Extension of CDC Study on School Mask Mandates shows No Relationship with Pediatric Case Transmission

Ambarish Chandra*
University of Toronto

November 8, 2021

Abstract

This paper extends the widely publicized CDC study on school mask mandates by Budzyn et al (2021). That study finds a negative association between school mask mandates and pediatric cases of Covid-19, using data up to September 4, 2021. Extension of the study to use more recent data, as well as a wider sample of counties, shows that there is in fact no correlation between school mask mandates and per-capita cases. In fact, cases appear to have fallen slightly faster in counties *without* mask mandates in the more recent data. The paper explains why the CDC result is spurious, using standard econometric techniques. As correlational studies do not provide causal effects, and in fact are likely to be systematically biased in a particular direction, it is misleading to base policies on these results.

*Associate Professor, Department of Management, UTSC; Rotman School of Management; and Department of Economics, University of Toronto, 105 St. George Street, Toronto, Ontario, M5S 3E6, Canada; Visiting Scholar, Stern School of Business, New York University (2021-22). Email: ambarish.chandra@utoronto.ca

1 Introduction

In a widely cited study by the Centers for Disease Control, Budzyn et al. (2021) find that, following school openings in the Fall of 2021, pediatric cases of Covid-19 increased faster in U.S. counties that did not have mask mandates in schools, compared to those that did. While the study is correlational, the results appear to have been taken as evidence by the CDC to continue recommending face masks for all children in the United States, regardless of community spread, vaccination rates or concerns about potential negative effects of masking.

This paper attempts to replicate the analysis of the CDC study. It then extends the analysis in two ways: by using a much broader sample of school districts, and by using more recent data, incorporating the CDC's restricted case data release of October 25, 2021. The results show that the negative correlation between mask mandates and pediatric cases does not persist in the larger sample. If anything, pediatric cases per capita fell slightly faster in non-masking counties over the more recent weeks than in counties with mask requirements.

Section 2 describes the data used, and the attempts to replicate Budzyn et al (2021) as closely as possible. Section 3 extends the Budzyn et al study to mid-October, using a wider sample of counties, and shows that this overturns their result. Section 4 explains why this is the case, using standard econometric reasoning to account for sample selection, unobserved effects and other confounders.

2 Data, Sample Construction and Replication

This study attempts to follow Budzyn et al (2021) as closely as possible. Data on pediatric and adult Covid-19 case rates, by county, were obtained from the CDC's Restricted Case Dataset, for which the most recent data release was on October 25, 2021. Data on school enrollments and mask policies are obtained from the data company MCH, the same source used by the CDC study. County level population figures for 2019, and school district to county mappings, were obtained from the U.S. Census Bureau.

Two factors make it impossible to exactly replicate the CDC study. First, Budzyn et al needed to make decisions on how to treat school districts that cross county lines, as well as those counties which contain multiple school districts. This is important because mask policies are set at the district level while case data are only available at the county level, and the two levels of geography do not map clearly to one another.[1] Budzyn et al use "the median school start date" for counties that have multiple school districts. This paper does the same. But Budzyn et al do not say how they treat districts that overlap across county lines.[2] This paper assigns each such district to each county of which it is a part, thereby duplicating some school districts.

[1] While mask policies are usually set at the district level, some states have attempted to impose blanket mask mandates or bans. Yet, individual school districts can often defy these directives, as was the case in some Florida school districts this Fall.

[2] When contacted, the corresponding author of the CDC study did not clarify this issue and declined to share the code used to construct the sample

Second, Budzyn et al obtain school masking rules, and other school district variables, from MCH, which in turn obtains these from phone surveys of school districts. MCH updates school district information frequently. This study uses MCH data that were current as of October 15, 2021. The CDC study used information from much earlier, which was likely to have been different as school district information changes regularly and the MCH information also sometimes conflicts with other sources.

The CDC study only keeps counties with "at least 3 weeks with 7 full days of case data since the start of the 2021-22 school year". Given the timing of that study, which ends on September 4, this implies that the sample dropped any county for which the median school start date was later than August 14, 2021.

