Vaccination nudges

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Abstract

A nudge changes people's actions without removing their options or altering their incentives. During the COVID-19 vaccine rollout, the Swedish Region of Uppsala sent letters with pre-booked appointments to inhabitants aged 16–17 instead of opening up manual appointment booking. Using regional and municipal vaccination data, we document a higher vaccine uptake among 16- to 17-year-olds in Uppsala compared to untreated control regions (constructed using the synthetic control method as well as neighboring municipalities). The results highlight pre-booked appointments as a strategy for increasing vaccination rates in populations with low perceived risk.

JEL: D78, H41, I18

Keywords: COVID-19, health policy, nudge, pre-booked, vaccination

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1 Introduction

COVID-19 vaccines are offered free of charge in all rich countries, but vaccination uptake mostly falls below 80 percent (Ritchie et al., 2020). Different measures are considered to increase vaccine uptake among hesitant individuals, such as cash incentives (direct payments and lotteries) and mandatory COVID-19 certificates (Campos-Mercade et al., 2021; Mills and Rüttenauer, 2021; Barber and West, 2022). These interventions can be expensive or intrusive, and the use of interventions that alter people's behavior without changing economic incentives or regulating behavior has thus received significant interest. A common approach is the use of nudges, which change the choice architecture to steer people's choices without limiting their options (Thaler and Sunstein, 2008). Two randomized controlled trials have studied the effects of nudges on COVID-19 vaccination uptake. Dai et al. (2021) found that text-based reminders effectively increased vaccination uptake from low vaccination levels early in the vaccination rollout. In contrast, Campos-Mercade et al. (2021) found no effect of three different types of nudges, starting from a higher vaccination level (70 percent).[1]

On July 15$^{\text{th}}$, 2021, Region Uppsala, one of Sweden's 21 regional governments and home to approximately 375,000 people, sent letters with pre-booked COVID-19 vaccination appointments to all residents aged 16 and 17. Other Swedish regions simply opened up bookings for this cohort. Similarly, other age groups could make their own appointments in Region Uppsala as well as in the rest of Sweden. The alternatives were to get vaccinated or not, and the decision by the regional authorities merely moved the default from the possibility of choosing an array of times or no time to the choice of a particular time. Not showing up (which one is not charged for) or cancelling the appointment remained possibilities. Thus, these letters in Region Uppsala provide a real-world example of an extensive nudge.

Our aim is to study whether these pre-booked appointments increased vaccine uptake. We use two empirical strategies to identify the effect. First, we use the synthetic control method to estimate the impact in Region Uppsala compared to other (untreated) Swedish regions. Second, we estimate the impact in municipalities in Region Uppsala compared to bordering (untreated) municipalities in other regions. We find a large and statistically

[1] They asked the participants to either (1) make a list of four people who would benefit from the participant getting vaccinated (social impact), (2) write down arguments that could best convince another person to get vaccinated (argument), or (3) participate in a quiz with information on the safety and effectiveness of COVID-19 vaccines (information).

significant effect of Region Uppsala's nudge on vaccine uptake, regardless of which of these two methods we use.

Important features of a nudge are that it neither removes nor adds alternatives and that it does not change the utilities associated with any of the available alternatives, other than through the presentation of the options (Thaler and Sunstein, 2008). The relevant nudge in this paper involves changing the default. Regarding this way of nudging, Madrian and Shea (2001) find that the decision to participate in a pension-savings program in which the employer matches one's own contribution is made far more often when it is made the default option. Similarly, Pichert and Katsikopoulos (2008) find green energy to be a more frequently chosen option when it is the default, and Li et al. (2013) report that registrations to organ donation registers increase with opt-out rules in comparison to opt-in rules.

The nudge may also work through social effects. Knowing that everyone else receives the same letter with a pre-booked appointment, teenagers who wish to be like other teenagers presume that their peers are now likelier to get vaccinated and so choose to do the same. Note that the recipients of the pre-booked appointments must still believe that the default option induces more of their peers to get vaccinated, although the independent effect of the default may be miniscule. A well-known feature of peer effects is that small changes in price (or the mental cost of making an appointment) can cause large equilibrium changes when peer consumption complements own consumption. This is because the small change will induce some portion of the peer group to get vaccinated, which, through the complement, causes some other portion to get vaccinated, etc. (Becker and Murphy, 2000).

Our results are consistent with those of Löfgren and Nordblom (2020), who construct a theoretical model that predicts the circumstances in which a nudge is likely to be effective. Their model shows that the likelihood of a nudge having an effect is higher for choices that the individual believes are unimportant. For choices that the individual considers important, nudges are less likely to have an effect. Since 16–17-year-olds are unlikely to suffer or die from either a COVID-19 infection (Kolk et al., 2021) or the side-effects of a vaccine (Patone et al., 2021), we should expect a considerable effect from the nudge studied in the present article. Indeed, the effect that we find is greater than those based on modest monetary payments or conditional cash lotteries in previous research. We note, however, that the young age of the individuals in our study also makes it unclear how generalizable our

findings are to older individuals, for whom the incentive to get vaccinated is greater.

Apart from adding to the health-economics literature on incentives and vaccination uptake (Campos-Mercade et al., 2021; Dai et al., 2021; Mills and Rüttenauer, 2021; Barber and West, 2022), we also contribute to the burgeoning literature on nudging (Madrian and Shea, 2001; Pichert and Katsikopoulos, 2008; Li et al., 2013; Löfgren and Nordblom 2020), the use of nudging as one tool, and the implementation of mandates and payments to increase vaccination rates.

