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

General comment:

This study investigates the radiative impacts in the Arctic (Ny-Ålesund, Svalbard) of biomass burning aerosols transported from Alaska during an especially intense event. Authors combine in-situ and remote sensing measurements, radiative transfer models, as well as Lagrangian, Eulerian and LES models to investigate the consequences of this event. This work is scientifically significant and of good quality, and is thus suitable for ACP. I have one major comment concerning the presentation of the results; I had difficulties understanding several sections of the manuscript, especially the introduction, due to shortcomings in the English language used. Details are given in the specific comments below.

Major comment:

1. 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. As an example, it can be hard to understand the following sentences:

"The effect of BB aerosol from the regional point of view is claimed to have stronger temporal variations indicating the change of the regional climate patterns (Wang et al., 2006) It might be especially important over the bright surfaces regarding changes in the surface and cloud albedo (Screen and Simmonds, 2010), which in particular may indicate a positive RF_{toa}"

Specific comments:

2. 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.
3. P2., l. 16, For IPCC results, Myhre et al. (2013b) might be a better reference than Pachauri et al., (2014)
4. P.2, l.34: Do you really mean reducing "values", not reducing data coverage?
5. 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).
6. 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.
7. P.7, l. 12: The a and d superscripts should be explained there, when they are first introduced, and not on page 8.
8. P. 9, equations 7 and 8: The text mentions RFnet and RFrel, but the equations give Fnet and frel.
9. P.9, l.15: If this product is from MODIS, this should be indicated.
10. P.9 l. 22: The "BRDF" acronym should be explained here.
11. P. 12 ll. 1-5: You could also compare to single scattering albedos used by Lund Myhre et al. (2007).
12. P. 12 l. 20: Is PM10 really reported in ppb, not µg m-3?
13. 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.
14. P. 15, l. 16: what do you mean here by a "real" value of albedo?

15. 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.
16. 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?
17. Pp. 15, 16: This section should include more paragraphs breaks to better separate the different ideas.
18. P. 16, l. 6: How would increase turbulence lead to higher variability in F_in?
19. 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?
20. 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.
21. P. 17, l. 31 and elsewhere: Can you explain what you mean by “robust” when referring to Fu-Liou? Do you mean more detailed?
22. Pp. 18-19: This section should include more paragraphs breaks to better separate the different ideas.
23. P. 18, ll. 13-18: I do not think it is needed here to remind the meaning of the different colors on Figure 4, since they are already explained on the Figure.
24. 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.
25. P. 19, ll. 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.
26. Figure 5: What are the reasons for the strong differences in RFE between MODTRAN and Fu-Liou for 9 July?
27. 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.
28. P. 20, ll. 13-14: Why not show RF directly, instead of this relative value? This should maybe be explained when the equations are discussed.
29. 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. topography 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.
30. 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 compared to typical snow and ice-covered surfaces in the Arctic?
31. Conclusion: If possible, use the full name of the quantities discussed in the conclusion, e.g. “heating rate”, instead of the “rh” notation.
32. P. 25, l. 4: Are these average values? Over what time window?
33. P. 25, l. 7: Are you really comparing modelled RF to observations in this study?

ADDITIONAL REFERENCES:

Sand, M., Samset, B. H., Balkanski, Y., Bauer, S., Bellouin, N., Berntsen, T. K., Bian, H., Chin, M., Diehl, T., Easter, R., Ghan, S. J., Iversen, T., Kirkevåg, A., Lamarque, J.-F., Lin, G., Liu, X., Luo, G., Myhre, G., Noije, T. V., Penner, J. E., Schulz, M., Seland, Ø., Skeie, R. B., Stier, P., Takemura, T., Tsigaridis, K., Yu, F., Zhang, K., and Zhang, H.: Aerosols at the poles: an AeroCom Phase II multi-model evaluation, Atmos. Chem. Phys., 17, 12197-12218, https://doi.org/10.5194/acp-17-12197-2017, 2017.

Myhre, G., D. Shindell, F.-M. Bréon, W. Collins, J. Fuglestvedt, J. Huang, D. Koch, J.-F. Lamarque, D. Lee, B. Mendoza, T. Nakajima, A. Robock, G. Stephens, T. Takemura, and H. Zhang, 2013: Anthropogenic and natural radiative forcing. In *Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change*. T.F. Stocker, D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Doschung, A. Nauels, Y. Xia, V. Bex, and P.M. Midgley, Eds. Cambridge University Press, pp. 659-740, doi:10.1017/CBO9781107415324.018.