It should already be clear that the CDC study, through its timing and choice of cutoff dates, heavily over-samples regions that open schools by mid-August. These regions include Florida, Georgia, Kentucky and other southern states. The CDC study does not examine data from New York, New Jersey, Massachusetts, Pennsylvania, Maryland and many other states that typically start schools in September. This is why the CDC analysis uses just 520 counties out of the 3142 counties in the United States. While this does not necessarily bias the results, it does call into question whether the results of that study can be representative of the entire country. Moreover, the states used in the CDC study opened schools in the middle of the severe Delta wave that peaked in southern states in the late summer.

This study attempts to replicate the CDC results. It finds 565 counties that satisfy the selection criteria used by Budzyn et al, in contrast to the 520 counties used in that study. The difference is likely due to the reasons provided above. Nevertheless, Figure 1 reproduces the main Figure in the CDC study and is very similar. It suggests a negative correlation between school mask mandates and pediatric Covid-19 cases, as counties with school mask mandates appear to exhibit smaller spikes in cases following school reopening than those without such mandates.

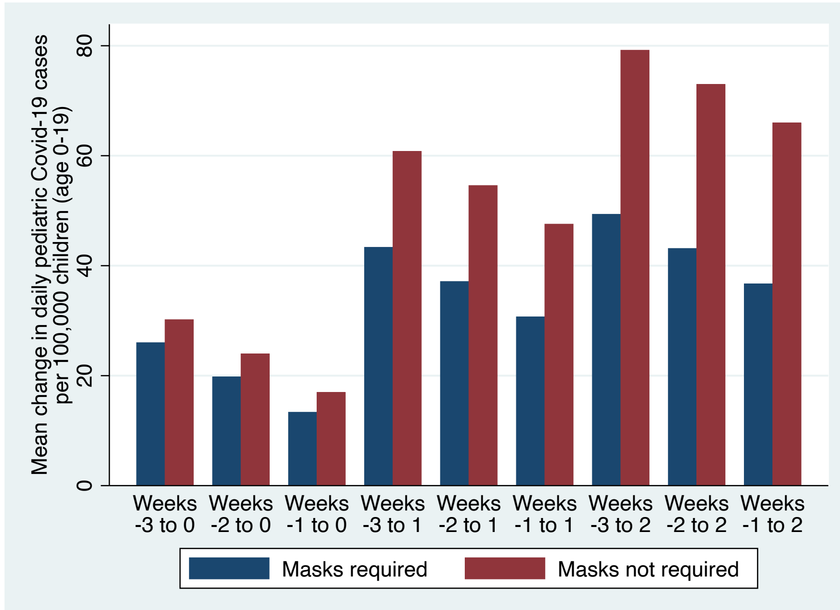
3 Extension of the CDC study

This section extends the time-period of the CDC study, which ends on September 4, 2021, to October 8, which is the most recent date for which complete covid case data are available from the CDC. Using more recent data achieves two goals—first, including all regions of the country, many of which only open schools after Labor Day. And second, examining whether the results of the more limited prior study are robust to a longer time interval.

The CDC figure in Budzyn et al is presented in terms of *changes* in cases, rather than in *levels*. This makes it difficult to compare differences in case transmission trends across different subsamples. For clarity, Figure 2 presents the same information as in Figure 1 but using the mean of weekly average pediatric cases per 100,000 children, rather than the weekly changes in this variable.

The results of Figure 2 show the same pattern as in the original figure in Budzyn et al, with higher spikes in cases following school openings in counties that did not impose mask requirements.

Figure 1: Recreation of Figure in Budzyn et al (2021)



Notes: This figure attempts to recreate the main Figure in Budzyn et al (2021). The sample consists of 565 counties that met the criteria used in the CDC study. This figure is larger than the 520 counties used by the CDC team, likely due to updated information from MCH.

In particular, by the end of the second week following school reopenings, non-masking counties had, on average, around 30 additional daily pediatric cases per 100,000 children, compared to counties with mask mandates,.

Figure 3 uses a longer time frame, though with the same 565 counties used so far. It exploits more recent data releases to present per-capita pediatric cases for five weeks following school openings, rather than just two, as was the case in the CDC study.

