Reviewer’s report

Title: Prevalence of Noroviruses in Children Hospitalized for Acute Gastroenteritis in Hohhot, China, 2012–2017

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Reviewer: Martin Chan

Reviewer's report:

Li and colleagues described molecular epidemiology of norovirus in young children under 5 in Hohhot, a city in northern part of China over a 6-year period from 2012 from 2017. They found GII.3 was the predominant VP1 genotype. This is interesting because globally GII.4 was the predominant genotype. Similar studies reporting norovirus genotype distribution have been widely available. What this study further adds to the literature is the detection of recombinant noroviruses. However, there are technical concern regarding the robustness of recombinant data that the authors need to address.

Major

1. It is surprising the authors amplified RdRp and VP1 genes by using two separate RT-PCR reactions to detect recombinant noroviruses. Ideally, a single amplicon covering both RdRp and VP1 regions should be used. Otherwise, there is a possibility that any "recombinant" may actually come from two different coinfecting norovirus genotypes. The authors should raise this limitation in their Discussion. I'd also highly recommend the authors to move away from separate PCRs to detect norovirus recombinants in their ongoing and future studies.

2. The authors concluded that the age group 0-23 months was most affected based on the highest number of cases in this group. This is not correct. They should have normalized case numbers with population structure and reported data as XXX cases/100,000 population stratified by age groups.

3. Figure 2 is very difficult to interpret. I'd suggest to just show the monthly number of positive cases (as bars) and monthly positive rate (as a line).

4. Lines 141-143: as I can see from Figure 2, those months with lowest number of cases usually had highest positive rate. Reporting norovirus activity merely on positive rate is vulnerable to small sample size. Should focus on norovirus-positive case number instead.

5. Table 1: add a column to report P values mentioned in the main text.

6. The manuscript would benefit from English and style-editing. E.g., line 21: "children", not "Children".
Minor
1. Line 65: Add a reference to support the claim that GII.3 predominated worldwide in the past decade, or revise.
2. Line 67: Not sure if Yerseke 2006a is a pandemic GII.4 variant.
3. Line 97: "prepared", not "pretreated".
4. Line 100: "real-time RT-PCR", not "Real-time PCR". Add "targeting ORF1/ORF2 junction" after "real-time RT-PCR".
5. Lines 119-120: "Numbers and Excel (MS office 2007), and percentages were computed for categorical." This sentence is difficult to interpret and appears to be incomplete as well.
7. Lines 146-147: why 296+26+10 did not add up to 306?
9. Lines 203-204: I seriously doubt the claim that norovirus activity is the highest in hot seasons in the southern hemisphere. In Australia, for example, norovirus activities usually peak around June to August, which is summer in northern hemisphere but winter in southern hemisphere.
10. Line 206: what do you mean by "easily detected"?
11. Lines 252-253: How did a better understanding of norovirus genotype distribution may help with control and prevention of AGE?
12. Table 2: (1) Would be helpful to further group GII.4 by variants. (2) RdRp, not RDRP
13. Figure 3: X axis label is missing.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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Quality of written English
Please indicate the quality of language in the manuscript:

Needs some language corrections before being published

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