanha and Imbens (2020). We use a local linear regression with a uniform kernel and a 12-months bandwidth. Bootstrapped standard errors are in parentheses. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

A Selection heterogeneity and external validity

As in Imbens and Angrist (1994) and Abadie (2003), we define compliance group (G_i) in the regression discontinuity design as follows:

$$G_i = \begin{cases} A & \text{if } D_i = 1 \text{ but } Z_i < \tau \\ C & \text{if } D_i = \mathbb{I}(Z_i \geq \tau) \\ N & \text{if } D_i = 0 \text{ but } Z_i \geq \tau \end{cases} \tag{A1}$$

Note that an individual is an always-taker if $G_i = A$, a complier if $G_i = C$, and a never-taker if $G_i = N$. Then we can identify the proportion of each compliance groups around the eligibility cutoff, P_τ^A, P_τ^C, and P_τ^N, in the following way:

$$P_\tau^A \equiv \mathbb{E}[G_i = A \mid Z_i = \tau] = \lim_{z \nearrow \tau} \mathbb{E}[D_i \mid Z_i = z] = f(\tau) \tag{A2}$$

$$P_\tau^C \equiv \mathbb{E}[G_i = C \mid Z_i = \tau] = \lim_{z \searrow \tau} \mathbb{E}[D_i \mid Z_i = z] - \lim_{z \nearrow \tau} \mathbb{E}[D_i \mid Z_i = z] = \beta_{\text{FS}} \tag{A3}$$

$$P_\tau^A \equiv \mathbb{E}[G_i = N \mid Z_i = \tau] = 1 - \lim_{z \searrow \tau} \mathbb{E}[D_i \mid Z_i = z] = 1 - \beta_{\text{FS}} - f(\tau) \tag{A4}$$

One way of exploring selection heterogeneity is to check the difference in the mean of covariate X_i around the cutoff. For example, we compare observable characteristics between always-takers and compliers by testing whether the following difference is zero:

$$\begin{aligned} &\mathbb{E}[X_i \mid G_i = C \,\&\, Z_i = \tau] - \mathbb{E}[X_i \mid G_i = A \,\&\, Z_i = \tau] \\ &\quad = \frac{P_\tau^C + P_\tau^A}{P_\tau^C} \cdot \left(\lim_{z \searrow \tau} \mathbb{E}[X_i \mid Z_i = z \,\&\, D_i = 1] - \lim_{z \nearrow \tau} \mathbb{E}[X_i \mid Z_i = z \,\&\, D_i = 1] \right) \end{aligned} \tag{A5}$$

The test can be implemented simply by estimating the conventional regression discontinuity parameter from the restricted sample of $D_i = 1$ (all vaccinated sample) following Kim and Lee (2017). Similarly, using the restricted sample of $D_i = 0$ (all unvaccinated sample), we can define and identify selection heterogeneity between never-takers and compliers by testing whether the following difference is zero:

$$\begin{aligned} &\mathbb{E}[X_i \mid G_i = N \,\&\, Z_i = \tau] - \mathbb{E}[X_i \mid G_i = C \,\&\, Z_i = \tau] \\ &\quad = \frac{P_\tau^C + P_\tau^N}{P_\tau^C} \cdot \left(\lim_{z \searrow \tau} \mathbb{E}[X_i \mid Z_i = z \,\&\, D_i = 0] - \lim_{z \nearrow \tau} \mathbb{E}[X_i \mid Z_i = z \,\&\, D_i = 0] \right) \end{aligned} \tag{A6}$$

Lastly, we assess the external validity of LATE to other compliance groups (Brinch et al. 2017, Bertanha and Imbens 2020). External validity can be defined as independence between potential outcomes and compliance types:

$$G_i \perp (Y_i(0), Y_i(1)) \mid Z_i \tag{A7}$$

If assumption (A7) holds, we can say that the local treatment effect can be also applied to the other compliance groups. In our study, we compare the mean of $Y_i(1)$ between compliers and always-takers and the mean of $Y_i(0)$ between compliers and never-takers jointly to test whether assumption (A7) holds near the eligibility cutoff. That is, we test jointly for the pair of restrictions:

$$\lim_{z \searrow \tau} \mathbb{E}[Y_i \mid Z_i = z \,\&\, D_i = 1] = \lim_{z \nearrow \tau} \mathbb{E}[Y_i \mid Z_i = z \,\&\, D_i = 1] \tag{A8}$$

$$\lim_{z \searrow \tau} \mathbb{E}[Y_i \mid Z_i = z \,\&\, D_i = 0] = \lim_{z \nearrow \tau} \mathbb{E}[Y_i \mid Z_i = z \,\&\, D_i = 0] \tag{A9}$$

Note that the term in large parentheses in equation (A8) and (A9) corresponds to the RD estimate for the sample of all vaccinated or all unvaccinated sample, respectively. Although the failure to reject equations (A8) and (A9) could lend support to external validity, the caveat of this test is that it does not have power to test all the sufficient conditions for assumption (A7) because the equality of means does not necessarily imply the equality of distributions (Bertanha and Imbens 2020). Also, the means of $Y_i(1)$ of never-takers and of $Y_i(0)$ of always-takers are still not identified, and therefore, the test result should be interpreted as weak evidence of external validity.

B Difference-in-differences estimation

In this section, we employ an event-study difference-in-differences (DID) framework as an alternative approach to our regression discontinuity method. The event-study DID model estimates how the differences in outcomes between those born in 1961 and 1962 evolve over time before and after the start date of the first and second dose vaccination. To do so, we use raw data from the credit card company where individual expenditures are aggregated in about ten-day intervals.[1] To be consistent with the credit card data, we also aggregate vaccination rate and air travel data, which are available at the daily level, into the same time intervals.

The estimation equation is as follows:

$$Y_{ict} = \lambda_t + \alpha \mathbb{I}[c = 1961] + \sum_{k \neq -1} \beta_k \mathbb{I}[t = k] \cdot \mathbb{I}[c = 1961] + \varepsilon_{ict} \tag{A10}$$

where Y_{iat} is the outcome of individual i in birth cohort c at time interval t. $t = 0$ is the time interval when those born in 1961 is eligible for their first dose. λ_t are time dummies, and $\mathbb{I}[c = 1961]$ is an indicator for the treatment group. The coefficients on the interaction terms, β_k, trace the trends of the differences between the treatment and control groups before and after the first dose eligibility date.

Figure A15 panel A shows the gap in vaccination rates for the first and second dose across time intervals. The trends simply mirror the differences in trends in Figure 1. Panels B and C present the estimates of β_k from equation (A10) and corresponding 95% confidence intervals (vertical line) for credit card and air travel data, respectively. In both panels, we find that the estimates are statistically not different from zero during the pre-treatment period ($k < 0$). The results are supportive of the common trend assumption, which is the key validity condition for the DID approach.

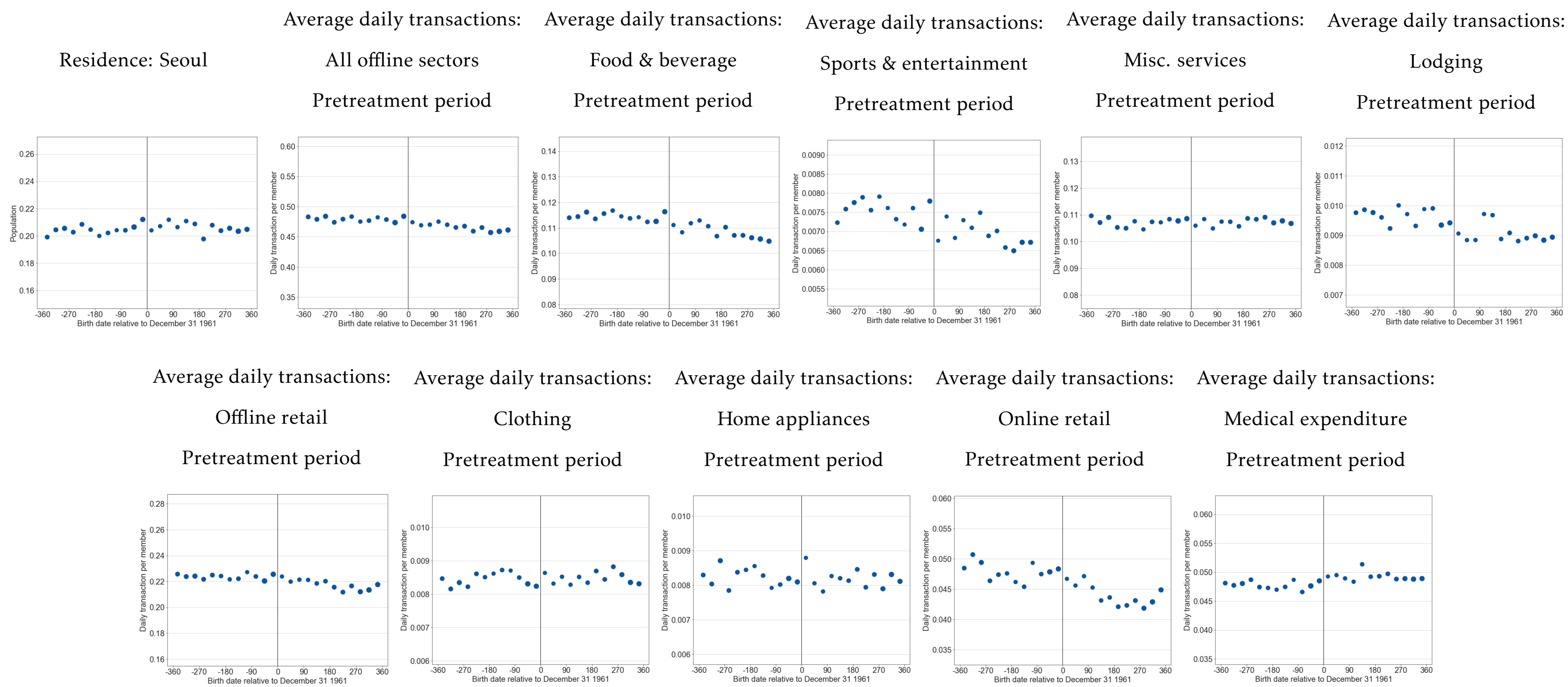
As shown in Figure A15, overall we find no evidence of risk compensation. We find a slight drop in credit card usage right after the first dose vaccination, corresponding to the RD result. We find a statistically significant increase after the second dose in the time interval beginning on September 5, but the effect is economically small (less than 1%).[2] Panel C presents the result for air trips. We find no significant difference between the two groups throughout the sample period.

[1] Time intervals are given in the data as follows. The first date of each time interval is May 1, 14, 27, June 7, 19, July 1, 13, 26, August 5, 16, September 5, 14, and 27. The first three intervals are before the start date of first dose eligibility for those born in 1961.

[2] It is difficult to interpret the estimate because it is ambiguous whether it reflects the effect of the second dose or the delayed effect of the first dose.

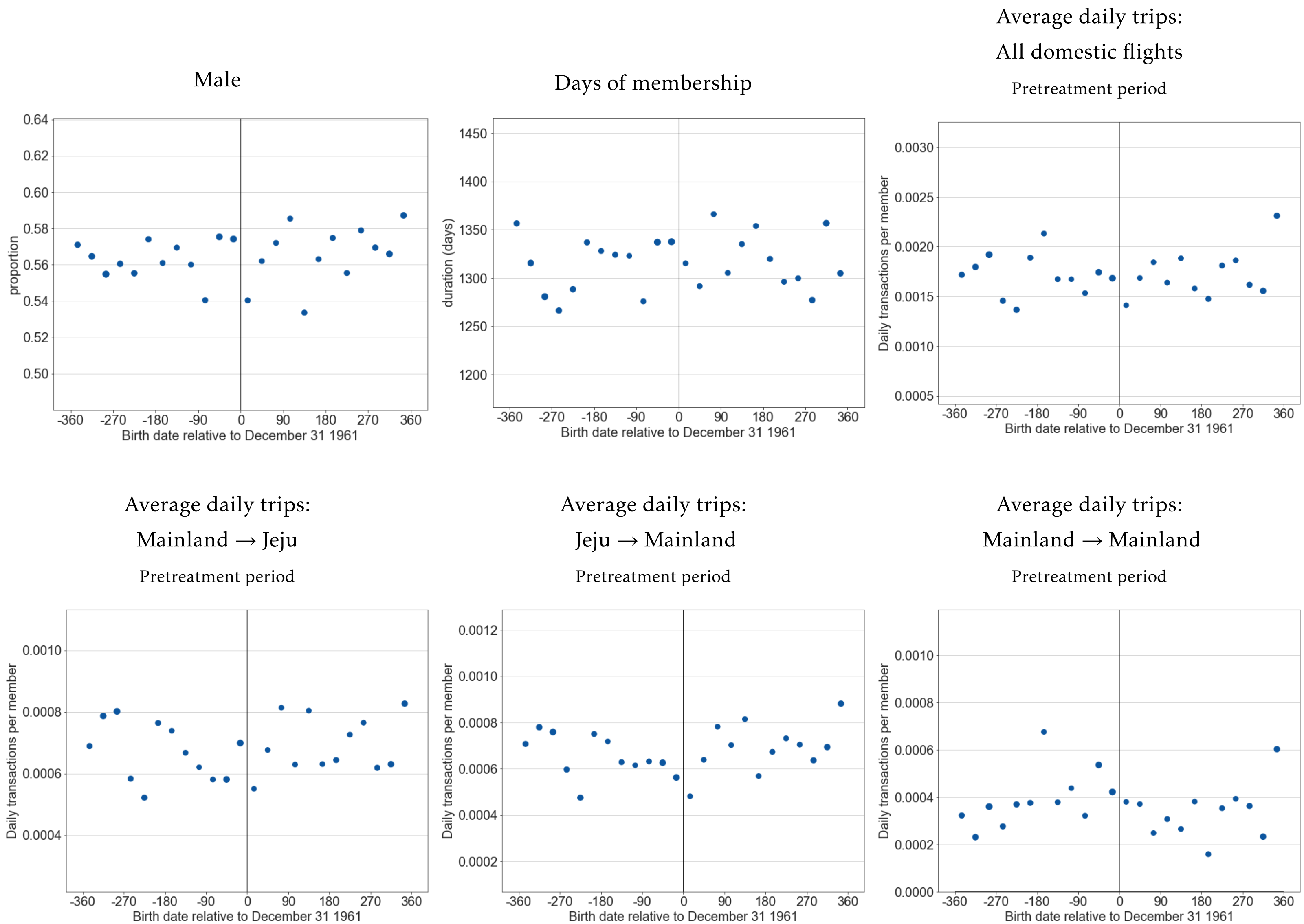
C Additional Figures and Tables

Figure A1: Covariate balance test (credit card data)