2 Methods and data

2.1 Empirical framework

Since we have access to both regional and municipal vaccination data, we conduct analyses on both levels to assess the effect of the nudging intervention. In the regional analyses, we measure the impact through a comparative case study approach that compares the trend in vaccination uptake between Region Uppsala and a set of untreated but similar regions. Specifically, we implement the synthetic control method to construct a synthetic Uppsala, which closely resembles the real Uppsala in terms of pre-intervention characteristics, from a combination of all other Swedish regions (Abadie et al., 2010; Abadie, 2021). The synthetic control method is specifically designed to measure the impact of policy interventions affecting one unit (e.g., country, region, or municipality) when only a small number of control units are available. It is a data-driven method used to estimate counterfactuals—i.e., what would have happened without the nudge—which automatically determines the weighted combination of untreated regions that provides the best match to the treated region with regard to pre-intervention outcomes and covariates. The weighted average vaccination uptake from the synthetic control group then provides the counterfactual trend of the vaccination share for Region Uppsala, that is, it predicts how the vaccination rates would have developed in the absence of the nudging intervention. For a detailed presentation of the method, see Abadie (2021). For recent implementations of the synthetic control method related to the COVID-19 pandemic, see, for example, Cho (2020), Mitze (2020), and Alfano et al. (2021).

Abadie and Gardeazabal (2003) proposed a nested optimization routine to simultaneously determine (i) a set of unit weights (one for each control)

that determine each untreated unit's contribution to the synthetic control and (ii) variable importance weights (one for each covariate) to prioritize a good match on strong predictor outcomes. The latter aspect is useful in small datasets where a perfect match cannot be expected for all included variables. The standard method for determining variable weights relies on pre-intervention variation in the outcome. However, our data have (almost) no variations in any region before the vaccination rollout that can be exploited. Instead, we determine variable importance by regressing the mean of the post-intervention outcomes among the control regions on standardized versions of the covariates. Then, we use the absolute standardized coefficients from this procedure to construct variable importance weights that—as in the original approach—are normalized to sum to one (Bonander, 2021). We also consider equal importance weights and the standard variable importance estimation procedure in sensitivity analyses. Following Abadie et al. (2010), we conduct inference using in-place placebo studies, where we estimate "effects" in each control region to assess uncertainty.

In the municipal analysis, we compare the eight municipalities in Region Uppsala to all eight municipalities that share a border with a treated municipality. The idea is that the geographical proximity should make the untreated neighbors a reasonable control group, as individuals on different sides of the border share similar social environments. We conduct a descriptive comparison of the vaccination development in the treated and neighboring municipalities and compare the final vaccination share in the treated municipalities to those of their neighbors. We also run ordinary least squares regressions with the share of vaccinated as the outcome variable, with neighbor fixed effects and covariates (see next section for details). Finally, we perform difference-in-differences and event study difference-in-differences estimation (Schmidheiny and Siegloch 2019), in which we contrast the increase in vaccinations in the treated municipalities with the increase in the neighboring municipalities.

2.2 Data

The outcome data in the present study are the share of vaccinated individuals from the Public Health Agency of Sweden, structured as regional-level weekly panel data covering all 21 Swedish regions (defined in Eurostat's Nomenclature of Territorial Units for Statistics [NUTS3]). We have data on the share of vaccinated individuals in the 16–17-year age group and the 18–

19-year age group from week 1 to 46 in 2021 for all 21 Swedish regions. Covariates are share of COVID-19 deaths in 2020 (also from the Public Health Agency of Sweden); share with at least three years of higher education in 2020 (Statistics Sweden); share of foreign-born in 2020 (Statistics Sweden); and the pre-intervention share of vaccinated 18–29-year-olds, which we use as a proxy for the general willingness to get vaccinated in each region. All variables refer to the entire population unless otherwise noted. For the vaccination data, cells with three or fewer observations were set to zero by the Public Health Agency of Sweden due to integrity reasons, so we have some missing data in weeks when very few individuals got vaccinated. This was more of a problem in the early stage of the pandemic, when mainly individuals with medical risks (e.g., chronic lung disease, cancer, and diabetes) in the 16–17 year age group were vaccinated.

For the municipal analysis, we use data on the vaccination share in two-week intervals and the total vaccination share in week 49. We received two-week (instead of one-week) data to reduce the number of cells with three or fewer observations. We also use the cumulative share of vaccinated individuals in week 49; with this outcome, we lose no data but do not have the time series. As control variables, we have the share of foreign-born, the share with at least three years in higher education (both from Statistics Sweden), and the share of COVID-19 deaths in 2020 reported by the Public Health Agency of Sweden.

3 Results

3.1 Regional analyses

Figure 1 plots the trends in the share of first-dose vaccinations among 16–17-year-olds in Uppsala and the rest of Sweden. The vertical line indicates when Region Uppsala sent out letters with pre-booked vaccination times to all 16–17-year-olds (week 28). In the final week that we observe (week 46), we can see that vaccinations reached 85 percent of the age group in Uppsala and 75 percent in the other regions (unweighted average). In Table 1, we can see that Uppsala clearly differs from the average of the 20 control regions in terms of pre-intervention characteristics, whereas synthetic Uppsala closely matches real Uppsala on predictors with high variable importance weights. Table 2 displays the region weights for synthetic Uppsala, which are a weighted combination of two regions: Östergötland and Stockholm.

