INSTITUTO DE ECONOMÍA

MAGÍSTER en ECONOMÍA

2020

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Katia Everke R.

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PONTIFICIA UNIVERSIDAD CATOLICA DE CHILE
INSTITUTO DE ECONOMIA
MAGISTER EN ECONOMIA

TESIS DE GRADO
MAGISTER EN ECONOMIA

Everke, Reyes, Katia Alejandra

Julio, 2020

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Katia Alejandra Everke Reyes

Comisión de microeconomía

Emilio Depetris-Chauvin, Francisco Silva

Santiago, Julio de 2020

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Katia Everke†

Advisors Professors: Emilio Depetris-Chauvin, Francisco Silva

Abstract

In 2015, Chile changed the target population for the national influenza immunization campaign and added children under six years old. Using national school attendance data, I test whether childhood flu vaccination has a positive short-term effect on educational outcomes. The intention-to-treat estimates suggest that the influenza vaccine positively impacts school attendance, which is especially visible during the flu season and for children from lower-income families. Moreover, I study whether age-eligible children attend more or less to school on the vaccination date since educational establishments serve as vaccination sites. I find no consistent evidence of manipulation on demand for immunization.

*Thesis written as a Master student at Pontificia Universidad Católica. I would like to thank my advisor professors Emilio Depetris-Chauvin and Francisco Silva for their continuos guidance and valuable feedback. I am especially grateful to my family, Sebastián Figari, Felipe González, Sarah Everke, Claudia De Goyenche and Roberto Cases for their support, advice and encouragment. All errors and omissions are my own.

†kaeverke@uc.cl.

1 Introduction

One of the greatest world health achievements is vaccines; they are responsible for significant public health improvements (Acemoglu and Johnson, 2007). Moreover, previous literature has shown that childhood vaccination impacts positively long-term educational outcomes, like school attendance and cognitive skills (Alsan, 2017; Oskorouchi et al., 2020). However, less is known about seasonal vaccines' short-run educational effects on children. I contribute to this literature by providing suggestive evidence about flu shot's short-run positive effects on school attendance.

The primary channel through which influenza vaccines affect school attendance is by preventing flu illness (WHO, 2020); vaccinated children are more likely to attend school during the flu season than not vaccinated ones. A large body of literature documents the detriments of school absenteeism; in particular, Grossman and Kaestner (1997) suggest that children who absent many school days achieve poorer grades. Likewise, Gottfried (2010) argues that the number of days that a child counts present positively affects learning outcomes. Furthermore, absences are also of concern to parents because they miss work and free school meals for their children.[1]

To shed light on the seasonal flu shot's short-term effects on educational outcomes, I exploit the Chilean yearly national influenza campaign from 2015 until 2019. The campaign seeks to immunize children under six years old (MINSAL, 2015). To facilitate access to the vaccines and ensure high immunization coverage, the Ministry of Health (MINSAL, henceforth) sends nurses to the schools to provide flu shots. The school's vaccination date determines the eligibility classification; if a child meets the age requirement on that day is age-eligible (eligible, henceforth) to the flu shot, and otherwise, not-eligible.

This study's primary data source is national administrative data from the Ministry of Education (MINEDUC, henceforth), which provides daily individual-level school attendance information. I also gather the school's exact vaccination date for 25 communes of the Metropolitan Region. After merging these bases, I can distinguish eligible from not-eligible children. Furthermore, I can also tell apart eligible-treated from eligible-not-treated children. Both groups meet the vaccine age requirement, but the first attended school on the vaccination date; meanwhile, the second did not.

The main data limitation of my study is that I can not observe whether a child got the flu shot. To overcome this challenge, I exploit eligible classification and argue that it is highly probable that eligible-treated children got an influenza vaccine, given that it is mandatory (MINSAL, 2010). Thus, to explore the flu shot effects on school attendance, I implement a Differences-in-Differences approximation and compare eligible-treated children's school attendance to eligible-not-treated ones.

Note that regardless of the mandatory nature of the flu shot, parents can avoid getting their children vaccinated by not sending them to school on the vaccination day. Vaccine skepticism is one of the top 10 threats to global health (BBC, 2019). Furthermore, parents are particularly

[1] In Chile, exists a national school alimentation program ("Programa de alimentación escolar") that provides free meals to 60% most vulnerable children.

hesitant about the flu shot (Kempe et al., 2020), and MINSAL recognizes anti-vaccine groups as a threat to immunization coverage (MINSAL, 2017). The possibility of avoiding the vaccine raises concerns about self-selection bias; comparing eligible-treated children's school attendance to the eligible-not-treated ones could be misleading. To overcome this issue, I implement an Event Study to test whether eligible children are more or less likely to attend school than not-eligible ones on the vaccination day.

The estimates indicate that eligible children are not less or more likely to attend school on vaccination dates than not-eligible ones.[2] Thus, there is suggestive evidence against self-selection. There could be multiple mechanisms behind these results; a plausible one is the lack of a prominent anti-vaccine movement. Importantly, I can not rule out other explanations and, therefore, can not conclude which mechanism drives the results neither if there is manipulation on supply for immunization.[3]

Since there is suggestive evidence against self-selection, I run an individual-level Differences-in-Differences regression for each academic year available in the database. I focus on short-term effects because the flu shot protection lasts less than a year (MINSAL, 2017). To ensure that the influenza vaccine drives the results, I run another regression that limits the data according to epidemiological weeks.

The intention-to-treat estimates show that eligible-treated children attend between 0.6 and 0.9 percentage points more to the school than eligible-not-treated ones during the flu season. In day terms, the results translate into 1.2 and 1.8 more days; hence, the influenza vaccine positively impacts school attendance. Nevertheless, most of these results are smaller or even fade away, considering all school year data; the estimates range between 0.4 and 0.6 percentage points (0.81 and 1.2 in day terms).

A possible underlying mechanism behind these results might be spillover effects. Herd immunity could protect non-vaccinated children against flu disease (Kim, 2014). Therefore my results could represent a lower bound of the actual impact. To explore this effect, I estimate whether having more eligible-treated classmates impacts school attendance. I find no evidence of spillover effects.

Another concern is whether eligible-not-treated children get vaccinated later on; if so, the results may be a lower bound. To shed light on this concern, I exploit school types as a proxy of income. In Chile, public schools correlate to families with lower socioeconomic status (Elacqua, 2012; Valenzuela et al., 2014). I hypothesize that eligible children from private schools are more likely to get the flu shot later if they missed the vaccination date than those from public schools; the results are consistent with this hypothesis due to larger coefficients for public schools. Eligible-treated children from public schools attend between 1.3 and 1.5 percentage points more to school weekly than

[2]In this regard, I contribute to the recent literature on health mistrust and its relation to the demand for immunization. See, for instance, Martinez-Bravo et al. (2018) and Lowes and Montero (2018).

[3]Another explanation could be that some parents are interested in getting their children vaccinated, and others are anti-vaccines, so the sum shows no jump or drop on school attendance on the vaccination date. Nevertheless, I argue that the evidence against self-selection holds because these groups would represent a minority.

eligible-not-treated ones. The latter corresponds to 2.6 and 3 more days to the school, respectively. Besides, during the flu season, the coefficients are larger and translate into 2 and 3.8 more school days.

To the best of my knowledge, this is the first economic study that assesses the short-term impact of seasonal flu vaccine on educational outcomes. In contrast to previous literature, I suggest that the vaccine's positive effects are more visible in children from low socioeconomic status families. Lastly, I also contribute to health mistrust research by showing suggestive evidence of no prominent anti-flu shots movements.

