Comparing bad apples to orange soda: Flaws and Errors in an Estimation of Years of Life Lost Associated With School Closures and COVID-19 deaths by Christakis, Van Cleve, and Zimmerman

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We are writing this openly-published letter to express deep concerns regarding the paper recently published in JAMA Network Open: *Estimation of US Children's Educational Attainment and Years of Life Lost Associated With Primary School Closures During the Coronavirus Disease 2019 Pandemic* **DOI: 10.1001/jamanetworkopen.2020.28786**

The paper by Christakis, Van Cleve, and Zimmerman (2020, abbrev. CVZ) is built upon multiple critically flawed assumptions, obvious misuse of the standard analytical tools, and clear mistakes in study design. Additionally, the analysis presented contains crucial mathematical and statistical errors that completely revert the main results, sufficient that if the estimates had been calculated according to the declared methodology, the results would completely contradict the stated conclusions and policy recommendations.

These are not idle criticisms. This study has received enormous public attention, and its results immediately appeared in discussions of public health policies around schools worldwide. The central question is resolving an evidence base for the inevitable trade-off between (a) the very real harms of missed education provoked by policies that decrease viral spread vs. (b) the resumption of education as a social good which increases viral spread.

This is an incredibly important public health question, and it demands careful cost-benefit analysis. To that end, this paper adds no usable evidence whatsoever.

Outline

The outline for the remaining text is as follows:

1. A brief summary of the main results of CVZ;
2. Calculation errors that, if corrected, reverse the results of the paper and the policy recommendations derived from them;
3. A summary of some of the most critical flaws in study design and assumptions used to obtain the key compared estimates of years of life lost.

Overview of the results

Briefly, the paper makes an attempt at calculating the YLL that may be attributable to school closures and compares this number to the YLL that we can already attribute to COVID-19 from the first half of 2020. It argues that the number of YLL from school closures is higher than that directly attributable to COVID-19. This is done by taking an estimate of the impact on eventual educational attainment of missing days of school, linking this to an estimate of 25% reduction in relative risk of death for each year of schooling, and finally applying this to life tables to attach a numeric estimate of YLL that would be associated with school closures if all the assumptions held true.

This series of assumptions is noted below:

> School closures => losing days of school => losing eventual educational attainment => losing benefits of schooling on health (25% reduction in death risk per year of school) => losing years of life

The authors use this finding to argue that "there is a 98.1% probability that the decisions to close US primary schools in March of 2020 could be associated with more eventual YLL than would be observed if these schools had remained open". Unfortunately, the analysis is filled with errors which, when corrected, imply the opposite.

Errors in calculations

Meta-analytic issues invalidate the main results of the paper. The meta-analysis in question is located in the eSupplementary Appendix of the paper, and is used to derive the 25% reduction in risk of death that is attributable to an extra year of schooling, which in turn drives the enormous predicted death toll for missed days of school. Even without errors, this estimate is tenuous, as it implies that an additional year of schooling is perhaps the most life-saving intervention in human history.

However, there are also several transcription errors in the meta-analysis where the input numbers are divorced from their cited texts.

- Mazumder (2008) has a value in the meta-analysis by CVZ of 0.65 (-0-1.3). Rather, the primary finding from this study (p.10) was a reduction in relative risk of "about 10 percent". Taking the exact value from table 3, section B, line 2 (as the author recommends), the true value for meta-analysis should be 0.89 (0.743-1.034).
- Clark and Royer (2013) has a value in the meta-analysis of 1 (0.94-1.07). This paper reports two corrected estimates of one additional year of schooling on mortality from the 1947 and 1972 changes to legislation, in Table 2 and Table 4 (first row/column). The proper estimate from Clark and Royer (2013) should read 1.022 (1.01-1.034).
- Lleras-Muney (2005) has a value in the meta-analysis of 0.42 (0.08-0.77). However, this is a misleading extraction from the paper; the author herself ran a series of regressions which could not be statistically distinguished from one another and so argued that the reduction of the baseline risk should be considered to be "3.6% (equivalent to a RR of 0.67) or greater" (Lleras-Muney 2005). Indeed, CVZ appear to have taken the single greatest value from each of these regression analyses to input into their meta-analysis, which while not an error per se is nevertheless misleading and cannot be considered best practice.

