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Interactive comment on "Impact of a Strong Biomass Burning Event on the Radiative Forcing in the Arctic" *by* Justyna Lisok et al.

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Italic font style denotes the Referee comments, while normal font - our answer.

We wanted to thank the reviewer for raising issues that limit the understanding of the paper as it helped us to improve the paper. We hope that the reviewer will be satisfied with the changes made to the new version of the paper.

Major comment

[...] I found it difficult to fully assess the quality of the paper due to English language and

grammar issues, which should be addressed before publication. Due to the number of such errors I am not able to point them all out in this review. [...]

The paper went through a major reorganization regarding English shortcomings along with English-proof reading.

Specific comments

Along with Myhre et al. (2013), the manuscript could include a citation to Sand et al., (2017), who investigated specifically the radiative forcing of aerosols in the Arctic in the AeroCom phase II models.

Indeed, both papers were significant for the section, thank you.

P2., l. 16, For IPCC results, Myhre et al. (2013b) might be a better reference than Pachauri et al., (2014)

The referee is right. Corrected.

P.2, l.34: Do you really mean reducing "values", not reducing data coverage?

True, we meant 'data coverage', thank you.

P.4, ll. 2 - 10: I think authors should clearly indicate here their own new/original contributions in this paper , and what work (e.g. simulations) was already performed for previous studies such as Markowicz et al. (2017b).

Corrected as requested. In the revised paper the section is written as follows:

Previously presented by scientific papers, and characterized in this research, was the study of smoke transport over the Arctic during July 2015. Markowicz et al., 2016a reported the temporal and spatial variability of aerosol single-scattering properties measured by in situ and ground-based remote sensing instruments over Svalbard and in Andenes, Norway. Moroni et al., 2017, discussed morphochemical characteristics and mixing state of smoke particles in Ny-Ålesund as indicated by DEKATI 12-stage low volume impactor, combined with scanning electron microscopy. Markowicz et al., 2017b on the other hand, presented a comprehensive description of smoke radiative and optical properties on a regional scale. The paper examined ageing processes of the smoke plume under study, while transported from the source region across the High Arctic. Simple Fu-Liou radiative transfer model, combined with NAAPS aerosol transport model, were used to determine the spatial distribution of aerosol single-scattering properties and RFs for the period of 5-15 July 2015, in the area to the north of 55^{o}N, where the transport of BB aerosol was observed.

In this paper, we utilise MODTRAN radiative transfer simulations and aerosol optical properties obtained from in situ and ground-based remote sensing instruments, to retrieve clear-sky direct RF over the area close to Ny-Ålesund. The research aims to estimate the biases connected with (i) hygroscopicity, (ii) variability of ω profiles, and (iii) plane-parallel closure of the modeled atmosphere. The main outcome of this research is the implementation of new methodology to retrieve the profile of ω at ambient conditions, utilising in situ measurements and lidar profiles (section 3.2). Simulated RFs were compared to simple radiative transfer model (section 3.5). Section 3.6 shows an example of RF distribution at the surface, in the vicinity of Kongsfjorden. The last part presents the influence of unstably stratified biomass burning air masses on the turbulence development, which is shown in section 3.7. Additionally, we confirmed the source region of the BB plume. A chemical weather model with satellite-derived biomass burning emissions was used to interpret the transport and transformations

pathways.

P7., section 2.2 : If I understand correctly, the in - situ measurements (e.g. SMPS, PSAP), are performed at the surface. Can you give reasons why these values are representative of the whole column , since the plumes extends at relatively high altitudes, and the Arctic surface and free troposphere are often decoupled.

In the revised manuscript, we added an explanatory section concerning our assumptions to ω and g retrieval, quoted below:

Vertical profiles of single-scattering properties at ambient conditions are used as input parameters to MODTRAN and Monte Carlo calculations. The retrieval is based on the in situ single-scattering properties, measured at the surface in dry conditions (denoted later on as superscript 'd'), and on vertical profiles of σ^a_{ext}, as well as RH at ambient conditions (hereinafter superscript 'a') from KARL lidar and radio-sounding data.