The remainder of the paper is organized as follows. Section 2 describes the literature review. Section 3 provides background information on the national immunization campaign. Section 4 and 5 summarize the data and empirical strategy. Sections 6 and 7 present the results and the robustness checks. Finally, Section 8 concludes.

2 Literature Review

This study draws upon research at the intersection of flu disease, health mistrust, children's health, and educational outcomes. Regarding health mistrust, Das and Das (2003) show that hesitancy in health workers can negatively impact vaccination rates. In the same line, Alsan et al. (2019) find that black men, paired with a racially concordant doctor, are more likely to get a flu vaccine and take blood tests. There is also evidence that medical skepticism could be a historical legacy. In this regard, Lowes and Montero (2018) find that places where French colonial governments tried to prevent sleeping sickness through medications that had severe side effects, the vaccination rates, and health trust diminished. The authors demonstrate that negative historical experiences with medicine and health mistrust constraint demand.

Furthermore, there is a strand of papers, exploiting informational disclosures to study their impact on medical hesitancy. In this manner, Alsan and Wanamaker (2018) find that after the Tuskegee experiment disclosure, medical mistrust raised, and demand for health diminished.[4] Likewise, Martinez-Bravo et al. (2018) document that the reveal of CIA espionage activities, i.e., undercover vaccination campaigns to locate Osama Bin Laden, triggered anti-vaccine campaigns and, consequently, damaged vaccines' credibility in Pakistan. The authors find that districts with more ideological affinity to Taliban the immunization rates reduced. Information and who spreads it plays a vital role in deciding whether to vaccinate children (Alatas et al., 2020; Chang, 2018).

In contrast to previous literature, I focus on the demand for the seasonal flu vaccine and find consistent evidence with no manipulation on vaccination dates. Importantly, I argue that a possible explanation behind this result is the lack of a prominent anti-vaccine movement. Nevertheless, I

[4]The Tuskegee experiment was conducted to determine the effects of syphilis on not-treated patients; to do so, black male patients were deceived and denied medical treatment.

cannot rule out other explanations, either manipulation on supply for immunization.[5]

This study also relates to a large body of literature on the relationship between health and educational outcomes. See, for instance, Almond et al. (2009), Almond and Currie (2011), Aizer et al. (2018), Currie et al. (2009) and Field et al. (2009). Specifically, economic research upon influenza mainly focuses on the fetal originals hypothesis and uses pandemics as natural experiments. In this vein, Almond and Mazumder (2005) and Almond (2006) compare cohorts in-utero exposed to the 1919 influenza pandemic with those that were not, and show that the treated cohorts had lower educational attainment and income. Moreover, Nelson (2010), Lin and Liu (2014) and Neelsen and Stratmann (2012) study the same pandemic effects on education, but in Brazil, Taiwan, and Switzerland, and find similar results. Finally, Kelly (2011) exploits another pandemic, the 1957 Asian Flu in the UK, and provide evidence that the cohorts in-utero during the influenza are shorter and have worse test scores.

In particular, I differ from previous literature by exploiting a national vaccination campaign instead of a pandemic. Besides, the treatment group in my study is not in-utero cohorts but children that attend school. Therefore, I emphasize the importance of immunization during childhood by showing its positive effects on school attendance.

More recent studies address the effects of childhood vaccination on educational outcomes. In particular, Driessen et al. (2015) find that age-appropriate measles vaccination induces a 7.4 percentage point increase in male school enrollment. Likewise, Bloom et al. (2011) suggest that full childhood vaccination for measles, polio, DPT, and TB increases cognitive test scores in the Philippines. Oskorouchi et al. (2020) exploit the Chinese setting to assess childhood vaccination effects on late adulthood educational outcomes and find a positive relationship. Finally, Alsan (2017) studies the impact of a mass immunization campaign in Turkey on educational outcomes and finds an increase of 2% in educational attainment for age-eligible children. It is important to remark that her study's main focus is the gendered spillover effects of eligible children. She argues that reducing eligible children's mobility benefits their older sister by improving their educational outcomes.

Overall, previous literature documents the long-lasting effects of non-seasonal-vaccines on educational outcomes. I differ from these studies by focusing only on the seasonal influenza vaccine's short-run impact on school attendance. In contrast to other vaccines, the influenza one's protection lasts less than one year; therefore, I use daily data to explore its effects on school attendance.

3 Background

MINSAL conducts every year a national influenza vaccination campaign to immunize the target population against the flu disease, which usually starts in the middle of March and lasts approx-

[5] Health centers of each commune are in charge of providing vaccines; they organize the vaccination calendar. If the vaccination date is at the beginning of the campaign, more children are age-eligible (fewer children have turned six years old). Therefore, there could be a strategic behavior of the supplier.

imately two months. In 2015 the target population changed from including children between six and twenty-fourth months of age to adding children up to five years, eleven months, and 29 days old. Health centers are responsible for providing influenza vaccines, which are mandatory and free (MINSAL, 2010). To facilitate access to flu shots and ensure high coverage, schools serve as vaccination sites. Importantly, health centers arrange a date with schools to send health personnel to administrate the vaccine.

The vaccination date determines whether a child is age-eligible; children under six years of age on that day are eligible. Nevertheless, anecdotal evidence shows that some health centers are flexible and even vaccinate children over the age threshold, which means that some not-eligible children could be eligible. The next sections discuss why this concern is unlikely to be relevant in this context.

Moreover, health centers reserve vaccines for not attending eligible children (eligible-not-treated). Getting the reserve flu shot implies a trip to the health center and the chance of losing the eligibility status. For example, if an eligible child missed school on the vaccination date and goes to the health center after turning six years old, she is no longer eligible. Furthermore, it is important to point out that parents can buy flu vaccines. Hence, allegedly, there could be eligible-not-treated children that were vaccinated. Section 6 discusses this matter.

4 Data

This study's primary data source is national administrative data from MINEDUC, which contains individual-level information on the daily school attendance from 2011 until 2019. I use it from 2015 because the change in the national vaccination campaign took place that year. Since I am interested in individuals around the required vaccination age, I restrict the data on children from pre-kindergarten, kindergarten, and first grade.[6]

To obtain each school's exact vaccination date from 2015 until 2019, I requested it directly to all Metropolitan Region communes; 25 of 52 municipalities sent the requested data. Note that some of them have no records every year, Table B.3 of Appendix B provides details about this. It is worth emphasizing that some schools have more than one vaccination day, and in the presence of multiple ones, they are not always in a row.

After merging the two datasets, I can distinguish between treated and control individuals, classified in terms of age-eligibility to the vaccine. Importantly, both groups are not the same in each empirical strategy; sections 4.1 and 4.2 explain the differences in detail. The eligibility classification is straightforward for schools with a unique vaccination date. However, it is no longer trivial in a multiple-dates-setting because a child's eligibility status could change on the different vaccination

[6]In Chile, children of kindergarten are usually between five and six years old. Hence, there are by construction not that many not-eligible ones. Therefore, I keep children from first grade in the data, comparable in characteristics to children of kinder and pre-kindergarten.

dates.[7] Therefore, I separate the data into two samples; the first sample includes schools with only one vaccination date, and the second one considers the rest of them.

The schools with multiple vaccination dates raise concerns about the window-setting, and, therefore, about the treatment assignment. Appendix A explains window-setting importance in detail. Moreover, they could give a hint of vaccine coverage failure on the first vaccination day. To shed light on this matter, I investigate whether multiple vaccination schools have a different attendance on the first day than the rest of them within the same commune. Table A.2 shows that school attendance is not statistically different on the first vaccination day. In other words, children from multiple vaccination date schools seem to be similar in manipulation terms to those from the rest of the schools. Nevertheless, I focus on the first sample to avoid misleading eligibility classifications and results due to the window-setting.

