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***Interactive comment on* “Standard climate models radiation codes underestimate black carbon radiative forcing” by G. Myhre and B. H. Samset**

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Anonymous Referee #3

General comment: This paper illustrates that the radiative forcing of BC in climate models is sensitive to the number of streams in the radiative transfer model. It shows that the 2-stream approximation led to a 10% underestimation of BC RF compared to executing the radiation code with 8-streams. The low bias is more severe over low albedo surfaces and under cloud-free conditions. The physical mechanism for this bias is fully explained and explored in the paper. The authors show that the low bias arises because 2-stream models forward scatter too much radiation leading to lower mean path length of solar radiation through the atmosphere, and hence reduces the opportunities

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for absorption. The result will presumably apply to any absorber, BC, aerosol, gas or otherwise so I believe it is most likely a generalised result. Though the bias is not large ($\sim 10\%$) it is nonetheless worth quantifying as it helps to explain discrepancies identified in an earlier study by Randales et al. (2013). In this way the present manuscript helps to complete the story / understandings gained from recent radiative forcing model intercomparisons. It is also useful information for climate model developers to have a paper quantifying the magnitude of this error associated with the 2-stream approximation. The paper is nice and short and really was quite straightforward and interesting to read. I therefore consider the paper to be of significant scientific merit and to be appropriate for publication in ACP with minor revisions.

Specific comments: I am not sure I agree with the final statement of the conclusions "Even so, radiation schemes in global models should be improved to provide more accurate calculation of present and future radiative forcing due to BC". Uncertainty in BC FR is really dominated by problems in simulating the BC abundance and optical properties. Also, increasing the number of streams in a radiative transfer model would increase the overall CPU and complexity of a climate model and so this may not be justifiable at this stage of climate model development. My interpretation is that the inclusion of multiple stream radiation is shown to improve the accuracy of BC RF accuracy and that the benefits of multi-stream radiation are certainly "worth considering" in aerosol-radiation and climate studies.

Response: We have modified the sentence to reflect that this could be considered. It may be other possibilities for improvements than using multi-stream radiative transfer schemes.

In the conclusions could the authors be a little more specific about the size of errors in BC RF stemming from BC abundance, BC optical properties, and BC forcing efficiency. For example, how does the error associated with the 2-stream approximation weigh up against problems in getting the vertical distribution right or other factors that affect the efficiency of BC absorption in the atmosphere. It is maybe asking too much from a

short paper but a little extra information I think would help to weigh this up in terms of model development priorities.

Response: The following is added: Burden of BC and the normalized RF has a standard deviation of the order of 50% relative to mean values for the 15 global aerosol models in AeroCom Phase 2 (Myhre et al., 2013). It would be quite useful to know how much additional cost (CPU) is consumed by opting for multi-stream over 2-stream. I know this would be different from model to model but even so, I think this information would help give an idea of the affordability of 8-streams. It would be nice to see the headline results in a table (global mean RF of BC, for clear and all-sky conditions showing the 2 and 8 streams, and then the ratio of 2-stream/8-stream). Maybe would be interesting to put the TOA SW fluxes in the table too to highlight any change in TOA balance from switching between 2 and 8 streams. In section 3.2 the authors have done something to keep the albedo of the clouds the same or at least to keep the TOA reflected SW flux the same in the 2-stream and 8-stream cases. Can the authors explain how they did this? I am not sure this is the best thing to do here. After all, if a climate model is upgraded to 8-stream then the change in cloud reflectance will occur and I think we really want to know the overall impact including any changes to scattering by clouds.

Response: The additional computer time between our 2-stream and 8-stream simulations is relatively small. However, this is not very relevant for climate model simulations where specific 2-stream approaches are adopted unlike our multi-stream code which is flexible in terms of number of streams. We have added that the modifications to clouds is due to constraint by measurements with the following addition: 'and close to measured fluxes'

In figure 3 the results switch now to comparing 2-stream with 16-streams rather than 2 versus 8. Was there a reason for this switch? It'd seem better maintain the same 8-stream configuration throughout the results.

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Response: All results now in 8-streams.

The wording and presentation of results could be improved in a few places to make it a little clearer and easier to follow (see minor suggestions below). Minor suggestions for clarity of text / figures: 1. Abstract line 5. Replace "absorption by BC" with "absorption by BC in the atmosphere". Response: Modified as suggested.

2. Abstract line 6. Replace "RF by 10%" with "the positive RF of BC by 10%". Response: Modified as suggested.

3. Abstract line 9. Replace "at high surface albedo" with "over high surface albedo". Response: Modified as suggested.

4. Introduction, line 16. Replace "mainfold" with "many". Response: Modified as suggested.

5. Introduction, page 2, line 6, "Further, one of the radiative transfer codes...". Can the authors say which one it was and/or what kind of radiative transfer method it had. Are the authors in this study using a similar or same radiative transfer to the Randales et al. study? Response: The following included: These two codes were denoted as number 3 and 4, respectively in Randles et al. (2013) and used in the current work

6. Section 2. Can the authors be a little more specific on how the aerosol fields were generated. It comes across as a bit confusing and vague in the way it is worded. Can they explain what aerosol species are included in the simulation other than BC. Response: More details added, see response to Reviewer 1.

7. Section 3.1, page 2, line 13. Replace "The radiative effect of clouds" with "The albedo of clouds". Response: Modified as suggested.

8. Section 3.2, page 3, line 13. Replace "enhances" with "increases". Response: Modified as suggested.

9. Section 3.3, page 3, line 12. Replace "the importance multiple" with "the importance

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of multiple". Response: Modified as suggested.

10. Figure 1. Would be good to be more explicit in the description of the figure caption that the data shown is the ratio of BC RF with 2-stream / BC RF with 8-stream. Why not also put a title on the figures "Clear-sky" "All-sky".

Response: Caption modified stating that it is ratio. 11. Figure 2. There are maybe too many lines on here, or at least it is not totally clear what the "BC only" and "All eff" refer to. The BC only case is maybe a distraction from the main point so maybe this could be omitted and simply explained in the text. The caption also has an awkward string "(a). (b)" in it. Maybe put the "(a)" at the beginning of the sentence.

Response: The figure and caption have been updated, and are now hopefully clearer. The new caption reads:

Figure 2: (a) BC RF normalized by abundance, as a function of altitude. Solid lines: 8-stream simulations. Dashed lines: 2-stream simulations. Colors represent all sky and clear sky conditions, and whether a full atmospheric simulation including Rayleigh scattering, water vapour and background aerosols was performed ("Full sim."), or if BC was the only radiatively active agent ("BC only"). (b) Ratio of 2-stream to 8-stream simulation results, for the four cases shown in panel (a).

12. Figure 3. Could the authors produce these results with higher resolution of surface albedo intervals? The lines look like they are going only in steps of 0.1 but the results seem very sensitive in the region where surface albedo is between 0 - 0.2 and I would guess are not at all linear as the lines presently suggest. Response: Fig 3 has been improved with a factor of 2 higher resolution than in the submitted version.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 26173, 2014.

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