Article details: 2020-0115	
Title	SARS-CoV-2 outbreak amongst physicians at a Canadian curling bonspiel: a descriptive observational study
Authors	Kelly W. Burak MD MSc(Epid), Sampson Law MSc, Chris Rice MDes, Jia Hu MD MSc, Christopher I. Fung MD, Allan K.H. Woo MD, Kevin Fonseca PhD, Amanda L.S. Lang PhD, Jamil N. Kanji MD, Bonnie L. Meatherall MD MSc
Reviewer 1	Stephanie Smith
Institution	Alberta Health Services, Edmonton, Alta.
General comments (author response in bold)	This is a descriptive study of an outbreak that occurred relatively early on in the NA COVID-19 pandemic. While there are some interesting points about this particular outbreak, I feel the authors are not focusing on the points of interest. There is ample literature on the symptoms of COVID-19 from very large cohorts so the description of symptoms in this small cluster does not add to our knowledge of the presentation of COVID-19. I think the more interesting aspect of this outbreak is the high attack rate. **[We agree with the editor and reviewer #1 on this point and have reworked the manuscript to focus on this.]** Even if you only include those who tested positive, 40/73 is still a much higher attack rate than described in other outbreaks including household clusters. Delving into the transmission dynamics and an explanation of how this occurred would be interesting. Given you had 10 people who had symptoms initially it would be interesting to map out their movement and figure more accurately describe the possible transmission dynamics. **[Unfortunately, this is not possible due to the very social nature of our bonspiel. Our survey tool did not cover these details, and going back several months later to ask each individual who they spoke with during social activities would prove very difficult, and likely would be inaccurate at this point. We highlight this in our manuscript on page 8:"The feasibility of studying the transmission dynamics of this outbreak is made challenging by the social nature of the bonspiel."** **However, providing a new format to Figure 1 (now bottom part of Figure 2) we have organized the curlers according to their teams. There is only one team who had no cases. Analysis of that team, showed that they did not attend any group social events, including buffet lunches, but they did curl the same number of games as other teams. This supports our conclusions on page 8: "Interestingly, the single team with no confirmed cases did not attend any social events outside of their curling games, suggesting that social activities associated with sporting events may be as, or more important, for transmission."]** I think the plan to do serology is an important one and I would suggest that you wait to publish the description of the outbreak until you have this information especially since the whole cohort was not tested with RT PCR to determine if any of asymptomatic patients were positive. **[We agree and have waiting until more serology results were available. At the time of resubmission we have SARS-CoV-2 IgG serology available on 58 / 73 (79.5% of the participants), with additional four tests pending. Recognizing the limitations of serology, we do believe it strengthens the paper and will be interest to the readers. We have added serology to the**

	Abstract (page 1), Methods (page 4), Ethics (page 5), Results (page 7), Interpretation (page 10), Limitations (page 10), Figure 2, Supplementary Table 2.] Some more specific comments: Page 5 line 30: why did you consider 28 days as opposed to 14 days which is the established incubation period. If you used this cut off would the symptomatic and asymptomatic numbers be the same? **[We recognize the incubation period is 14 days but wanted to collect a bit more data around travel. The difference between travel in symptomatic (8/56 = 14.2%) and asymptomatic (3/17 = 17.6%) curlers was also not significantly different at 14 days (p=0.734). Results (page 7) updated to read "Eleven participants reported international travel in the 2 weeks before the bonspiel (14.2% in symptomatic vs. 17.6% in asymptomatic; p=0.734), with travel to USA being the most common destination."]** Page 5 line 41: when were the participants tested in relation to symptom onset - this may help us to understand the false negatives **[Not addressed as per the editors instruction.]** Page 6 line 5: I feel the reported sensitivity of the testing is not accurately calculated. By my calculation there were 34 patients who tested positive on the first test and and 6 who tested negative on the first test but tested positive on the second test. The sensitivity would therefore be 34/(34+6) = 85%. I do not think you can include the people who had symptoms and tested negative as being false negatives especially since there is no mention that they had other resp viruses ruled out. This is an important point especially since you had 10 participants who had symptoms when the started the Bonspiel - it is possible that some of these people actually had another respiratory virus as it was still resp viral season. **[Not addressed as per the editors instruction.]**
Reviewer 2	Jean-Pierre Pellerin
Institution	Unité de médecine familiale, Centre Hospitalier de Verdun, Verdun, Que.
General comments (author response in bold)	SARS-CoV2 virus outbreak amongst physicians at a curling bonspiel in Canada : an observational study Observational study is generally a poor method to illustrate a health process. The sampling is often inadequate because small and not randomized; lot of missing data is seen; variability of the data may adversely affect the results. In this study, we found an observational study with a non-randomized sample of subjects but this sample is composed almost essentially of healthcare workers who are relatively in the young age (54 are less than 60 years old), who are in good health and who are concerned in participation to a research process. This give a strength to the study because no lost to follow-up; every participant agree to answer to the survey tool. This sample should not be different than another in front of the disease. The survey tool in pages 26 to 28 is complete and seems valid. There does not seem to be a lack of important informations. **[Thank you.]** Statistical analyses are numerous and varied. The authors do not link thisrtain of their analyses to their results. What conclusions do they draw from the Kruskal