A dataset limitation is that it reports the birth date in months. Hence, I cannot identify whether a child born in the vaccination date month is eligible.[8] To avoid misleading categorizations, I drop these children from the data. Since most vaccination dates are in April, there are fewer children born on that month.

4.1 Attendance on Vaccination Days

To explore whether there are self-selection and manipulation on vaccination days, I compare eligible (treated) and not-eligible (control) children's school attendance on that day. Table 1 presents summary statistics.[9] According to the stats, the fraction of eligible children is 55%. The only characteristic that is not significantly different between both groups is the percentage of children in public schools. However, the rest variables have similar means except for the school grade. The latter is not surprising because, by construction age-eligible children are under six years old, and usually in first grade, children turn seven years old. Even though most variables are statistically different across groups, this is not a problem for the empirical approach because individual fixed effects capture time-invariant factors (Angrist and Pischke, 2008).

To understand better what happens around the vaccination day, I compare average daily attendance between eligible and not-eligible children around vaccination days from 2015 until 2019.[10] Figure B.2 shows that there is no visible jump or drop in attendance on the vaccination date. Moreover, Figure 1 also suggests no visual manipulation on $t = 0$ and indicates that both groups of interest follow similar trends before the vaccination date.

A possible heterogeneity behind these results could rely on income level differences; parents with

[7] For instance, if the vaccination dates are 4th and 19th of May and a child turns six on the 16th of May, is age-eligible?

[8] Suppose a child's birth date was in March 2009, and the 2015 vaccination date on his school is 25th March. This child could be age-eligible if he were born before the 25th, but not after the 25th.

[9] Table A.1 presents summary statistics for multiple vaccination schools.

[10] I calculate $\triangle\bar{A} = \bar{A}_{Et} - \bar{A}_{NEt}$, where $\bar{A}_{Et}$ corresponds to average daily attendance from eligible children and $\bar{A}_{NEt}$ from not-eligible ones.

worse socioeconomic status could value the flu shot more because it is free, and schools provide it. Figure B.3 presents visual evidence against this hypothesis because, again, there is no jump or drop on vaccination dates attendance for public schools.

4.2 Annual Attendance

To assess the flu shot impact on school attendance during each academic year, I aggregate the dataset at week level to allow more data variation.[11] I focus only on eligible children and compare eligible-treated to eligible-not-treated ones.[12] Table 2 provides yearly descriptive statistics for both groups. On average, the proportion of eligible-treated in the sample is 90%. The excess representation of eligible-treated is not surprising because it corresponds to the mean attendance on a typical school day. Even though the groups differ in characteristics, the differences will be captured by fixed-effects presented on the empirical strategy (Angrist and Pischke, 2008).

Furthermore, Figure 2 shows the average weekly attendance of eligible-treated and eligible-not-treated children from 2015 until 2019 before and after the vaccination date (dotted red lines). Overall, both group's school attendance seems to have parallel trends. After the vaccination date, the eligible-treated school attendance shows no prominent jump compared to the eligible-not-treated children.

5 Empirical Strategy

5.1 Manipulation on Vaccination Dates

To investigate the presence of manipulation and to explore self-selection on vaccination dates, I exploit daily attendance data and use an Event Study approach. To do so, I estimate the following equation:

$$attend_{i,g,m,d,y} = \alpha_i + \sum_{k=-10}^{10} \beta_k event_{i,d,m,y,t-k} treat_i + \gamma_d + \theta_m + \delta_y + \sigma_g + \epsilon_{i,g,d,m,y} \tag{1}$$

where $attend_{i,g,m,d,y}$ is a dummy variable that signals if the child i enrolled in grade g attended school on the day d of the month m on the year y. $treat_i$ is a dummy variable that takes the value of 1 if the individual is eligible for the influenza shot. $event_{d,m,y,t-k}$ is a dummy variable that indicates that an event occurred in the day t. α_i are individual fixed effects that control for time-invariant factors. σ_g are school grade fixed effects. γ_d, θ_m, and δ_y are day, month, and year fixed effects that control for time trends and enable the comparison between different vaccination days. $\epsilon_{i,g,d,m,y}$ is the error term clustered at school level, which allows within school correlation.

[11] If I use daily data, I would have to add more fixed effect, which hinders the data variation.

[12] Note that not every individual in the database has attendance records every month, thus I restrict the weekly data to individuals that have records of 9 or 10 months a year.

The vaccination date differs by school and year, so I denote these dates by $t = 0$ and index all days relative to that day. I observe the children's school attendance ten workdays before and after the vaccination day. Thus, the event time runs from 10 to -10. It is worth remarking that the vaccination date artificial centering on $t = 0$ means that the parallel trends do not follow the chronological order like a traditional Differences-in-Differences. The common trends assumption implies that without the treatment, the control and treatment group would have the same trend over time (the mean unobserved difference would be 0) (Angrist and Pischke, 2008).

The coefficient of interest is β because it captures the individual's response to the vaccination date. Since I omit the event time $t = -1$ when I run the equation 1, the coefficient measures the vaccination date response relative to the day before the treatment. A positive $\hat{\beta}$ suggests that eligible children are more likely to attend school on vaccination days than not-eligible ones. The contrary occurs with a negative coefficient. In case of a null effect, it shows no manipulation, and, consequently, no self-selection bias evidence.

5.2 Impact on Children's School Attendance

To identify the impact of -highly probable- getting vaccinated against influenza on school attendance, I use a Differences-in-Differences approach. I estimate the following intention to treat (ITT) equation for each year available in the data set:

$$Y_{i,t} = \alpha_i + \beta dpos_t \times dtreat_i + \gamma_t + \varepsilon_{i,t} \tag{2}$$

where $Y_{i,t}$ denote weekly school attendance of individual i. *dpos* takes the value of one for weeks after the vaccination week, while *dtreat* is also a dummy variable that indicates eligible-treated children. α_i are individual fixed effects that capture time-invariant factors such as school grade.[13] γ_t are week of the year fixed effects that control time trends. Finally, $\epsilon_{i,t}$ is the error term clustered at the school level, allowing within-school error correlation.

The coefficient of interest, β, shows how eligible-treated children's weekly school attendance changes after the immunization campaign. Since vaccination protects against influenza viruses, eligible-treated children likely attend more to the school than eligible-not-treated ones. Hence, I expect $\hat{\beta}$ to be positive.

The vaccination effect should be especially visible during the flu season because most cases occur during that time of the year (MINSAL, 2017). To reassure that the results are due to the flu shot, I limit the data according to epidemiological weeks. Specifically, I consider data until the epidemiological curve goes down, and there are less than 50 cases per week.[14] This estimation revels

[13]Importantly, in this approach, the school grade is captured by the individual fixed effects because it is time-invariant. Nevertheless, in the Event Study approximation, the school grade is time-variant; I run a regression that includes different years and, therefore, there are individuals across time in different grades.

[14]The epidemiological curve changes every year; thus, I consider data until different weeks each year of the database. Specifically, from 2015 until 2019, I limit the data until the 38th, 39th, 30th, 37th, and 43rd week. Figure B.1 shows

whether the vaccination impact on the flu season drives the results of all school week data, so it is heartening to find that $\hat{\beta}$ is positive. However, note that there could be a null impact considering all school weeks but a positive one in the censored-data.

