## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

## ARTICLE DETAILS

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| **TITLE (PROVISIONAL)** | Effect of Modeling Slum Populations on Influenza Spread in Delhi |
| **AUTHORS** | Chen, Jiangzhuo; Chu, Shuyu; Chungbaek, Youngyun; Khan, Maleq; Kuhlman, Christopher; Marathe, Achla; Mortveit, Henning; Vullikanti, Anil; Xie, Dawen |

## VERSION 1 - REVIEW

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| **REVIEWER** | Ayaz Hyder, Assistant Professor<br>College of Public Health, The Ohio State University, United States |
| **REVIEW RETURNED** | 01-Apr-2016 |

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| **GENERAL COMMENTS** | In this study, the authors simulated the spread of influenza using a network model with and without slum neighborhoods in Delhi, India. The main finding of the study was that inclusion of slums in the network model increased the speed and intensity of the epidemic. These results were sensitive to the transmission but not seeding (of initial infections) assumptions in the model. The authors suggested that their results are important for policy makers to consider in light of the significant differences due to accounting for slums in the system. I have some major and minor comments on this submission that I hope will be helpful for the authors to consider.<br><br>Major comments:<br>1. A main concern I have is regarding model validation. There seems to be none in the submission, which is somewhat alarming since data on influenza infections probably exists for a large city like Delhi, India. Without model validation the results remain purely theoretical. There are several ways for the authors to validate their results. First, they could estimate the reproductive number (R0) from previous influenza epidemics in the city and calibrate their model to within the range of past epidemic dynamics. Second, the authors could calculate several of the epidemic metrics, which they have defined in the current study, from past epidemics and see how well simulated and observed epidemic metrics match. Lastly, some sort of validation of the attack rate or clinical attack rate by age group would be most helpful as well.<br><br>2. Another main concern I have is how the comparison is done between epidemics on each network (Network 1 and Network 2 in the submission). The stated objective of the study is to evaluate the effect of slums on infectious disease dynamics. For this objective, the approach taken by Laskowski et al. (2011) may be more appropriate where they studied how shifted demographics affected epidemic metrics. The current approach of the authors does not seem appropriate because it does not specify what the baseline model looks like. In other words, is Network 1 how the real world looks like such that adding slum demographic and activity patterns is |

a more realistic representation of the real world? If so, then the next question is whether epidemic dynamics are similar under the current spatial configuration of slums and what would happen if we randomly distributed slums throughout Delhi? If epidemic dynamics were similar under both scenarios, then spatial configuration of slums does not matter and everything is simply being driven by demographics and activity patterns. If epidemic dynamics were not similar, then spatial configuration does matter and more work would need to be done to determine any additional impact of demographics and activity patterns.

3. Throughout the submission the authors point to how much worse the epidemic would be if slums are not incorporated into the model. This is somewhat misleading without establishing how epidemic impacts are currently being modeled, which is not mentioned in the current submission.

4. I think much more sensitivity analysis should be done to properly assess the reliability of the numerous parameters that are involved in ABMs. For example, the authors do not show a more realistic seeding scenario where infections would be seeded randomly regardless of whether they occurred in slums or not.

5. In several places, there is very little or no detail or references for how the activity data were collected, assessed for quality, and description of uncertainty or possible measurement errors in the data. For example, on Page 5, the paragraph on population generation lists partnerships with commercial companies but the reader has no idea of how that data was collected and what it looks like. Also, would that data be open to others to replicate this study. A bit more of how the population was generated should be included in this study rather than just referring the reader to previous work. At the bottom of page 5, the authors mentions number of zone by categories of population size without giving any references to where the data source. Same goes for household size and activity patterns mentioned on page 6, infectious and incubation period on page 7.

