Reviewer’s report

Title: No MERS-CoV but positive influenza viruses in returning Hajj pilgrims, China, 2013-2015

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Reviewer: Joshua Petrie

Reviewer’s report:

The authors present a case report detailing respiratory virus surveillance among pilgrims returning from the Hajj. They tested symptomatic and asymptomatic travelers for influenza, human metapneumo virus, RSV, season coronaviruses, and MERS-CoV. No MERS cases were identified, but influenza was identified in 5.7% of participants, overall, including a substantial proportion of those without fever. My specific comments are below.

Background:

The background is entirely focused on MERS-CoV, which is not sufficient for a study that examined several respiratory viruses and found no MERS-CoV infections. Stating the importance of MERS-CoV surveillance as rationale for the study is fine, but more background on Hajj-associated respiratory infections is needed.

Case Presentation:

"A total of 847 returning pilgrims were recruited…” It is unclear how these participants were actually recruited. Were symptomatic participants identified before being approached to participate in the study. If so, how and by whom?

Were those in the group without symptoms completely asymptomatic or just without fever? Was there any history of symptoms in this group?

Is the total number of Chinese Hajj pilgrims known? Were all points of entry included in the study?

Were each of the specimen types listed collected from each participant? How were specimens tested? Were all PCR, it is not clear?

A significant difference is noted "for influenza A and B between travelers with fever and without symptoms", but it is not clear what is actually being tested here. More importantly, this study was not designed to test any specific hypothesis, but rather to enumerate the number of respiratory infections in travelers returning from the Hajj. Therefore, any statistical testing such as this is really not appropriate.
The numbers of participants are quite a bit different year to year. Is there an explanation for this and how could it have affected results? The 2014-2015 influenza season was particularly severe and here represents the fewest number of participants.

Conclusions:

The word "prevalence" is used in the title and throughout the manuscript. This word has a specific meaning in epidemiologic studies which is different from how the authors use it here.

I would hesitate to conclude that the proportion positive in this study (5.7%) is substantially different from the UK study cited (7%).

The authors state that the studies provide evidence that the proportion of pilgrims with influenza is relatively stable year to year. I would hesitate to make this conclusion given year to year variation in recruitment for this study and variable intensity of annual influenza epidemics.

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

No

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

Unable to assess

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

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:

Needs some language corrections before being published

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