It is also worrisome that the authors have included both Lleras-Muney (2005) and Mazumder (2008) in this meta-analysis, as Mazumder (2008) was a later paper based on the same dataset as Lleras-Muney (2005) but using a more rigorous analytic method. It is clearly inappropriate to include both of these studies in the same meta-analytic model.

Moreover, it is questionable that the authors have included this estimate from Mazumder (2008) in their paper at all, given that this author reexamined the work after noting a programming error (Mazumder, 2010) – the corrected estimates according to the erratum published in the text of the article imply no effect of schooling on mortality and in fact show that there is "little compelling evidence suggesting a causal link between schooling and mortality", undermining the entire premise of the CVZ paper.

In addition to the transcription mistakes, the meta-analysis itself is statistically incorrect. CVZ specify that they use an inverse-variance model with extra weight to US studies, however this is mathematically impossible given the confidence bounds of the included research. This can be seen quite clearly in the supplementary appendix - Clark and Royer (2013) with a confidence bound of 0.94-1.07 is weighted 0.01 while Lleras-Muney (2005) with a bound of 0.08-0.77 is weighted at 27.90. Using these bounds and the formula for variance to calculate the inverse variance of each study gives 227.31 and 8.07 respectively.

Even if the Lleras-Muney (2005) estimate is double-weighted, as the authors claim, the fact that it is weighted 2,790x more heavily than the more precise estimate of Clark and Royer (2013) is mathematically flawed. Correctly applying an inverse-variance weighting for the included studies gives a result of 0.95 (0.92-0.97). Excluding the Lleras-Muney (2005) paper and correcting the transcription errors gives a result of 0.97 (0.93-1).

Correcting the above would reduce the estimate of YLL for school closures to 0.65 million, vs. 2.97 million using the counterfactual of keeping schools open, completely reversing the main finding of the paper even without other errors taken into account.

Design, assumption and conceptual flaws

The authors suggested a causal chain that consists of two highly questionable links: *(1)*, missing school is linked to overall educational attainment, and, *(2)*, attainment is then linked to the length of life. The first link relies entirely on a single Argentinian study by Jaume and Willén (2019) of the long-term effects of teacher strikes on educational attainment of children who attended school during this time. In addition to the inappropriate assumptions that present-day US children are directly comparable with Argentinian children from the 1970s and 80s and that teacher strikes have equal effect as remote learning during the lockdown, CVZ ignores *the explicit warning that Jaume and Willén (2019) provide in their paper against using the presented results causally*.

The second link relies on the unwarranted assumption that lost years of schooling can be directly translated into lost years of life. Even if the causal relationship between years of schooling and years of life can be assumed, educational attainment does not explain *all the variance* in longevity, which is the implicit assumption of the deterministic one-to-one translation used in the paper. In essence, CVZ assumes that Argentinian children from decades ago are perfectly representative of children across all the United States learning from home in 2020, and that the losses that have previously been associated with missing days of school are instead causally related. Neither of these assumptions are viable.

Based on the above, the authors proceed to an even more questionable comparison of the YLL due to the lockdown school closures and YLL due to the direct COVID-19 deaths in the US through 30 May 2020. It is incorrect to compare fulfilled lifespan losses of those who died in the still-ongoing pandemic with the predicted future lifespan losses for the children generations affected by the lockdown. The authors effectively compare YLL of the deaths that happened to a small fraction of the population to the future (and highly speculative) YLL that will happen to a nation-wide population of school children.