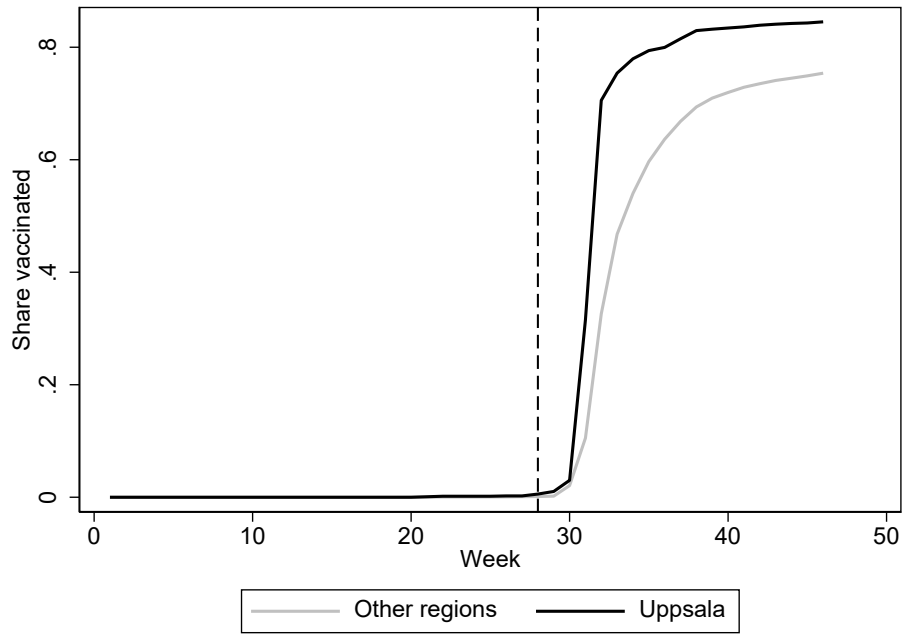

Figure 1. First-dose vaccinations in Uppsala (treated) and average of all other 20 Swedish regions

Table 1. Vaccination share predictor means

	Uppsala	Synthetic Uppsala	Average of 20 Control regions	V
Share foreign-born	.189	.189	.160	.69
Share high education	.180	.164	.138	.07
Share of COVID-19 deaths	.00104	.00101	.000838	.06
Share vaccinated (18–29 y)	.07676	.07325	.07360	.19

Notes: The period for each predictor is 2020, except for Share vaccinated (18–29 y), which refers to the mean share for all pre-intervention weeks. Variable importance weights (V) were determined by regressing the mean of the post-intervention outcomes among all controls on the covariates.

Table 2. Region weights in synthetic Uppsala

Region	Weight	Region	Weight
Stockholm	.205	Västra Götaland	0
Södermanland	0	Värmland	0
Östergötland	.795	Örebro	0
Jönköping	0	Västmanland	0
Kronoberg	0	Dalarna	0
Kalmar	0	Gävleborg	0
Gotland	0	Västernorrland	0
Blekinge	0	Jämtland	0
Skåne	0	Västerbotten	0
Halland	0	Norrbotten	0

The left panel in Figure 2 shows the difference in the share of first-dose vaccinations between Uppsala and synthetic Uppsala for the treated age group (16–17 years old). There is a clear difference in the share vaccinated between Uppsala and synthetic Uppsala in the post-treatment period, which peaks in week 32 at 30.6 percentage points. In week 46, the final week of measurements, the difference is 12.0 percentage points (72.4 in synthetic Uppsala and 84.5 in actual Uppsala). In the middle panel in Figure 2, we compare the effect estimated for Uppsala with the effect of placebo interventions implemented in the other 20 regions. Reassuringly, we can see that no other region has an effect estimate close to the one in Uppsala. However, we can see considerable variations, especially regarding the time point when vaccinations shares started to increase rapidly. The right panel in Figure 2 ranks the post-intervention effect sizes across all regions, showing that Uppsala has by far the largest estimated effect (with a placebo-based p-value of 1/21=0.047[2]). Overall, the analysis implies a large and persistent effect of the intervention.

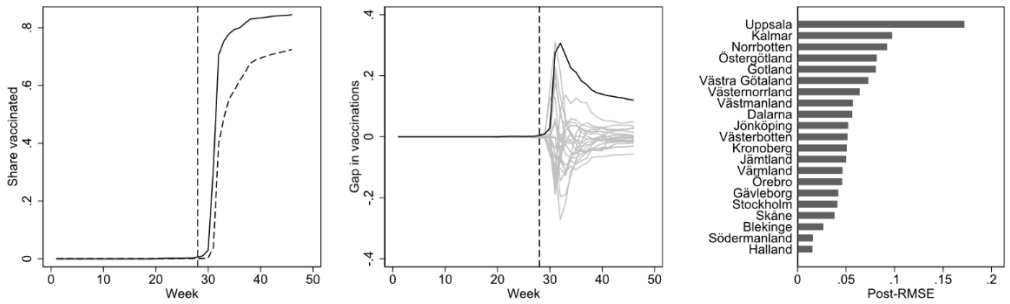

Figure 2. Effect (left), placebo (middle), and post-intervention effect size (right) plots. The left panel shows the share of first-dose vaccination by week in Uppsala (black) and synthetic Uppsala (dashed) among 16–17-year-olds. The middle panel shows effects estimated by assessing the vaccination share gaps between Uppsala and its synthetic counterpart (black) and equivalently defined placebo gaps in all 20 control regions (gray). The right panel shows the post-intervention root mean squared error (RMSE) in vaccination uptake from the synthetic control analysis in Uppsala and all other regions.