Spillover Effects

A possible mechanism underlying equation 2 estimates could be spillover effects; herd immunity could indirectly protect a not-vaccinated child by being surrounded by eligible-treated classmates (Kim, 2014). Therefore, the estimates could represent a lower bound of the actual flu shot effects. To assess for spillover effects, I implement the following specification:

$$Y_{i,t} = \alpha_i + \beta dpos_t \times dspill_i + \gamma_t + \varepsilon_{i,t} \tag{3}$$

where $dspill_i$ is the percentage of eligible-treated children in the same school class as child i. The rest of the variables are the same as equation 2. The coefficient β shows, whether being surrounded by eligible-treated classmates, impact weekly school attendance. If $\hat{\beta}$ is positive, it suggests a positive spillover effect. In other words, herd immunity, having more eligible-treated classmates, impacts positively on weekly school attendance by reducing the probability of getting the flu. The contrary occurs if $\hat{\beta}$ is negative.

6 Results

6.1 Main Results

Vaccination Day

Figure 3 presents the estimates of equation 1. There is no visual evidence of manipulation on demand for immunization on vaccination dates; the coefficient in $t = 0$ is not statically different from zero. In other words, eligible children seem to be not more or less likely to attend school on vaccination days than not-eligible ones. Crucially, the results suggest no self-selection on that day. There could be multiple explanations behind these results, a plausible one is the lack of a prominent anti-vaccine movement, but I can not rule out other mechanisms. Yet, there could be manipulation on supply for immunization; nevertheless, this possibility does not affect my results because I am interested in the demand response.

Importantly, section 3 discusses that anecdotal evidence shows that not-eligible children could be eligible ones. Hence, the estimates could be a lower bound of the actual effect. To explore this, I include children above the age threshold, i.e., those who turn six years old from January until the month before the vaccination date. As expected, figure B.4 indicates a larger coefficient but still non-significantly different from zero.

the epidemiological weeks for each year.

Furthermore, figure 3 shows that after the vaccination day, the eligible children attend significantly less to the school than not-eligible ones. A straightforward explanation behind this drop in attendance could be flu shot's side effects, which can last one or two days after the vaccination (MINSAL, 2017). Figure 4 gives insight into this matter; it suggests that eligible-treated children attend more to school one week after the vaccination than eligible-not-treated ones. Thus, it is unlikely that flu shot's side effects explain the attendance drop because eligible-treated children should attend less than eligible-not-treated ones just after the vaccination day. There could be multiple underlying mechanisms behind this result, e.g., not-eligible children attend more; however, I can not conclude which one is driving the results.

Impact on School Attendance

Table 3 presents the main Differences-in-Differences estimates. As explained in Section 5, I run the equation 2 for each year in the database, the columns tell them apart. Only 2016 and 2017 show statistically significant estimates at the 5% level; eligible-treated children attend 0.6 and 0.4 percentage points (pp., henceforth) more to school weekly than eligible-not-treated ones. In terms of magnitude, each of these estimates represents changes of 0.68% and 0.45% of the sample mean, respectively.[15] Hence, in day terms, eligible-treated children attend 1.2 and 0.8 more days than the comparison group (0.25 and 0.16 more weeks).[16]

To reassure that the flu shot drives these results, I limit the data according to epidemiological weeks. Table 3 also reports the estimates limiting the data; almost all coefficients are positive and statistically significant. The significant estimates range between 0.6 and 0.9 pp. across the years, which translates in an increase of 0.7% and 1.02% over the sample mean. In day terms, eligible-treated children attend between 1.2 and 1.8 more days to school (0.24 and 0.37 more weeks). These results are consistent with the hypothesis that the vaccination effects should be more visible during the flu season.

Why the flu shots effects are smaller or even fade away, considering all school year data? Multiple mechanisms could be behind these results, e.g., eligible-not-treated children's parents prevent school absences after the influenza season since their children already missed more school days than eligible-treated ones. I can not determine which underlying mechanism is responsible for reducing or disappearing the influenza vaccine impact.

Spillover Effects

A possible underlying mechanism behind my results might be spillover effects. Table 4 displays the estimates of equation 3; neither considering all data nor limiting it according to epidemiological

[15] Mean attendance from 2015 until 2019 is 0.88%, 0.883%, 0.887%, 0.889% and 0.862%, respectively.
[16] This calculation considers 180 school days and 36 school weeks in an academic year.

weeks are significant coefficients. Thus, the results suggest no spillover effects, and consequently, no herd immunity.

A large body of epidemiological literature suggests that infectious disease contagion is non-linear (Kermack and McKendrick, 1927,9,9). Furthermore, herd immunity works above a vaccination threshold (Hethcote, 2000); according to Plans-Rubió (2012), the required percentage to establish influenza herd immunity ranges between 75% to 90%. To test whether non-linearity is driving the null results, and to explore herd immunity thresholds, I restrict the percentage of eligible-treated children in a classroom. To assess the robustness of this, I study coverage above 50%, 65%, 75%, 85%, and 90%.

Figure 5 indicates that even by restricting *dspill*, there are no spillover effects. Overall, the coefficients tend to get more positive with a higher percentage, but none is statically significant, and most of them are negative. Thus, I cannot conclude anything regarding the proposition of Plans-Rubió (2012). Since there is no consistent evidence of spillover effects, the estimates of Table 3 seem not to be a lower bound of the actual impact.

6.2 Heterogeneous Effects by School

The main limitation of this study is that I can not observe who gets vaccinated. To overcome this challenge, I argue that it is highly probable that eligible-treated children get the flu shot, given that it is mandatory (MINSAL, 2010). However, eligible-not-treated children could be got vaccinated later because health centers reserve flu shots for not attended children, and parents can buy them particularly. Thus, the estimates of equation 2 could be a lower bound of the actual effect. To shed light on this matter, I exploit school types as a proxy of income; in Chile, public schools are correlated to lower-income families (Elacqua, 2012; Valenzuela et al., 2014). I hypothesize that eligible-not-treated children from private schools are more likely to get vaccinated later than the ones from public schools; hence the estimates should be smaller for the first group.

To explore this heterogeneous effect, I run specification 2 for public and private schools separately. Table 5 shows consistent evidence with the hypothesis; eligible-treated children from public educational establishments attend more to school weekly than the ones from private establishments. Crucially, the coefficients for public schools are larger than the ones from Table 3, and for private establishments are smaller. Specifically, eligible-treated children from public schools attend between 1.3 and 1.5 pp. more to school weekly than eligible-not-treated ones. These effects represent a 1.5% and 1.7% increase over the corresponding sample mean. In day terms, it translates to 2.6 and 3 days. In comparison, only in 2018, there is a significant estimate for private schools, 0.4 pp. (0.8 more days).

Moreover, table 5 also shows the estimates of equation 2, limiting the data according to epidemiological weeks. The coefficients are larger and more significant during the flu season, which reassures that the influenza vaccine drives all year data results. Eligible-treated children from public schools attend between 1 and 1.8 pp. more to school during the influenza season than the comparison group (2 and 3.8 more days). In contrast, the impact on children from private establishments ranges between 0.5 and 0.8 pp (1 and 1.6 more days).

7 Robustness

Parallel Trends

The central identifying assumption behind both empirical strategies is common trends; in the absence of the influenza vaccine, the treated and control individuals' school attendance would have evolved similarly. Yet, this assumption is not directly testable.

Note that for the Event Study approach, the lagged coefficients of equation 1 provide insight into the parallel trends' assumption. Figure 3 shows that the point estimates fluctuate around zero until the vaccination date. Therefore, there is visual evidence of common trends.

To study parallel trends on the Differences-in-Differences approximation, I estimate the following equation each year in the spirit of Granger (1969):

$$attend_{i,w} = \alpha_i + \sum_{l=-5}^{14} \beta_l event_{i,w,t-l} treat_i + \gamma_w + \epsilon_{i,w} \tag{4}$$

where $attend_{i,w}$ denotes weekly school attendance of individual i. $event_{i,w,t-l}$ is a dummy variable that indicates that an event occurred in the week, $treat_i$ is a dummy variable that equals one if the individual is eligible for the influenza shot and attended school on the vaccination date (eligible-treated). α_i and γ_w are individual, week of the year fixed effects. $\epsilon_{i,w}$ is the error term clustered at school level. β_l corresponds to the lagged coefficients.