Even if performed correctly (which is not the case), the estimation of YLL relies upon a mathematical model *period life table* from the demographic/actuarial toolkit. When life expectancy estimate for a synthetic cohort in a period life table is interpreted as a forecast of remaining years of life for specific people, the underlying assumption is that the age-specific profile of death rates observed for the synthetic cohort today is not going to change in the future (Preston et al. 2001). Since mortality keeps improving, and even faster than experts had routinely expected (Oeppen and Vaupel 2002), the period life table assumptions applied to estimate the remaining years of life for school children will be much more uncertain than that for the mostly elderly people dying from COVID-19.

Moreover, there are crucial errors in the estimation of the YLL *directly* attributable to COVID-19. CVZ calculated this using the COVID-19 deaths recorded in the US until May 30th (CDC 2020) and the 2017 life tables (Arias and Xu 2019) that provide age specific estimates of the remaining life expectancy: for each COVID-19 death they looked up the estimate of remaining years of life associated with the corresponding age, then summed up all these YLL. There were two unwarranted simplifications of the data involved, as CVZ wrote they analysed the death records "assuming that deaths occurred in the middle of the 10-year span and that 85 was the maximum age of death". As can be seen in the raw data of the CDC's COVID-19 death database (CDC 2020), both of these data processing assumptions are incorrect. They result in assigning younger age to many people who died of COVID-19, which are later on associated with more remaining years of life. In particular, the assumption that all deaths of people older than 85 were happening exactly at age 85 has a major impact on the YLL calculation, significantly overestimating the value – effectively this assumes that all those who died for example at ages 87, 93 or 105 had the remaining life expectancy of those aged 85. And the proportion of people dying of COVID-19 at age 85 and more is very sizeable: *23% of males and 40% of females, based on the current COVID-19 death records (CDC 2020)*.

Next, it is also clearly wrong to compare an inference of potential future YLL from school closures to only the direct mortality losses due to COVID-19, given that we know that there are definite secondary impacts of COVID-19 as well. This counterfactual implicitly assumes that the only harm from the disease is people who die of it, and as is now extraordinarily clear from the literature, there are secondary harms relating to hospitalization and additional chronic/convalescent issues ('Long COVID'). Moreover, the counterfactual used by CVZ assumes that more cases of COVID-19 would have no impact on school attendance or the ability of school teachers to keep offering the classes, which is at best unlikely.

Finally, CVZ recognized that should the schools had been kept open, the spread of the virus would have been increased, resulting in more COVID-19 deaths, hence more YLL due to the pandemic. To provide this estimate, CVZ rely upon two papers:

1) Auger et al. (2020) modelled the increase in county level *crude death rates* associated with schools being open during the pandemic and got a relative risk ratio estimate of 2.85;

2) Courtemanche et al. (2020) modelled the effectiveness of various policy interventions in reducing the *spread of the virus* and among other results found a statistically insignificant reduction of the spread associated with school closures – based on this CVZ derived the relative risk ratio of 1 (no difference) from their analysis.

There are multiple critical considerations regarding the applicability of these two point estimates from the two contradicting papers, but the way these two point estimates were synthesized by CVZ is flawed: they averaged the risk ratios of 1 and 2.85 to obtain the "middle ground" estimate of 1.93. CVZ then performed Monte Carlo modelling using these estimates, which is unlikely to be useful given the crude and inaccurate aggregation of these figures. CVZ further applied this estimate as a simple scaling factor multiplying the previous estimate of the total YLL due to the direct COVID-19 deaths. Neither county level risk ratios of crude death rates, nor US population level risk ratios of the virus spread associated with schools staying open, can be simply averaged together and used directly to scale up YLL.

Concluding remarks

None of the years of life lost quantities that CVZ estimated and compared may be treated as solid evidence providing new insights, since the calculation of every single one of them involved flawed assumptions and errors. While comparing apples to oranges might be issue enough, this paper does not even do that - the final result is more akin to comparing bad apples to orange soda given how demonstrably mistaken the methodology, calculation, and data appear to be. There are further issues not delineated here for brevity.

Given the entirety of the above, we respectfully suggest the editors and authors consider correcting and/or retracting the paper.

Replication

All our re-calculations can be replicated using the code from https://github.com/ikashnitsky/pppr-jamano

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