For clarity, we repeat the reviewer comments in normal font, followed by our answers in italic font.

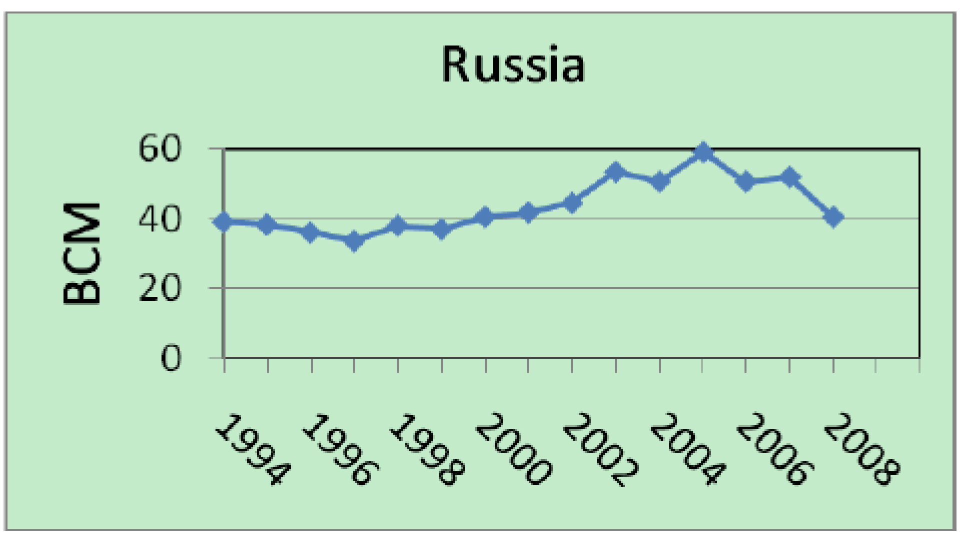
The manuscript investigates the role of previously poorly resolved emissions (residential combustion and gas flaring) on the modelled seasonal cycle of Arctic BC concentrations. It is an original contribution to the discussion of Arctic aerosol sources, transport and losses, and as such deserves to be published. Overall, the manuscript is well written and the methodological limitations are openly discussed. However, I agree with the anonymous referee that the conclusions regarding the role of gas flaring and residential combustion should be tuned down unless a sensitivity analysis addressing some of the large uncertainties in the model set-up are addressed.

We thank you for the overall positive assessment. We have tuned down the title as suggested, while at the same time including a sensitivity studies and adding additional observational evidence.

Specific comments:

1) There are large uncertainties related to the newly introduced emissions (e.g. gas flaring BC emission factor, intra-annual variability, emission height) as well as to the BC removal processes. I recommend a sensitivity analysis regarding these uncertainties so that the study could better constrain the role of different assumptions and processes in the modeled fields. The sensitivity simulations do not need to be run over the whole simulation period but can concentrate on e.g. 1 year period. The large uncertainties should also be briefly mentioned in the abstract and the conclusions.

There are likely trends in the gas flaring emissions but and they can vary from year to year as illustrated in the chart below (Elvidge et al., 2009) but available data does not allow for more detailed assessment of seasonal variation as well as distribution of changes across the many oil production.



The heights of the flares varies, but stacks are typically quite low (some 20-60 meters), which does not introduce substantial uncertainty in the model results at long distances from the flares. The emission factors are indeed uncertain, but our results scale linearly with the emission factor *(unless taking into account spatial variation of the emission factors, which is possible but for which we have no basis for estimation), so a sensitivity study would be rather trivial.*

2) The cited study regarding the correlation between fuel use and HDD (Quayle and Diaz, 1980) is over 30 years old. How well does this study apply to the modern, better insulated housing with much more electrical gadgets that heat the indoor air? Is the base temperature of 15_C from the Quayle and Diaz study or from more recent research, and what is it based on?

The cited study is one of the most systematic in the meteorological literature. There are newer ones (especially in the engineering literature), but in our opinion the cited study is still the best reference for our purpose. The law of heat conduction dictates that energy requirements depend on the temperature gradient (i.e., difference between outside and inside temperature). The HDD concept simply makes use of this law and should not need an update. Notice that the emission data itself accounts for possible improvements in insulation or changes in the energy source used for heating during recent decades.