Minor comments:
1. What software was used in the modeling and analysis?
2. It would be good to describe what “varied activities” means on line 12 on page 6.
3. Page 7, line 14, later should be latter.
4. Since the focus is on flu, it may be useful to quantify “mild, strong and catastrophic” into effective R0 or the basic R0. Does catastrophic mean pandemic flu?
5. Page 10, line 28, please give the reader an idea about the regression analysis. Was it a GLM model? How were variables selected? Was is backward or forward selction? Also, why not include other sociodemographic and economic variables? These additional variables are probably more predictive of the burden of flu, similar to work by Charland et al. (2011).

Laskowski, Marek, et al. "The impact of demographic variables on disease spread: influenza in remote communities." Scientific Reports 1 (2011).

Charland, Katia M., et al. "Socio-economic disparities in the burden of seasonal influenza: the effect of social and material deprivation on rates of influenza infection." PloS one 6.2 (2011): e17207.

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| **REVIEWER** | Supriya Kumar<br>University of Pittsburgh, USA |
| **REVIEW RETURNED** | 22-Apr-2016 |

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| **GENERAL COMMENTS** | This is a very interesting study. My main critique of the manuscript is its lack of detail in describing the data that went into parameterizing contacts in slum v/s non-slum populations. What were the data, how were they collected (was there informed consent?), how were they analyzed, and have they been presented elsewhere? If not, this analysis should be first presented here. The study is an important addition to the literature, but its data sources need to be explained and cited clearly to promote confidence in the study's accuracy. Though two previous papers on methods used to create these populations have been cited, a more thorough description of the sources of data and the methods is warranted here.<br><br>It is not clear why Network 1 is "slum free." Is this not also a network based on census data and LandSat imagery? Why are slum household sizes the same as non-slum household sizes in Network 1? And if Network 2 is more realistic because of additional contact data, those data and analyses that went into using them in these models must be presented in this paper so that we can gauge how realistic the Network 2 model is.<br><br>Minor comments are presented by page and line number below.<br><br>Page 3<br>Line 43: It is unclear why the study assumes different daily activities of slum residents compared to non-slum residents.<br><br>Page 4<br>Line 12: Results are presented in the introduction. We're not used to seeing this format in Public Health, but I leave it to the journal editors to make a call on this.<br><br>Line 32: "Replicates" should be changed to "runs" if appropriate. I assume the multiple runs are not really replicates of each other.<br><br>Line 42: The study seems to draw an analogy between slums and poor, dense areas, in general. Slums may be an extreme case of such high-density areas, which are also visible in developed countries. It would strengthen the introduction to clearly state this, citing all the papers that have found a relationship between poverty and influenza in the US of late (Hadler, Sloan, Yousey-Hindes, Kumar, etc.), as well as a relationship between poverty (in slums) and ebola in Liberia.<br><br>Page 5<br>Line 28: Some detail on how ABMs are better than other models at handling heterogeneity in individual characteristics and contact networks would be good.<br><br>Line 47: Thus the number of individuals are the same in both populations. Do the authors mean networks (1 and 2)?<br><br>Line 53: Where do these data or estimates of the population in each slum zone come from?<br><br>Page 6 |