Since I am interested in the pre-treatment effects, I focus on the lags (β_{-5}, $\beta_{-4}...\beta_{-2}$). When I run the regression, I omit $t = -1$ purposely, so the point estimates are measured relative to that day. If the lagged coefficients are not statistically different from zero, then the parallel trends assumption is supported. Figure 4 shows the results. Overall, the lagged point estimates fluctuate around zero; hence, the central identifying assumption is supported.

8 Conclusion

This research suggests that flu shot positively impacts school attendance in the short-term; the effects seem to be even larger for children from low-income families. Additionally, I find no consistent evidence of spillover effects, i.e., herd immunity, even by studying different coverage thresholds. Moreover, I provide suggestive evidence that eligible children are not more or less likely to attend school on vaccination days.

To the best of my knowledge, this is the first economic study that assesses flu shot short-run effects on school attendance. I differ from previous literature by emphasizing the importance of immunization against influenza during childhood, especially for children from low socioeconomic status families. I also contribute to the growing research on health mistrust by providing suggestive evidence against manipulation on demand for immunization on vaccination days.

Nevertheless, my study has some limitations; first, I can not observe who gets the flu shot. To overcome this challenge, I argue that age-eligible children who attended school on the vaccination dates highly probable got the flu shot because it is mandatory (MINSAL, 2010). Second, there are external validity concerns; I study communes only from the Metropolitan Region, which differs from other Chilean Regions. Furthermore, using school type as proxy for income is hardly extrapolate to other countries. Nevertheless, external validity concerns do not threaten causal identification but signal the impossibility to extrapolate the results to different contexts. Third, I can not assess which underlying mechanisms are responsible for reducing or disappearing the influenza vaccine impact after the flu season.

Finally, my findings have implications for public vaccine policies because they suggest that influenza immunization campaign investments can potentially improve school attendance, especially in more socioeconomic vulnerable children. To miss a school day for those children could be highly detrimental not only because of negative impacts on learning outcomes (Gottfried, 2010; Grossman and Kaestner, 1997) but also due to free school meals lose and their parent's cost of probably missing work.[17] Future research should dig deeper into the potential long-run effects of seasonal vaccines, underlying mechanisms, and manipulation on immunization supply.

[17]As explained before, Chile has a national school alimentation program that provides free meals to 60% most vulnerable children.

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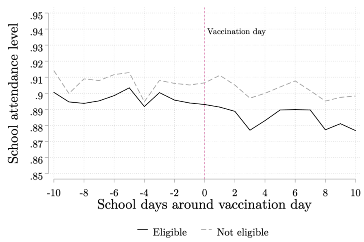
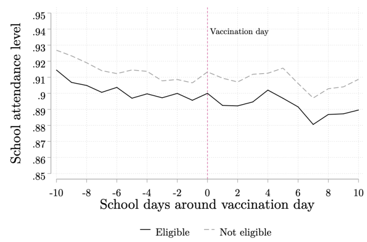
Figures and Tables

Figure 1: Average daily attendance of eligible and not-eligible children from 2015 until 2019



(a) 2015



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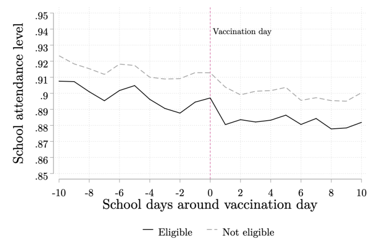

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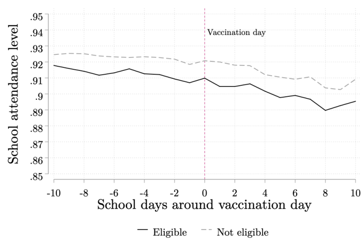

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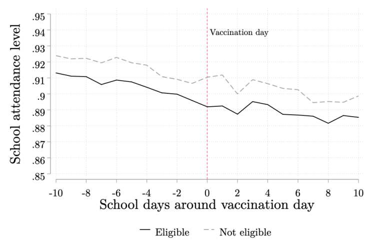
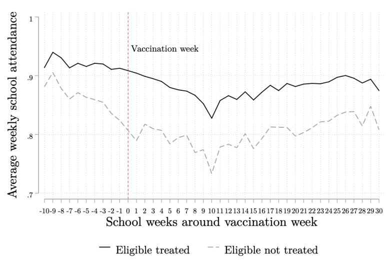
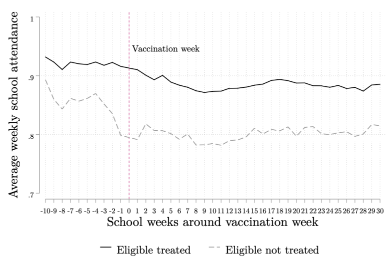


(e) 2019

Figure 2: Average weekly attendance of eligible-treated and eligible-not-treated children



(a) 2015



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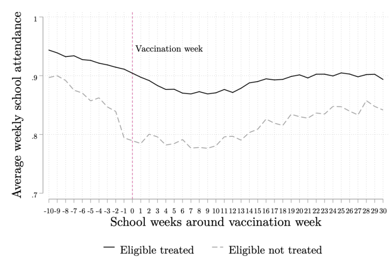

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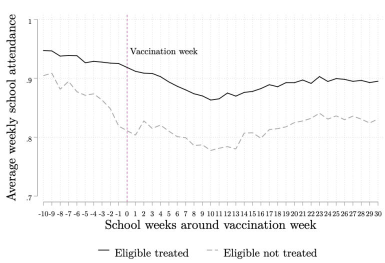

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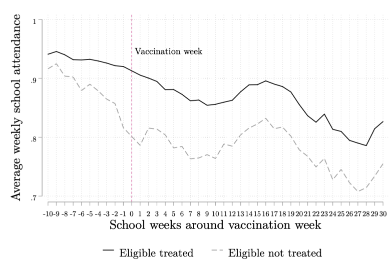
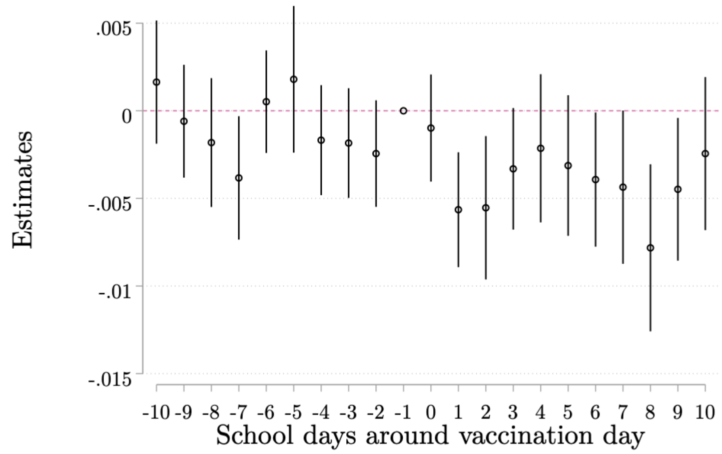


(e) 2019

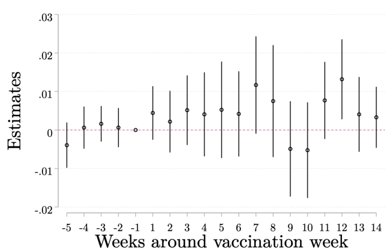
Notes: I omit the weekly school attendance on $t = 0$, because it shows a mechanical jump.

