Author's response to reviews

Title: Hand hygiene instruction decreases illness-related absenteeism in elementary schools: a prospective cohort study

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Version: 3 Date: 22 March 2012

Author's response to reviews: see over
Dear Dr. Neilan,

Thank you for your continued consideration of our manuscript, entitled “Hand hygiene instruction decreases illness-related absenteeism in elementary schools: a prospective cohort study.” We much appreciate the helpful review provided and have taken care to adequately address each concern. Our responses are included below and a clean version of the manuscript has been submitted for further review. We believe the revisions made have substantially strengthened the text and look forward to hearing your thoughts. In particular, we obtained additional statistical assistance from a PhD level researcher and were able to obtain classroom sizes to conduct a stronger statistical analysis. Please let us know if you have any questions or if additional information is needed. Thank you again for your time and consideration.

Sincerely yours,

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We would like to thank the reviewers again for their helpful comments. Please find our responses to each reviewer comment below.

Referee 1

1. The potential of English proficiency on the childrens’ understanding

Thank you for bringing up this very valid concern. Instruction was completed in individual classrooms. Both instructors and teachers ensured that all children both participated in and understood the lesson. Both schools were taught in English and all children had a good grasp of the English language. The lessons were also made specific to the child’s grade and a question and answer session as well as interactive teaching was conducted. We added a sentence to the methods to clarify.

2. Vaccination rates were unknown and vaccination may have been the primary driver of reduced infection (vaccination rates may be high across both arms resulting in the effect towards the null)

We agree with this point made by the reviewer and therefore added the following to the limitations: “We also did not have data on influenza vaccination rates of children in the participating schools.”

3. While random allocation may make all potential confounders equal in both groups the sample sizes in each arm were small hence the potential for siblings as a source of infection at home and not school students.

We appreciate this suggestion and agree. All children (intervention and control arms) had an equal chance of exposure to factors outside of school. This includes not only siblings but also any afterschool activities, sports, parties and any other activities they may be involved with. Unfortunately we were unable to control for these factors and feel each child has an equal ability to be exposed to factors outside the classroom.

Referee 2
Major compulsory revision

I appreciate the improvements the authors have made to the manuscript. However, I am still concerned about
the statistical analysis not accounting for clustering. I am not sure I can follow the explanation given by the
authors. Surely they must have collected data at classroom level in order to distinguish between intervention
and control. Therefore it should be possible to compare the classroom level rates in intervention and control
using a simple t-test. These are standard procedures in a cluster allocated trial. Please see the literature (e.g.
Donner/Klar, Hayes/Moulton). Failure to take into account clustering produces confidence intervals that are
much narrower than would be realistic. This flaw in the analysis must be addressed in my opinion. Otherwise I
cannot recommend this study for publication.

Thank you for the helpful statistical advice. We obtained additional statistical assistance from a PhD
level researcher. We were able to obtain classroom sizes as well as the requested literature and did perform
the requested t-test to control for clustering. When we attempted this our significance went away. We conducted a power analysis for illness-related absences during influenza season and found
that our sample (n = 31) provided us with power < .2 for a two-sided t-test at alpha < 0.05. This
suggests that our sample based on the clusters is grossly underpowered for the manner of analysis
suggested by the reviewer. However, another study (Stebbins et. al. Pediatric Infectious Journal, 2011)
reported that the intraclass correlation for illness-related school absences was 0.01 (p. 4). This
suggests that class-level clustering is small to minimal for illness-related school absences. Given the
magnitude of uncorrected association (unadjusted odds ratios = 1.37 for the illness-related absences
during influenza season) and the potential low intraclass correlation, the study would have found this
association significant had we had access to individual-level data that would have enabled us to
correct for the class-level clustering in a random-intercept modeling. Given the lack of statistical
significance after cluster correction, however, we revised our text to indicate this and included it in the
limitations section.

Referee 3

1. The results presented come from a non-randomized, short period intervention study. I am unsure whether
significance testing makes any sense if groups are not randomized at the beginning. Does a sig. result show a
pre-existing difference or an intervention effect? Or somewhat in-between?

Thank you for your comment. Both schools were randomized by grades in the beginning of the study.
We assigned odd grades to the control group and even grades to the intervention group. Although we
agree the groups are small and the study period short but we do believe that the slight decrease in
absences during the flu season is related to the improved hand hygiene in the intervention group.

2. I must raise the “September-issue” again. Was September excluded from the calculations or not? This is still
not clear to me.

Sorry for the confusion and thank you for noticing this. We have now excluded September from the new analysis.

3. Most severely, authors report illness-related absenteeism rates (= the % of absences due to illnesses in
comparison to all absences) only, instead of total absences. As the illness-related absenteeism rate does not
only depend on the number of illness-related absences but on the number of non-illness-related absences, too,
a change in the illness-related absenteeism rate could completely be caused by a change in the non-illness-
related absences. Moreover, the authors did not collect data on non-infectious reasons for absenteeism (see
authors reply).

The first and central sentence in the discussion, that HH can reduce the illness related absences, is therefore
not backed by the (reported) results. This somewhat hidden but striking problem did only become clear to be
as I thought about the effect of a non-randomized design and the authors reply again. Authors need to report
total absences in control and intervention group as well as illness-related absences for both schools separately
(tables 2a and b) to back their statement.
Due to this comment and insight we obtained new data (number of children per classroom) from the schools, to calculate percent total absent days as well as percent illness-related absent days, rather than the previous ratio. We found that both total absences and illness-related absences were lower in the intervention group during flu season, supporting our statement that hand hygiene with education can reduce illness-related absences. Results were consistent with previous analyses. The methods and results sections were amended accordingly. Thank you.

Referee 4

There is a significant amount of literature about the use of non-pharmaceutical interventions, including hand washing/sanitizing, and its impact on absenteeism. There are even a few comprehensive studies. This paper breaks no really new ground, but from a public health policy and practice perspective, it makes clear that one cannot just put a NPI intervention in place and expect it to work. It reinforces the need to include instruction and periodic refresher instruction. I would suggest that you look at information at www.pipp.pitt.edu

Thank you for this great resource! Relevant publications of PIPP were added to the introduction and discussion (reference number 15, 20).