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| | Line 7: what was the distribution of household sizes in these two populations? The average is less informative than the distribution.<br><br>Line 11: the activity survey is mentioned. How were individual activities collected and analyzed? Are these presented elsewhere? Can a summary be presented here?<br><br>Line 15-18: Again, please cite a study or present the analyses in more detail. Did degree differences vary by age? We might expect that the difference between slum and non-slum degree would be greater among adults than among children. Was this the case, and if yes, did it impact age-specific infection rates? How closely did agent behavior in the model match behavior observed in the survey?<br><br>Line 34: how much data (from how many individuals) was this average duration based on?<br><br>Line 46 reads like a discussion section. I personally like this style of discussing the implications of findings before even presenting the model. However, it is not what some may be used to in the public health field.<br><br>Page 7<br>Line 21. Citation required.<br>Line 25: are there asymptomatic agents?<br><br>Page 8<br>Line 9: it is unclear what is meant by “non-conservative” in this context.<br>Line 18: for mild flu, there appears to be hardly an epidemic on network 1. Maybe state this rather than present what seems like an extremely high estimate of number of days earlier?<br>Line 35: Figure 2?<br><br>Page 9<br>Line 9: are people placed in geographic subgroups (slum v/s non-slum) based on residence or where they got infected?<br><br>Line 19: are these cumulative attack rates?<br><br>Line 30: this should be part of the discussion. A similar result was observed by Kumar et al. with inequalities decreasing with increasing transmissibility of the virus. This should be cited and discussed in the context of the present study.<br><br>Line 32: "the changes in contact rates" should be changed to "the higher contact rates" if appropriate.<br><br>Line 50: can the authors preface this section with a rationale for this experiment? What seeding scenario might be expected in reality? Do national or international migration patterns suggest particular seeding patterns? Or discuss this in the discussion section?<br><br>Page 10<br>Line 34: In many parts of the paper, cumulative attack rate may be a clearer term than infection rate.<br><br>Page 11:<br>Table 1: Can we see standardized coefficients so that we can compare between factors? |

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| | p-values: <0.001 may be a more appropriate way to present this. No need to present a significance column when p-values are presented.<br><br>Page 12<br>Line 16: It would be interesting to see if random seeding results in different epidemic rates in slum and non-slum populations. This would tell policy makers how many weeks ahead of non-slum areas slum areas reach some relevant threshold attack rate, and whether it would be useful to target slum areas with resources before non-slum areas.<br><br>Discussion in general: Why do the authors think females had a higher attack rate? What can they tell us about gender differences in contact numbers, duration, and mixing with children? We might expect that women worked outside the home at a lower rate than did men. Was this true? If yes, is the result of higher attack rate among women counter-intuitive? How does it tie in with observed higher mortality among women during the 1918 pandemic in India?<br><br>A paragraph on limitations would be useful. What was left out of the model? Sanitation, behavior, etc. |

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| **REVIEWER** | Yongkuk Kim<br>Dept. of Mathematics & Institute for Mathematical Convergence, Kyungpook National University, KOREA |
| **REVIEW RETURNED** | 26-Apr-2016 |

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| **GENERAL COMMENTS** | 1. Please clarify the claim: "Results are not sensitive to the number of initial infections" on page 7.<br>1. In the reference 19, Epidemics. 2017 should read as Epidemics. 2011<br>1. It would be better to discuss 'the effect of intervention' in the Discussion section. |

**VERSION 1 – AUTHOR RESPONSE**

Response to Reviewers' Comments on Manuscript: Effect of Modeling Slum Populations on Influenza Spread in Delhi

We would like to thank the editor and the reviewers for their helpful and constructive comments. Please find below the editor's and reviewers' comments, each followed by our response.

General Comment
The reviewers asked for more data in a number of areas. We attempted to satisfy these requests herein, as itemized below. Before doing so, we wish to state that we omitted lot of details in order to keep the text and number of figures within the prescribed limits. Much of our data are not amenable to verbal descriptions; plots are necessary to represent the data. We have, accordingly, prepared supplemental information (SI) to provide these data.

Response to editor's comments
1. Please add more detail to the title e.g. include the study setting.

The title has been
changed to "Effect of Modeling Slum Populations on Influenza Spread in Delhi"

to include the study setting.

2. ‘Our results show:..’ onwards in the Introduction. Are you referring to the results of the current study? If so then this paragraph should be removed/ moved to the Discussion section.

Yes, we are referring to the results of the current study. Per your suggestion, it has been moved to the Results and Discussion sections.