	Wallis test and the Mann-Whitney test? **[We did not use KW test or MW test in the results presented in the resubmission. They have been removed from the Data Analysis methods.]** In table 1, almost eight out of nine comparisons are nonsignificant. This could mean that most of the time, the 'Confirmed and Presumed' do not differ from the 'Asymptomatics'. Is this real or due to the small sample size that probably prevents these comparisons. Do you have some explanation or comment? **[We have simplified Table 1 to only show curling and social events. Yes, we agree our small sample size could be contributing to no difference found (lack of power due to sample size). However, looking closely at the only team with no cases, provides further support to our hypothesis that transmission likely occurred in the bar / lounge where people congregated to socialize and share meals.]** **Results (page 7) - "An analysis of curling and social events at the bonspiel (Table 1), highlighted a significantly greater proportion of symptomatic cases attended the buffet style lunches at the curling rink (98.2% vs. 70.6%; p=0.002). Only one team had no cases (Figure 2), and no members of this team attended the buffet lunches or other social events".]** **Interpretation (page 8) – "Interestingly, the single team with no confirmed cases did not attend any social events outside of their curling games, suggesting that social activities associated with sporting events may be as, or more important, for transmission".]** In figure 2, the frequency of symptoms is based only on descriptive mode, either between confirmed and presumption or either between all the confirmed symptoms. Some symptoms are more frequent than others. Are they the same that are reported in the literature? **[Yes, the frequency of symptoms are similar to other published studies. However, as we do highlight through the manuscript our rates of anosmia are quite high, for example, compared to the meta-analysis we reference.]** Same comments in Figure 3. Medians indicate that some symptoms appear earlier than others. Is that significant? Is this consistent with what is already known in the literature? The authors do not comment but it seems that the more frequent symptoms start earlier. That should be mentioned or computed. **[We are not aware of this being described in detail in the literature. This analysis was actually done at the request of Dr. Deena Hindshaw, Chief Medical Officer of Health in Alberta. She was interested knowing the timing of symptoms and updating Alberta's case definition based on our early outbreak. She believes this is the most interesting aspect of our paper. Most of the common symptoms early on are very non-specific upper respiratory track illness symptoms (could be the common cold or flu) and hence the current recommendation to self-isolate if sick. Although anosmia appears very specific for COVID-19, not isolating until day 4 (our median onset of anosmia), would mean people would be at increased risk of spreading the virus if waited to isolate until they developed this symptom. We have added a line to highlight the importance of our finding on page 9: "Our study, demonstrates that anosmia tends to occur a few days after onset of other symptoms (Figure 3b), and can occur without other symptoms**

	such as nasal congestion or rhinorrhea in 16% of patients." We added… "However, waiting to isolate until this specific symptom is present could led to risk of spread to others".] Figure 4 clearly shows the relationship between the duration of symptoms and the duration of confinement. **[Agree, and we updated the graph to illustrate this in another way.]** In page 12 (line 3 to 5) : Specifically, confirmed and presumptive cases were more likely to have attended the lunches than those with non-specific or no symptoms (98.0% vs 77.3%; p=0.003). You should explain where those numbers come from. Table 1 does not mention them and they are not found in the table. **[This has been removed as we now have new case definitions.]** The sensitivity reported in this study is of the same caliber as that found in the medical literature. This highlights the limitations of the test (RT-PCR) applied to a healthy population. The sensitivity of the test in sicker patients approaches 85-95%. That should also be mentioned. **[Not addressed as per the editors instruction.]** The frequency of symptoms reported (Nasal congestion, cough, fever, fatigue, anosmia, diarrhea, dyspnea) is for all practical purposes similar to those found in the literature. **[Agree, although I believe we do add some interesting findings about onset of symptoms, and specifically further information around timing of anosmia.]** This study tells us little about the types of symptoms and how often COVID-19 infection occurs in a population. However, it shows us how community-wide spread is possible and rapid among the population and even in healthy population. **[Agree, this was not the purpose of our study. The new Figure 1, highlights that COVID-19 is occurring more commonly now and that we are bracing for the "second wave".]** I propose that the authors agree to the minor corrections or explanations requested and that their study be published. **[Thank you!]**
Reviewer 3	Elise Jackson
Institution	Internal Medicine Residency Program, The University of British Columbia, Vancouver, BC
General comments (author response in bold)	Overall, I do not feel that this paper adds significantly to the COVID literature. The stated objectives of the paper were to "describe the symptoms associated with the coronavirus disease 2019 (COVID-19) and the sensitivity of the reverse transcriptase polymerase chain reaction (RT-PCR) testing" (page 2 line 16). Both of these have been extensively researched and documented in previous literature, and therefore this paper offers very little novel information. As a case report, it may be interesting given the specific context of a group of physicians spreading COVID in a social gathering, but does not contribute to our understanding of the pathophysiology or natural history of the disease. **[Not addressed as per the editors instruction.]** Furthermore, there is information missing that weakens the strength of the