[2] Abadie et al. (2010) suggest using the ratio between the post-to-pre-intervention root mean squared error (RMSE) in the outcome variable (vaccination uptake) to handle differences in pre-intervention fit across the placebo analyses when assessing significance, which is neither feasible nor necessary with our data given that the RMSE in the pre-intervention period is zero in almost all analyses.

There are three regions with vaccination shares among 16- and 17-year-olds that are very close to the Uppsala vaccination rate toward the end of the study period: Gotland, Norrbotten, and Västerbotten. These three regions reached the Uppsala vaccination shares several weeks later but still stand out compared to the rest of Sweden. All three regions are small and remote (Gotland is an island in the Baltic Sea, and the others are the two northernmost regions). They are also among the five regions with the lowest share of foreign-born individuals and about average with regard to education levels. Even so, their effect estimates are considerably smaller than the estimated effect in Uppsala (Figure 2, right panel).

To further scrutinize the findings, we assess the vaccination share for the age group 18–29 years in the Uppsala region. Since they were not treated with pre-booked appointments, we do not expect them to have a higher vaccination rate than the same age group in synthetic Uppsala. However, there may be spillovers in the treatment; increased vaccinations in the treated age group may increase vaccinations among friends and relatives in the older age group. Appendix Figure A1 shows the results for the age group 18–29 years. In the final week, the difference is 5 percentage points (79.4 in synthetic Uppsala and 84.9 in actual Uppsala). With a placebo-based p-value of 0.286, we interpret this difference as a chance finding. In Appendix Figure A2, we estimate the effect on second-dose vaccinations for 16–17-year-olds, which yields similar results as in our main analysis (placebo-based p-value: 0.047). Sensitivity analyses in which we give equal importance to all predictors and use the variable optimization algorithm suggested by Abadie and Gardeazabal (2003) also show similar results (Appendix Figures A3–A4).

3.2 Municipal analyses

Figure 3 shows vaccination shares for 16–17-year-olds in the treated municipalities (Enköping, Heby, Håbo, Knivsta, Tierp, Uppsala, Älvkarleby, and Östhammar) and untreated neighboring municipalities (Avesta, Gävle, Norrtälje, Sala, Sandviken, Sigtuna, Upplands-Bro, and Västerås) for the final available data (week 49). The share of vaccinated people was 85.1 (95% confidence interval [CI]: 83.2, 87.0) percent in the treated municipalities compared with 72.2 (95% CI: 68.3, 76.1) percent in the neighboring untreated municipalities, a difference of 12.9 percentage points (95% CI: 9.0, 16.8).