Response to reviewer 1’s comments
1. Major Concerns:
(a) A main concern I have is regarding model validation. There seems to be none in the submission, which is somewhat alarming since data on influenza infections probably exists for a large city like Delhi, India. Without model validation the results remain purely theoretical. There are several ways for the authors to validate their results. First, they could estimate the reproductive number (R0 ) from previous influenza epidemics in the city and calibrate their model to within the range of past epidemic dynamics. Second, the authors could calculate several of the epidemic metrics, which they have defined in the current study, from past epidemics and see how well simulated and observed epidemic metrics match. Lastly, some sort of validation of the attack rate or clinical attack rate by age group would be most helpful as well.

This paper does not simulate a particular outbreak in India, rather it is a parametric study. The goal of the paper is to show that for a given scenario, how the outcomes differ between network 1 and network 2. In other words, given the same set of parameters, the results are significantly different when slum specific attributes are accounted for, as is the case in network 2. To check the robustness of the results, we use 3 different transmission rates (corresponding to R0 1.05, 1.26, 1.40 for network 1 and 1.123, 1.39 and 1.54 for network 2). R0 corresponding to catastrophic flu is similar to estimated R0 of 1.45 for H1N1 in India [1]. Authors in [2] considers a R0 of 1.35 in Delhi. This information has been added to Table 1 in the revised paper as well as to the “Disease Model” in the paper.

(b) Another main concern I have is how the comparison is done between epidemics on each network (Network 1 and Network 2 in the submission). The stated objective of the study is to evaluate the effect of slums on infectious disease dynamics. For this objective, the approach taken by Laskowski et al. (2011) may be more appropriate where they studied how shifted demographics affected epidemic metrics. The current approach of the authors does not seem appropriate because it does not specify what the baseline model looks like. In other words, is Network 1 how the real world looks like such that adding slum demographic and activity patterns is a more realistic representation of the real world? If so, then the next question is whether epidemic dynamics are similar under the current spatial configuration of slums and what would happen if we randomly distributed slums throughout Delhi? If epidemic dynamics were similar under both scenarios, then spatial configuration of slums does not matter and everything is simply being driven by demographics and activity patterns. If epidemic dynamics were not similar, then spatial configuration does matter and more work would need to be done to determine any additional impact of demographics and activity patterns.

The state of the art representation of the social network of Delhi is given in Xia et al. (2015). This is being treated as a baseline and is labeled as “Network 1” in our study. Network 2 adds slum demographic and activity

patterns to households in Network 1 that are located in the slum regions of Delhi. These slum regions and their spatial configurations are not randomly assigned; slum polygons data is obtained from MapMechanics.com and used to map specific spatial locations of slums in network 2. We have added sentences to the manuscript to address these issues, to make them clearer.

(c) Throughout the submission the authors point to how much worse the epidemic would be if slums are not incorporated into the model. This is somewhat misleading without establishing how epidemic impacts are currently being modeled, which is not mentioned in the current submission.

Current models are either compartmental models or scaled lattice representations which do not have the spatial resolution to represent slums, or they are network models like Network 1. Currently, there is no model in the literature that considers the role of slums in measuring epidemic dynamics. This is why Network 1 is so useful as a base case and why we consider it, and the influenza epidemic results generated with it: it provides a very large and detailed social contact network whose properties we know, and that does not account for slums. The starting point for the slum-inclusive Network 2 is Network 1; therefore we can contrast properties of Network 1 with those of Network 2 as we do in the SI.

(d) I think much more sensitivity analysis should be done to properly assess the reliability of the numerous parameters that are involved in ABMs. For example, the authors do not show a more realistic seeding scenario where infections would be seeded randomly regardless of whether they occurred in slums or not.

Figure 4 in the original manuscript shows the impact of seeding infections in slum regions, non-slums regions and randomly in the entire region.

(e) In several places, there is very little or no detail or references for how the activity data were collected, assessed for quality, and description of uncertainty or possible measurement errors in the data. For example, on Page 5, the paragraph on population generation lists partnerships with commercial companies but the reader has no idea of how that data was collected and what it looks like. Also, would that data be open to others to replicate this study. A bit more of how the population was generated should be included in this study rather than just referring the reader to previous work. At the bottom of page 5, the authors mentions number of zone by categories of population size without giving any references to where the data source. Same goes for household size and activity patterns mentioned on page 6, infectious and incubation period on page 7.

