The authors would like to thank the Reviewer#1 for his careful review of our manuscript. We addressed each comment individually in the following electronic supplement, and have revised the manuscript accordingly.

RC : This paper presents a thorough and detailed model investigation of the factors affecting transport of several plumes of BC and CO to the Arctic, comparing the influences of different pollution sources and the spatial distribution, amount and types of precipitation. There is a limited comparison to aircraft measurements that shows the model is doing a reasonable job of capturing the general features of the observations, though not doing a perfect job of simulating the exact concentrations or resolving the finer structure of the plumes (and I would not expect it to). Overall I think the work is of a very high scientific quality, and the main issue I have is that it is too long. However, I think it is probably not suitable to be split into 2 papers, nor are there any sections that could easily be cut without detracting from the rigour of the analysis, so I think it will just have to remain long. Possibly, parts of Section 2 might work as an appendix/supplement. Additionally, some of the written English is phrased in strange ways and some sentences do not quite make sense, so it could do with some work. I recommend publication in ACP subject to the following minor revisions.

AC : We thank the anonymous reviewer for providing helpful comments. As mentioned by the reviewer himself/herself, it is not suitable to be split into 2 papers as it would compromise the second paper. We have tried to reduce parts of the paper, as suggested by Reviewer#2, but the length is still important.

— *RC : P1L13-15 The way this is phrased makes it sound like the source determines the APT. I think this is because you have said "is" rather than "was", which makes it sound more like a general statement rather than a specific statement about the plumes studied in this paper. This type of error occurs throughout the paper and is mostly benign but can sometimes be confusing, such as in the example above. As a general rule of thumb, the work you have done is in the past (e.g. "the campaign took place" or "we ran the model") but things you do in the paper itself should be in the present (e.g. "in this study we describe. . . " or "Figure 5 shows . . . "). It gets more tricky, for example when you come to the conclusions- the specific plumes you studied were affected by precipitation but plumes in general are affected by precipitation.*

AC : We agree with the reviewer that using present may give the impression of a general comment and may be confusing for the reader. We put the phrase in the past tense. More generally, we have decided to apply the reviewer's suggestion along the full manuscript. We use present when it refers to the work we have performed and the results obtained, and we use the past when statements refer to conclusions.

— *RC : P3L8-9 "Schwarz et al showed. . . ." I'm not sure what you are trying to say with this sentence, other than to mention the study by Schwarz et al. It seems like you do a better job of saying the same thing in the next sentence, so this one isn't really necessary*

AC : We removed that sentence.

— *RC : P3L15 What results from Koch and Hansen ? Do they confirm them or do they just agree with them ? (i.e. both could be wrong)*

AC : Koch and Hansen (2005) highlighted an overestimation of BC concentrations in the upper Arctic troposphere. The modeling study of Breider et al. (2014) found similar results. We modify the word "confirms" by "agrees with".

— *RC : P5L6-10 Please give details of how the SP2 was calibrated. Actually, you should also say how the CO box was calibrated. Just one sentence for each would probably be sufficient if a standard method was used.*

AC : The SP2 was calibrated using the recommended calibration material (Fullerene soot, as produced, Lot#F12SO11, Alfa Aesar Inc., Ward Hill, MA). CO was measured every second from VUV fluorescence with an accuracy of 10% (Gerbig et al., 1999).

— *RC : P5L14-15 "Absolute uncertainty of BC particle mass is within 10%, the uncertainty of the derived total BC mass mixing ratio is about 30%." I am not sure I follow the logic here. Also 10% is not an absolute uncertainty, it is a relative uncertainty- a percentage is relative by definition. Given you do not mention particle size in your analysis, the only relevant errors are A) The systematic uncertainty in your BC calibration and B) The statistical uncertainty in the derived BC mass concentrations. A) is down to a combination of the sampling time, concentrations and flowrate, but is easy as you can just pick a time when you think your concentrations are constant and look at the variation in your time series. You could express this as a relative error (e.g. +/- 10%) or absolute (e.g. ± 2 ng/kg) B) is more difficult as you have two factors- firstly the random variation in your calibration slope (in other words how accurately does your particular slope recreate the mass of the calibration material, if you repeated your calibration exactly how much would the slopes differ ?) (see Laborde et al. (2012b)) and secondly how well does your calibration material represent the instrument response to the BC you measure in the atmosphere ? As Laborde et al. (2012a) showed, the SP2 responds differently to different BC types. I understand the observations present a fairly minor part of your paper but if you are going to present them and quote an uncertainty it should be done correctly, which at present I don't think it is.*