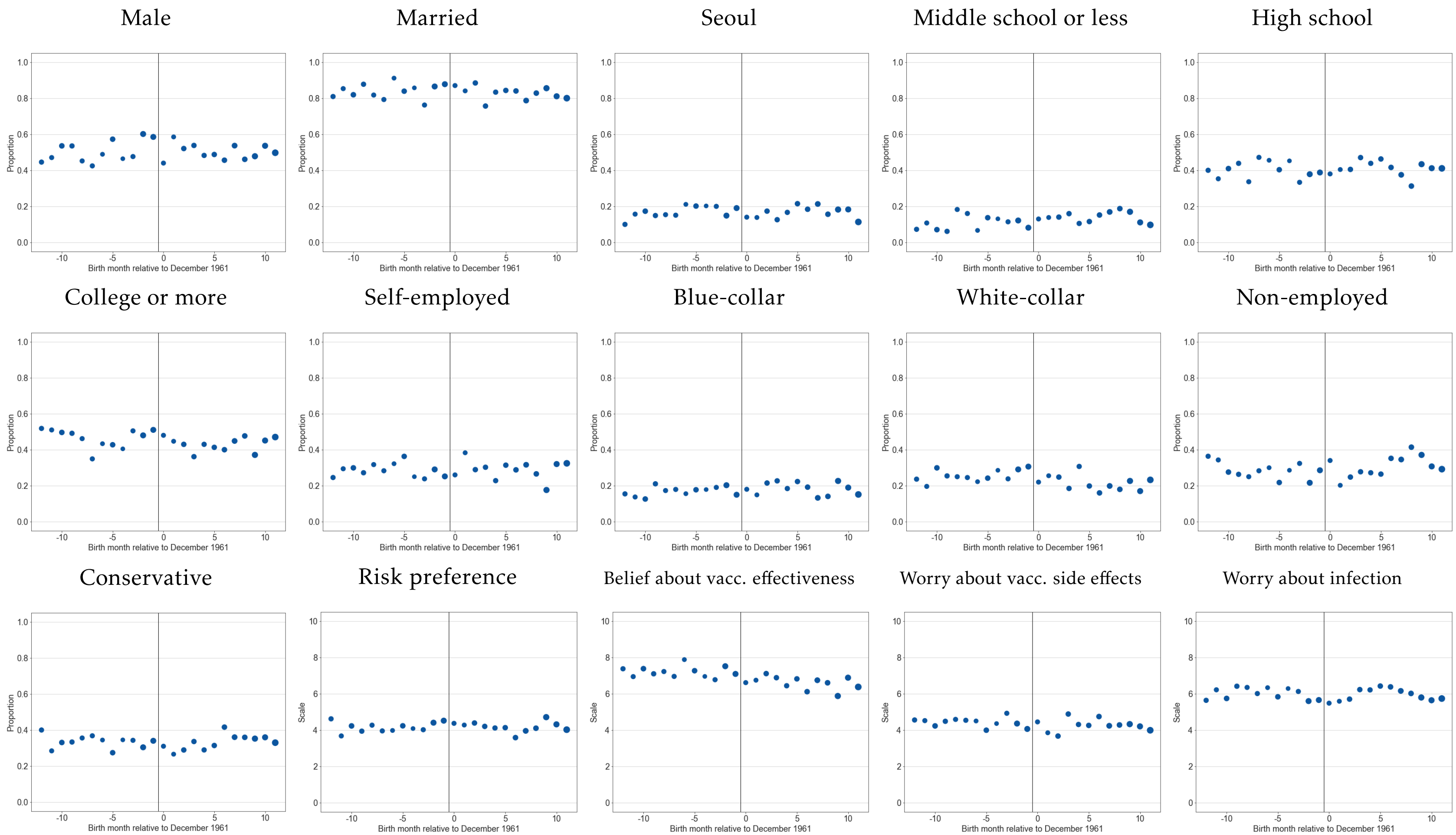
Notes: These figures show the means of covariates around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin. Pre-treatment period is January 1, 2021–June 5, 2021. The spending categories are as follows: food and beverage, sports and entertainment, miscellaneous services (beauty salons, education, fuel), lodging, offline retail (supermarkets, department stores, cars), clothing (clothes, accessories, cosmetics), home appliances (furniture, electronics), medical expenditure (hospitals, pharmacies), and online retail.

Figure A2: Covariate balance test (airline data)



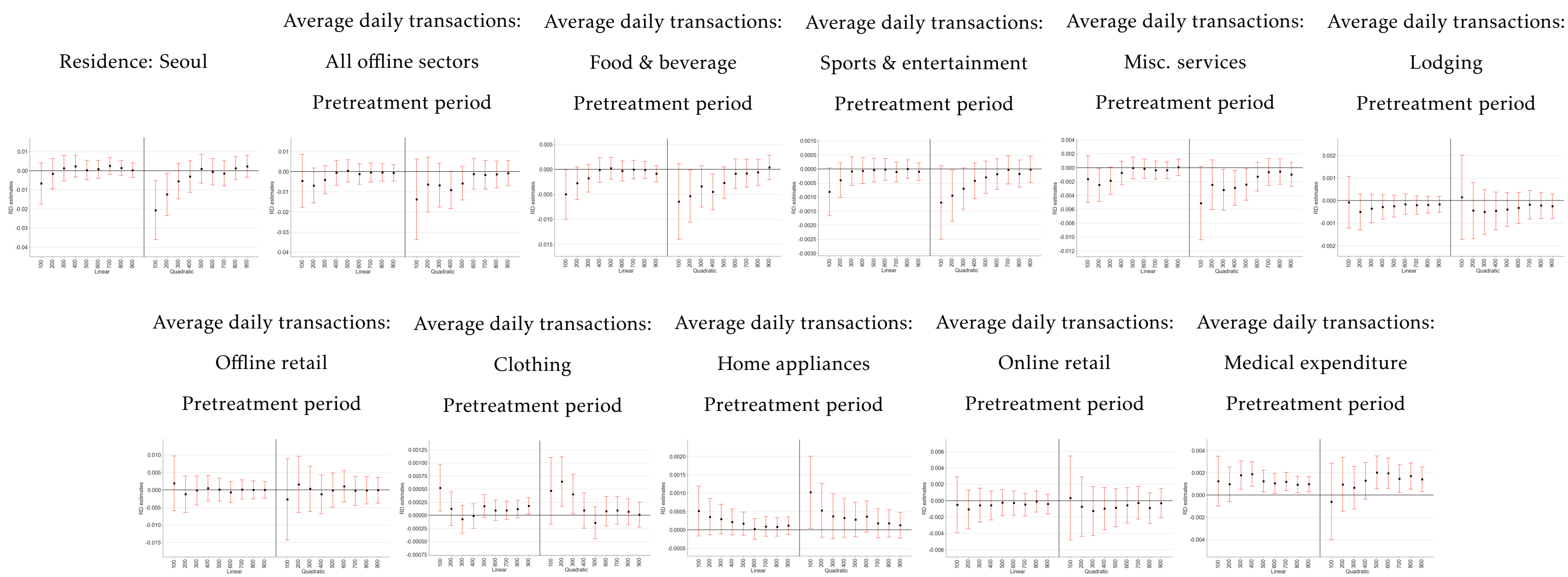
Notes: These figures show the means of covariates around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin. Pre-treatment period is January 1, 2021–June 5, 2021.

Figure A3: Covariate balance test (survey data)



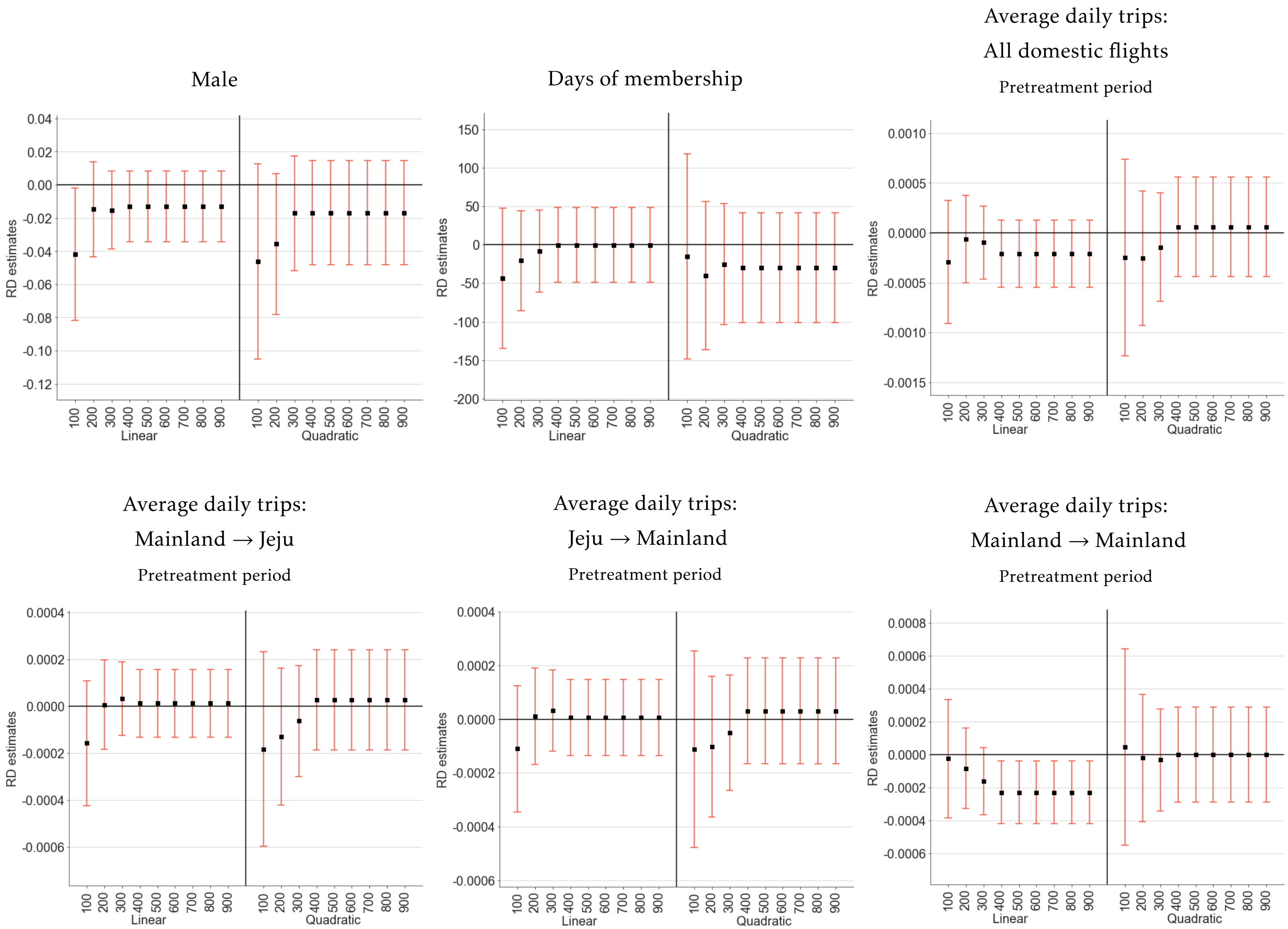
Notes: These figures show the means of covariates around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin. "Conservative" is an indicator of "strongly conservative" or "weakly conservative". "Risk preference": 0 - strongly risk-averse, 10 - strongly risk-taking. "Belief about vaccine effectiveness": 0 - expect no effects, 10 - expect strong effects. "Worry about vaccine side effects": 0 - not worried at all, 10 - very worried. "Worry about COVID-19 infection": 0 - not worried at all, 10 - very worried.

Figure A4: Sensitivity to bandwidth and polynomial degree: Covariate balance test (credit card data)



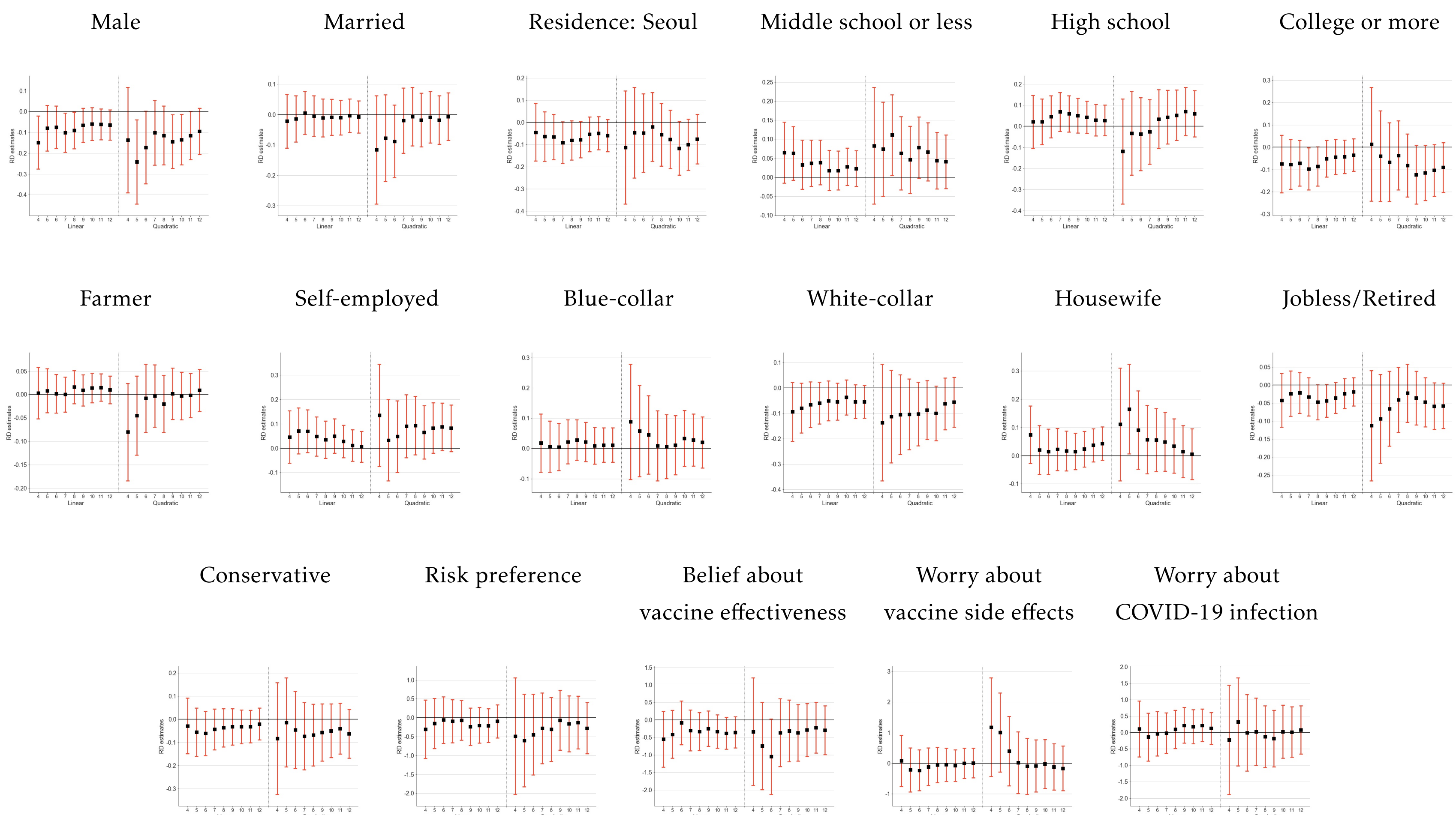
Notes: Each figure displays estimates of β from local polynomial regression of equation (1) at different bandwidths from 100 to 900 days in increments of 100 days, for linear and quadratic polynomial specifications. Degree of $f(\cdot)$ and the bandwidth size are on the x-axis. Coefficient estimates and 95% confidence bounds are on the y-axis. The running variable is birth date. Pre-treatment period is January 1, 2021–June 5, 2021. The spending categories are as follows: food and beverage, sports and entertainment, miscellaneous services (beauty salons, education, fuel), lodging, offline retail (supermarkets, department stores, cars), clothing (clothes, accessories, cosmetics), home appliances (furniture, electronics), medical expenditure (hospitals, pharmacies), and online retail.