A supplementary information section has been added where details on survey data and population generation methods have been provided. MapMechanic.com is given under Methods as the source of the Delhi slum geographic zones. The method for determining the number of people in a slum zone is described, and a plot of slum zone population size distribution is given in SI. The generation of households inside of slums is described. The generation of the social network from the population is also described. A plot of household size distributions is given in SI. Also, references are given for the incubation and infectious period distributions used in this work. These expanded descriptions and references to SI are given under Methods.

2. Minor comments:
• What software was used in the modeling and analysis?

Modeling and analysis was done using EpiFast. This has been added to the Disease Model along with a reference for EpiFast.

• It would be good to describe what “varied activities” means on line 12 on page 6.

We now address this in the SI. Figure 3 of SI shows the number of edges of each type: home, work, shopping, other, school, and college. For each of these types, the following are provided: the number of edges where both individuals are slum, both individuals are nonslum, one individual is slum and the other nonslum, and the total number of edges over the entire network. The “other” category of edge means edges that represent any activity not included in the other five activity categories. The number of slum-to-slum edges that are “other” type is about 4× that for nonslum-to-nonslum edges. This is all the more significant because about 87% of individuals are nonslum. This is what we mean by saying: “According to the activity survey, slum individuals have more varied activities than non-slum individuals.” This is in the SI now.

• Page 7, line 14, later should be latter.

Fixed.

• Since the focus is on flu, it may be useful to quantify mild, strong and catastrophic into effective R0 or the basic R0. Does catastrophic mean pandemic flu?

Mild, strong and catastrophic correspond to R0 1.05, 1.26, 1.40 for network 1 and 1.123, 1.39 and 1.54 for Network 2. This has been added to Table 1 in the revised paper and Disease Model under Methods. Catastrophic can be interpreted as pandemic flu. The R0 for catastrophic is close to the estimated R0 for H1N1 2009 pandemic in Delhi.

• Page 10, line 28, please give the reader an idea about the regression analysis. Was it a GLM model? How were variables selected? Was is backward or forward selection? Also, why not include other sociodemographic and economic variables? These additional variables are probably more predictive of the burden of flu, similar to work by Charland et al. (2011).

The model applied here is a linear regression model. We began with a large number of independent variables which included slumzone population, average degree for each different activity (home, work, shopping, other, school, college), average degree for all activities in total, number of edges in each slumzone, network density in each slumzone, average degree within slumzones for each of the six activity types and in total, average degree of nodes connected to nonslums for activity types 2-6 and total and average household size in each slum zone. Next we identified the mutually correlated variables which were average degree for home, average degree for shopping, average degree within slum for home, and all average degree connected in non-slum. We removed the correlated variables.

Then we conducted variable selection using using a bidirectional elimination method, which is a combination of both forward and backward elimination, testing at each step for variables to be included or excluded. It corresponds to R function “both.” These details have been added to the paper in the Results Section.

Response to reviewer 2’s comments

1. Major comments:

(a) This is a very interesting study. My main critique of the manuscript is its lack of detail in describing the data that went into parameterizing contacts in slum v/s nonslum populations. What were the data, how were they collected (was there informed consent?), how were they analyzed, and have they been presented elsewhere? If not, this analysis should be first presented here. The study is an important addition to the literature, but its data sources need to be explained and cited clearly to promote confidence in the study’s accuracy. Though two previous papers on methods used to create these populations have been cited, a more thorough description of the sources of data and the methods is warranted here.

More details on the data, construction of the population, and data analysis has been added to Section Methods of the paper. There is additional detail in the new Supplemental Information (SI).

