In the following, we repeat the reviewer's comments in normal font and give responses in *blue italic*.

Response to Anonymous Referee #1

Here, the authors have exploited the release of radioactive isotopes (137Cs, 133Xe) during the Fukushima Dai-Ichi nuclear power plant accident to constrain modelled aerosol lifetimes in 19 global models. The paper suggests that the majority of models underestimate aerosol lifetimes compared to observations. Deviations between modelled and observed lifetimes are greatest in the Arctic. Comparison of the modelled and observed passive tracer (133Xe) indicate errors in simulated transport mechanisms. However, results from the aerosol tracer 137Cs indicate that the pervasive low bias in Arctic (and global) aerosol (within the majority of models) is driven by too fast removal in the PBL and free troposphere. This study provides a novel method for evaluating global models and I recommend publication after the authors address the minor comments detailed below:

We would like to thank anonymous reviewer#1 for the positive feedback on our paper.

Page 24516, line 11: '....also contribute to the Arctic aerosol underestimates' Underestimate or underestimates is a verb used here as a noun. Please correct by replacing with 'low bias' or re-structuring the final sentence.

The sentence is rewritten and now reads "...also contribute to the underestimation of the Arctic aerosol concentrations."

Page 24516, line 20:
Do the authors really need to abbreviate accumulation mode to AM? If it is necessary, AccM might be better as Aitken mode also begins with A (capital). In general I'm unsure why accumulation mode needs to be abbreviated, ACP has no word limit.

We used the same abbreviation in the previous ACP-paper (Kristiansen et al, 2012) that this study is based on, and we would like to keep using the same style in this follow-up paper.

Page 24516, line 25:
Please add citation.

We added the reference
Andreae, M. O.: Atmospheric aerosols versus greenhouse gases in the twenty-first century, Phil. Trans. R. Soc. A (2007) 365, 1915–1923 doi:10.1098/rsta.2007.2051, 2007.

Page 24517, line 24-25: 'Compared to aircraft measurements models seem to overestimate aerosol concentrations in the middle and upper troposphere. Thus, a short lifetime appears necessary to reproduce such observations.' It is unclear what this statement is in reference to (please cite). To my knowledge AEROCOM has shown a general overestimation of BC mass in the upper troposphere. BC lifetime is not analogous to accumulation sulphate due to the aging step implemented in most model and the size partitioning of primary emissions. (see below)

In the sentence the reviewer is referring to we are already citing "(e.g. Samseth et al., 2014)". In this section we review the literature investigating aerosols in general, and at this point do not compare sulphate and BC directly. However, in the Discussions chapter (two last paragraphs of Section 6) we further highlight the differences between sulphate and BC and mention the aging of BC.

Page 24518, line 24:
As discussed above, I'm unsure why it is necessary to introduce another abbreviation (FD-NPP) shortening to Fukushima seems sufficient.

We used the same abbreviation in the previous ACP-papers (Kristiansen et al, 2012, Stohl et al 2011 and Croft et al 2014) that this study is based on, and we would like to keep using the same style in this

follow-up paper. This abbreviation has also been adopted by many other authors and seems accepted in the literature about the Fukushima accident.

Page 24519, lines 3-4: 'They further explained that elemental carbon or BC were not likely the transport carriers...'
Change to: They further explained that elemental carbon (EC) or black carbon (BC) particles were unlikely to be transport carriers
Changed