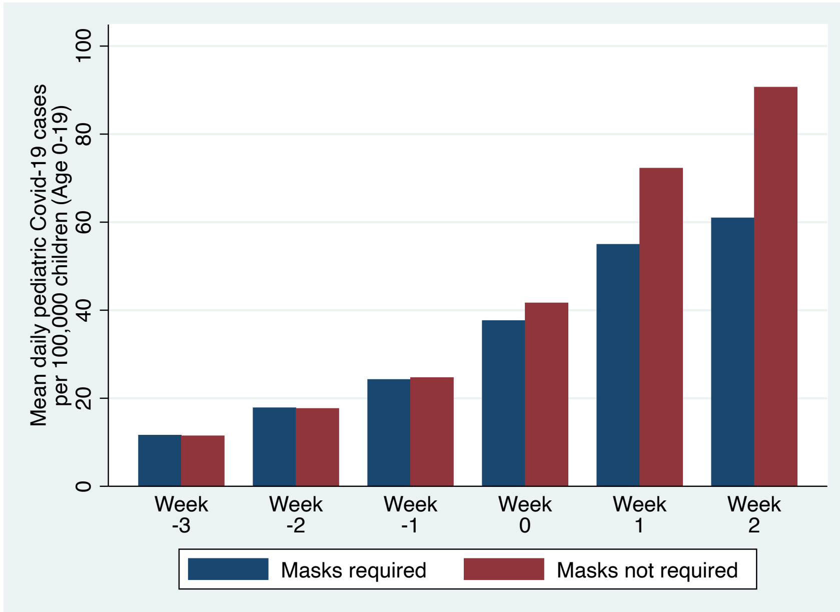
Two points are apparent from this figure: first, while cases appeared to spike in schools in the first two weeks after school reopenings, they then quickly declined in later weeks. In fact, the CDC study ended at exactly the peak of school case numbers for this sample of counties. Second, by the end of the fifth week after reopening, counties without mask mandates had only slightly higher pediatric cases per capita than those with such requirements: the difference was 9 average daily cases per 100,000 children, as opposed to 30 at the end of the second week.

Moreover, Figure 3 suggests that pediatric cases fell *faster* in counties without mask mandates in the three-week period that follows the end of the Budzyn et al analysis.

Finally, Figure 4 uses a much larger sample of 1832 counties, rather than the 565 used so far. This is possible because the more recent data releases allow an examination of school districts that opened as late as September 14, 2021, which provides a much broader representation of districts around the country.[3] This is especially important as there are clear geographic divides in school opening dates with southern U.S. states being far more likely to open in August. The figure shows

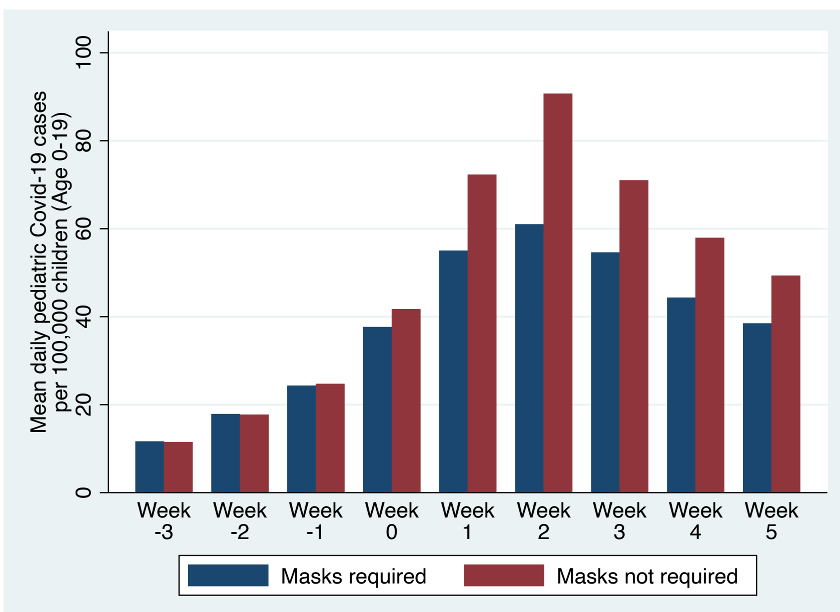
[3]There are 3142 counties in the United States, but many cannot be classified with regard to school mask policies, due to either insufficient data from MCH or the particular criteria used by Budzyn et al.