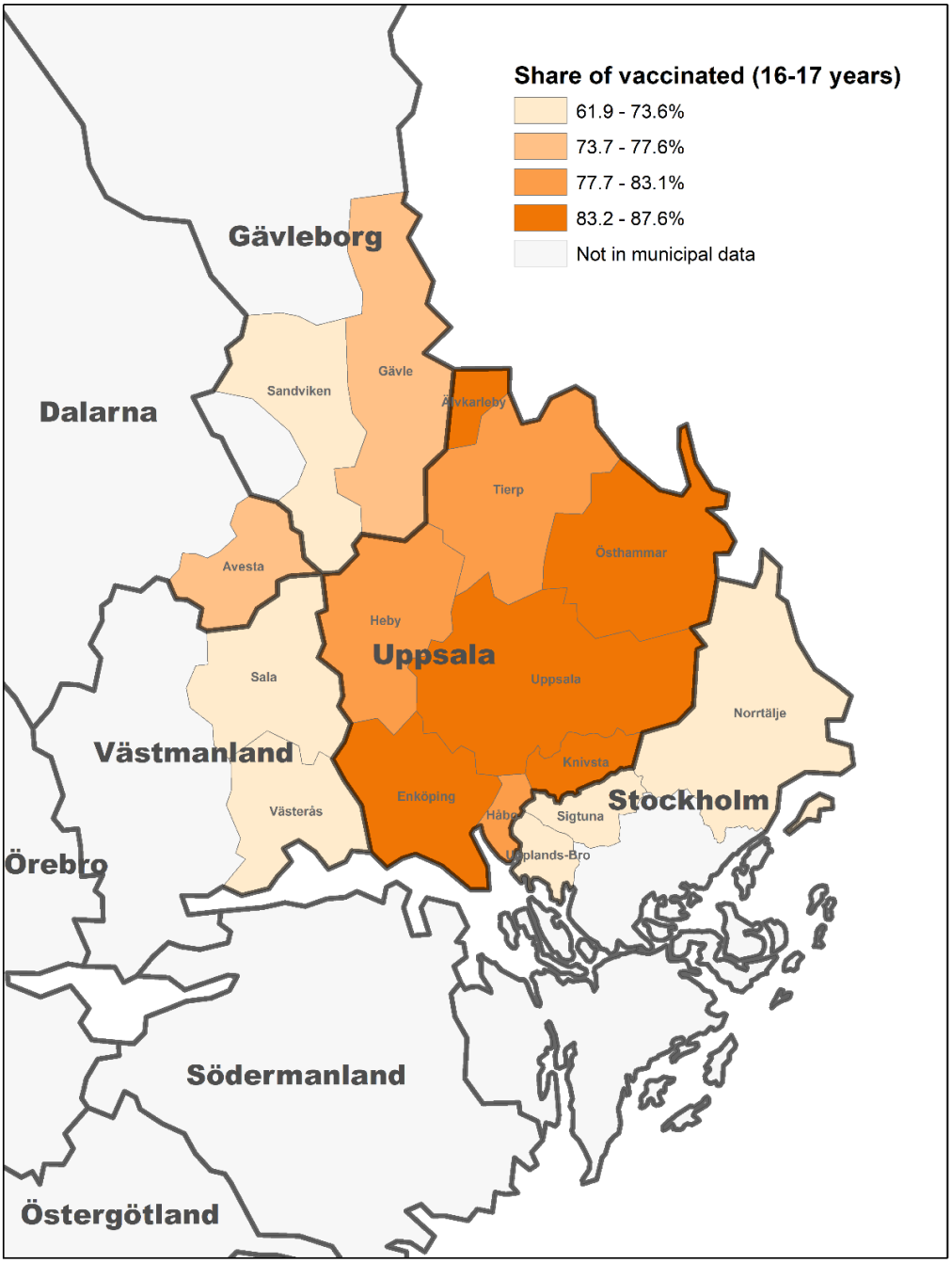

Figure 3. Share of vaccinated 16-17-year-olds in the treated and neighboring municipalities

Figure 4 plots the trends in the share of first-dose vaccinations among 16–17-year-olds in the treated municipalities and the neighboring municipalities. The vertical line indicates when Region Uppsala sent out letters with pre-booked vaccination times to all 16–17-year-olds (week 28). As in the regional analysis, the share of vaccinated individuals is considerably higher in the treated municipalities compared to their untreated neighbors.

Appendix Table A1 compares summary statistics for the observed covariates between each treated municipality and their neighbors. We find meaningful differences in some neighbor groups, indicating that it is important to adjust the municipal comparisons for observables even within neighbor groups. In Table 3, we present the results from ordinary least squares (OLS) regressions, with the cumulative vaccination share in week 49 among 16–17-year-olds as the dependent variable. In column 1, we include only a treatment dummy; in column 2, we include three control variables (share foreign-born, share high education, and COVID-19 deaths); in column 3, we include neighbor indicators (a dummy variable for each treated municipality, indicating its neighbors); and in column 4, we include all of the above. The treatment estimate is not statistically different from zero in column 4 (p=0.11), but the point estimate is still considerable, and we must consider the limited degrees of freedom in a model with 16 observations and 12 control variables. The estimated treatment effect varies from 8.1 to 12.9 percentage points. As in the regional analysis, we find no evidence of an effect on vaccine uptake among 18-29-year-olds after adjusting for observable confounders (Appendix Table A2). This result suggests that the residual confounding within neighbor groups is small after adjusting for observables, assuming the same sources of bias are present in both age groups (see e.g. Lipsitch et al., 2010).

A difference-in-differences estimation (without control variables) suggests an average effect in the post-treatment period of 15.6 percentage points (95% CI: 11.6, 20.0; CI computed using wild cluster bootstrap (Cameron et al., 2008)). Figure 5 contains time-specific effect estimates and 95 percent CIs (i.e., an event study difference-in-differences estimation), showing how the effect changes over time according to the municipality-level data. Like the regional analysis, we can see that the treatment effect is massive early on, and although it decreases over time, the vaccination share in the treated municipalities is considerably higher in the final time period.

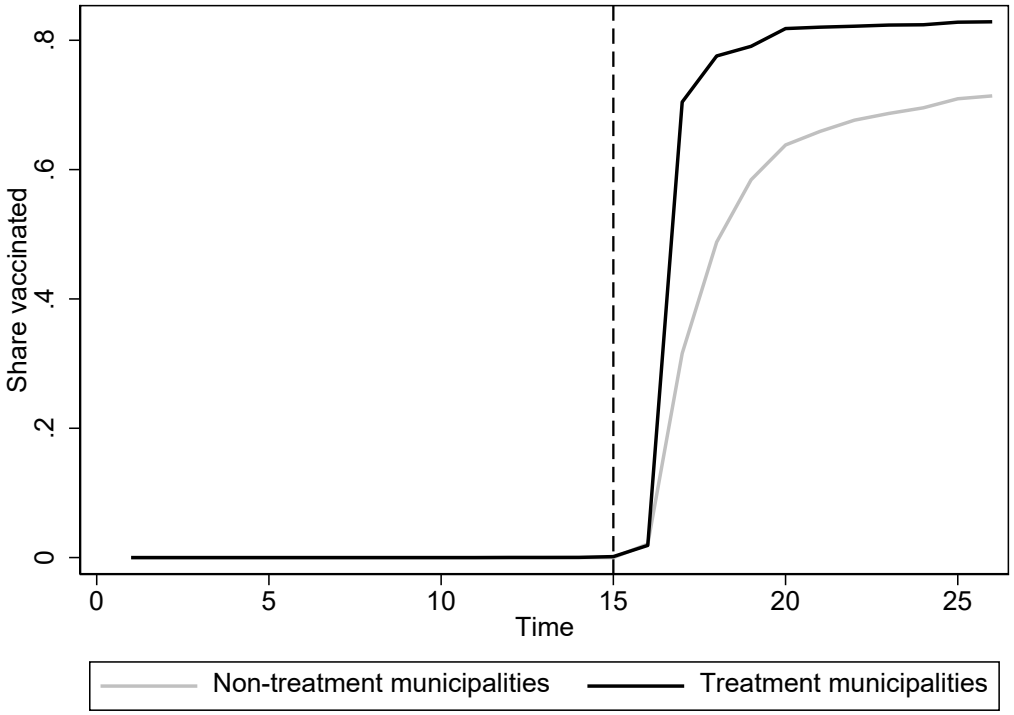

Figure 4. First-dose vaccinations in treated municipalities (located in Uppsala) and in their neighboring municipalities (outside Uppsala).