Figure 3: Event Study estimates



Notes: Estimates of equation 1. The coefficients measure the impact of the vaccination date relative to $t = -1$.

Figure 4: Differences-in-Differences estimates, parallel trends



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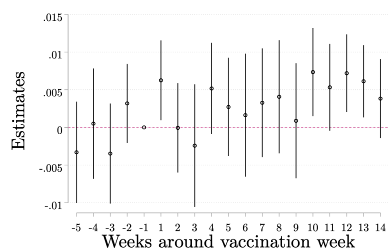

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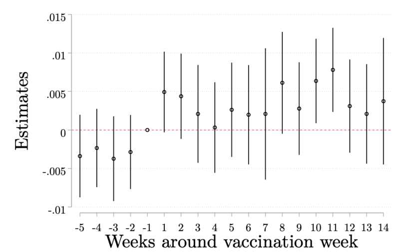

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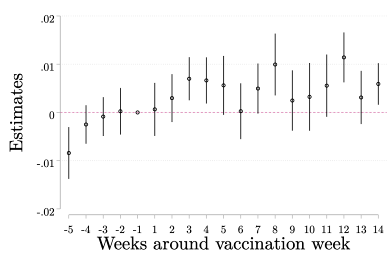

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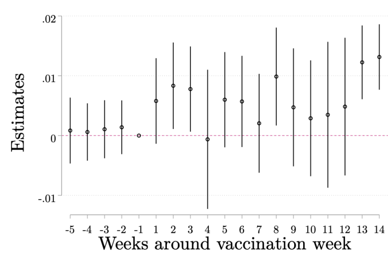
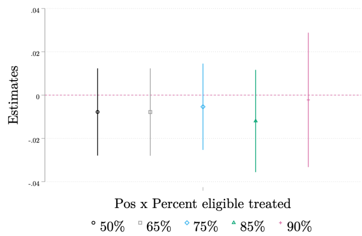


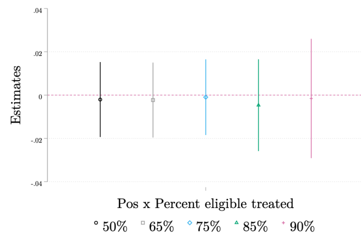
(e) 2019

Notes: Estimates of equation 4 for each year. The coefficients measure the impact of the vaccination date relative to $t = -1$.

Figure 5: Spillover effects considering different percentage of eligible-treated children in a classroom



(a) 2015



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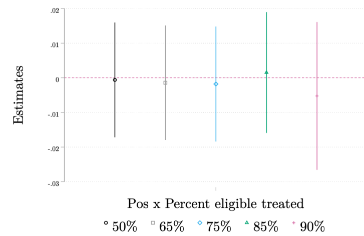
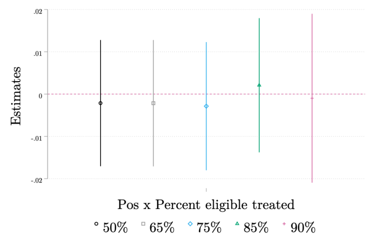
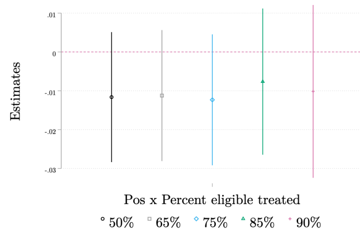


(c) 2017



(d) 2018



(e) 2019

Notes: Estimates of equation 3 for each year and considering different percentage of eligible-treated thresholds.

Table 1: Summary statistics for eligible and not-eligible children

	Eligible		Not eligible		
	Mean	SD	Mean	SD	Diff
ONE VACCINATION DATE SCHOOLS:	(1)	(2)	(3)	(4)	(5)
Age (under six)	1	0	0	0	-1***
Gender (men)	0.48	0.5	0.524	0.5	0.044***
Rural school	0.019	0.137	0.017	0.128	-0.002***
Grade (first grade)	0.032	0.176	0.95	0.216	0.918***
Grade (kindergarten)	0.566	0.496	0.049	0.216	-0.517***
Grade (pre kindergarten)	0.402	0.49	0	0.02	-0.402***
Mean attendance before vaccination date	0.904	0.153	0.916	0.138	0.012***
Mean annual attendance	0.884	0.105	0.9	0.093	0.016***
Public school	0.276	0.447	0.274	0.446	-0.002
Number of individuals	126,938		104,639		

Notes: *Gender*, *rural school*, *grade*, *attended first vaccination date* and *public school* are binary variables. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Yearly summary statistics for eligible-treated and eligible-not-treated children

	Eligible treated		Eligible not treated		
	Mean	SD	Mean	SD	Diff
2015:	(1)	(2)	(3)	(4)	(5)
Gender (men)	0.48	0.5	0.472	0.5	-0.008
Rural school	0.009	0.0934	0.01	0.1	0.001
Grade (first grade)	0.002	0.176	0.027	0.163	-0.005
Grade (kindergarten)	0.595	0.491	0.576	0.494	-0.019
Grade (pre-kindergarten)	0.373	0.484	0.396	0.489	0.023**
Mean weekly attendance	0.889	0.089	0.805	0.117	-0.084***
Mean attendance before vaccination week	0.919	0.099	0.857	0.137	-0.062***
Public school	0.26	0.439	0.264	0.441	0.004
Number of individuals	12,804		1,461		
2016:	(1)	(2)	(3)	(4)	(5)
Gender (men)	0.48	0.5	0.485	0.5	0.005
Rural school	0.02	0.141	0.022	0.148	0.002
Grade (first grade)	0.04	0.197	0.031	0.174	-0.009**
Grade (kindergarten)	0.578	0.494	0.551	0.498	-0.026**
Grade (pre-kindergarten)	0.382	0.486	0.417	0.493	0.035***
Mean weekly attendance	0.892	0.094	0.8	0.138	-0.091***
Mean attendance before vaccination week	0.921	0.109	0.85	0.154	-0.071***
Public school	0.262	0.44	0.291	0.454	0.029***
Number of individuals	18,836		1,957		
2017:	(1)	(2)	(3)	(4)	(5)
Gender (men)	0.473	0.5	0.47	0.5	-0.003
Rural school	0.019	0.136	0.022	0.147	0.003
Grade (first grade)	0.034	0.181	0.024	0.152	-0.01***
Grade (kindergarten)	0.577	0.494	0.558	0.497	-0.019**
Grade (pre-kindergarten)	0.389	0.488	0.418	0.493	0.029***
Mean weekly attendance	0.895	0.09	0.809	0.119	-0.086***
Mean attendance before vaccination week	0.927	0.1	0.858	0.139	-0.069***
Public school	0.251	0.434	0.252	0.434	0.001
Number of individuals	21,569		2,338		
2018:	(1)	(2)	(3)	(4)	(5)
Gender (men)	0.481	0.5	0.489	0.5	0.007
Rural school	0.015	0.121	0.02	0.139	0.005**
Grade (first grade)	0.034	0.18	0.028	0.164	-0.006
Grade (kindergarten)	0.567	0.5	0.563	0.496	-0.004
Grade (pre-kindergarten)	0.4	0.49	0.41	0.492	0.01
Mean weekly attendance	0.896	0.088	0.811	0.125	-0.085***
Mean attendance before vaccination week	0.931	0.101	0.86	0.149	-0.071***
Public school	0.259	0.438	0.301	0.459	0.042***
Number of individuals	25,566		2,384		
2019:	(1)	(2)	(3)	(4)	(5)
Gender (men)	0.48	0.501	0.49	0.5	0.011
Rural school	0.015	0.121	0.042	0.201	0.027***
Grade (first grade)	0.023	0.15	0.016	0.124	-0.008***
Grade (kindergarten)	0.561	0.496	0.547	0.498	-0.014
Grade (pre-kindergarten)	0.415	0.493	0.437	0.496	0.022
Mean weekly attendance	0.87	0.094	0.785	0.117	-0.085***
Mean attendance before vaccination week	0.931	0.1	0.871	0.132	-0.06***
Public school	0.255	0.436	0.262	0.44	0.007
Number of individuals	26,470		3,004		