Figure A5: Sensitivity to bandwidth and polynomial degree: Covariate balance test (airline data)



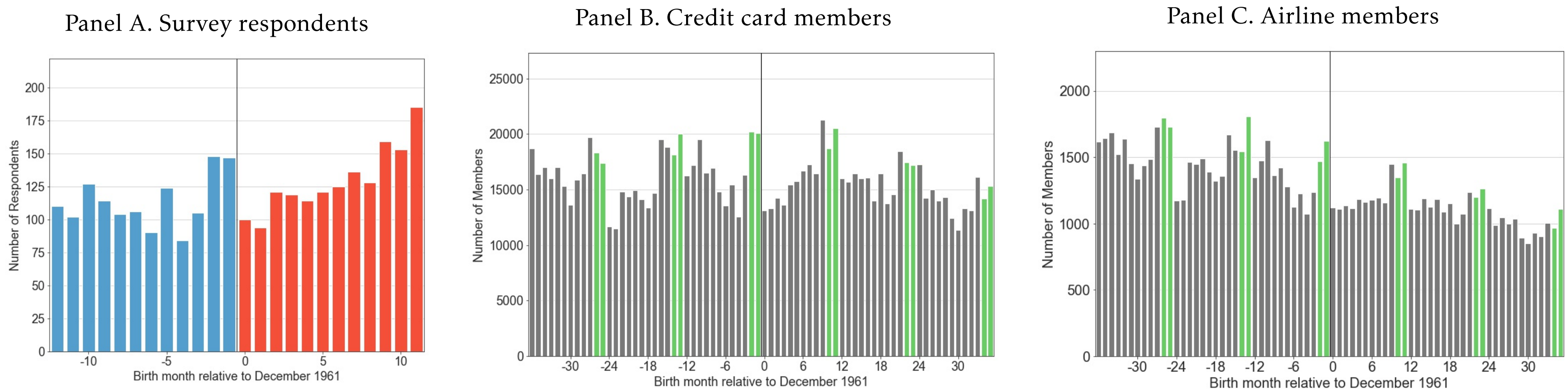
Notes: Each figure displays estimates of β from local polynomial regression of equation (1) at different bandwidths from 100 to 900 days in increments of 100 days, for linear and quadratic polynomial specifications. Degree of $f(\cdot)$ and the bandwidth size are on the x-axis. Coefficient estimates and 95% confidence bounds are on the y-axis. The running variable is birth date. Pre-treatment period is January 1, 2021–June 5, 2021.

Figure A6: Sensitivity to bandwidth and polynomial degree: Covariate balance test (survey data)



Notes: Each figure displays estimates of β from local polynomial regression of equation (1) at different bandwidths from 4 to 12 months in increments of 1 month, for linear and quadratic polynomial specifications. Degree of $f(\cdot)$ and the bandwidth size are on the x-axis. Coefficient estimates and 95% confidence bounds are on the y-axis. The running variable is birth month. "Conservative" is an indicator of "strongly conservative" or "weakly conservative". "Risk preference": 0 - strongly risk-averse, 10 - strongly risk-taking. "Belief about vaccine effectiveness": 0 - expect no effects, 10 - expect strong effects. "Worry about vaccine side effects": 0 - not worried at all, 10 - very worried. "Worry about COVID-19 infection": 0 - not worried at all, 10 - very worried.

Figure A7: Density of running variable

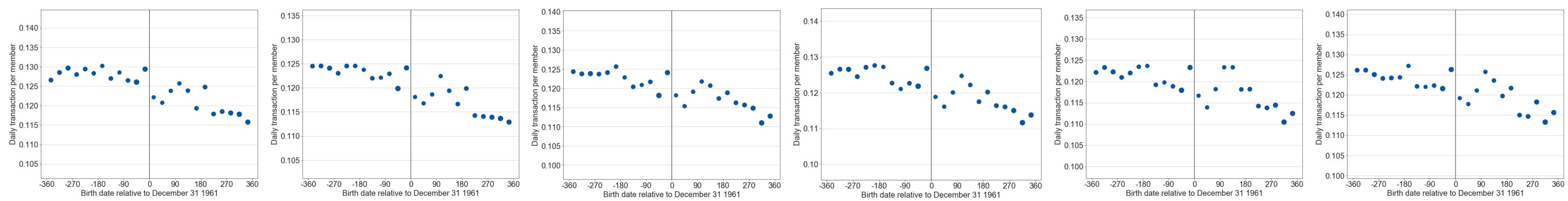


Notes: These figures show density of population in each data around the vaccination eligibility cutoff (vertical line). The period used for calculating the number of users in panels B and C is January 1, 2021–June 5, 2021. The green bars indicate the number of members who were born on January and Febuary.

Figure A8: Effects of vaccine eligibility on social distancing behaviors by category (credit card data)

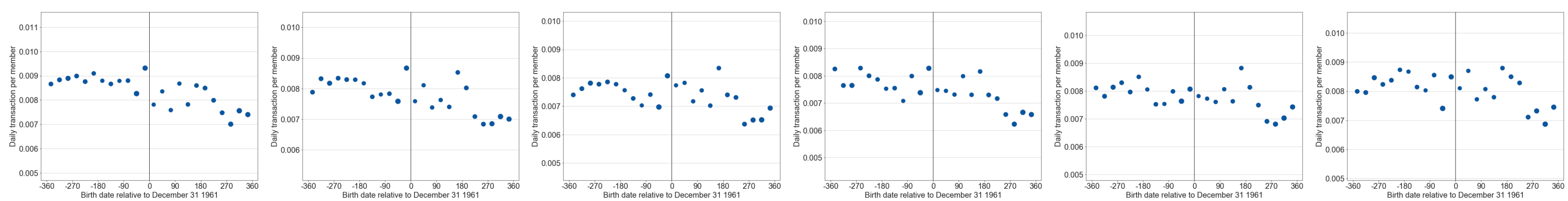
Panel A. Average daily transactions: food & beverage

T1 (June 7–June 30) T2 (July 1–July 25) T3 (July 26–Aug 25) T4 (Aug 26–Sept 4) T5 (Sept 5–Sept 26) T6 (Sept 27–Oct 7)



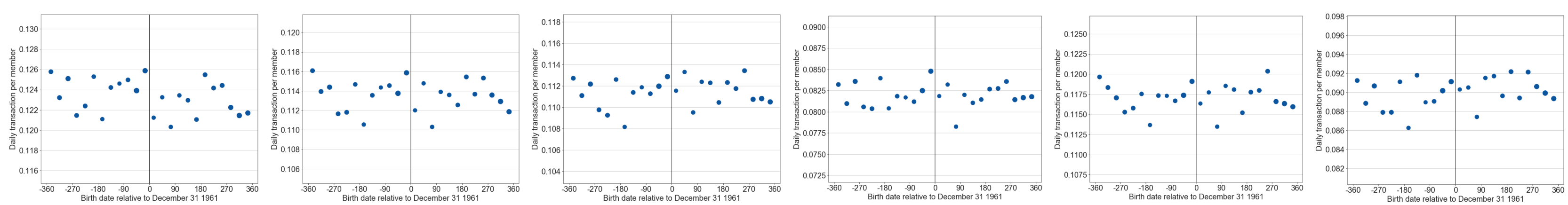
Panel B. Average daily transactions: sports & entertainment

T1 (June 7–June 30) T2 (July 1–July 25) T3 (July 26–Aug 25) T4 (Aug 26–Sept 4) T5 (Sept 5–Sept 26) T6 (Sept 27–Oct 7)



Panel C. Average daily transactions: miscellaneous services

T1 (June 7–June 30) T2 (July 1–July 25) T3 (July 26–Aug 25) T4 (Aug 26–Sept 4) T5 (Sept 5–Sept 26) T6 (Sept 27–Oct 7)

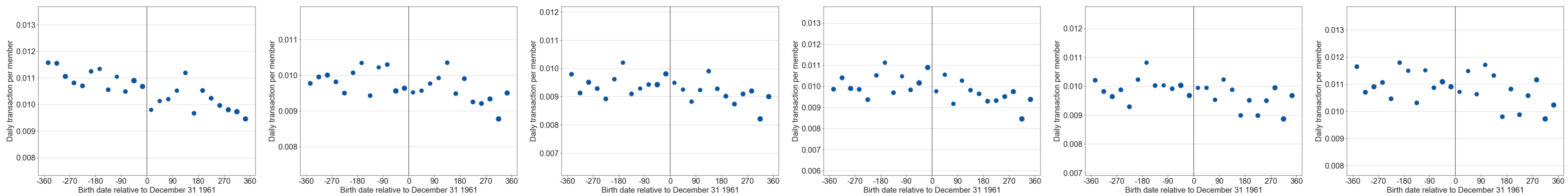


Notes: These figures show reduced-form effects of the outcome variables in credit card data around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin. The spending categories are as follows: food and beverage, sports and entertainment, miscellaneous services (beauty salons, education, fuel), lodging, offline retail (supermarkets, department stores, cars), clothing (clothes, accessories, cosmetics), home appliances (furniture, electronics), medical expenditure (hospitals, pharmacies), and online retail.

Figure A8: Effects of vaccine eligibility on social distancing behaviors by category (credit card data, continued)

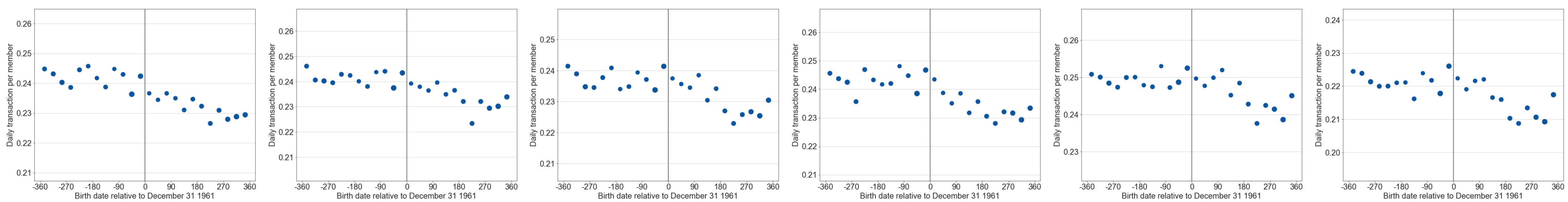
Panel D. Average daily transactions: lodging

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)



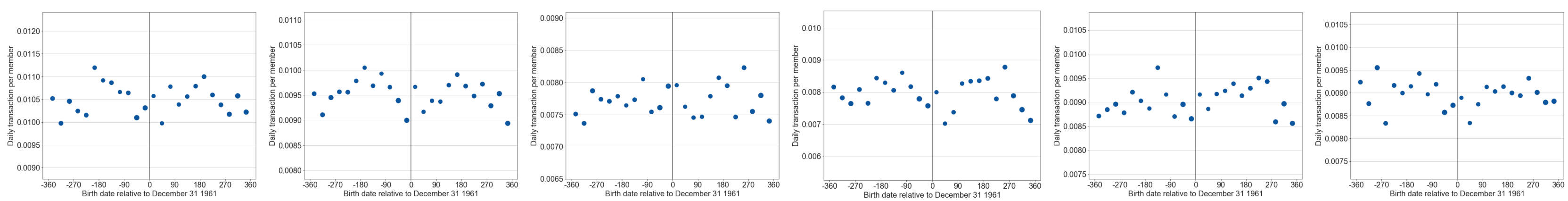
Panel E. Average daily transactions: offline retail

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)



Panel F. Average daily transactions: clothing

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)

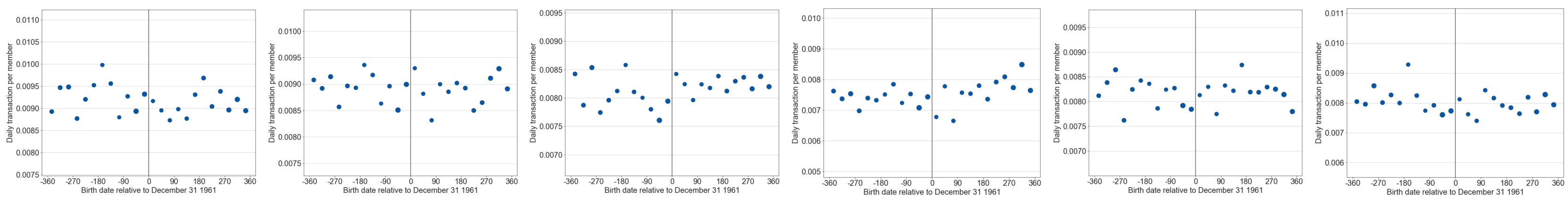


Notes: These figures show reduced-form effects of the outcome variables in credit card data around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin. The spending categories are as follows: food and beverage, sports and entertainment, miscellaneous services (beauty salons, education, fuel), lodging, offline retail (supermarkets, department stores, cars), clothing (clothes, accessories, cosmetics), home appliances (furniture, electronics), medical expenditure (hospitals, pharmacies), and online retail.