In the reference to temporal variability of range-corrected signal, measured at 532 nm by Micropulse Lidar, Markowicz et al, 2016a, characterize smoke plume as a rather well-mixed layer of BB aerosol extending from around 4 - 6 km on 9th to 0 - 3.5 km later on. Both contributions of BB-like aerosol in the NAAPS AOD, estimated on the level as high as 80$\%$, and the similarity between columnar and in situ aerosol extensive properties such as α (Markowicz et al, 2016a), suggest that smoke plume may have crossed PBL and mixed with the lowermost part of the troposphere. Additionally, very little aerosol load existing above smoke plume plays a minor role in affecting the radiative properties of the atmosphere and therefore may be neglected. This is why, in the presented methodology, we assume no changes in chemical composition vertically, so that most of the possible vertical variability of ω^a at ambient conditions, is attributed to changes in RH. Therefore, we approximate initial profiles of ω^d and R^d_{eff} by setting them up to the values of in situ measurements and consider them constant with altitude. By introducing hygroscopic growth model for particles with known size distribution, one may obtain ω^a profile as well as g^a.

P.7, l. 12: The a and d superscripts should be explained there, when they are first introduced, and not on page 8.

Corrected as suggested.

P. 9, equations 7 and 8: The text mentions RFnet and RFrel, but the equations give Fnet and frel.

Thank you, this was our mistake while copy-pasting to latex.

P.9, l.15 : If this product is from MODIS, this should be indicated.

Indeed, thank you.

P.9 l. 22: The "BRDF" acronym should be explained here.

Corrected.

P. 12 ll. 1 - 5: You could also compare to single scattering albedos used by Lund Myhre et al. (2007).

Thank you for your helpful comment, we referred also to Lund Myhre et al. (2007) in the section under consideration.

P. 12 l. 20: Is PM10 really reported in ppb, not $\mu g m^{-3}$?

Thank you, the text was corrected to "the mass mixing ratio".

P. 15 l. 10 " and additional no change in the irradiances from the reference simulation" it is not clear what you mean by this sentence.

Indeed, we rephrased the sentence.

P. 15, l. 16: what do you mean here by a "real" value of albedo?

Indeed, we rephrased the sentence.

Figure 3. There are several issues with this figure. First, the caption does not seem to match the contents, as the "Rad" quantities, which seem to be observations, are not explained in the caption. The caption mentions Fu - Liou results that are apparently not shown. The quantities do not seem to be daily mean values. In addition, RF quantities in panel b should use different colors/symbols than the F results in panel a , as the current choices is very confusing.

Thank you, we didn't notice that the caption was ill-copied. In the revised manuscript, the caption matches the figure.

We changed the colors/symbols in the b panel for the clarity.

Figure 3: What are the reasons for the differences between F and ModF results at the end of the period, after 12h on 11 July ?

This difference is a result of low cloud appearance at around noon 11^{th} July, as explained in the section 3.1. In the revised version of the manuscript we removed all cloud-contaminated data from this figure, also the F_{in} after 11:30 July 11.

Pp. 15, 16: This section should include more paragraphs breaks to better separate the different ideas.

The paragraph breaks were added.

P. 16, l. 6: How would increase turbulence lead to higher variability in F_{in}?

We apologize for this linguistic shortcoming. The higher variability of F_{in} on 10^{th} is a direct effect of the appearance of cumulus clouds. They, in turn, result from: (1) the aerosol activation based on the most common mechanism of cloud formation and (2) the instability of the atmospheric dynamics, as this is the reason why cumulus clouds are formed rather than other clouds.

After rephrasing, this sentence should be as follows:

We may expect that higher variability of Rad F_{in}, visible by comparison to the 9^{th} July, together with an appearance of clouds inside the smoke plume, are likely to result from both a possible BB aerosol activation and increased turbulence. Further to this, a number of high- and mid-level cumulus clouds are reported around noon and in the afternoon (Markowicz et al., 2016).

P. 17, l. 17: Explain the meaning of "RFE" when it is first introduced. For what reason is RFE a more accurate quantity for intercomparisons?

Corrected as suggested. RFE is a more accurate quantity for inter-comparison only when intrinsic properties of the plume are taken into consideration as it was stated in the further part of the sentence. However, in the revised version of the manuscript this sentence, after rephrasing of the paragraph was omitted.

P. 17, l. 31: It is not clear here for someone unfamiliar with these codes that DISORT is included within MODTRAN and not a standalone radiative transfer model. Consider rephrasing this sentence.

Corrected as suggested.

P. 17, l. 31 and elsewhere: Can you explain what you mean by "robust" when referring to Fu - Liou? Do you mean more detailed?

We apologize for this ill-translation. We meant 'fast' and 'less-complicated' in terms of solvers of the radiative transfer equations. It was improved in the revised manuscript.