AC : This was indeed a mistake. The instrument provides acumulation model refractory BC mass mixing ratio with a total relative uncertainty of 30% **(Laborde et al., 2012, Schwarz et al., 2013). The recent paper by Schwarz et al. (2017) details how the instrument had been calibrated and how the measurements had been corrected for the ACCESS campaign.**

— *RC : P7L23 You note that the height of the emissions injection is very important- how good a job does the plume rise model do ? How does it work out the buoyancy of a particular fire ? Please provide a brief summary*

AC : During the ACCESS airborne campaign, flights were only performed in the remote Arctic region. Validating the fire injection heights would require measurements of vertical profiles of BC over the source regions. The performance of the plume rise model has nevertheless been studied in detail in some papers (Grell et al., 2011 ; Sessions et al., 2011). They have shown that the plume-rise model embedded in WRF-Chem improves the injection heights when compared to the satellite-observed ones. These two papers are quoted in the manuscript. Appropriate fire properties are obtained from a synergy between remote sensing observations, land use and carbon fuel datasets to determine in which columns the fires are located and the plume rise is simulated explicitly (Grell et al., 2011).

— *RC : Figure 2a is there a reason why potential temperature is more useful than just temperature ?*

AC : The potential temperature is constantly increasing with altitude, suggesting strong atmospheric stability in this study. It has also been used to underline in

Fig. 10d and Fig. 14 that the transport of BC in biomass buring plumes followed isentropes. Validating this parameter is therefore useful.

— *RC : P10L20 wrong OH and transport- the way this is written it is not quite clear what you mean. Wrong transport ? Vertical or horizontal ? Or just transport ? Do these factors and the studies you reference explain why CO is underestimated specifically between 6 – 9km ?*

AC : This sentence was unclear. It was not referring to WRF-Chem. We have re-written it to be clearer : The small underestimation in CO between 6 and 9 km is a common feature observed by most models (Emmons et al., 2015 ; AMAP, 2015). Variability in models, run with the same emissions, appears to be driven by differences in chemical schemes influencing modelled OH and/or differences in modelled vertical export efficiency of CO from mid-latitude source regions to the Arctic (Monks et al., 2015).

— *RC : Section 3.2 I think you are overselling the agreement between model and measurements. For example "The two profiles are well correlated with maximum CO values of 200 ppbv at* $7-8$ *km, associated with elevated BC values reaching 25 ng* kg^{-1}*." Actually the maximum in the model CO is* 150 *ppb at 6.5km. Compare to P32L10 : this is a better way of describing the model/measurement comparison. The general features were well captured.*

AC : The rewiewer might have been confused about that sentence. We just compare here the two measured profiles of CO vs BC, not model vs observations. This comparison is done later, e.g. P11L8. We have added the word "the two measured profiles" to avoid confusion.

— *RC : P11 L 1 "the influence of flaring emissions in this area is insignificant". Insignificant for what ? At what scale ? Later on in the paper you talk about some flaring plumes so it can't have been insignificant within those plumes.*

AC : The term "in this area" was not clear. The influence of flaring emissions on the vertical profiles of CO and BC sampled by the aircraft is insignificant. It has however a small influence on the background pollution off the coast of Scandanavia, as simulated by the model but not in the plumes sampled by the aircraft. According to a comment of Reviewer#2, we have decided to removed this sentence.

— *RC : P11L7-8 "The model shows appreciable skill in capturing the vertical profile of BC, but overestimates the BC mixing ratio between 2 and 3 km of altitude." Looking at the median BC concentrations, the 40km model overestimates between 1.5 – 7.5km then underestimates higher than 7.5km. Additionally, if you just went with a flat BC concentration of around 6 or 7 ng/kg the medians would probably show similar agreement. Now, I am not saying that the agreement is terrible- actually it OK, perhaps reasonably good, and it seems to capture the general features of the observations without doing a perfect job. But you read the text and it sounds like the model is doing an amazingly good job, which figures 3 and 8 show it isn't, it's just doing a reasonable job. So I think you should just tone down how well you claim the model and observations agree.*

AC : We modified the sentence as follows : "The model shows appreciable skill in capturing the general structure of the vertical profile of BC, but overestimates the BC mixing ratio in the mid-troposphere".