Figure A8: Effects of vaccine eligibility on social distancing behaviors by category (credit card data, continued)

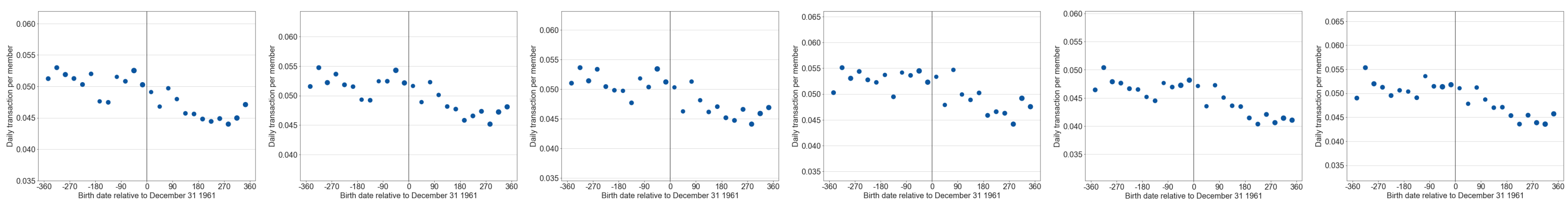
Panel G. Average daily transactions: home appliances

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)



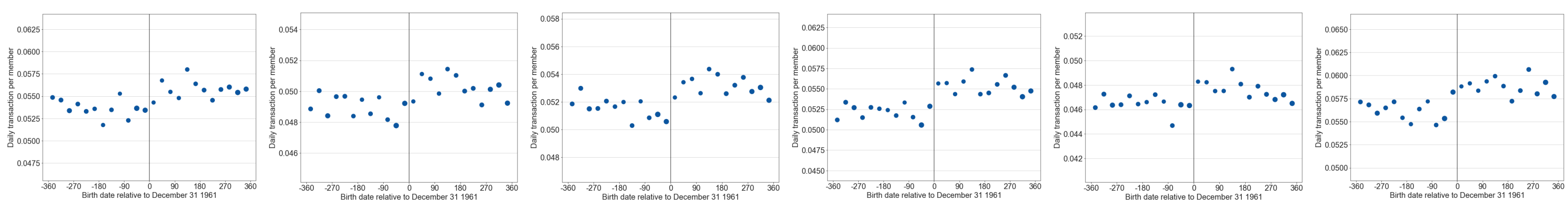
Panel H. Average daily transactions: online retail

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)



Panel I. Average daily transactions: medical expenditure

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)

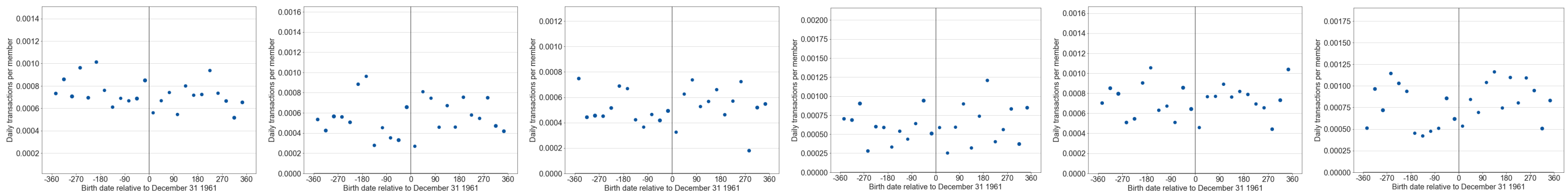


Notes: These figures show reduced-form effects of the outcome variables in credit card data around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin. The spending categories are as follows: food and beverage, sports and entertainment, miscellaneous services (beauty salons, education, fuel), lodging, offline retail (supermarkets, department stores, cars), clothing (clothes, accessories, cosmetics), home appliances (furniture, electronics), medical expenditure (hospitals, pharmacies), and online retail.

Figure A9: Effects of vaccine eligibility on social distancing behaviors by category (airline data)

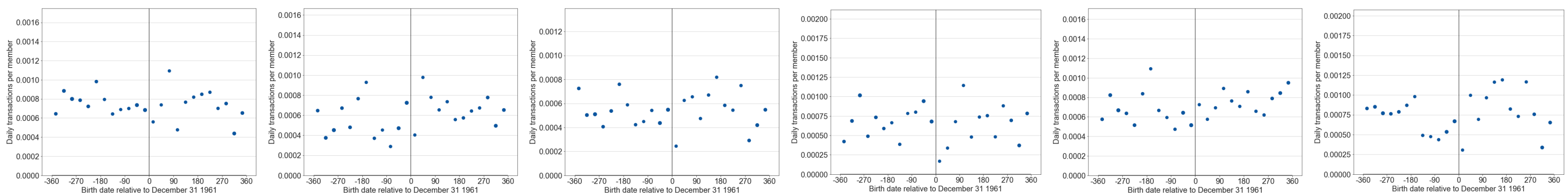
Panel A. Average daily trips: Mainland → Jeju

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)



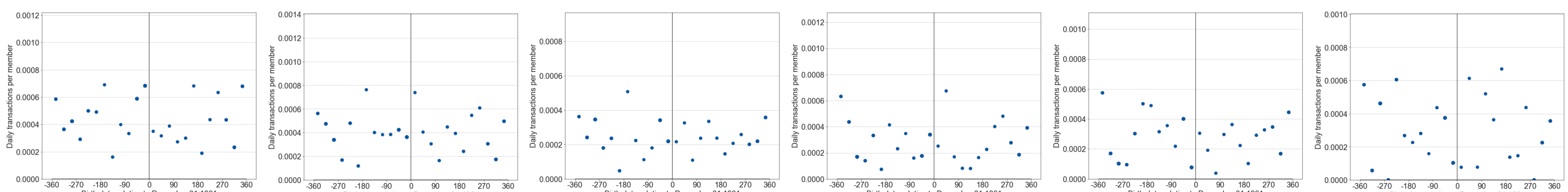
Panel B. Average daily trips: Jeju → Mainnland

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)



Panel C. Average daily trips: Mainland → Mainland

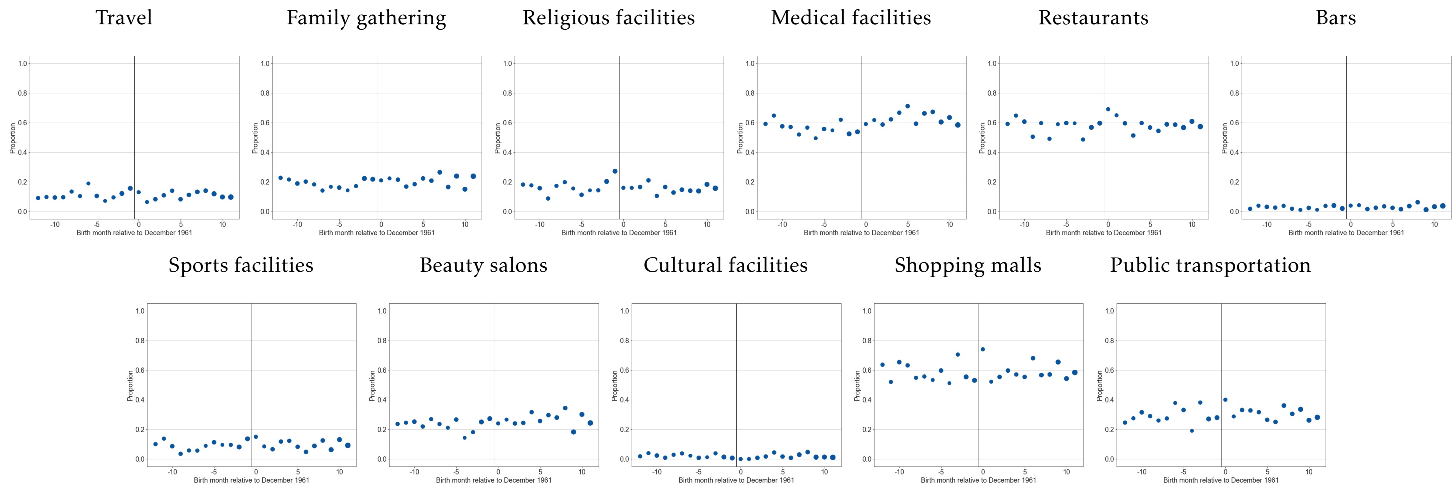
T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)



Notes: These figures show reduced-form effects of the outcome variables in airline data around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin.

Figure A10: Effects of vaccine eligibility on social distancing behaviors by category (survey data)

Panel A. Engaged in social activities



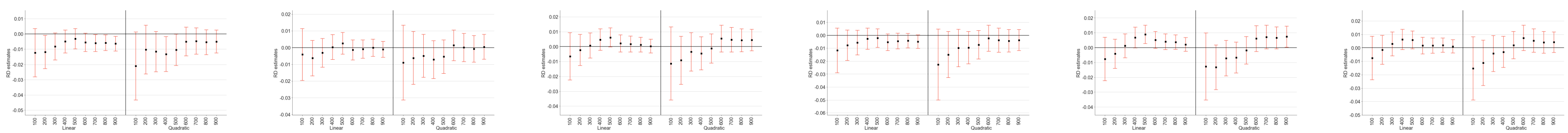
Panel B. Ever engaged in social activities without mask

Notes: These figures show reduced-form effects of outcome variables in survey data around the vaccination eligibility cutoff (vertical line). The size of the dots corresponds to the number of observations in each bin.

Figure A11: Sensitivity to bandwidth and polynomial degree: Effects of vaccine eligibility on social distancing behaviors (credit card data and airline data)

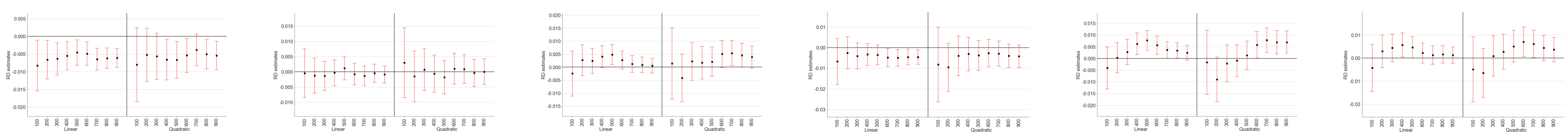
Panel A. Average daily offline transactions (without covariates)

T1 (June 7–June 30) T2 (July 1–July 25) T3 (July 26–Aug 25) T4 (Aug 26–Sept 4) T5 (Sept 5–Sept 26) T6 (Sept 27–Oct 7)



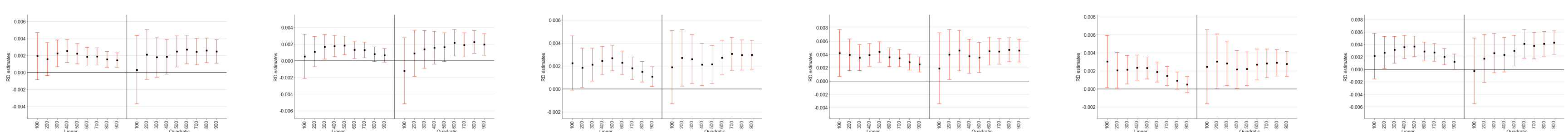
Panel B. Average daily offline transactions (with covariates)

T1 (June 7–June 30) T2 (July 1–July 25) T3 (July 26–Aug 25) T4 (Aug 26–Sept 4) T5 (Sept 5–Sept 26) T6 (Sept 27–Oct 7)



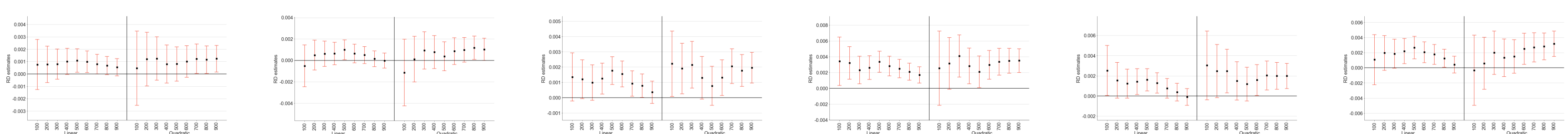
Panel C. Average daily transactions: medical expenditure (without covariates)

T1 (June 7–June 30) T2 (July 1–July 25) T3 (July 26–Aug 25) T4 (Aug 26–Sept 4) T5 (Sept 5–Sept 26) T6 (Sept 27–Oct 7)



Panel D. Average daily transactions: medical expenditure (with covariates)

T1 (June 7–June 30) T2 (July 1–July 25) T3 (July 26–Aug 25) T4 (Aug 26–Sept 4) T5 (Sept 5–Sept 26) T6 (Sept 27–Oct 7)

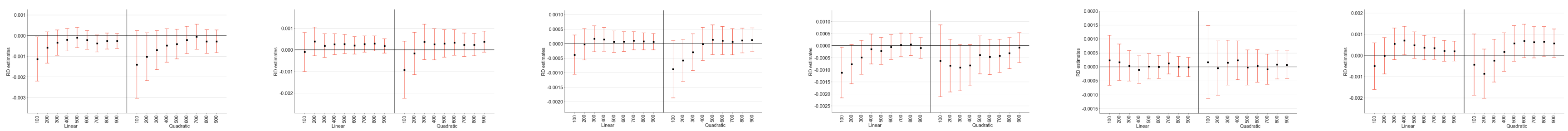


Notes: Each figure displays estimates of β from local polynomial regression of equation (1) at different bandwidths from 100 to 900 days in increments of 100 days, for linear and quadratic polynomial specifications. Degree of $f(\cdot)$ and the bandwidth size are on the x-axis. Coefficient estimates and 95% confidence bounds are on the y-axis. The running variable is birth date. Covariates in credit card data include "residence: Seoul" and "average daily transactions" of each category in pre-treatment period. Covariates in airline data include "male", "days of membership", and "average daily domestic trips in pre-treatment period."

Figure A11: Sensitivity to bandwidth and polynomial degree: Effects of vaccine eligibility on social distancing behaviors (credit card data and airline data, continued)

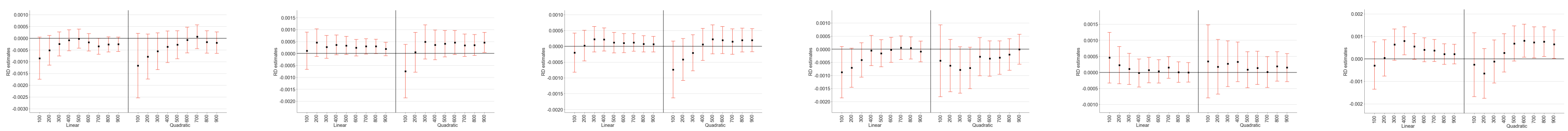
Panel E. Average daily domestic trips

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)



Panel F. Average daily domestic trips (with covariates)

T1 (June 7–June 30) | T2 (July 1–July 25) | T3 (July 26–Aug 25) | T4 (Aug 26–Sept 4) | T5 (Sept 5–Sept 26) | T6 (Sept 27–Oct 7)

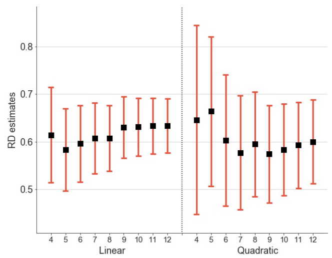


Notes: Each figure displays estimates of β from local polynomial regression of equation (1) at different bandwidths from 100 to 900 days in increments of 100 days, for linear and quadratic polynomial specifications. Degree of $f(\cdot)$ and the bandwidth size are on the x-axis. Coefficient estimates and 95% confidence bounds are on the y-axis. The running variable is birth date. Covariates in credit card data include “residence: Seoul” and “average daily transactions” of each category in pre-treatment period. Covariates in airline data include “male”, “days of membership”, and “average daily domestic trips in pre-treatment period."