(b) It is not clear why Network 1 is “slum free.” Is this not also a network based on census data and LandSat imagery? Why are slum household sizes the same as non-slum household sizes in Network 1? And if Network 2 is more realistic because of additional contact data, those data and analyses that went into using them in these models must be presented in this paper so that we can gauge how realistic the Network 2 model is.

Yes, Network 1 is based on census data and it contains the same number of individuals as Network 2. However Network 2 individuals differ from Network 1 in terms of their activities. Even though the types of activities performed are the same, the duration and frequency of the activities are different. Also the number of people per household in the slum zones of Network 2 are different than Network 1. The average slum household size is about 3 times that of the average non-slum household size. The details of slum versus non-slum activities and other relevant information has been added to the Methods section and in SI.

2. Minor comments:

• Page 3, Line 43: It is unclear why the study assumes different daily activities of slum residents compared to non-slum residents.

The activity types are the same but the activity patterns in slum and nonslum are different.

• Page 4, Line 12: Results are presented in the introduction. Were not used to seeing this format in Public Health, but I leave it to the journal editors to make a call on this.

The results have been moved to “Results” and “Discussion” sections.

• Line 32: Replicates should be changed to runs if appropriate. I assume the multiple runs are not really replicates of each other.

Replicates are often used to capture random variation in the experiment, but both “runs” and “replicates” have been used in the literature. It has been changed to runs in the paper.

• Line 42: The study seems to draw an analogy between slums and poor, dense areas, in general. Slums may be an extreme case of such high-density areas, which are also visible in developed countries. It would strengthen the introduction to clearly state this, citing all the papers that have found a relationship between poverty and influenza in the US of late (Hadler, Sloan, Yousey-Hindes, Kumar, etc.), as well as a relationship between poverty (in slums) and ebola in Liberia.

This has been added to the Introduction, along with the references Hadler et al. 2016, Sloan et al. 2015, Yousey-Hindes et al. 2011 and Kumar et al. 2015.

• Page 5, Line 28: Some detail on how ABMs are better than other models at handling heterogeneity in individual characteristics and contact networks would be good.

More details have been added at the end of the Methods Section.

• Line 47: Thus the number of individuals are the same in both populations. Do the authors mean networks (1 and 2)?

Yes. It has been fixed in the manuscript.

• Line 53: Where do these data or estimates of the population in each slum zone come from?

MapMechanics.com provides the GIS information on the spatial location of slum polygons. This is explained in the Methods Section.

• Page 6, Line 7: what was the distribution of household sizes in these two populations? The average is less informative than the distribution.

The distributions are now in a figure in the SI.

• Line 11: the activity survey is mentioned. How were individual activities collected and analyzed? Are these presented elsewhere? Can a summary be presented here?

A brief summary has been added in SI.

• Line 15-18: Again, please cite a study or present the analyses in more detail. Did degree differences vary by age? We might expect that the difference between slum and non-slum degree would be greater among adults than among children. Was this the case, and if yes, did it impact age-specific infection rates? How closely did agent behavior in the model match behavior observed in the survey?

Yes, we have added this in the SI. We provide plots by activity types; by

durations of activity types; by slum, nonslum, mixed, and total groups of individuals; by age; and by adults interacting with children. Figure 3 in the manuscript shows that age specific infection rates are different.

• Line 34: how much data (from how many individuals) was this average duration based on?

These numbers are based on all of the data of Network 2. In fact, there is now a plot in the SI that breaks these data down by activity type, by types of individuals forming edges, and in a last column in the plot, an “all” category shows precisely the values that support this statement in the main text. This is all called out in the Methods Section, including details of the plot.

• Line 46 reads like a discussion section. I personally like this style of discussing the implications of findings before even presenting the model. However, it is not what some may be used to in the public health field.

It appears more readable in its current place, but will make a reference elsewhere if you wish.

• Page 7, Line 21. Citation required.

A citation has been added.

• Page 7, Line 25: are there asymptomatic agents?