— *RC : P11L9 The* 30% *error in the SP2- please relate this back to the previous comment on statistical vs systematic error. Also please give numbers for the model biases.*

AC : The resulting mean bias on BC is 1.5 ng kg^{-1} and the corresponding normalized mean bias if 27%. It is much lower that biases reported for most models in the Arctic region (AMAP, 2015). Eckhardt et al. (2015) indeed reported that BC concentrations in July-September are overestimated in the mean of intercompared models by 88%.

— *RC : P11L13 Here you say the CO between 6-9km is due to biomass burning emissions, but previously you said the underestimation in CO was due to wrong OH concentrations and transport. Could it not be that the model is underestimating the biomass burning CO at this altitude ? Is that what you mean by transport ? Please clarify*

AC : The underestimation in CO mentioned previously was a general features in CTM. Here we wanted to mention the origin of the plumes we sampled in this ACCESS study. For sake of clarity we modified the last sentence : "In Sect. 4, we discuss the origin and transport of plumes leading to this noticeable increases of CO and BC between 6 and 9 km of altitude, associated with higher ozone mixing ratios."

— *RC : P13L11 You say the AOD underestimation is due to simplified SOA, but you haven't given any details of how the AOD is calculated. I don't think you can say this is due to missing SOA when you don't have a good handle on even the size distribution, let alone composition or refractive index.*

AC : This is correct. We don't know the exact reason of the model underestimation in this region. AOD is computed through a Mie code embedded in the model. The representation of the size distribution and complex refractive index strongly influences the result. The simplified SOA mechanism is a potential cause, but we can't say it is the main one. We removed that comment.

— *RC : P18L13 are the BC enhancements you are talking about in the model or in the observations ? I assume model but it's not clear*

AC : We added the word "modeled".

— *RC : P18L23 The agreement between model and measured BC is "very good"- Again I would say it's reasonable but I wouldn't say it's "very good". The model plumes in figure 5 are too diffuse and some are missing, such as the smaller amounts of BC associated with the CO plume at 8km at 0915. You get the general features. For me, "very good" would be if you could plot model vs measurement for each grid box as the aircraft passed through it and get something approaching a 1 :1 line, though I doubt that could happen in a study like this.*

AC : We corrected the sentence, replacing "very good" by "reasonable".

— *RC : Figure 8 You could do with a longer time average of the measurement data as currently it's difficult to see the structure when the markers all overlap. I also wonder if there is a way to make the aircraft easier to see as the parts that stand out are the parts where it disagrees with the model. Also it might help if the x-axis was north/south or east/west as several points in the discussion relate to spatial location and this is difficult to see in this and subsequent plots*

AC : We follow the reviewers's suggestion in applying a sliding window to calculate longer time average of the measurement data. In situ measurements are averaged

using a 2-min running mean. We have added larger white dots behind the measurements to make them easier to see versus the modeled cross-sections. We have also highlighted by magenta circles the eight airmasses discussed in the Sect. 4.4 to facilitate the reading.

— *RC : P21L18 If the aerosol from flaring had been removed by precipitation, wouldn't you still see the CO ?*

AC : We thank the reviewer for pointing this. Indeed, if the decrease of BC in flaring plumes was only due to precipitation, the CO should have remained unchanged. This is not the case, suggesting that transport of plumes from flaring sources is not only directed towards the northern coast of Norway. We have decided to remove this last sentence.

— *RC : P21L25 Didn't you say in the previous section that the flaring plumes didn't exist ?*

AC : No, we had said previously that the aircraft did not sample any flaring plume. But flares have a small influence in the area of the study (between northern Norway and Svalbard archipelago). Those flaring airmasses have been identified in Fig. 8 and 9 and highlighted by circles in the new version of the manuscript.