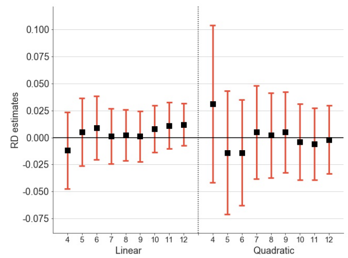
Figure A12: Sensitivity to bandwidth and polynomial degree: Effects of vaccine eligibility (survey data)

Panel A. Without covariates

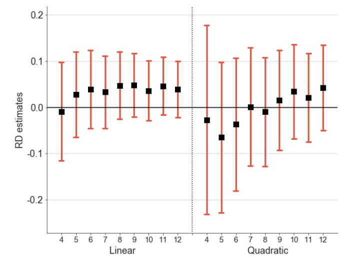
First dose vaccine take-up rate



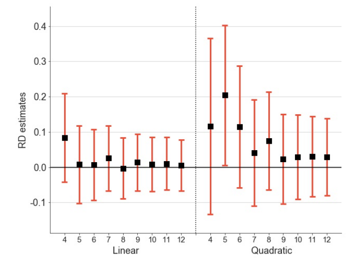
Engaged in social activities



Ever engaged in social activities without mask

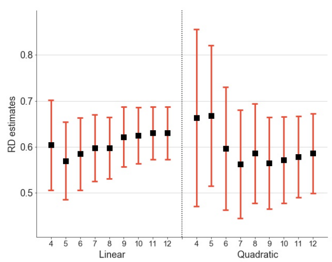


Avoided contact with others

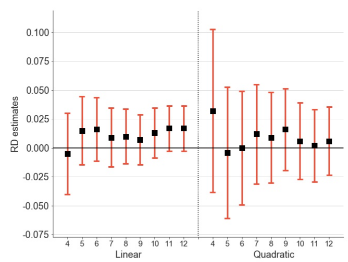


Panel B. With covariates

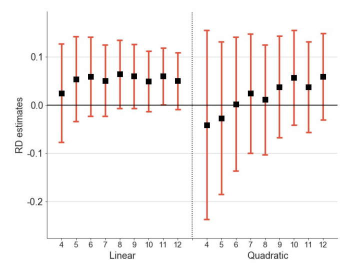
First dose vaccine take-up rate



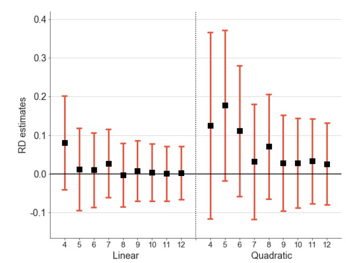
Engaged in social activities



Ever engaged in social activities without mask



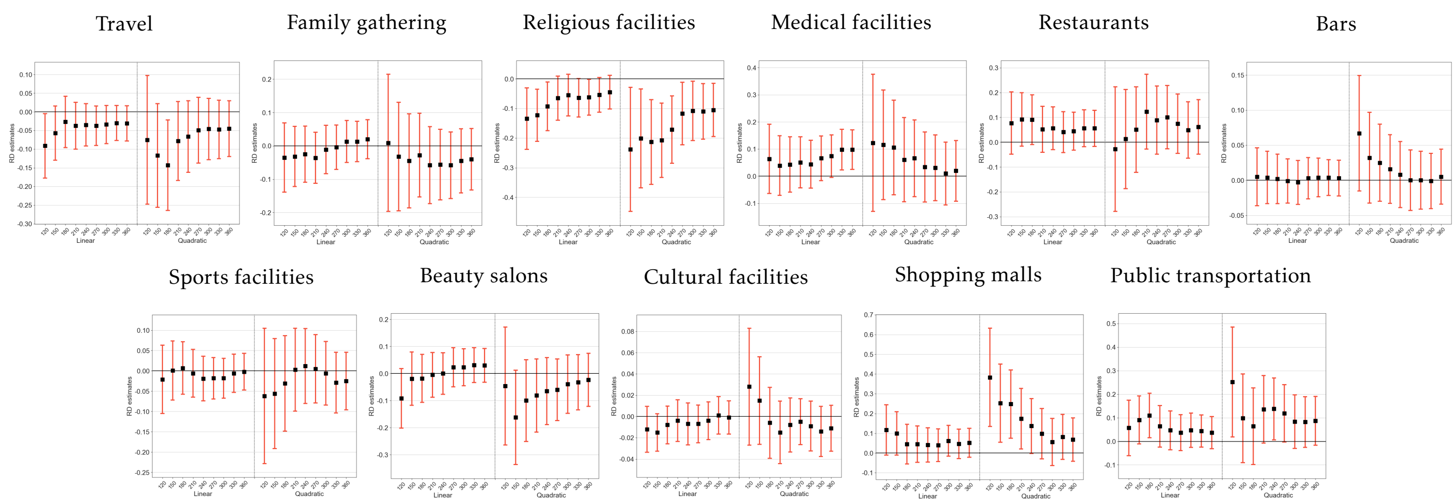
Avoided contact with others



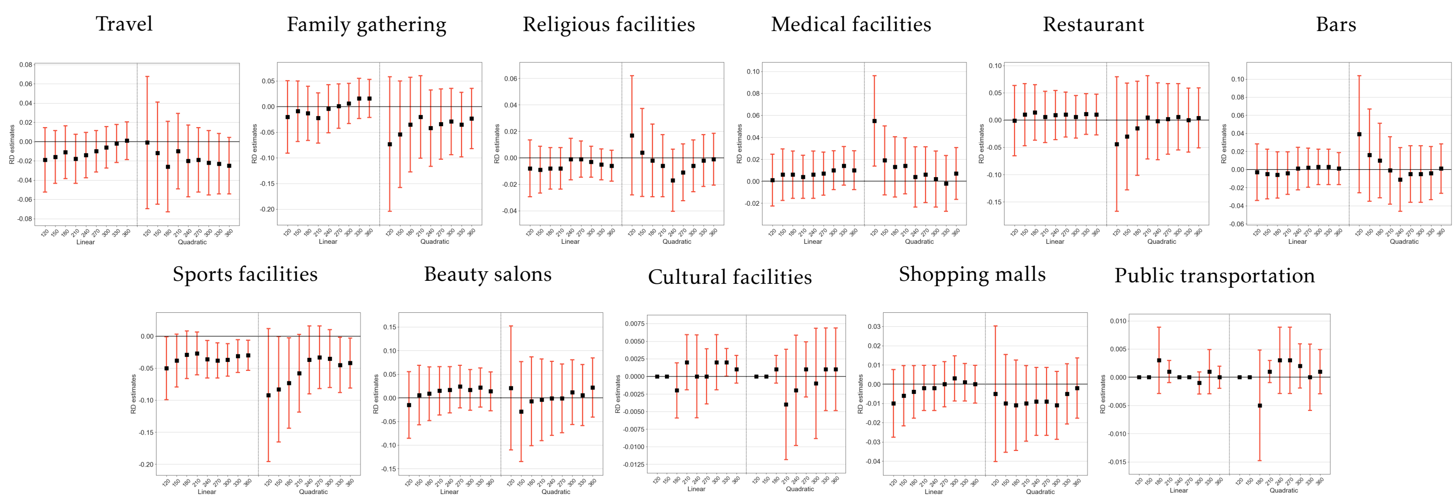
Notes: Each figure displays estimates of β from local polynomial regression of equation (1) at different bandwidths from 4 to 12 months in increments of 1 month, for linear and quadratic polynomial specifications. Degree of $f(\cdot)$ and the bandwidth size are on the x-axis. Coefficient estimates and 95% confidence bounds are on the y-axis. The running variable is birth month.

Figure A13: Sensitivity to bandwidth and polynomial degree: Effects of vaccine eligibility on social distancing behaviors (survey data)

Panel A. Engaged in social activities



Panel B. Ever engaged in social activities without mask

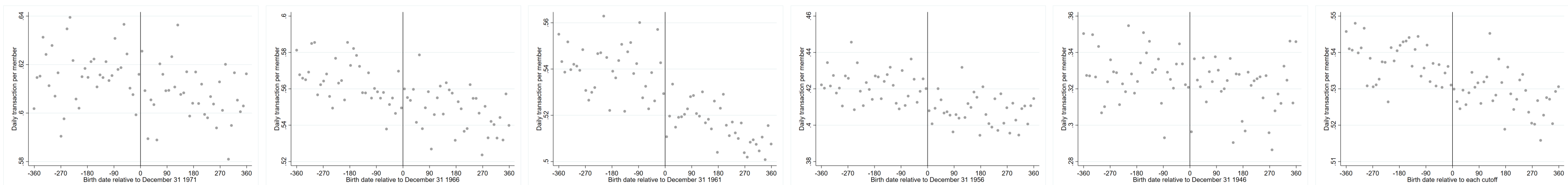


Notes: Each figure displays estimates of β from local polynomial regression of equation (1) at different bandwidths from 4 to 12 months in increments of 1 month, for linear and quadratic polynomial specifications. Degree of $f(\cdot)$ and the bandwidth size are on the x-axis. Coefficient estimates and 95% confidence bounds are on the y-axis. The running variable is birth month.

Figure A14: Effects of vaccine eligibility on social distancing behaviors at different age cutoffs (credit card data and airline data)

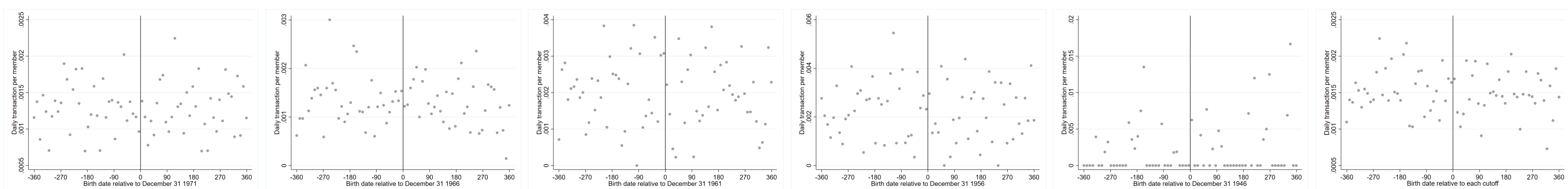
Panel A. Average daily offline transactions

1971–1972 cohort | 1966–1967 cohort | 1961–1962 cohort | 1956–1957 cohort | 1946–1947 cohort | Stacked-up



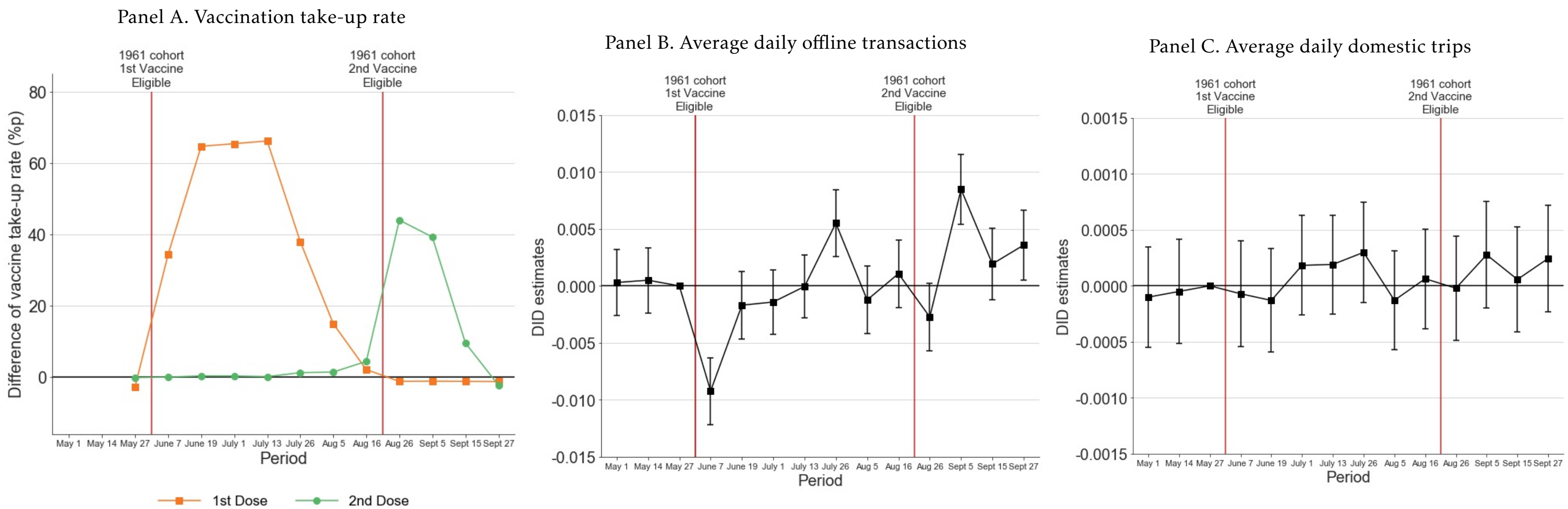
Panel B. Average daily domestic trips

1971–1972 cohort | 1966–1967 cohort | 1961–1962 cohort | 1956–1957 cohort | 1946–1947 cohort | Stacked-up