Figure 2: Presentation of the main CDC figure in levels



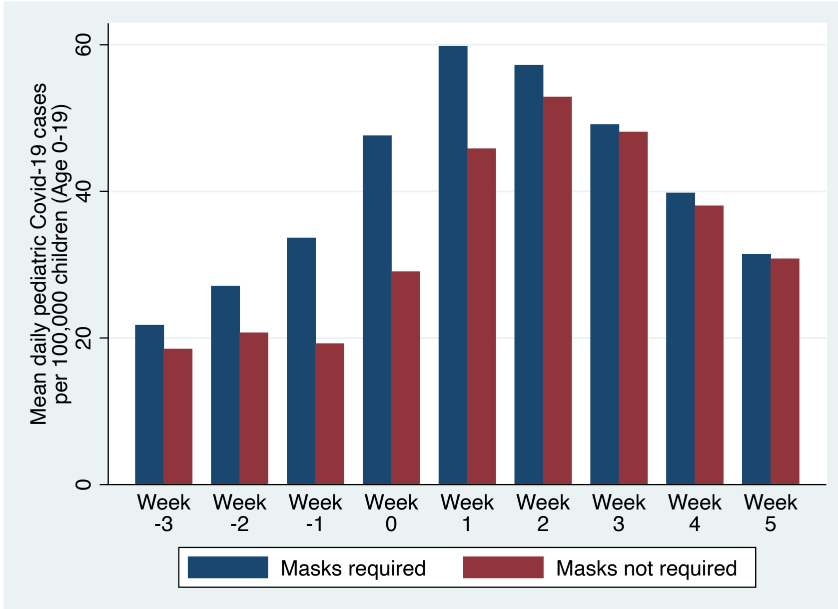
Notes: This figure attempts to recreate the main Figure in Budzyn et al (2021) but expressing cases in levels rather than in changes. The sample consists of 565 counties that met the criteria used in the CDC study. This figure is larger than the 520 counties used by the CDC team, likely due to updated information from MCH.

Figure 3: Extension of CDC sample to use more recent data



Notes: This figure extends the analysis of Budzyn et al (2021) to use more recent data, but restricting attention to the 565 counties analyzed earlier.

Figure 4: Extension of CDC sample to 1609 counties



Notes: This figure extends the analysis of Budzyn et al (2021) to use more recent data, and also uses information on 1832 counties that were open for at least 5 weeks as of the most recent CDC data release.

that there is essentially no relationship between mask mandates and pediatric cases. Counties that required masks in schools actually saw a slightly larger spike in cases in the weeks immediately before and after school opening, but by the second week after school reopenings there is no difference in cases per capita between the two sets of counties, and this remains the case through to the end of week 5.

4 Explanation for the discrepancy in findings, and discussion of sources of bias

The reversal of the results found in the CDC paper by Budzyn et al should not be surprising. The original study established simple correlations, which should not have been taken to be causal. This section explains why inferring causality from the CDC study is a mistake, using standard econometric techniques.

If mask mandates were assigned at random, as, for example, in Abaluck et al. (2021), then the relationship between these mandates and case outcomes may have a causal interpretation. But the treatment in the CDC study is not only non-random, but likely to be highly correlated with unobserved factors in systematic ways.

School districts that mandate masks are likely to be systematically different from those that do not for many reasons. First, the behaviour of school districts themselves: the former are likely to invest in other measures to mitigate transmission, such as ventilation, cohorting, daily screening of children, and maintaining distance between children in classrooms and other school locations.

Thus, the treatment of mandating masks is likely to pick up these other measures as well due to their likely correlation.

