Author's response to reviews

Title: Clinical Features and Risk factors for Severe and Critical Pregnant Women with 2009 Pandemic H1N1 Influenza Infection in China

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Author's response to reviews: see over
Dear Jonathan A McCullers and Tonilynn Manibo,

Thank you for your comments to our manuscript. We are very grateful to have a chance to revise it.

As requested we have dealt with the thoughtful comments of each of the reviewers individually and present these below:

**Point-by-point responses to the reviewers’ comments:**

**Response to Reviewer 1:**

*Discretionary revisions:*

1. Minor grammar points should be corrected in editing: use of the verbs was/were and occasional commas.

**Response:** Thanks for your suggestions. We have corrected the grammatical errors. The manuscript has been checked by the English native speaker to minimize the mistakes.

2. Page 4: This sentence is lacks a verb and the word “succeeded” is misspelled: Among 186 patients who received mechanical ventilation, 83 patients with non-invasive ventilation (NIV) and 38 succeeded.

**Response:** Thanks for your reminder. We had corrected it: Among 186 patients who received mechanical ventilation, 83 patients were treated with non-invasive ventilaton (NIV) and 38 were succeeded.

3. In the discussion, the new text on the unexpectedly low rate of underlying illness could be better focused. Instead of the conclusion that “These findings suggest that environmental factors influence asthma prevalence”, it might be more accurate to say that “These results suggest that the lower prevalence of pre-existing asthma in our sample reflects prevalence of the disease in the Chinese population”.

**Response:** Thanks for your good suggestion. In the discussion, we had corrected it as your suggestion: These results suggest that the lower prevalence of pre-existing asthma in our samples reflects prevalence of the disease in the Chinese population.

4. I am not sure about the conclusion: “Another reason for the low prevalence of underlying diseases among pregnant is that 73% of our patients came from rural area or those who were unemployed. The poor living conditions made them less likely to have health checkup before pregnancy.” It seems likely that underlying conditions would have been identified during the hospital admission, so that the hypothesis of a lack of detection prior to admission was an
explanation for an underlying comorbid illness is hard to support. Perhaps these sentences could simply be deleted.

Response: Thanks for your advices. In fact, there is no causal connection between the poor living conditions and the low prevalence of underlying diseases. The underlying conditions would have been identified during the hospital admission. So, physical examination after admission does not affect the incidence of the underlying disease. We agree to delete this explanation.

5. The sentence: “Risk analysis showed that BMI, and a PaO2/FiO2 ≤ 200 were risk factors for maternal death” should be modified to indicate the at risk BMI and note that the value was measured on admission (i.e. >30).

Response: Thanks for your suggestion. We had added BMI as a risk factor in the manuscript and noted the value was measured on admission (i.e. ≥30).

6. In the conclusions, the authors state: “septic shock was one of the contraindications”, when they actually probably mean that “septic shock should be considered a contraindication”

Response: Thanks for your good suggestion. In the revised version, we had re-written the conclusions as you suggested: Septic shock should be considered a contraindication for pregnant women when they treated with NIV.

Response to Reviewer 2:

Major Compulsory Revisions

Major Compulsory Revisions

1. It is still not clear how the final multivariable models were selected. From your response I understand that variables with a statistical significance p<0.05 in univariate analyses were included in the multivariate models. Were all these variables retained in the final multivariable models? Since in tables 3, 4 and 5 only 1 or 2 variables per multivariable analysis are presented, one may think that only variables showing a certain level of significance were retained in the final models and presented in the tables. Does that mean that all other variables were not longer significant after adjusting? If all variables introduced in the multivariable analyses were retained in the final models, their corresponding odds ratios should be presented.

Response: Thanks for your good advice. All variables with a statistical significance p<0.05 in univariate analyses were retained in the final multivariable models. In the old version, all other variables we had not presented in the tables were no longer significant after adjustment. According to your suggestion, we had presented the all corresponding odds ratio which was retained in the final models. Please see the new table 3,4,5.
2. Model for factors associated with death: in table 3, in addition to PaO2/FiO2, higher BMI is also independently associated with death. This should be mentioned in the manuscript and in the abstract.

Response: Thanks for your good suggestion. In the manuscript and in the abstract, we had added higher BMI is also independently associated with death.

Minor Essential Revisions

3. Abstract, methods:
Details concerning the RT-PCR are not necessary. Should include a brief description of statistical analysis

Response: Thanks for your suggestion. We had re-written the part of abstract, methods.

4. Introduction, line 4: specify country

Response: Thanks for your suggestion. We had added the country in the new version.