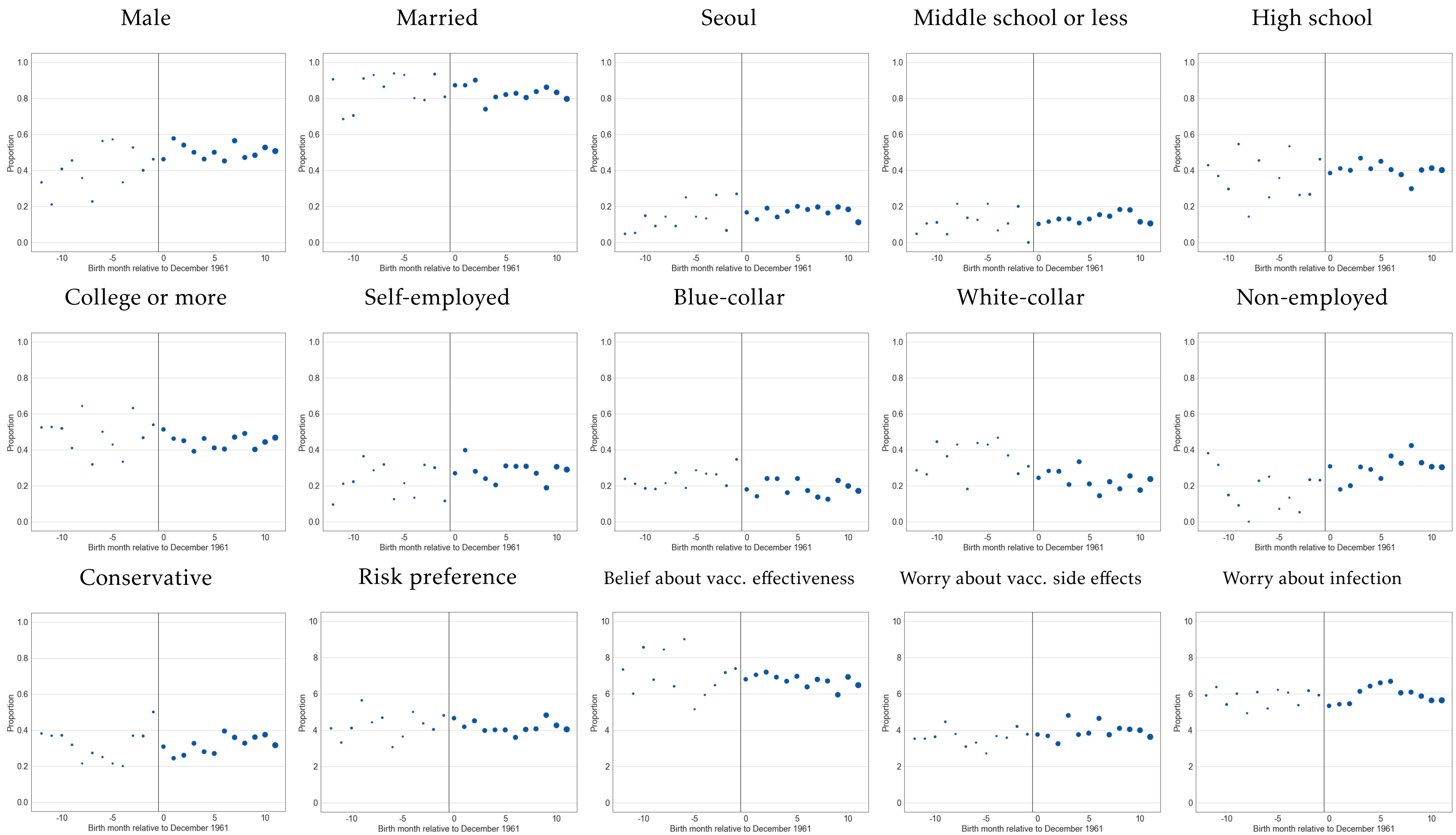
Notes: These figures show reduced-form effects of outcome variables in credit card data (panel A) and airline data (panel B) around different vaccination eligibility cutoffs (vertical line). The size of the dots corresponds to the number of observations in each bin. The period used for estimating the effects is the first 10–12 days after vaccination became eligible for each cohort.

Figure A15: Event-study DID results



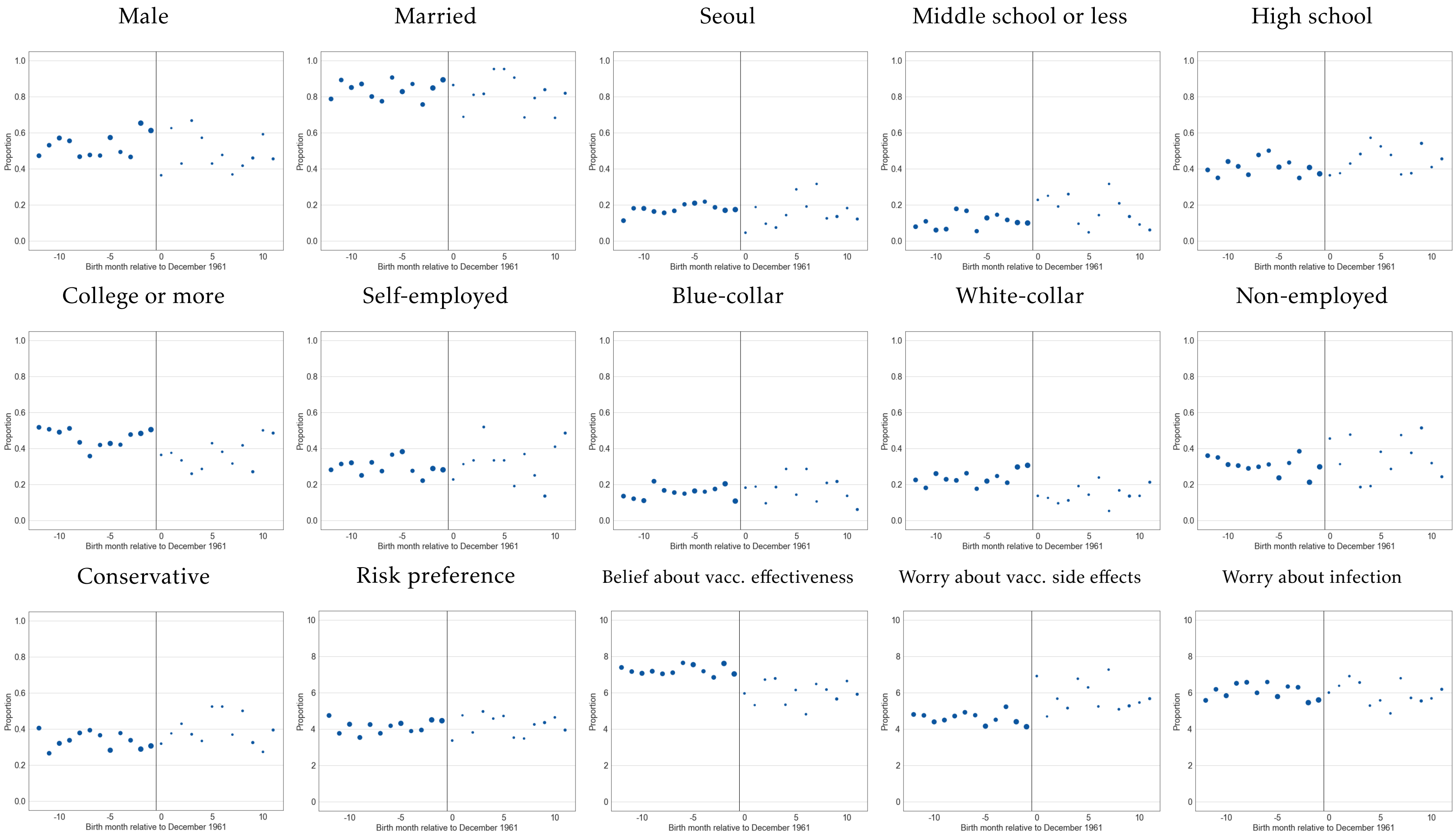
Notes: Panel A represents the difference in vaccine take-up rate between those born in 1961 and 1962. Panels B and C represent event-study DID estimates of equation (A10). Coefficients and 95% confidence bounds are on the y-axis. See Appendix B for details.

Figure A16: Selection heterogeneity test: always-takers vs compliers



Notes: These figures show the mean characteristics around the vaccination eligibility cutoff (vertical line) for always-takers and compliers. The size of the dots corresponds to the number of observations in each bin. "Conservative" is an indicator of "strongly conservative" or "weakly conservative". "Risk preference": 0 - strongly risk-averse, 10 - strongly risk-taking. "Belief about vaccine effectiveness": 0 - expect no effects, 10 - expect strong effects. "Worry about vaccine side effects": 0 - not worried at all, 10 - very worried. "Worry about COVID-19 infection": 0 - not worried at all, 10 - very worried.

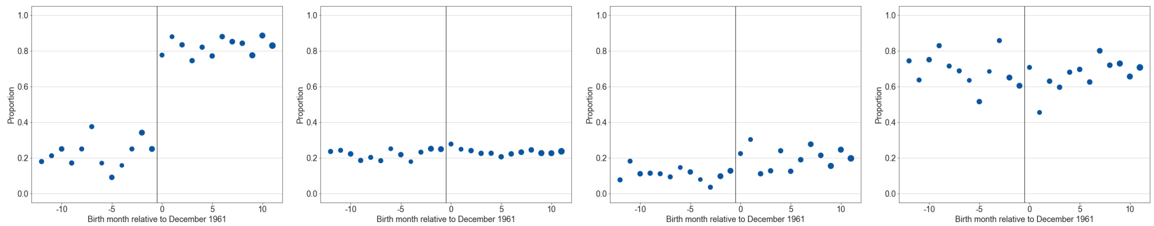
Figure A17: Selection heterogeneity test: never-takers vs compliers



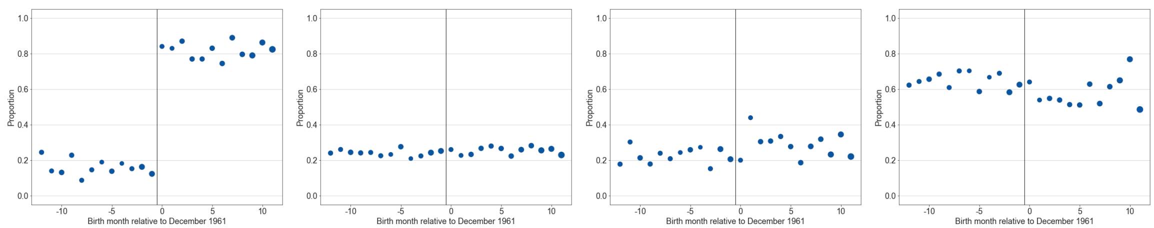
Notes: These figures show the mean characteristics around the vaccination eligibility cutoff (vertical line) for never-takers and compliers. The size of the dots corresponds to the number of observations in each bin. "Conservative" is an indicator of "strongly conservative" or "weakly conservative". "Risk preference": 0 - strongly risk-averse, 10 - strongly risk-taking. "Belief about vaccine effectiveness": 0 - expect no effects, 10 - expect strong effects. "Worry about vaccine side effects": 0 - not worried at all, 10 - very worried. "Worry about COVID-19 infection": 0 - not worried at all, 10 - very worried.

Figure A18: Heterogeneous effects (survey data)

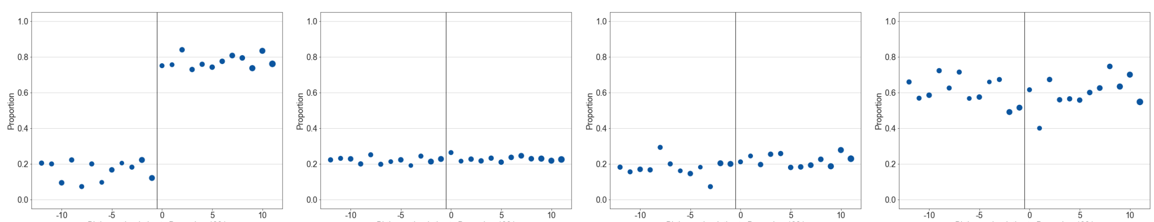
Spouse: vaccine eligible



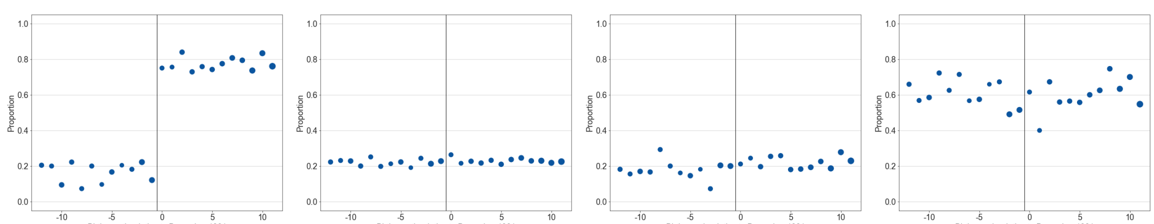
Spouse: vaccine ineligible



Belief about vaccine effectiveness: high (index ≥ 8)



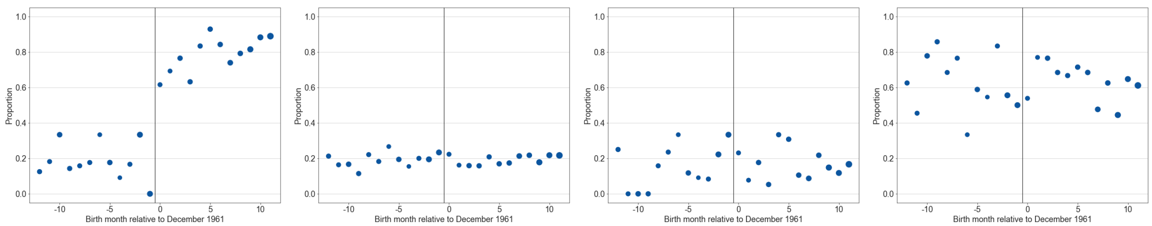
Belief about vaccine effectiveness: low (index < 8)



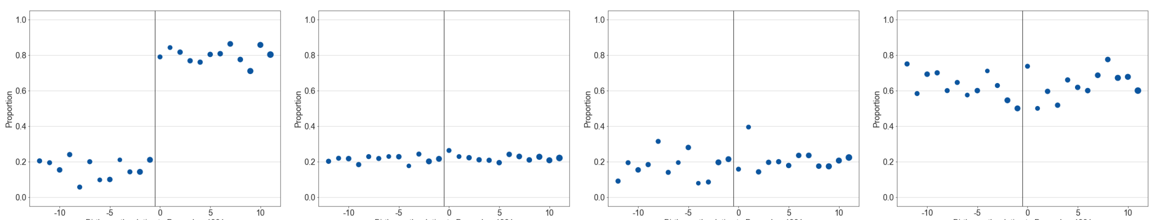
Notes: These figures show reduced-form effects of the outcome variables in the survey data around the vaccination eligibility cutoff (vertical line) separately for each subgroup. As in Figure 3, the dependent variable is first dose vaccine take-up rate, "engaged in social activities," "ever engaged in social activities without mask," and "avoided contact with others," respectively. The size of the dots corresponds to the number of observations in each bin. "Belief about vaccine effectiveness": 0 - expect no effects, 10 - expect strong effects.