These are just susceptible individuals. Not infected (symtomatic or asymptomatic). All exposed agents become symptomatically infected.

• Page 8, Line 9: it is unclear what is meant by non-conservative in this context.

It meant estimates will be under-estimated. Have changed “non-conservative”.

• Page 8, Line 18: for mild flu, there appears to be hardly an epidemic on network 1. Maybe state this rather than present what seems like an extremely high estimate of number of days earlier?

We state this as suggested but just for completeness, we keep the number as well.

• Page 8, Line 35: Figure 2?

Yes figure 2. Fixed.

• Page 9, Line 9: are people placed in geographic subgroups (slum v/s non-slum) based on residence or where they got infected?

Based on residence.

• Page 9, Line 19: are these cumulative attack rates?

Yes, cumulative. Have updated the paper to clarifiy this.

• Page 9, Line 30: this should be part of the discussion. A similar result was observed by Kumar et al. with inequalities decreasing with increasing transmissibility of the virus. This should be cited and discussed in the context of the present study.

Done.

• Page 9, Line 32: ”the changes in contact rates” should be changed to ”the higher contact rates” if appropriate.

Done.

• Page 9, Line 50: can the authors preface this section with a rationale for this experiment? What seeding scenario might be expected in reality? Do national or international migration patterns suggest particular seeding patterns? Or discuss this in the discussion section?

A paragraph has been added to preface this section. The seeding scenarios considered in the paper are all plausible scenarios. Cholera epidemic in the coastal slums of West Africa demonstrated how epidemics can quickly spread in slums. More text on this topic has been added to the discussion section.

• Page 10, Line 34: In many parts of the paper, cumulative attack rate may be a clearer term than infection rate.

Done.

• Page 11: Table 1: Can we see standardized coefficients so that we can compare between factors? p-values: < 0.001 may be a more appropriate way to present this. No need to present a significance column when p-values are presented.

The table has been modified as requested. The coefficients are comparable across the three models. Note the dependent variable Y represents cumulative infection rates for mild, strong and catastrophic flu.

• Page 12, Line 16: It would be interesting to see if random seeding results in different epidemic rates in slum and non-slum populations. This would tell policy makers how many weeks ahead of non-slum areas slum areas reach some relevant threshold attack rate, and whether it would be useful to target slum areas with resources before non-slum areas.

Results of random seeding (total Delhi population), are shown in Figure 4 of the original paper. A follow on paper will discuss the problem of resource allocation in slum and non-slum areas.

• Discussion in general: Why do the authors think females had a higher attack rate? What can they tell us about gender differences in contact numbers, duration, and mixing with children? We might expect that women worked outside the home at a lower rate than did men. Was this true? If yes, is the result of higher attack rate among women counterintuitive? How does it tie in with observed higher mortality among women during the 1918 pandemic in India?

We have two plots in the SI that address these questions. One is a plot of average degree, by age, broken down by male/female and slum/nonslum. We find that in the slums, females in the 20-60 age range have greater degree than males. Also, females have more connections to children than do males. For this latter point, in many epidemic settings, children are major transmitters of disease. This is because they have many connections and they are longduration (at school). Thus, women having greater infection rates than men is consistent with these observations.

• A paragraph on limitations would be useful. What was left out of the model? Sanitation, behavior, etc.

This has been added to the discussion.

Response to reviewer 3's comments

1. Please clarify the claim: "Results are not sensitive to the number of initial infections" on page 7.

This meant that whether the initial infections were 20 or less or more, the outcomes were the same. In other words, the initial condition with respect to the number of infections did not change the results. Additional clarification has been added to the paper.

2. In the reference 19, Epidemics. 2017 should read as Epidemics. 2011

Fixed.

3. It would be better to discuss 'the effect of intervention' in the Discussion section.

The interventions will be studied in detail in a complementary paper. They are outside the scope of this study.