Table 3. Determinants of share of vaccinated 16-17-year-olds in treated and neighboring municipalities

	(1)	(2)	(3)	(4)
Treatment	0.129***	0.094***	0.115***	0.081
	(0.018)	(0.014)	(0.026)	(0.036)
Neighbor indicators	No	No	Yes	Yes
Share foreign-born	No	-0.531***	No	-0.354
		(0.114)		(0.331)
Share high education	No	0.215**	No	0.134
		(0.084)		(0.232)
COVID-19 deaths	No	6.375	No	-7.534
		(5.195)		(21.964)
Constant	0.722***	0.774***	0.748***	0.826***
	(0.013)	(0.027)	(0.039)	(0.101)
R^2	0.782	0.900	0.928	0.967

Notes: The dependent variable is the share of 16–17-year-olds vaccinated in week 49 in the 16 included municipalities. Ordinary least squares regressions controlling for Treatment (pre-booked appointments), Neighbor indicators (one dummy variable for each treated municipality, indicating its neighbors), as well as the control variables Share foreign-born, Share high education, and COVID-19 deaths. * p<0.1, ** p<0.05, *** p<0.01.

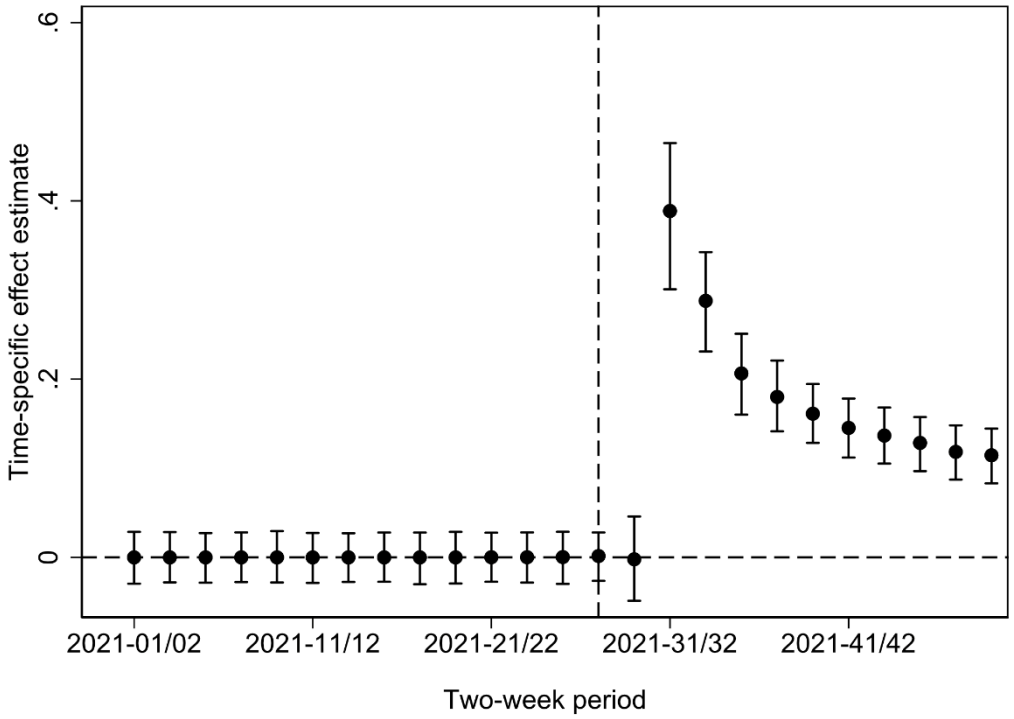

Figure 5. Time-specific coefficients and 95% wild cluster bootstrap confidence intervals from the difference-in-differences estimation

4 Discussion and conclusion

Our regional analysis suggests that pre-booked vaccination appointments increased vaccination uptake among 16–17-year-olds in the Uppsala region by about 10.3 percentage points, compared to a counterfactual uptake of 74.2 percent (in week 46). The municipal analyses also suggest an effect of 8.1–12.9 percentage points (in week 49). Although our estimates may be biased due to unobserved confounding, they are substantial, robust, and specific to the treated age range over two identification methods and datasets. They are also theoretically plausible. The effect we find is considerably higher than the effects found for modest monetary payments or conditional cash lotteries to increase COVID-19 vaccinations. Campos-Mercade et al. (2021) find that a monetary payment of 200 Swedish kronor (about $24) increased vaccinations by 4.2 percentage points (from a baseline of 71.6 percent) in a random sample of Swedes aged 18–49 years. Barber and West (2021) report that a conditional cash lottery in Ohio increased the vaccination share in the state population by 1.5 percent. In a study on nudges to

increase COVID-19 vaccination uptake, Dai et al. (2021) find that text-based reminders can effectively increase vaccination uptake from low initial vaccination levels in the overall population, at least in the early stages of the vaccination rollout. Conversely, Campos-Mercade et al. (2021) find no effects of three different nudges on COVID-19 vaccination uptake when vaccination uptake is already above 70 percent.