Figure A18: Heterogeneous effects (survey data, continued)

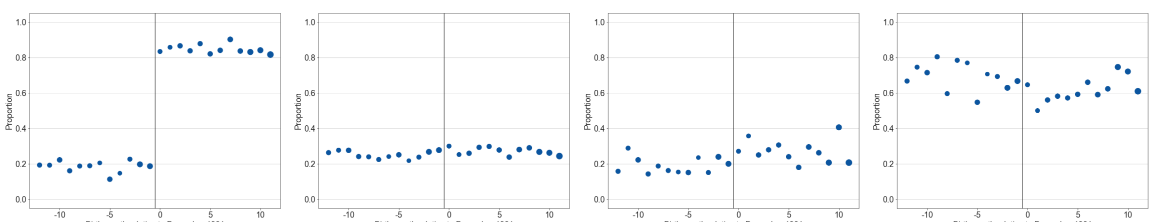
Education: middle school or less



Education: high school



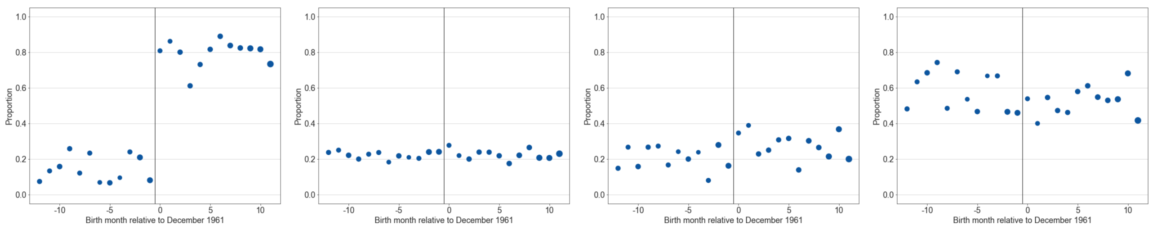
Education: college or more



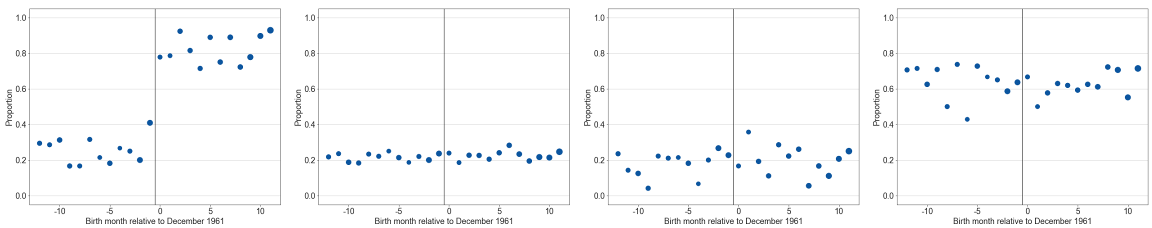
Notes: These figures show reduced-form effects of the outcome variables in the survey data around the vaccination eligibility cutoff (vertical line) separately for each subgroup. As in Figure 3, the dependent variable is first dose vaccine take-up rate, "engaged in social activities," "ever engaged in social activities without mask," and "avoided contact with others," respectively. The size of the dots corresponds to the number of observations in each bin.

Figure A18: Heterogeneous effects (survey data, continued)

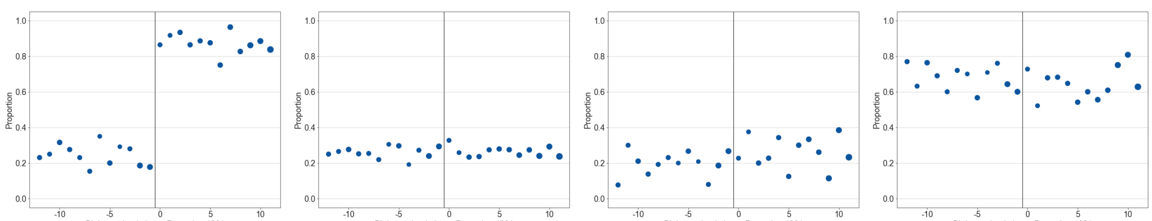
Occupation: self-employed



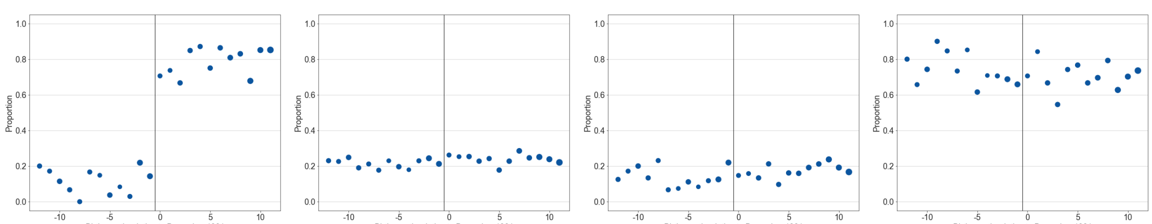
Occupation: blue-collar



Occupation: white-collar



Occupation: non-employed



Notes: These figures show reduced-form effects of the outcome variables in the survey data around the vaccination eligibility cutoff (vertical line) separately for each subgroup. As in Figure 3, the dependent variable is first dose vaccine take-up rate, "engaged in social activities," "ever engaged in social activities without mask," and "avoided contact with others," respectively. The size of the dots corresponds to the number of observations in each bin.

Table A1: Covariate balance test (credit card data and airline data)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Credit card data	Residence: Seoul	Average daily transactions in pre-treatment period				
		All offline sectors	Food & beverage	Sports & entertainment	Misc. services	Lodging
RD estimates (β)	0.0003	−0.0027	−0.0014	−0.0001	−0.0008	−0.0002
	(0.0027)	(0.0032)	(0.0012)	(0.0002)	(0.0008)	(0.0003)
Mean of dep var. in [-365 days,0)	0.2044	0.4795	0.1145	0.0076	0.1073	0.0097
Observations	730	730	730	730	730	

	(7)	(8)	(9)	(10)	(11)
	Average daily transactions in pre-treatment period				
	Offline retail	Clothing	Home appliances	Online retail	Medical expenditure
RD estimates (β)	−0.0004	0.0000	0.0003	−0.0017*	0.0017***
	(0.0018)	(0.0001)	(0.0002)	(0.0009)	(0.0005)
Mean of dep var. in [-365 days,0)	0.2238	0.0085	0.0082	0.0478	0.0478
Observations	730	730	730	730	730

	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. Airline data	Male	Days of membership	Average daily trips in pre-treatment period			
			All flights	Mainland → Jeju	Jeju → Mainland	Mainland → Mainland
RD estimates (β)	−0.01323	−0.37668	−0.00021	0.00001	0.00001	−0.00023**
	(0.01089)	(24.76827)	(0.00017)	(0.00007)	(0.00007)	(0.00010)
Mean of dep var. in [-365 days,0)	0.56311	1314.01	0.00171	0.00067	0.00065	0.00038
Observations	33,613	33,613	33,613	33,613	33,613	33,613

Notes: This table represent the RD estimates of equation (1). We use a local linear regression with a uniform kernel and a 365-days bandwidth. The pre-treatment period is January 1, 2021–June 5, 2021. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table A2: Covariate balance test (survey data)

	(1)	(2)	(3)	(4)	(5)	(6)
	Male	Married	Residence: Seoul	Middle school or less	High school	College or more
RD estimates (β)	−0.064	−0.008	−0.036	0.023	0.028	−0.035
	(0.037)	(0.027)	(0.028)	(0.024)	(0.037)	(0.037)
Mean of dep var. in [−12 months,0)	0.512	0.841	0.169	0.108	0.400	0.469
Observations	2,916	2,916	2,916	2,916	2,916	2,916

	(7)	(8)	(9)	(10)
	Self-employed	Blue-collar	White-collar	Non-employed
RD estimates (β)	0.017	0.011	−0.054	0.023
	(0.034)	(0.029)	(0.033)	(0.034)
Mean of dep var. in [−12 months,0)	0.286	0.170	0.259	0.281
Observations	2,916	2,916	2,916	2,916

	(11)	(12)	(13)	(14)	(15)
	Conservative	Risk preference	Belief about vaccine effectiveness	Worry about vaccine side effects	Worry about COVID-19 infection
RD estimates (β)	−0.020	−0.095	−0.355	−0.012	0.125
	(0.035)	(0.223)	(0.228)	(0.246)	(0.247)
Mean of dep var. in [−12 months,0)	0.334	4.185	7.209	4.403	5.976
Observations	2,916	2,916	2,916	2,916	2,916

Notes: This table represents the RD estimates of equation (1). We use a local linear regression with a uniform kernel and a 12-months bandwidth. “Political preference”: 1 - strongly conservative, 5 - strongly progressive. “Conservative” = strongly conservative + weakly conservative. “Risk preference”: 0 - strongly risk-averse, 10 - strongly risk-taking. “Belief about vaccine effectiveness”: 0 - expect no effects, 10 - expect strong effects. “Worry about vaccine side effects”: 0 - not worried at all, 10 - very worried. “Worry about COVID-19 infection”: 0 - not worried at all, 10 - very worried. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table A3: McCrary test

Panel A. Survey data									
	bandwidth (months)								
cutoff	4	5	6	7	8	9	10	11	12
-4	−2.51	−2.74	−2.19	−1.30	−0.54	0.60	0.91	1.05	1.13
-3	5.05	3.62	3.45	3.56	3.28	2.89	3.02	2.88	2.69
-2	2.22	−0.95	1.57	1.92	1.92	1.69	1.39	1.43	1.38
-1	−2.01	−2.02	−4.27	−3.86	−3.55	−3.17	−3.02	−2.90	−2.55
0	51.17	0.16	−0.82	−2.93	−3.50	−3.83	−3.84	−3.85	−3.81
1	5.08	2.24	3.73	3.75	1.87	0.65	−0.25	−0.86	−1.35
2	5.33	−0.54	1.42	3.37	4.48	3.72	2.94	2.13	1.38
3	−0.92	−1.51	−2.66	−1.61	0.29	2.08	2.26	2.20	1.90
4	69.38	0.66	−0.19	−1.81	−1.83	−0.68	0.82	1.27	1.57
Panel B. Credit card data									
cutoff	4	5	6	7	8	9	10	11	12
-4	−52.51	−62.85	−60.47	−41.59	−12.21	14.30	33.79	48.57	58.71
-3	9.16	13.87	30.77	50.67	66.74	81.50	90.77	93.81	93.38
-2	22.96	51.19	66.67	71.26	73.31	74.09	76.40	75.37	70.07
-1	−13.65	−22.22	−24.81	−24.87	−25.93	−26.18	−27.19	−26.23	−23.94
0	−20.71	−45.16	−66.53	−77.55	−84.40	−90.43	−95.04	−93.82	−89.23
1	20.63	29.14	15.54	−10.62	−35.62	−55.49	−66.23	−74.51	−79.82
2	13.08	29.74	47.74	47.64	32.70	18.06	1.22	−15.18	−29.18
3	−22.33	−18.61	−4.39	22.01	46.08	50.58	43.50	31.05	17.43
4	0.70	−10.45	−13.91	0.90	22.85	42.37	49.83	50.37	45.53
Panel C. Airline data									
cutoff	4	5	6	7	8	9	10	11	12
-4	−4.99	−5.77	−6.92	−5.69	−2.25	1.42	4.25	6.52	8.12
-3	−0.14	−0.11	2.74	6.11	8.88	11.45	13.25	14.02	14.24
-2	2.08	5.86	8.52	10.30	11.35	11.84	12.53	12.74	12.32
-1	0.81	0.04	−0.38	−0.71	−0.84	−0.71	−0.83	−0.44	0.20
0	−4.84	−7.47	−10.10	−11.68	−12.75	−13.40	−13.50	−12.84	−11.59
1	2.66	1.97	−0.53	−4.13	−7.54	−10.12	−11.25	−11.84	−12.09
2	1.92	4.60	6.59	5.92	3.68	1.40	−1.14	−3.45	−5.39
3	−1.87	−0.49	1.83	5.48	8.25	8.43	6.79	4.38	2.05
4	−0.57	−2.32	−1.71	1.22	4.69	7.23	8.07	7.68	6.38

Notes: This table presents t-statistics of the RD manipulation test (McCrary test) for the smoothness of frequency density of survey data using the `rddensity` package in Stata. Bin size 1 month is used.

Table A4: Effects of vaccine eligibility on social distancing behaviors by category (credit card data)

	(1)	(2)	(3)	(4)	(5)	(6)
	T1 (June 7–June 30)	T2 (July 1–July 25)	T3 (July 26–Aug 25)	T4 (Aug 26–Sept 4)	T5 (Sept 5–Sept 26)	T6 (Sept 27–Oct 7)
	Panel A. Food & beverage					
RD estimates (β)	−0.00222	−0.00096	0.00011	−0.00060	0.00084	0.00068
	(0.00148)	(0.00143)	(0.00142)	(0.00174)	(0.00143)	(0.00165)
Mean of dep var. in [-365 days,0)	0.13606	0.13086	0.13071	0.13205	0.12817	0.13178
Observations	730	730	730	730	730	730
	Panel B. Sports & entertainment					
RD estimates (β)	−0.00047	−0.00002	0.00042*	0.00031	0.00044*	0.00048
	(0.00029)	(0.00027)	(0.00026)	(0.00032)	(0.00026)	(0.00032)
Mean of dep var. in [-365 days,0)	0.00951	0.00885	0.00834	0.00857	0.00866	0.00882
Observations	730	730	730	730	730	730
	Panel C. Miscellaneous service					
RD estimates (β)	−0.00203**	−0.00094	0.00013	−0.00119	−0.00056	0.00010
	(0.00097)	(0.00097)	(0.00090)	(0.00100)	(0.00094)	(0.00101)
Mean of dep var. in [-365 days, 0)	0.12436	0.11409	0.11167	0.08093	0.11764	0.08825
Observations	730	730	730	730	730	730
	Panel D. Lodging					
RD estimates (β)	−0.00010	0.00026	0.00001	−0.00024	0.00004	0.00043
	(0.00034)	(0.00034)	(0.00034)	(0.00042)	(0.00033)	(0.00044)
Mean of dep var. in [-365 days,0)	0.01164	0.01045	0.00998	0.01076	0.01049	0.01159
Observations	730	730	730	730	730	730
	Panel E. Offline retail					
RD estimates (β)	−0.00296	−0.00111	0.00056	−0.00301	0.00157	0.00128
	(0.00239)	(0.00233)	(0.00232)	(0.00257)	(0.00228)	(0.00237)
Mean of dep var. in [-365 days,0)	0.24992	0.24947	0.24518	0.25056	0.25536	0.22963
Observations	730	730	730	730	730	730
	Panel F. Clothing					
RD estimates (β)	0.00004	0.00012	−0.00007	0.00002	0.00032	0.00005
	(0.00022)	(0.00020)	(0.00023)	(0.00024)	(0.00020)	(0.00023)
Mean of dep var. in [-365 days,0)	0.01067	0.00958	0.00782	0.00816	0.00913	0.00902
Observations	730	730	730	730	730	730
	Panel G. Home appliances					
RD estimates (β)	−0.00016	0.00006	0.00035	−0.00001	0.00036	0.00007
	(0.00027)	(0.00027)	(0.00024)	(0.00029)	(0.00023)	(0.00033)
Mean of dep var. in [-365 days,0)	0.00934	0.00899	0.00822	0.00750	0.00829	0.00804
Observations	730	730	730	730	730	730
	Panel H. Online retail					
RD estimates (β)	−0.00225*	−0.00173	−0.00206	−0.00199	−0.00095	−0.00170
	(0.00134)	(0.00139)	(0.00136)	(0.00151)	(0.00112)	(0.00123)
Mean of dep var. in [-365 days,0)	0.05493	0.05688	0.05554	0.05695	0.05117	0.05506
Observations	730	730	730	730	730	730
	Panel I. Medical expenditure					
RD estimates (β)	0.00242***	0.00183***	0.00235***	0.00357***	0.00196***	0.00310***
	(0.00072)	(0.00067)	(0.00066)	(0.00092)	(0.00075)	(0.00100)
Mean of dep var. in [-365 days,0)	0.05287	0.04868	0.05134	0.05204	0.04600	0.05583
Observations	730	730	730	730	730	730