References

[1] Jesan T, Menon G, Sinha S. Epidemiological dynamics of the 2009 influenza A(H1N1)v outbreak in India. Current Science (00113891); 4/10/2011, Vol. 100 Issue 7, p1051.
[2] Xia H, Nagaraj K, Chen J, Marathe MV. Synthesis of a high resolution social contact network for Delhi with application to pandemic planning. Artif Intell Med 2015;65(2):113-130.
[3] K. Bisset and M. Marathe. A cyber-environment to support pandemic planning and response. DOE SciDAC Magazine 2009, 13, 36-47.
[4] Bisset, K., Chen, J., Feng, X., Kumar, V.A., Marathe, M.: EpiFast: a fast algorithm for large scale realistic epidemic simulations on distributed memory systems. In: Proceedings of the 23rd International Conference on Supercomputing (ICS), pp. 430439 (2009)

**VERSION 2 – REVIEW**

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| **REVIEWER** | Ayaz Hyder<br>Ohio State University, United States |
| **REVIEW RETURNED** | 15-Jun-2016 |

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| **GENERAL COMMENTS** | Great job at the revisions. |

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| **REVIEWER** | Supriya Kumar<br>Graduate School of Public Health, University of Pittsburgh, Pittsburgh, USA |
| **REVIEW RETURNED** | 21-Jun-2016 |

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| **GENERAL COMMENTS** | This is an important addition to the literature, focusing on the need to include additional detail on demographic factors (household size) and activities (encounters in space and time) in agent-based models. The authors have done a good job addressing my concerns. I would suggest clarifying whether this is a model of Delhi state, or the municipal corporation. I also suggest including a statement about whether IRB clearance was sought for the surveys in Delhi. There are a few remaining minor comments below.<br><br>Page 3<br>Please standardize the tense in the introduction. Paragraph 2 uses past tense, while paragraph 3 uses present tense. Remove "Authors in" in line 39. Please cite Fallah et al. 2015.<br><br>Page 13 line 47<br>Were models compared using AIC (in R-function “both”)? If yes, this should be stated. Please clarify if inclusion of both average degree and average household size was warranted by AIC or similar comparison between models.<br><br>Page 14. Table caption.<br>It is unclear to me what the last sentence means.<br><br>SI file<br>Page 1, Paragraph 1:<br>“typical census data”<br>Does the census provide average household size or microdata for sub-regions within the city? If it is like the US, we would expect household sizes to reflect reality in smaller geographic areas like the equivalent of census tracts. If such data are not available, this should be stated to explain what "typical" census data are in India. Currently, I am still confused as to why Network 1 is not a better reflection of realistic household sizes.<br><br>Table 1<br>reversed Network 1 and Network 2 cumulative rates?<br><br>Survey data<br>Was duration collected in seconds? Is this similar to the American Time Use Survey? Were geo-locations of work, school, college, shops, travel, and other collected? If not, how were these assigned in the model?<br><br>Figures 4 and 5 report time in seconds, but the description of the survey suggests respondents reported hourly activities. Please clarify. What was the time-step in the model?<br><br>Figure 6 is an important addition. Was average number of contacts within home > outside home for women? What was the age breakdown of survey respondents? Was there a sufficient sample size to make conclusions for each age-year? |

## VERSION 2 – AUTHOR RESPONSE

SI file

1. Census data is by districts and sub-districts.
2. You are right; the cumulative rates as reported are indeed reversed. Thank you for catching our error.
3. Duration was collected in hourly intervals for India but the time step in the model is in seconds so as to capture interactions between individuals at higher resolution levels for other populations for which the data is available in seconds or minutes. Geolocations of work, school, college, shop etc. were collected by MapMyIndia.com
4. Yes, average number of contacts within home is greater than outside home for women. Two new figures have been added in the SI file. Figure 1 provides the age distribution of the survey respondents and Figure 8 provides the average degree of males and females for home and non-home activities.