Pp. 18 - 19: This section should include more paragraphs breaks to better separate the different ideas.

Corrected as suggested.

P. 18, ll. 13 - 18: I do not think it is needed here to remind the meaning of the different colours in Figure 4, since they are already explained on the Figure.

We agree, thank you for this suggestion.

P. 19, l. 4 and elsewhere: The correct reference is Lund Myhre et al. (2007), not Myhre et al., since "Lund Myhre" is the last name of the first author.

Corrected as suggested.

P. 19, l. 12 - 15 : This section would be clearer if the analysis of Figure 4 started with this remark , since the most obvious result from Figure 4 is that there is a very good agreement for RF between MODTRAN and Fu - Liou.

We agree with the reviewer and changed the text accordingly.

Figure 5: What are the reasons for the strong differences in RFE between MODTRAN and Fu - Liou for 9 July ?

The main reason for the modeled discrepancies in RFE are (1) the differences in inputs to models, in particular, the assumed aerosol optical properties and secondarily PW as well as (2) the distinction between solvers of the radiative transfer equations used in both models, that may give different results even though the exact inputs are assumed. The latter issue is more widely described in the following paper: Myhre, G. et al,2009: Intercomparison of radiative forcing calculations of stratospheric water vapour and contrails, METEOROL Z, 18(6), pp585-596.

Note, this part of the section was moved to the appendix B. This was requested by the Referee 1 being concerned that the inter-comparison between RTM models was not the main subject of the manuscript and additionally unnecessarily lengthened the paper.

P. 20, l. 7: " In the previous sections, we discussed the RF computed for a single cell " maybe this should also be mentioned explicitly in the beginning of the previous sections, e.g. at the beginning of 3.2.

We decided to add this information in the description of models.

P. 20, l l . 13 - 14: Why not show RF directly, instead of this relative value? This should maybe be explained when the equations are discussed.

In the revised manuscript, we added the following information to the 2.3.4 section with 3D Monte Carlo equations:

The results from 3D Monte Carlo model, as mentioned earlier, are used to characterise the spatial variability of RF and therefore to diagnose possible uncertainties resulting from using single-column radiative transfer models, represented by MODTRAN and Fu-Liou codes. Taking into account the above goals, we resigned from performing time-consuming simulations of daily mean broadband RFs for the model domain; and instead, we relied on the relative value of RF calculated for 1 λ, with respect to its value at TOA at a given zenith angle. Such an approach allowed for defining higher spatial resolution.

Figure 6: There are also several issues with this figure. First, the colorbar should include a label. Since values go from negative to positive, it would be a lot clearer to use a divergence colormap where 0 is indicated by a special color, for example white. It is also unclear to a reader unfamiliar with the "ICA" terminology what is the exact difference between panels a and b. I understand that the point is to study the effect of e.g. topog raphy on the RF calculations, but consider writing a more explicit caption, and consider including in the text an explanation of the difference between these two calculations and the aim of this 2 - panel comparison.

The label to the colorbar was added. Regarding the divergence colormap, we kindly disagree with the referee, as this would limit the number of colors used for the negative

RFs. As the area of a positive RF is very small, we feel that this change is not of a great importance. Instead, we added a black line to the colorbar highlighting 0 value.

Figure 6: Results seem to show a negative RF over high - albedo surfaces. Other studies (e.g. Sand et al., 2017) often showed a positive RF of BB aerosols over snow and ice. Is this due to the high single - scattering albedo here ? To a relatively low surface albedo c ompared to typical snow and ice - co vered surfaces in the Arctic ?

Fig.6 in the manuscript and the work by Sand et al. (2017) present radiative forcing at different levels. The figure shows aerosol radiative forcing at the surface while Sand et al (2017) at the top of the atmosphere. Aerosol radiative forcing at the surface is typically negative.

Conclusion: If possible, use the full name of the quantities discussed in the conclusion , e.g. "heating rate", instead of the "rh" notation .

Corrected as suggested.

P. 25, l. 4: Are these average values? Over what time window?

This averages refer to the BB event, in particular, 14:00 July 9th - 11:30 July 11th. We changed the sentence accordingly.

P. 25, l . 7: Are you really comparing modelled RF to observations in this study?

We apologize for this shortcoming in English. We have changed the sentence to match the actual meaning. Nevertheless, note that we also added a comparison of modeled and measured Fs.

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