

Interactive comment on “Global radiative effects of solid fuel cookstove aerosol emissions” by Yaoxian Huang et al.

Anonymous Referee #2

Received and published: 4 February 2018

The manuscript by Huang et al. presents a modeling study of the radiative effects of solid fuel cookstoves, both globally and specifically in India. There is a lot of scientific and policy interest in this topic, given the potential climate co-benefits of cookstove intervention programs and the uncertainty associated with quantifying this, with several recent papers making a range of estimates that differ in sign. The contribution of this article is a welcome addition to the field, focusing particularly on details of aerosol cloud interactions, and considering the effect of BC ice nucleation, which has not been considered in such studies previously. Overall the manuscript is thus appropriate in scope for ACP. It is also generally clear, well organized, and easy to read. I only have a few comments that are detailed below; there is some ambiguity regarding how the authors are arriving at their uncertainty estimates, and some of the motivation for the

C1

scope of their analysis (e.g., considering just India, or not considering the impact of co-emitted GHGs) could be strengthened. Addressing these would constitute minor revision.

Major Comments:

37: An important conclusion, which I mainly agree with in spirit. However, it might be stated a little bit softer for a few reasons. First, the uncertainty of up to a factor of two in estimating concentrations would seem to contribute to the overall uncertainty in the net radiative effects. Second, this study is of radiative effects, not the climate response, and the wording should reflect that. Third, it's the result of only a single model, which may not be as definitive as presented.

The measurements used for comparison come from very different time periods (2010 for IMPROVE, 2009-2013 for Europe, 2000-2008 for AMS data, and 1993-2016 for AERONET). How does this impact the evaluation of modeled concentrations and AOD, given that the model uses emissions from 2010 and that there have been large changes in emissions over this time period?

215 - 220: Some previous studies have suggested that the resolution of global scale models leads to a bias that makes it difficult to match AOD from AERONET in these regions. Could this partially explain the low bias?

Fig 6 and Section 3.4.1 (and everywhere these numbers are quoted in the text): Suddenly the results have errors associated with them (concentrations and AOD did not. . .). What is the meaning of the error estimates? Are these the standard deviations over the timeframe modeled? If so, that needs to be more clearly stated when presenting these numbers in the abstract and conclusion (that +/- is modeled temporal standard deviation). And then I wonder why similar deviations were not considered for discussion of concentrations or AOD. Further, temporal variability is very different than e.g. an estimate of uncertainty owing to sources of model error or approximations, such as the ranges provided for the RF of the simulations including BC IN that stem from uncer-

C2

tainty in the MFE. These ranges can't be directly compared, and yet they're presented in e.g. the abstract without distinguishing their different meanings. At present the non BC-IN ranges come across as uncertainty estimates that seem much too small (I doubt the authors believe that the aerosol RF in any single model could be that accurate).

Introduction: I didn't get a good sense from the introduction why there is a particular interest in India as separate from the globe in this study (as opposed to China, or any other country with significant cookstove use). I'm not suggesting that the authors do more simulations for other regions, but if they wish to include the India-specific results it would make more sense to include a bit more rationale for this emphasis.

439 - 447: I think it's worth recognizing that there are climate impacts from GHG emissions as well. So, considering not just the aerosol emissions, these may be large enough to make the net climate impacts of cookstove emissions positive (Lacey 2017). It is somewhat artificial to envision a scenario wherein only the carbonaceous aerosol cookstove emissions are affected by stove replacements.

Minor Comments:

12: Not clear – “updated” related to what? A particular previous study? Later it becomes evident what is meant (first to include BC as IN), but perhaps it could be worded differently here.

74: Clarify whether the Butt 2016 study included just aerosols or also GHGs in the DRE.

80: Similarly, Ethiopia is the 3rd largest in Lacey 2017, but that's including GHGs in 2050, which is a slightly more specific statement than as presented here.

128: CAM5-Chem not CAM5-chem

Fig 1: Sorry if I just missed it, but did the authors state how they are defining urban vs rural in their classification of measurement sites?

C3

276-279: What is the BC mass absorption coefficient (MABS) at 550 nm in this model?
See e.g. Koch ACP 2009

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-894>, 2017.

C4