Notes: This table represents the RD estimates of (1). We used a local linear regression with a uniform kernel and a 365-days bandwidth. The spending categories are as follows: food and beverage, sports and entertainment, miscellaneous services (beauty salons, education, fuel), lodging, offline retail (supermarkets, department stores, cars), clothing (clothes, accessories, cosmetics), home appliances (furniture, electronics), medical expenditure (hospitals, pharmacies), and online retail. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table A5: Effects of vaccine eligibility on social distancing behaviors by category (airline data)

	(1)	(2)	(3)	(4)	(5)	(6)
	T1 (June 7–June 30)	T2 (July 1–July 25)	T3 (July 26–Aug 25)	T4 (Aug 26–Sept 4)	T5 (Sept 5–Sept 26)	T6 (Sept 27–Oct 7)
	Panel A. Mainland → Jeju					
RD estimates (β)	−0.000072	0.000059	0.000098	−0.000060	−0.000023	0.000182
	(0.000125)	(0.000109)	(0.000103)	(0.000171)	(0.000134)	(0.000181)
Mean of dep var. in [-365 days,0)	0.000765	0.000537	0.000505	0.000618	0.000724	0.000732
Observations	33,613	33,613	33,613	33,613	33,613	33,613
	Panel B. Jeju → Mainland					
RD estimates (β)	0.000066	0.000110	0.000042	−0.000248	0.000045	0.000343*
	(0.000130)	(0.000121)	(0.000101)	(0.000173)	(0.000127)	(0.000180)
Mean of dep var. in [-365 days,0)	0.000753	0.000546	0.000528	0.000697	0.000665	0.000711
Observations	33,613	33,613	33,613	33,613	33,613	33,613
	Panel C. Mainland → Mainland					
RD estimates (β)	−0.000226	0.000035	−0.000013	0.000036	−0.000098	0.000127
	(0.000159)	(0.000139)	(0.000103)	(0.000140)	(0.000106)	(0.000147)
Mean of dep var. in [-365 days,0)	0.000461	0.000397	0.000250	0.000283	0.000286	0.000294
Observations	33,613	33,613	33,613	33,613	33,613	33,613

Notes: This table represent the RD estimates of equation (1). We use a local linear regression with a uniform kernel and a 365-days bandwidth. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table A6: Effects of vaccine eligibility on social distancing behaviors by category (survey data)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Social activities										
Panel A. Engaged in social activities	All social activities	Travel	Family gathering	Religious facilities	Restaurants	Bars	Sports facilities	Beauty salons	Cultural facilities	Shopping malls	Public transportation	Medical facilities
RD estimates (β)	0.012	−0.031	0.020	−0.045	0.056	0.003	−0.002	0.030	−0.001	0.052	0.037	0.098***
	(0.010)	(0.024)	(0.030)	(0.029)	(0.037)	(0.013)	(0.023)	(0.032)	(0.008)	(0.037)	(0.035)	(0.037)
Mean of dep var. in [−12 months,0)	0.229	0.114	0.190	0.170	0.572	0.027	0.091	0.236	0.021	0.538	0.281	0.561
Observations	2,910	2,916	2,915	2,916	2,913	2,916	2,916	2,916	2,916	2,916	2,914	2,914
		Social activities										
Panel B. Ever engaged in social activities without mask	All social activities	Travel	Family gathering	Religious facilities	Restaurants	Bars	Sports facilities	Beauty salons	Cultural facilities	Shopping malls	Public transportation	Medical facilities
RD estimates (β)	0.039	0.001	0.016	−0.006	0.010	0.001	−0.030**	0.014	0.001	0.000	0.000	0.010
	(0.031)	(0.010)	(0.019)	(0.006)	(0.019)	(0.009)	(0.012)	(0.021)	(0.001)	(0.005)	(0.001)	(0.009)
Mean of dep var. in [−12 months,0)	1.182	0.020	0.054	0.007	0.059	0.012	0.024	0.076	0.002	0.004	0.001	0.014
Observations	2,910	2,916	2,915	2,916	2,913	2,916	2,916	2,916	2,916	2,916	2,914	2,914

Notes: This table represents the RD estimates of equation (1). We use a local linear regression with a uniform kernel and a 12-months bandwidth. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table A7: Heterogeneous effects of vaccine eligibility on social distancing behaviors (survey data)

	(1) First vaccine take-up rate	(2) Engaged in social activities	(3) Ever engaged in social activities without mask	(4) Avoided contact with others
	Spouse: vaccine eligible			
RD estimates (β)	0.593***	0.018	0.025	0.061
	(0.040)	(0.014)	(0.038)	(0.048)
	0.199	0.217	0.145	0.672
	1,728	1,724	1,724	1,713
	Spouse: vaccine ineligible			
RD estimates (β)	0.682***	0.010	0.096*	−0.081
	(0.044)	(0.016)	(0.052)	(0.059)
	0.158	0.243	0.225	0.641
	1,188	1,186	1,186	1,175
	Belief about vaccine effectiveness: high (index $\geq$ 8)			
RD estimates (β)	0.670***	0.013	0.038	0.017
	(0.039)	(0.015)	(0.043)	(0.049)
	0.189	0.236	0.186	0.693
	1,545	1,542	1,542	1,531
	Belief about vaccine effectiveness: low (index < 8)			
RD estimates (β)	0.598***	0.014	0.047	0.004
	(0.044)	(0.015)	(0.044)	(0.055)
	0.168	0.220	0.176	0.611
	1,371	1,368	1,368	1,357

Notes: This table represents the RD estimates of equation (1) separately for each subgroup. We use a local linear regression with a uniform kernel and a 12-months bandwidth. "Belief about vaccine effectiveness": 0 - expect no effects, 10 - expect strong effects. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table A7: Heterogeneous effects of vaccine eligibility on social distancing behaviors (survey data, continued)

	(1) First vaccine take-up rate	(2) Engaged in social activities	(3) Ever engaged in social activities without mask	(4) Avoided contact with others
	Education: middle school or less			
RD estimates (β)	0.506***	−0.038	−0.072	0.126
	(0.090)	(0.025)	(0.086)	(0.108)
	0.184	0.193	0.156	0.633
	363	361	361	361
	Education: high school			
RD estimates (β)	0.646***	0.012	0.018	0.064
	(0.046)	(0.017)	(0.047)	(0.059)
	0.165	0.213	0.179	0.625
	1,182	1,179	1,179	1,168
	Education: college or more			
RD estimates (β)	0.676***	0.032**	0.091*	−0.084
	(0.042)	(0.015)	(0.047)	(0.054)
	0.187	0.255	0.193	0.688
	1,309	1,308	1,308	1,299

Notes: This table represents the RD estimates of equation (1) separately for each subgroup. We use a local linear regression with a uniform kernel and a 12-months bandwidth. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

Table A7: Heterogeneous effects of vaccine eligibility on social distancing behaviors (survey data, continued)

	(1) First vaccine take-up rate	(2) Engaged in social activities	(3) Ever engaged in social activities without mask	(4) Avoided contact with others
	Occupation: self-employed			
RD estimates (β)	0.650***	0.008	0.110*	0.010
	(0.053)	(0.020)	(0.060)	(0.071)
	0.144	0.223	0.209	0.570
	836	835	835	827
	Occupation: blue-collar			
RD estimates (β)	0.532***	0.008	−0.020	−0.024
	(0.075)	(0.022)	(0.073)	(0.087)
	0.251	0.214	0.182	0.643
	517	516	516	514
	Occupation: white-collar			
RD estimates (β)	0.702***	0.008	0.046	−0.001
	(0.054)	(0.020)	(0.063)	(0.072)
	0.239	0.261	0.199	0.675
	684	683	683	676
	Occupation: non-employed			
RD estimates (β)	0.643***	0.036*	0.007	0.022
	(0.055)	(0.020)	(0.052)	(0.066)
	0.120	0.216	0.142	0.739
	865	862	862	857

Notes: This table represents the RD estimates of equation (1) separately for each subgroup. We used a local linear regression with a uniform kernel and a 12-months bandwidth. ***: Significant at 1%, **: Significant at 5%, *: Significant at 10%

D Survey Questionnaires

Hello, this is (*surveyor's name*) a researcher at the Gallup Research Institute in Korea. Seoul National University is conducting a "COVID-19 Vaccine Effect Survey", and I would appreciate if you could take a moment. The responses and personal information will be used only for statistical purposes and kept confidential according to Article 33 of the Statistics Act.

◯ What city (state) do you live in? Please tell us based on your current address. *We do not know the region because we draw random phone numbers.

1. Seoul 2. Busan 3. Daegu 4. Incheon 5. Gwangju 6. Daejeon 7. Ulsan 8. Sejong 9. Gyeonggi 10. Gangwon 11. Chungbuk 12. Chungnam 13. Jeonbuk 14. Jeonnam 15. Gyeongbuk 16. Gyeongnam 17. Jeju

SQ1) Could you please tell us your birth year and month according to your resident registration?

(   ) year (   ) month

SQ2) Gender:

1. Male 2. Female

Q1) In the past week, how often have you intentionally avoided contact with others? (Please read the items)

1. Not at all. 2. Not much. 3. Sometimes. 4. Often. 5. Always.

9. Do not know / Prefer not to answer

From now on, we will ask you about wearing masks in daily life'.

Q2) In the past month, have you traveled for more than one night? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not traveled.
2. I have traveled, and I kept my mask on at all times.
3. I have traveled, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q3) In the past month, have you had social gatherings with family or friends? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not had social gatherings.
2. I have had social gatherings, and I kept my mask on at all times.
3. I have had social gatherings, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q4) In the past month, have you visited places of worship, such as churches, cathedrals, and temples? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not visited such places.
2. I have visited such places, and I kept my mask on at all times.
3. I have visited such places, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q5) In the past month, have you visited health facilities such as hospitals and public health centers? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not visited such places.
2. I have visited such places, and I kept my mask on at all times.
3. I have visited such places, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

From now on, we will ask you about your experiences during the past week.

Q6) In the past week, have you visited restaurants or cafes? (If you have,) Did you keep your mask on at all times except while eating, or did you occasionally take it off?

1. I have not visited such places.
2. I have visited such places, and I kept my mask on at all.
3. I have visited such places, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q7) In the past week, have you visited nightlife venues (amusement restaurants) such as bars and karaokes? (If you have,) Did you keep your mask on at all times except while eating, or did you occasionally take it off?

1. I have not visited such places.
2. I have visited such places, and I kept my mask on at all times.
3. I have visited such places, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q8) In the past week, have you visited gyms or fitness centers? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not visited such places.
2. I have visited such places, and I kept my mask on at all times.
3. I have visited such places, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q9) In the past week, have you visited hair and beauty salons, barber shops, spas or public baths? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not visited such places.
2. I have visited such places, and I kept my mask on at all times.
3. I have visited such places, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q10) In the past week, have you visited cultural facilities such as museums, concert halls, and movie theaters? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not visited such places.
2. I have visited such places, and I kept my mask on at all times.
3. I have visited such places, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q11) In the past week, have you visited shopping facilities such as supermarkets, local markets, and department stores? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not visited such places.
2. I have visited such places, and I kept my mask on at all times.
3. I have visited such places, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

Q12) In the past week, have you used public transportation such as bus, train, and metro? (If you have,) Did you keep your mask on at all times, or did you occasionally take it off?

1. I have not used public transportation.
2. I have used public transportation, and I kept my mask on at all times.
3. I have used public transportation, and I occasionally took my mask off.
9. Do not know / Prefer not to answer

From now on, we will ask you about Covid-19 vaccination'.

Q13) Are you vaccinated against Covid-19? (Please read the items)

1. I received my first dose.
2. I received my second dose
3. Unvaccinated
9. Do not know / Prefer not to answer

Q13-1) Which Covid-19 vaccine did you receive? In the case of cross-vaccination, please state the two vaccines in the order you received them.

(1) (   ) (2) (   )

1. AstraZeneca
2. Pfizer
3. Janssen
4. Moderna
5. Novavax
6. Do not know / Cannot remember
9. Prefer not to answer

Q13-2) Will you receive a Covid-19 vaccine in the future? (Please read the items)

1. Definitely will receive
2. Probably will receive
3. Probably will not receive
4. Definitely will not receive
9. Do not know / Prefer not to answer

Q14) How effective do you believe Covid-19 vaccines are in preventing infection? Please respond on a scale from 0 to 10, with 0 being 'not effective at all', 5 being 'moderately effective', and 10 being 'very effective'.

99. Do not know / Prefer not to answer

Q15) How concerned are you about adverse events and side effects caused by Covid-19 vaccines? Please respond on a scale from 0 to 10, with 0 being 'not at all concerned', 5 being 'somewhat concerned', and 10 being 'very seriously concerned'.

99. Do not know / Prefer not to answer

Q16 Are you currently married?

1. Yes
2. No
9. Do not know / Prefer not to answer

Q16-1) What is your spouse's birth year and month according to his/her resident registration?

(   ) year (   ) month

9. Do not know / Prefer not to answer

Q16-2) Has your spouse received Covid-19 vaccine? (Please read the items)

1. Received first dose
2. Received second dose
3. Unvaccinated
9. Do not know / Prefer not to answer

Q16-3) Which Covid-19 vaccine did your spouse receive? In the case of cross-vaccination, please state the two vaccines in the order he/she received them

(1) (   ) (2) (   )

1. AstraZeneca
2. Pfizer
3. Janssen
4. Moderna
5. Novavax
6. Do not know / Cannot remember
9. Prefer not to answer

From now on, we will ask you about personal preferences'.

Q17) How afraid are you of Covid-19 infection? Please respond on a scale from 0 to 10, with 0 being 'not afraid at all', 5 being 'somewhat afraid, and 10 being 'very afraid'.

99. Do not know / Prefer not to answer

Q18) How risk-averse are you in general? Please respond on a scale from 0 to 10, with 0 being 'completely avoid any risk', 5 being 'moderate', and 10 being 'ready to take any risk'.

99. Do not know / Prefer not to answer

Thank you very much for answering the questions so far. Finally, we would like to ask you a few more questions for data classification. We promise that these items will never be used for purposes others than statistical data classification.

DQ1) What is the highest level of education you have completed (received)?

1. Graduated from middle school or less
2. Graduated from high school
3. Attended / graduated from college
4. Attended / graduated from graduate school
9. Prefer not to answer

DQ2) What is your occupation?

1. Agriculture/Forestry/Fishery
2. Self-employment
3. Skilled Labor/Service Jobs
4. Office/Administrative Jobs (Office/Technical Jobs, Business/Administrative Jobs, Professional Jobs/Freelancer)
5. Keeping house
6. Student

7. Unemployed
8. Retired
9. others (Please specify:)
99. Do not know / Prefer not to answer

DQ3) What best describes your political orientation? (Please read the items)

1. Very conservative
2. Slightly Conservative
3. Moderate
4. Slightly progressive
5. Very progressive
9. Do not know / Prefer not to answer

Thank you very much for responding to the survey.