It may be that the effect is more pronounced in younger age groups. Löfgren and Nordblom (2020) argue that nudges should be more effective for choices that are considered unimportant by the individuals making them. Since 16–17-year-olds are unlikely to suffer from severe illness or death in case of a COVID-19 infection, whereas the risk is considerably higher for older individuals (Kolk et al., 2021), we should not expect the effect of the pre-booked vaccination appointments to be as large in the general population. Additionally, while previous studies consider lighter nudges, we study the impact of a nudge that changes the default alternative, something that has been shown to be impactful when considering choices in other domains (e.g., Madrian and Shea, 2001; Pichert and Katsikopoulos, 2008; Li et al., 2013).

In summary, pre-booked appointments seem to provide a simple and effective nudge that could be used more broadly to increase vaccine uptake in the future (e.g., for COVID-19 booster doses or vaccinations for other viruses).

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Declaration of competing interests

The authors declare that they have no competing financial interests or personal relationships that have influenced the work reported in this article.

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Appendix

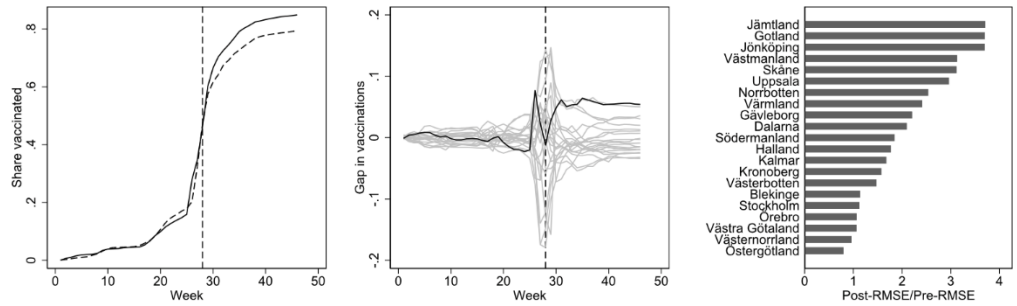

Figure A1. Effect (left), placebo (middle) and post-intervention effect size (right) plots for first-dose vaccinations among 18-29-year-olds. The left panel shows the share of first-dose vaccinations by week in Uppsala (black) and synthetic Uppsala (dashed). The middle panel shows effects estimated by assessing the vaccination share gaps between Uppsala and its synthetic counterpart (black) and equivalently defined placebo gaps in all 20 control regions (gray). The right panel shows the ratio between post-intervention root mean squared error (RMSE) to the pre-intervention RMSE from the synthetic control analysis in Uppsala and all other regions. The specification in this analysis differs from our main analysis in two ways: (i) since we can exploit variation in the pre-intervention outcomes to determine variable importance, we rely on the standard original variable importance optimization from Abadie and Gardeazabal (2003), and (ii) for the same reason, we also standardize the effect sizes in the right panel by the pre-intervention RMSE to account for the fact that the pre-intervention fit can vary across regions, as suggested by Abadie et al. (2010).

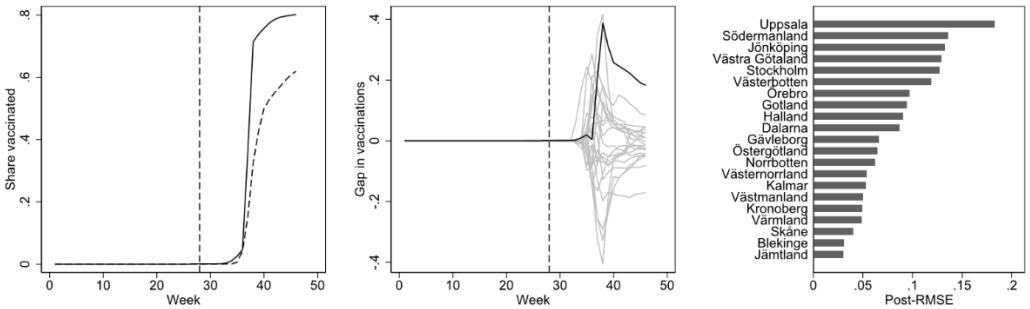

Figure A2. Effect (left), placebo (middle) and post-intervention effect size (right) plots for second-dose vaccinations among 16-17-year-olds. The left panel shows the share of second-dose vaccinations by week in Uppsala (black) and synthetic Uppsala (dashed). The middle panel shows effects estimated by assessing the vaccination share gaps between Uppsala and its synthetic counterpart (black) and equivalently

defined placebo gaps in all 20 control regions (gray). The right panel shows the post-intervention root mean squared error (RMSE) from the synthetic control analysis in Uppsala and all other regions.