	conclusions. For example, only 6 of the 17 asymptomatic subjects were tested (page 5, line 46) - it would have been helpful to test all subjects in order to truly be able to assess transmission, including asymptomatic spread. I understand that this was a retrospective analysis, and that testing was done based on the guidelines at the time, but this lack of information would also make this more appropriate for a case report. **[This has been reworked as a descriptive study. We agree with the limitations of our paper and have expanded our limitations. The addition of serology also strengthens the paper, although the timing of serology was not ideal either. This is also addressed in the limitations.]** I think more elaboration would also be required about the precautions taken (e.g. were masks used? was transmission increased among those who did not follow appropriate precautions?), **[This has been moved to the Introduction and expanded upon. Everyone present was following the public health guidelines at the time. "In early March, local public health guidelines in Alberta included limiting indoor gatherings to groups of less than 250 persons, and there were no recommendations for using facemasks in public places. During the bonspiel, enhanced safety measures included use of hand sanitizers, discouraging pre- and post-game handshakes, and sanitizing curling stones with disinfectant wipes between games. Despite these measures, an outbreak of COVID-19 occurred following the event."]** what definitions were used to determine whether someone was considered a "presumptive" or "non-specific" case, etc. Particularly, as it is reported that the median duration of symptoms in the "non-specific" cases was 3 days, which seems unlikely to be COVID. **[Thank you for pointing this out. We have now adopted the PHAC and WHO case definitions (Supplemental Materials – Clinical Definitions). Based on these definitions we have only two individuals labelled as "non-specific symptoms" and they are highlighted in the Updated Figure 4 (shown to have very short durations of symptoms). We agree that these are not likely COVID-19 cases and are not counted in our attack rate.]** The calculation of a "76.7% attack rate (confirmed or presumptive cases)" (page 2, line 51) also does not match the data presented in the paper. There were 40 confirmed cases, 11 presumptive and 5 non-specific cases, out of 73 participants. Therefore the attack rate of confirmed + presumptive cases would be (40+11)/73 = 69.9%, whereas the 76.7% would have to include the non-specific cases as well (40+11+5)/73 = 76.7%. Again, given that the median duration of symptoms was 3 days in these patients, it is not clear to me that this truly represents COVID positive patients. **[The attack rate is now estimated to be 74% based on confirmed or probable cases (based on PHAC or WHO criteria). Supplemental Table 2 provides a comparison of PHAC, WHO criteria and our serology results in our 16 symptomatic participants who were RT-PCR negative or if RT-PCR was not done.]** I am also unsure of how the calculation of PT-PCR sensitivity was performed, and am unable to replicated the 70.8% figure based on the data presented. The

	sample size is also small enough such that it seems that a calculation of test sensitivity would be unreliable. **[Not addressed as per the editors instruction.]** Finally, I do not think that the conclusions of the paper are broadly applicable. Given the now widespread use of masks and social distancing, which - from what is reported in the paper about precaution measures - was not done at the bonspiel, the findings are not relevant in the context of what has now been adopted to prevent transmission. Additionally, the only activity that was significantly associated with increased transmission was a buffet-style lunch, which again is not relevant given the societal changes that have since been implemented to reduce spread. **[Respectfully, we disagree. If people were following public health recommendations about masks, and shared meals, I do not think that we would be facing the surge in cases we are seeing following Thanksgiving (see NEW Figure 1). Our experience should act as a reminder to individuals that these new public health measures need to be taken seriously as we face the second wave, while we still try to remain active and well by participating in sports.]**