— *RC : Section 4.4 The discussion may be easier to follow here if you circled the plumes on one of the figures*

AC : We agree. This has been done in Fig. 8.

— *RC : P23L2 you say the fire injection lofted the plumes to 6km, but doesn't fig 10d shows that the emissions from the 8th initially remained below 4km. I'm not really sure I follow what figure 10d adds to the analysis*

AC : This is correct, thanks for pointing this. The rapid uplift up to 6 km is due to WCBs not pyroconvection. The sentence has been removed. Fig. 10d illustrates the merge of the two BB over eastrn Siberia on 12 july and their transport to the Arctic following isentropes.

— *RC : P24L20 How does figure 9 show European influence ?*

AC : Below 4 **km, the BC concentrations are dominated by the anthropogenic contribution (Fig. 9). As discussed in Sect. 4.4, this area is influenced by European emissions. To clarify the sentence, we have written "European anthropogenic influence".**

— *RC : P24L24 Please define KFCuP in the text. You refer several times to KFCuP as if it is a process itself. In the real atmosphere it is actually "convective clouds" that do things that the model is trying to represent.*

AC : We had defined KFCuP in Table 1. We replaced "KFCuP" occurences by "convective clouds" when the manuscript speaks about processes and keep "KFCup" only when the cumulus scheme is described.

— *RC : P26L4 It's frowned upon these days to refer to it as a coating, as that may not represent the actual morphology of the particle. Maybe say BC in BB plumes is more internally mixed or*

something like that. I also saw another point where you referred to coatings, please also change this.

AC : This has been corrected P24L31, P28L3 and P33L16.

— *RC : P26L11-14 I don't know what these last 2 sentences add to the analysis*

AC : We have noted that the wet deposition processes were efficient in removing BC-containing particles during transport. But in Fig. 6, we had shown that clouds in this study are mostly mixed-phased or ice clouds. The last two sentences have been kept to quote studies underlying the role of impaction scavenging in removing BC in mixed-phased or ice clouds.

— *RC : P27L19 The removal efficiencies may be low for large rain drops but not for drizzle*

AC : Here we are talking about cumulus parameterized clouds. Drizzle mostly occurs in low-level stratocumulus clouds that are resolved by the model and do not depend on KFCuP mechanism.

— *RC : P29L6 The 5th percentile of the measured or the modelled CO concentration ? Also why not do this for the BC as well ? The average lifetime of aerosols is of the order of a week because of deposition processes, mostly wet deposition. So the lifetime of aerosol that escapes wet deposition is longer. If you mean you looked at figure 3 and saw that the minimum BC in the observations was basically zero at all levels then say that. But if that was the case I still don't see the harm in taking the 5th percentile like you do with the CO.*

AC : This is correct. But according to a major comment of Reviewer#2, we have modified the calculation of the transport efficiency of BC using the ratio of BC in the base run on BC in the NoWetAll run. This is more appropriate for a model estimate. Therefore, the background concentrations are not used anylonger.

— *RC : P29L22 Perhaps this is not the case in the model but in the real atmosphere BC would only be lost if the cloud precipitated. If it evaporated the BC would still be there.*

AC : We agree with the reviewer. However we do not consider losses for the *atmosphere* but rather losses for the *plumes*. When there is activation of clouds droplets, BC is not lost for the atmosphere but is transferred from interstitial aerosol to cloud-borne aerosol (nucleation scavenging). If the cloud droplets reach the sizes of precipitating rain drops, it will act as a deposition process from the plume.

— *RC : P32L16 Can you suggest how the discrepancy/difference might be resolved ? Is it just because there is less precipitation in this study ?*

AC : We have suggested three reasons to explain the differences between the two papers : different year (BC emissions differ), less precipitation and a difference in the methodology. Matsui's study is only based on observations. The BC-to-CO ratios are therefore normalized by BC-to-CO ratios over the sources. If a plume emitted from a specific source diverges before reaching the receptor area, the transport efficiency calculated by Matsui's method will decrease. In our modelling study, the transport efficiencies are only influenced by precipitation and therefore higher. They illustrate the role of deposition during transport rather than the contribution of the transport itself.