The base temperature is of course debatable. Many studies have used 17 or 18 degrees (also Quayle and Diaz, 1980). We have decided to use a lower temperature, exactly because nowadays houses are better insulated and people living at high latitudes (where emissions are particularly important for the Arctic) turn on space heating only when temperatures are lower than when, say, people living in the United States (for which many of the studies have reported threshold temperatures) would turn heating on. We have tested the sensitivity of our results to changing the threshold temperature to 17 degrees and the resulting seasonal emission cycle is only minimally less strong than shown in the paper.

3) What is the linear weighting of space heating and cooking between 15 and 55 N based on? How will this assumption affect the simulated Arctic BC concentrations?

This is admittedly a subjective choice. To do this more objectively would require more information on exactly the source type of the emissions rather than the aggregate residential emission sector which was available for modeling here. This is left for future study and will need to be integrated directly into the development of emission inventories. The effect on Arctic surface BC concentrations is small since they are determined mainly by the high-latitude emissions. But of course the seasonal cycle in the Arctic is somewhat damped by the transition to emissions without a seasonal cycle at low latitudes.

4) What is the emission size of BC of 0.25 um based on? One would expect the emission size to differ between the different emission sources. How is this expected to affect the simulation results? How about the fact that BC wet deposition seems to be assumed independent of the hygroscopicity of the particles?

FLEXPART has rather simple aerosol removal parameterizations. Therefore, we consider our study a sensitivity study, not a full impact analysis, for which more sophisticated aerosol models should be used. The size of 0.25 um is not the emission size (which for most sources is smaller) but shall represent the size of the particles as they are transported in the atmosphere. We do not treat changes in the aerosol size distribution. Also the lack of treatment of the change of hygroscopicity is a shortcoming, as mentioned in the paper. We have now added a specific discussion subsection called "Relative importance of seasonality in emissions and aerosol *removal", where we discuss these issues, also in the light of seasonal cycles of constant-lifetime tracers. Chemical ageing of the BC would lead to slower removal in winter than in summer, which would amplify the seasonal cycle, which is needed anyway, since our model still overestimates summer concentrations.*

5) P. 9581, L. 19-24: Explain explicitly how the differences in the relative enhancement imply show that the enhancement is due to enhancement is mainly due to differences in transport pathways between seasons.

Transport is faster in winter than in summer. This creates a seasonal cycle, which is stronger for shorter-lived species than for longer-lived species (i.e., smaller fractions of the emitted species can reach the Arctic in summer for both species, but with larger differences between seasons for the shorter-lived species). Furthermore, for a short-lived species, the source region is mainly the high latitudes, where the seasonal variability of the emissions is strong; for a longer-lived species, the source regions also include areas further south where the seasonal emission cycle is weaker. This produces a stronger enhancement of the seasonal cycle by seasonal emission variability for the shorter-lived species.

6) P. 9582, L. 3: '5%' should be '5 percentage points'

Yes, indeed, this is better. However, we have now reformulated the sentence such that it now reads: "Overall, for daily resolved residential combustion emissions, the annual mean enhancement for the Arctic north of 66 N compared to annually constant emissions is 68%, compared to the 63% enhancement when using monthly mean emissions." We think that is even better.

7) The implied very strong interannual variability of the measured and modeled monthly means should be somehow indicated ("broken" y axis in figure 8 or a table) to get a better idea of the model performance against the measurements. If one argues that HDD improves the match between observations and the model, this should be evident also in a year-to-year comparison.

In principle, we agree. However, the problem here is the relatively short timeseries of three years, which means that ranges are not representative. We have tried plotting all individual monthly means, but this requires extended y axes, thus, reducing the "interesting" area of the plots, and it also makes the plots more messy overall. Thus, it is impossible to show all six panels in one figure. A solution would be to separate the plots into six separate figures. However, then the results for different stations cannot be compared so easily anymore. In the end, we left the figure as it is.

8) Make sure that in the final figures the fonts for titles, colour bars, etc. are large enough! Currently many of them are impossible to read.

This is a consequence of the landscape format of ACPD. In ACP, these figures would occupy a full portrait page and thus would be much more easy to read than in the present version. We will check this carefully in the page proofs.