Second, school districts have representatives and decision makers who reflect the attitudes and behaviour of the community. Communities that are concerned about the spread of Covid-19 are also likely to implement other measures, even outside of schools. These could include closing or restricting capacity in restaurants; restricting children and adults from accessing other indoor activities; requiring masks in non-school settings; and community members voluntarily reducing their social contacts in order to reduce their personal risk of infection. It is very likely that school districts which implement mask mandates also have lower community spread for these, correlated, reasons.

Third, there is likely to be temporal correlation in private and public behavioural changes, in addition to the cross-sectional effects described above. Public health measures are often introduced when case counts are high, a fact that is generally well publicized in the media. Community members are likely to be aware of high transmission, and therefore more likely to change their own behaviour—by meeting fewer contacts, being less likely to dine out etc.—at exactly the time public restrictions are introduced.

Overall, private behavioural changes are highly likely to be positively correlated with public health measures to reduce cases, both cross-sectionally and temporally.

Econometric techniques can correct for these issues, even with non-random assignment of mask mandates across school districts. Techniques like Instrumental Variables can correct for bias by exploiting information on the extent and timing of measures that may have been introduced, at least partially, for reasons uncorrelated with the confounding variables.

The extensions presented in this paper are also simple correlations, and also do not reveal the true causal effect of mask mandates. However, there is an important difference: while the presence of correlation does not imply causality, the *absence* of correlation can often rule out causality, especially if the direction of bias can be reasonably anticipated.

In the context of the pandemic, the direction of bias can be anticipated quite well. Private behavioural responses—by individuals, families and employers—are likely to be positively correlated with local public health measures, for the reasons described above. Public health measures themselves should be expected to reduce case numbers. Therefore, the bias in the estimated coefficient from a naive regression of case outcomes on public health measures will be negative, as shown in Appendix A.

Therefore, the absence of correlation between mask mandates and case transmission, as shown in the larger sample, is evidence against the hypothesis that mask mandates reduce school transmission, as long as the bias in the mandate–case relationship can be consistently signed.

In conclusion, this study has established that the results of the CDC study by Budzyn et al (2021) do not persist when using a larger sample with more recent data and with a broader selection of counties. The spurious negative correlation between school mask mandates and pediatric cases should not be taken to have a causal interpretation. It is misleading to use the Budzyn et al study as evidence in favor of continued mask requirements in schools.

References

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A Appendix: Econometric Methods

Consider a relationship between an outcome variable, y, and a treatment variable, x, in a population. If x can be randomized across sub-populations, then the causal effect of varying x on y can be inferred. In the CDC study, y measures the change, over a certain number of weeks, in pediatric cases at the county level, while x is a binary variable that takes the value 1 if most school districts in the county mandate masks, and 0 otherwise. A linear relationship between y and x can be expressed as follows:

$$y_{it} = \beta_0 + \beta_1 x_{it} + u_{it} + \epsilon_{it}$$

where i denotes cross-sectional units such as counties, states, or countries, and t denotes time measured daily, weekly etc. Here u_{it} denotes variables that can affect case growth rates, but are unobserved or difficult to measure, such as private behavioral changes in the population. ϵ is an error term that is uncorrelated with x.

If u cannot be observed and therefore cannot be included in the estimation, then a simple regression if y_{it} on x_{it} will only yield a causal estimate of the coefficient on x, if the following condition is satisfied:

$$\mathbb{E}\left[u + \epsilon | x\right] = 0$$

This expression simply says that the expected value (or average) of the unobserved terms in the regression is independent of the observed explanatory variable.

If x is assigned at random then the condition above is satisfied, and the estimated coefficient on x has a causal interpretation. But the treatment in the CDC study is not only non-random, but likely to be highly correlated with unobserved factors in systematic ways.

Therefore, the bias in the estimated coefficient from a naive regression of case outcomes on public health measures will be negative, as shown in the expression below:

$$\text{Bias}\left(\tilde{\beta}_1\right) = \beta_2 \tilde{\delta}$$

where β_1 is the coefficient on observed public health measures, β_2 is the coefficient on unobserved private measures, and $\tilde{\delta}$ is the correlation between the private and public health responses (Wooldridge, 2010).