Notes: *Gender*, *rural school*, *grade* and *public school* are binary variables. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Differences-in-Differences weekly estimates

	Weekly School Attendance				
Year	2015	2016	2017	2018	2019
	(1)	(2)	(3)	(4)	(5)
All year:					
Pos × Eligible treated	0.004	0.006**	0.004**	0.002	0.005
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Observations	525641	799578	880788	1022540	1043125
R^2	0.1990	0.2130	0.2061	0.2064	0.2362
Epidemiological weeks:					
Pos × Eligible treated	0.009***	0.006**	0.008***	0.003	0.006**
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Observations	365358	571812	429205	697023	869309
R^2	0.2111	0.2119	0.2282	0.2173	0.2351

Notes: Differences-in-Differences estimates of the impact of being eligible treated on weekly school attendance. In each regression individual and week fixed effects are included. The first panel uses all academic school attendance data, and the second one focus on epidemiological weeks. Cluster at school level. Standard errors in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Spillovers effects

	Weakly School Attendance				
Year	2015	2016	2017	2018	2019
	(1)	(2)	(3)	(4)	(5)
All year:					
Pos × Percent eligible treated	-0.010	-0.000	-0.004	-0.007	-0.008
	(0.010)	(0.009)	(0.007)	(0.007)	(0.009)
Observations	46519	68292	77819	76331	93509
R^2	0.2066	0.2531	0.2247	0.2411	0.2355
Epidemiological weeks:					
Pos × Percent eligible treated	-0.005	-0.003	-0.001	-0.008	-0.010
	(0.010)	(0.008)	(0.008)	(0.007)	(0.008)
Observations	32301	48768	37887	52041	78195
R^2	0.2192	0.2470	0.2471	0.2475	0.2366

Notes: Differences-in-Differences estimates of the impact of having eligible-treated classmates on weekly school attendance. In each regression individual and week fixed effects are included. The first panel uses all academic school attendance data, and the second one focus on epidemiological weeks. Cluster at school level. Standard errors in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Differences-in-Differences weekly results by school type

	Weekly School Attendance									
	Public Schools					Private Schools				
Year	2015	2016	2017	2018	2019	2015	2016	2017	2018	2019
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
All year:										
Pos × Eligible treated	0.013**	0.015***	0.006	0.002	0.014**	0.001	0.002	0.004	0.004*	0.005
	(0.005)	(0.005)	(0.004)	(0.004)	(0.006)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)
Observations	128470	212339	220969	259577	245643	397171	587239	659819	733450	755321
R^2	0.2424	0.2521	0.2384	0.2347	0.2822	0.1896	0.1948	0.1932	0.1928	0.2261
Epidemiological weeks:										
Pos × Eligible treated	0.018***	0.013***	0.010**	0.002	0.015***	0.005*	0.003	0.008***	0.006**	0.005
	(0.007)	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
Observations	86886	151782	107360	177755	203917	278472	420030	321845	499302	633064
R^2	0.2602	0.2511	0.2594	0.2459	0.2771	0.2009	0.1931	0.2158	0.2038	0.2229

Notes: Differences-in-Differences estimates of the impact of being eligible treated on weekly school attendance by school type. In each regression individual and week fixed effects are included. Cluster at school level. The first panel uses all academic school attendance data, and the second one focus on epidemiological weeks. Standard errors in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendices

A Multiple Vaccination Date Setting

Table A.1: Multiple vaccination date schools summary statistics for eligible and not-eligible children

	Eligible		Not eligible		
	Mean	SD	Mean	SD	Diff
SAMPLE 2: multiple vaccination dates					
Gender (men)	0.481	0.5	0.519	0.5	0.037***
Rural school	0.02	0.14	0.023	0.151	0.003
Age (under six)	1	0	0	0	-1***
Grade (first grade)	0.023	0.153	0.944	0.231	0.003***
Grade (kindergarten)	0.557	0.497	0.056	0.23	-0.501***
Grade (pre-kindergarten)	0.419	0.493	0	0.021	-0.419***
Mean attendance before vaccination date	0.902	0.153	0.926	0.131	0.024***
Mean annual attendance	0.897	0.123	0.916	0.109	0.019***
Public school	0.107	0.309	0.114	0.319	0.008**
Number of individuals	12,476		8,726		

Notes: *Gender*, *rural school*, *grade*, *attended first vaccination date* and *public school* are binary variables. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: OLS estimates, multiple vaccination date schools

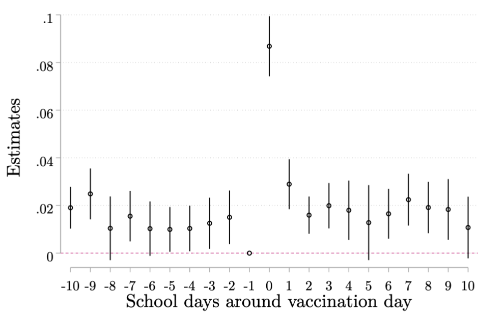
	School Attendance on the first vaccination date				
Year	2015	2016	2017	2018	2019
	(1)	(2)	(3)	(4)	(5)
Multiple vaccination dates	-0.033	-0.003	0.001	0.000	0.019
	(0.035)	(0.021)	(0.015)	(0.012)	(0.017)
Observations	253	322	402	464	480
R^2	0.1024	0.1341	0.0671	0.0437	0.0649

Notes: Regression at school level. *Multiple vaccination date* takes the value of 1 for schools with more than one vaccination date. In each regression commune fixed effects are included. Standard errors in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

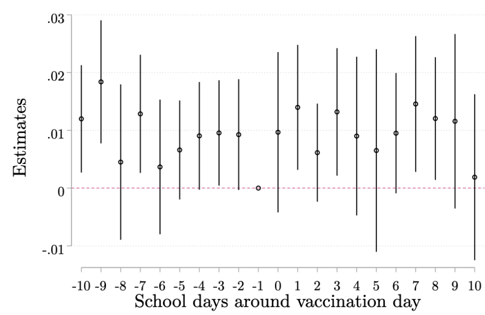
A.1 Window-setting

The vaccination dates determine the flu shot eligibility classification; a child under six years old on that day is eligible. The multiple vaccination date setting presents a challenge because, in this set-up, window-setting requires assumptions. The latter could lead to different results and, therefore, to misleading ones. For example, figure A.1a denotes $t = 0$ as the first vaccination date that eligible children attended school, for the rest of them (not-eligible and eligible ones that did not attend school on any vaccination date) $t = 0$ always signals the first vaccination day. Conversely, figure A.1b signals the first vaccination date as $t = 0$ for everyone. Both graphs suggest very different results; the first one shows that eligible children attend more likely to school on the vaccination day; on the contrary, the second one indicates no manipulation.

