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Title: Impact of COVID-19 on hand hygiene performance measured using group electronic monitoring: a multicentre observational study

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Reviewer 1: Dr. Olivier Drouin

Institution: **CHU Sainte-Justine**

General comments (author response in bold)

Overall, interesting, novel, well-executed study on an important topic, which may have relevance past the current COVID-19 epidemic on a particularly difficult (but important) topic.

I have a few comments:

ABSTRACT:

1. The Methods would benefit in being clearer about the study period, rather than states "first wave of the pandemic".

We agree and have made this change as above.

2. In the Results, the numerator and denominator should be defined (it is in the body of the abstract, but should be defined here as well).

We agree and we have made this change in abstract.

INTRODUCTION

1. Concise, well written. The abstract could benefit to hinting to other studies that have studied the topic. A quick search allowed me to find the following:

- Moore, L. D., Robbins, G., Quinn, J., & Arbogast, J. W. (2021). The impact of COVID-19 pandemic on hand hygiene performance in hospitals. *American Journal of Infection Control*, 49(1), 30-33.

- Hess, O. C., Armstrong-Novak, J. D., Doll, M., Cooper, K., Bailey, P., Godbout, E., ... & Bearman, G. (2020). The impact of coronavirus disease 2019 (COVID-19) on provider use of electronic hand hygiene monitoring technology. *Infection Control & Hospital Epidemiology*, 1-3.

Despite those previous reports, the study is very relevant given both the Canadian context and the number of sites involved, so reference to these other studies would not diminish the relevance of the work presented.

We agree with the reviewer. These studies were published since our submission and we have updated the manuscript as suggested by the Editor, to reflect this literature.

METHODS

1. While I understand that some of the Methods details may have been described in previous papers from the same group, some details may be very relevant to the reader of the current paper. This includes what is described in the last sentence of the first paragraph.

a. Who were the email reports sent to? At what frequency? Were they generic or individualized? Research from behavioural sciences show that some of those elements can make a sizeable difference on the effectiveness of interventions. They are also pertinent from a QI perspective if someone was to try to replicate this study's success.

We thank the reviewer for this comment. As noted, the intervention is well described in 2 other peer-reviewed published papers. Feedback is provided in multiple ways including email, posters and in-person huddles/meetings and we have now described these various ways in the paper. We have not added additional details primarily because this intervention was in place in background throughout the study period (except for critical care units that began January 6th) and therefore the large increase in performance seen coinciding with the start of wave one of the pandemic is unlikely to have been a result of these background interventions.

2. The Methods appear appropriate and described in sufficient details. I am just not sure of the two methods to divide the periods for the segmented regression. Why two? Because the first one (based on peak COVID-19 inpatient census) would vary by hospital and would make comparison between hospitals difficult to interpret? If so, would the 2nd represent a sensitivity analysis?

We agree that the prior version of methods was unclear and we have now clarified that there were 2 different exposure variables. We have also clarified the different models used (as above).

3. I commend the authors for referring to the STROBE statement. However, this should be mentioned somewhere in the Methods and not only as an Appendix. [Editor's note: this could be mentioned in the first paragraph of the Methods citing both the statement and the appendix.]

We agree and have added this to the text.

4. While it is in the Intro, the operational definition of HH adherence should be in the Methods.

We agree. This has been moved from the Introduction to the Methods section.

RESULTS

1. Do I understand from Table 1 that 6 hospitals do not have bed monitored on the inpatient ward (and only in critical care unit)? This could benefit from one line in the Methods. It is also slightly confusing that Table 1 refer to pre-COVID period for all data except for the COVID-19 patient-days.

We agree with the reviewer that this was confusing and have corrected this. We can reassure the reviewer that all beds were monitored throughout the entire study – but critical care only began to receive the intervention described above on January 6. This is the only difference in terms of the background intervention that was in place across the units. In terms of the time periods, we have named and defined these in both the methods and Table so this is hopefully more clear. Finally, we move the COVID-19 case numbers to the new Table 2 with footnote providing the dates to avoid confusion about the time period of these case numbers.