— *RC : P33L30 "found to be more important" It is not clear what you mean by this. If you mean the cumulus clouds remove more of the low-level aerosol by scavenging or washout than by uplifting it then say that. It's better to say what is actually happening.*

AC : Yes, the reviewer exactly understood the message. We modified the sentence accordingly.

— *RC : P34L1 This last sentence is an odd way to end a paper. It sounds like you are using the model to validate the measurements when actually it was the other way round. Also this wasn't the main focus of your analysis. It seems to me that the main important conclusions were 1) BC is transported more efficiently into arctic from high-latitude BB sources than from east-Asian anthropogenic sources because it rains less at higher latitudes 2) The ways in which the large-scale vs subgrid convective clouds affected the BC distributions differently. It would be good if you could highlight these more in the abstract/conclusions in terms of the physical processes your results suggest are actually taking place in the real atmosphere, rather than abstract terms like grid-scale and APT*

AC : The last sentence of the paper has been removed. We have reformulated the abstract and conclusions in focusing on the physical processes : we have for instance replaced "grid scale precipitation" by "large scale coulds" and "subgrid parameterized clouds" by "convective updrafts".

— *RC : Finally, given you have actual observations it seems like there is a missed opportunity to calculate TEBC based on the measured values of BC and CO. You have hinted at your reasons for not doing so in the comparison to previous studies calculating TEBC but I'm not sure it's clear exactly why not.*

AC : We have only observations over northern Norway. Calculations of TEBC based on measurements would require also measurements of CO and BC over the different emission sources (anthropogenic, fires and flaring).

Technical corrections

— *P3L25 "lead to in the" remove 'in'*

AC : Done.

— *P3L27-31 This is a very long sentence, please split it into at least 2.*

AC : Done.

— *P8L4 add "resolution" before "to adequately"*

AC : Done.

— *P11L12 remove "but"*

AC : Done.

— *Figure 3 please make the plots slightly taller to show the detail better*

AC : The vertical bars for the median values have been replaced by diamonds of

the same color as the mean values. This makes the plot clearer.

— *P11L19 This is a very long sentence, please split it into at least 2.*

AC : P11L19 is very short. Maybe the reviewer wanted to point P11L13-16. This long sentence has been removed according to a previous comment.

— *P12 L4 remove "that"*

AC : Done.

— *Figure 4 the scale is between 0 – 1 but the text mentions values up to 2.5*

AC : This is true. But we have chosen this color scale on purpose to hightlight the contrast between the source regions and the rest. The colorbar contains arrows on the top and bottom of it to show that data exist above (resp. below) the highest (resp. lowest) contour level.

— *Figure 5 are (a) and (b) really the same grid ? Panel (a) looks much more blocky. Also label (%) on panel (c) colorscale*

AC : Fig. 5a is provided by GPCP data at 1° resolution. They have been interpolated on the WRF grid, which gives this blocky feature. We have removed "same grid" to avoid confusion. We have also added the missing label.

— *P15L1 "than in altitude", do you mean "that at higher altitudes" ?*

AC : Done.

— *P18L6 "plumes transported in altitude" do you mean "at altitude" ?*

AC : We wanted to say "in the upper troposphere". This has been corrected.

— *P18L20 "shifted a bit"- how far ? Saying "a bit" is probably a bit too colloquial for a paper (but not a review !)*

AC : To be more quantitative, the plume enriched in CO is shifted towards the north by one pixel in the model simulation, which means 40 km. It has been precised in the new version.

— *P21L17 "not due to" do you mean "did not lead to" ?*

AC : Done.

— *P27L34-35 This sentence doesn't make sense*

AC : The new sentence is the following : "The combination of low-level scavenging in the Arctic region and transport decrease from mid-latitudes is the cause of the low summertime BC concentrations."

— *Figure 13 caption Maybe rephrase to "The points are colored by the time (in hours) before the release of the trajectories in FLEXPART-WRF)"*

AC : Done.

— *P30L25 You may consider not using the word "septenrional" as it is not a commonly used word.*

AC : We say that BB sources are located far north.

— *P32L4 Again change "a bit" to "slightly"*

AC : Done.

— *P33L4 Remove "very"*

AC : Done.

— *Finally, thankyou for your interesting (but long!) paper*

AC : We also thank the anonymous reviewer for providing helpful comments.