Figure A.1: Event study estimates for multiple vaccination date setting



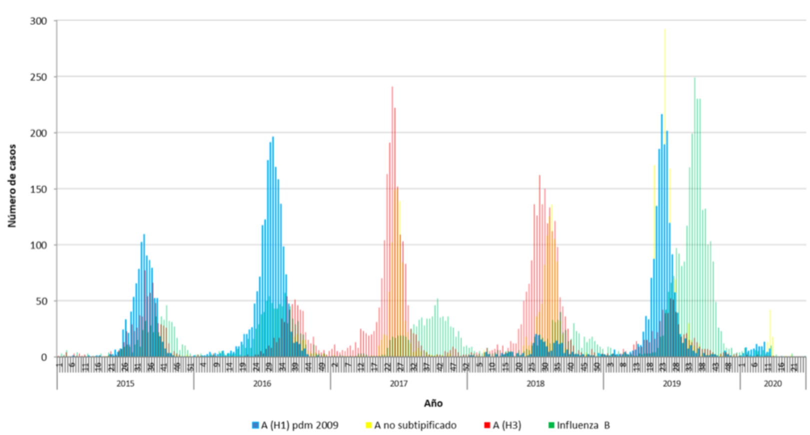
(a) 2015



(b) 2016

B Figures and Tables

Figure B.1: Epidemiological weeks by year



Source: "Sección Virus Respiratorios y Exantemáticos. Departamento Laboratorio Biomédico. Instituto de Salud Pública de Chile" 2020

Figure B.2: Difference in daily attendance between eligible and not-eligible children

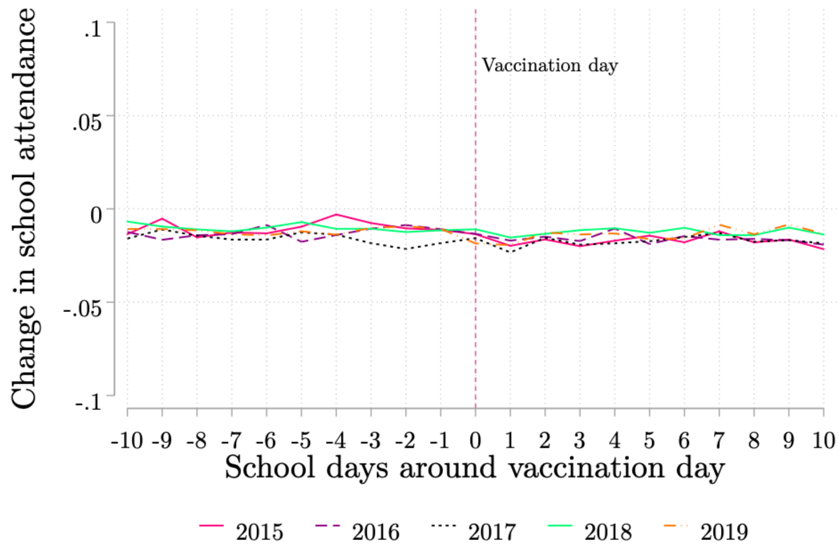


Figure B.3: Eligible and not-eligible children's daily school attendance by type of school

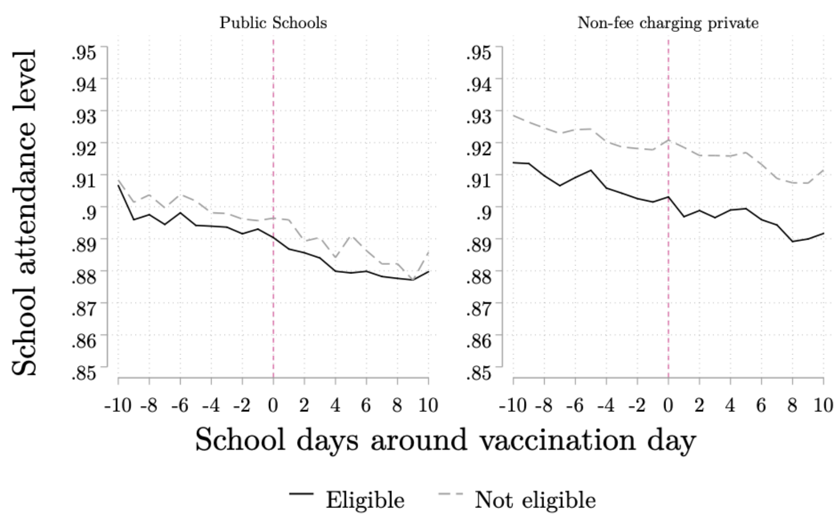


Figure B.4: Robust event study estimates

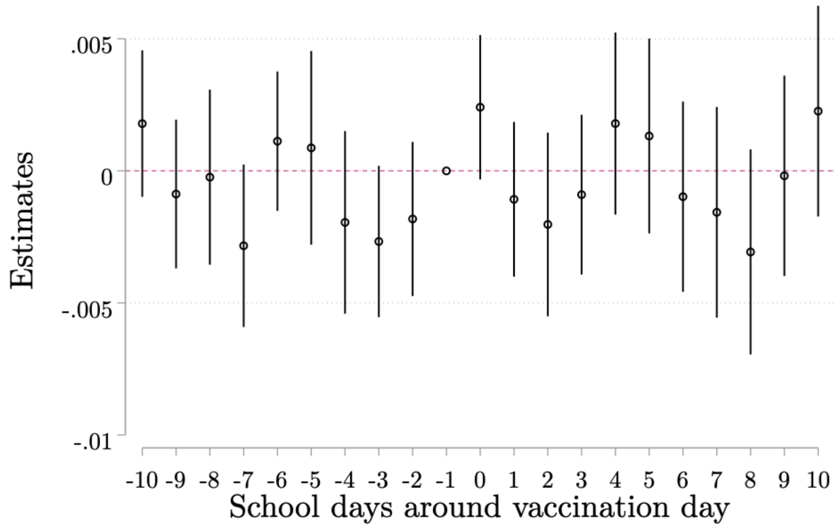


Table B.3: Communes of the dataset

Years:	2015	2016	2017	2018	2019
	(1)	(2)	(3)	(4)	(5)
COMMUNE NAME:					
Maria Pinto	Yes	Yes	Yes	Yes	Yes
La Pintana	Yes	Yes	Yes	Yes	Yes
El Bosque	Yes	Yes	Yes	Yes	Yes
Lo Espejo	Yes	Yes	Yes	Yes	Yes
Recoleta	Yes	Yes	Yes	Yes	Yes
Santiago	Yes	Yes	Yes	Yes	Yes
La Reina	Yes	Yes	Yes	Yes	Yes
Peñaflor	Yes	Yes	Yes	Yes	Yes
Providencia	No	Yes	Yes	Yes	Yes
La Granja	Yes	Yes	Yes	Yes	Yes
El Monte	No	Yes	Yes	Yes	Yes
Pudahuel	Yes	Yes	Yes	Yes	Yes
La Florida	Yes	Yes	Yes	Yes	Yes
La Cisterna	No	Yes	Yes	Yes	Yes
Renca	Yes	Yes	Yes	Yes	Yes
Quinta Normal	Yes	Yes	Yes	Yes	Yes
Huechuraba	Yes	Yes	Yes	Yes	Yes
San Ramón	No	Yes	Yes	Yes	Yes
San Bernardo	Yes	Yes	Yes	Yes	Yes
Paine	Yes	Yes	Yes	Yes	Yes
Pedro Aguirre Cerda	No	Yes	Yes	Yes	Yes
Buin	No	No	No	Yes	Yes
Puente Alto	Yes	Yes	Yes	Yes	Yes
San Miguel	No	No	Yes	Yes	Yes
Lo Prado	Yes	Yes	Yes	Yes	Yes
Number of communes	18	23	24	25	25
Sample eligible and not-eligible (daily)					
Number of schools	269	312	374	447	451
Number of individuals	30,980	40,532	47,377	55,597	57,091
Sample eligible (weekly)					
Number of schools	245	312	374	437	450
Number of individuals	14,265	20,793	23,907	27,950	29,474