2. I would have liked to see some results on variation between sites in HH adherence and/or the impact of COVID census. The 12 different sites are a strength of this study and I feel like the authors could have better taken advantage of it. Are some hospitals driving the effect seen? The same way that baseline HH adherence was different at baseline (Table 1), the effect of the pandemic could have been different between hospitals.

We agree and as noted above we have now added a Table 2 that provides site level data to allow the reader to see the differences between sites during the different time periods. This Table also includes the COVID-19 case number differences by site to allow comparison of these with changes in hand hygiene rates.

DISCUSSION

1. It is only briefly mentioned, but I think it is fascinating that the peak in HH adherence seemed to have been at (or even before) the onset of COVID-19 in Ontario. And somewhat paradoxically, as the number of cases INCREASED, the adherence DECREASED with time. A brief discussion on the potential drivers for this effect could have been insightful: accustomization to the risk? Wearing off of the novel risk perception? Reminder fatigue? Other personal/institutional/provincial explanation?

We agree with the reviewer's comment and have integrated some of these potential hypotheses into the discussion.

2. The authors suggest that local # of cases was a bigger driver of hand hygiene (through increased risk perception) than provincial # of cases, but again, that does not explain at all the peak 0-4 days after the FIRST case at each hospital. Still, the authors bring important points which warrants further study. The return to baseline 90 days into the biggest pandemic most living person has lived through is sobering to infection control and hand hygiene efforts in the future.

We agree with the reviewer and as already noted by Statistician above, we have removed the prior comparison between the two exposure variables. This should address the reviewer's comment as well.

3. The discussion would benefit from at least a brief comparison to other studies on the topic.

We agree and we have added the prior studies to both the introduction and also the discussion.

FIGURES

1. Once printed in black & white, both figures are very difficult to read, both with regards to the X-axis, as well as histogram bars and trend lines. The chosen 3 time periods should also be made more visible on the graph to help the reader better visualize the impact they had.

We agree with adding the study periods and have done so. We have left the Figure in color in hopes that it can be published in color rather than grayscale. If this is a problem, we can adjust at the Editor's direction.

2. Also, why 3-day increments rather than the more visible and more intuitive 7 days increments?

We agree and we have changed to 7-day increments as suggested by the Statistician reviewer.

3. Figure 1 represents the province-wide average of HH? And the province total # of COVID-19 cases? If so, the title should be clearer

We agree. The legend now says: Figure 1. Hand hygiene adherence across 12 acute care hospitals in association with hospital census of COVID-19 patients and overall new daily COVID-19 cases in Ontario.

Reviewer 2: Dr. Daiva Nielsen

Institution: McGill University

General comments (author response in bold)

The authors report findings from an innovative study that evaluated hand hygiene adherence in 12 Ontario hospitals using a group electronic monitoring system over the course of the COVID-19 pandemic. The results indicate an improvement in hand hygiene adherence among both wards and critical care units early in the pandemic when cases began to rise in Ontario and COVID-19 patients began to be admitted to hospital. However, while both Ontario daily cases and hospital cases were significantly associated with increased hand hygiene adherence, hospital cases was a stronger predictor. Adherence decreased before the peak of daily cases in Ontario (and prior to the peak in hospital cases) and ultimately returned to baseline levels (within 90 days). The authors conclude that healthcare worker perceived risk is a strong enabler of increased adherence to hand hygiene practice and that the observed high level of adherence during the pandemic further validates the group electronic monitoring system.

General comment:

- This is timely and relevant work. The investigative measures were obtained with appropriate (hospital census, daily provincial case numbers) and novel methods (electronic monitoring system). The statistical approach was well described. Overall, the authors have interpreted their observations well and discussed the limitations of the study, although this reviewer has comments/questions for the authors to consider.