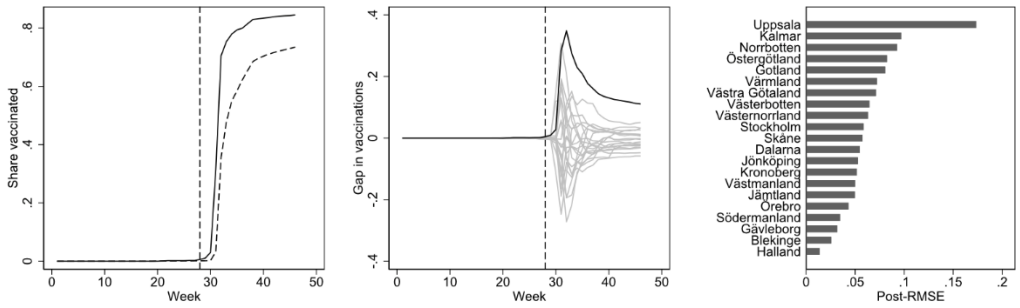

Figure A3. Effect (left), placebo (middle) and post-intervention effect size (right) plots, using the equal weights for the included covariates. The left panel shows the share of first-dose vaccination by week in Uppsala (black) and synthetic Uppsala (dashed) among 16-17-year-olds. The middle panel shows effects estimated by assessing the vaccination share gaps between Uppsala and its synthetic counterpart (black) and equivalently defined placebo gaps in all 20 control regions (gray). The right panel shows the post-intervention root mean squared error (RMSE) from the synthetic control analysis in Uppsala and all other regions.

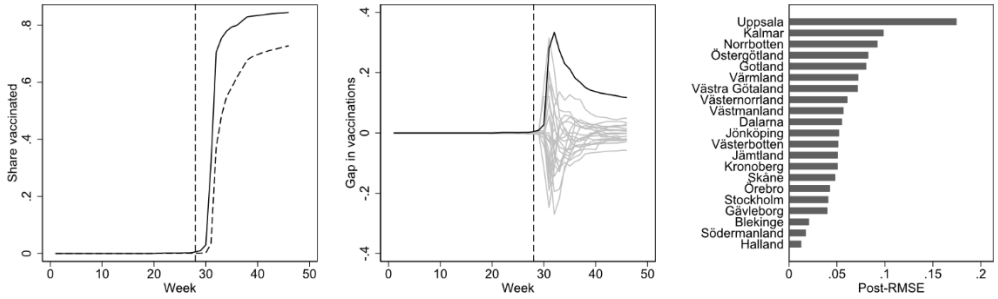

Figure A4. Effect (left), placebo (middle) and post-intervention effect size (right) plots, using the algorithm suggested by Abadie et al. (2010). The left panel shows the share of first-dose vaccination by week in Uppsala (black) and synthetic Uppsala (dashed) among 16-17-year-olds. The middle panel shows effects estimated by assessing the vaccination share gaps between Uppsala and its synthetic counterpart (black) and equivalently defined placebo gaps in all 20 control regions (gray). The right panel shows the post-intervention root mean squared error (RMSE) from the synthetic control analysis in Uppsala and all other regions.

Table A1. Control variable comparison between treated municipalities and their neighboring municipalities

	Treated municipality	Average of neighboring municipalities
	Enköping	Sala, Västerås, Upplands-Bro
Share foreign-born	0.161	0.225
Share high education	0.208	0.230
Share of Covid-19 deaths	0.00256	0.00272
	Håbo	Sigtuna, Uppsala
Share foreign-born	0.157	0.326
Share high education	0.193	0.218
Share of Covid-19 deaths	0.00107	0.00270
	Knivsta	Norrtälje, Sigtuna
Share foreign-born	0.144	0.245
Share high education	0.369	0.178
Share of Covid-19 deaths	0.00112	0.00263
	Uppsala	Norrtälje
Share foreign-born	0.221	0.135
Share high education	0.423	0.168
Share of Covid-19 deaths	0.00187	0.00254
	Östhammar	Norrtälje
Share foreign-born	0.096	0.135
Share high education	0.149	0.168
Share of Covid-19 deaths	0.00230	0.00254
	Tierp	Gävle
Share foreign-born	0.131	0.159
Share high education	0.151	0.216
Share of Covid-19 deaths	0.00107	0.00217
	Älvkarleby	Gävle
Share foreign-born	0.149	0.159
Share high education	0.155	0.216
Share of Covid-19 deaths	0.00630	0.00217
	Heby	Avesta, Gävle, Sala, Sandviken
Share foreign-born	0.126	0.165
Share high education	0.145	0.178
Share of Covid-19 deaths	0.00161	0.00227

Table A2. Determinants of share of vaccinated 18-29-year-olds in treated and neighboring municipalities

	(1)	(2)	(3)	(4)
Treatment	0.063**	0.020	0.047	0.013
	(0.023)	(0.012)	(0.038)	(0.028)
Neighbor indicators	No	No	Yes	Yes
Share foreign-born	No	-0.552***	No	-0.331
		(0.100)		(0.256)
Share high education	No	0.485***	No	0.312
		(0.074)		(0.180)
COVID-19 deaths	No	-2.883	No	-16.042
		(4.555)		(16.973)
Constant	0.745***	0.768***	0.769***	0.821***
	(0.016)	(0.024)	(0.057)	(0.078)
R^2	0.344	0.897	0.723	0.964

Notes: The dependent variable is the share of 18-29-year-olds vaccinated in week 49 in the 16 included municipalities. Ordinary least squares regressions controlling for Treatment (pre-booked appointments), Neighbor indicators (one dummy variable for each treated municipality, indicating its neighbors), as well as the control variables Share foreign-born, Share high education, and COVID-19 deaths. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.