Major comments:

- I would appreciate if the authors could elaborate on their statement that not all hand hygiene moments may have been appropriate. Since it is likely that this behaviour occurred, it seems that there is some inaccuracy in describing the increased hand hygiene moments as “adherence”. Are there concerns over increased use of hand sanitizer on unnecessary occasions given increased attention to antimicrobial resistance? Perhaps the authors should acknowledge that while electronic monitoring systems can circumvent some issues known to impact direct observation to assess adherence (sampling and observer biases), direct observation, if conducted accurately, currently remains the only method available to evaluate appropriateness of hand hygiene moments.

We agree that our prior sentence was confusing and we have revised it as follows: “GEHMS provide a unit level estimates of HH adherence that cannot differentiate the specific moments of HH. This is particularly important as the system cannot distinguish whether the rise in adherence was due to additional HH events outside of appropriate moments of HH.” We do not view this as a significant methodologic concern given that improvements in hand hygiene adherence measured through GEHMS have been associated with reductions in infection outcomes in the literature (Leis et al, Clin infect Dis 2020; Kelly et al, AJIC 2016).

- According to Table 1, baseline adherence was widely variable across the hospitals included in the analysis. It would be helpful if the authors could justify why analyses were not adjusted for baseline hand hygiene adherence.

We agree and we have addressed this in 2 ways. First Table 2 now shows hand hygiene by site across the different study periods. Second, our interrupted time series evaluated the difference in performance over time.

- The Results presently do not describe the comparison of adherence in wards vs. critical care units, but this is presented in Figure 2 and mentioned somewhat in the

Interpretation. It would be useful to state this comparison in the Results. Also, in the Interpretation the authors state that the results were similar between wards and critical care units. However, although the pattern was the same, adherence was much higher in wards vs. critical care units even before the admission of the first COVID-19 patient. Is this because the wards received feedback for a longer period than the critical care units (as noted in the Introduction)? It would be helpful if this was briefly discussed.

We agree with the reviewer that the difference between critical care and wards is noted in the Figure but was not specifically noted in the Results and we have now added this as follows: “The absolute HH adherence was higher on wards compared to critical care units, but without significant differences in changes over time.” The reason for the absolute difference may be due to differences between sites (the number of critical care units varied by site), more recent implementation of hand hygiene interventions, and also inherent differences in critical care units such as the larger number of HH opportunities and contextual factors like business and acuity of patients. The absolute difference noted is not a focus in our study but we assessed the relative difference over time which was similar across all units. We have now summarized this difference in the results and again in the discussion as follows: “Our regression model adjusted for correlation within units and hospitals and findings were consistent across both wards and critical care units suggesting a generalizable behavior change in response to COVID-19.”

- The authors conclude that future research should determine whether strategies that focus on healthcare worker personal risk perception result in sustainable improvements in hand hygiene, but, as the author’s note, personal risk perception may not reflect actual risk and it seems that personal risk perception could also increase unnecessary hand hygiene moments.

Therefore, perhaps the authors should acknowledge that additional strategies evaluating other possible targets to improve adherence remain important areas for investigation.

We thank the reviewer for this comment. We agree that perceived risk appears to be a more important driver than actual risk and we believe this is an important learning from this study that can inform future quality improvement intervention. We are not concerned that hand hygiene would be performed when unnecessary since healthcare workers are trained on the moments of hand hygiene (and especially before and after patient contact) and we did not observed rates above 100% of opportunities across our hospitals suggesting there is still opportunity for improvement.

Minor Comments

- The Methods section first describes the hospital-based analysis, followed by the dates-based analysis. However, the dates-based analysis seems to be discussed first in the Results and displayed first in Figure 1. I would suggest that the authors either present the hospital-based results first in the Results (and present as Figure 1) or, alternatively, edit the Methods to describe the dates-based analysis first.

We agree and now that we have added Table 2, the hospital based analysis comes first which should address this comment.

- There is a grammatical error on page 8 Line 41: “...provide a unit-level estimates”. Either make “estimates” singular or remove “a”.

We have fixed this.

- The x-axes in the figures are difficult to read. Please improve the fonts and/or sharpen the quality of the images to improve readability of these axes.
We agree and have modified the Figure as suggested above.