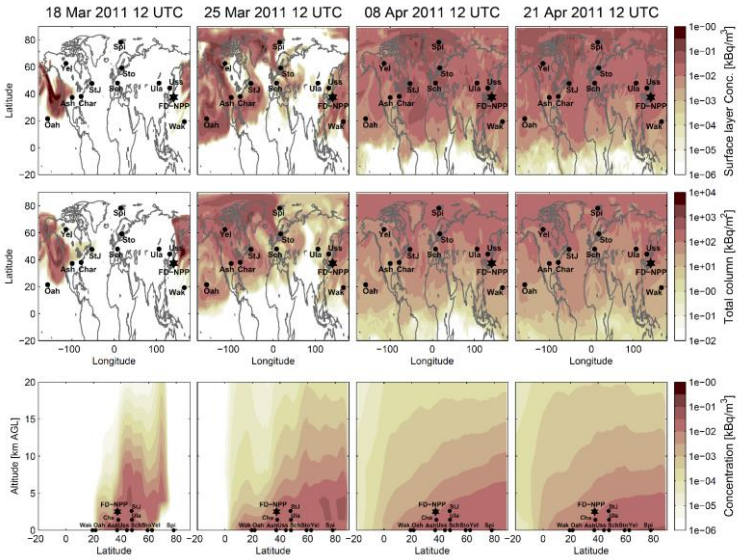
Page 24519, line 20: 'Cesium from FD-NPP'
Was the Cesium (and Xenon) isotope emitted only from Fukushima? Both were released in large quantities but the text includes no explicit discussion of the uniqueness of the emission. If alternative sources exist how would this effect the results?
No other sources of the radioactive isotopes were simulated by the models.
As discussed in Stohl et al (2011) and Kristiansen et al (2012) which this study is based on, there exists a highly variable background of 133Xe in the atmosphere due to emissions from nuclear facilities (Wotawa et al., 2010). Our model does not simulate that background. However, the atmospheric background levels of 137Cs and 133Xe were defined as the measured mean activity concentrations before the FD-NPP accident (August 2010 to 11 March 2011), and subtracted from all measurement values after the FD-NPP accident. The background of the radionuclides is very low with values which rarely exceed the level of detection at most stations. Thus, the effect of the background subtraction was negligible for 137Cs while a small effect could be seen at some stations for 133Xe. We use the background-corrected measurement values in the comparison to the modelled values which also does not include any background, hence the comparisons should be suitable.

We have added a few sentences about this issue in the paper (5th paragraph of the Introduction) including the below reference.

Reference: Wotawa, G. et al (2010) Computation and analysis of the global distribution of the radioxenon isotope 133133 based on emissions from nuclear power plants and radioisotope production facilities and its relevance for the verification of the Nuclear-Test-Ban treaty, Pure Appl. Geophys., 167, 541–557, 2010

Page 24519, line 29:
'fact' not facts
Changed

Figure 2:
To aid understanding of the transport pathways it would be useful to add a fourth (and possibly fifth) column showing the distribution in the first week of emission (2-5 days). Please also consider showing the full latitudinal range (90S to 90N) or using a NH satellite projection centered at the pole. Finally, rainbow color bars are inaccessible to the color-blind and provide a distorted perception of data (http://www.climate-labbook.ac.uk/2014/end-of-the-rainbow/) please consider converting to a hue saturated scale.
We added another column showing the distribution in the first week (18 March 2011), and we have increased the latitudinal range to (20S to 90N). We also changed the color bar according to the reviewers comments.



New figure 2.

Page 24525, lines 15-19: 'Before day 15,…'
As discussed above additional plots in figure 2 would make this easier to follow.
Changed according to the above comment.

Page 24526, line 8: 'Both measured and mean modelled lifetimes are shorter below 50N suggesting less efficient aerosol removal at high-latitudes…'
…in spring. Please refer to time period. Previous work in the literature suggests results at high-latitude may be different in late summer. It would be useful here to include some discussion of the seasonality of high-latitude aerosol lifetime.
We added the time period (March to June). In our study we don't have the possibility to study the seasonality due to the limited time period of the measurements. However, in the introduction we discuss findings from other studies on seasonality. Particularly we mention the study by Zhang et al (2015) who showed that the lifetime depends on season and emission type. Substantially lower BC lifetime is found in summer, likely due to relatively strong wet removal, than in other seasons, and open-fire emissions that have higher initial injection heights which leads to generally longer lifetime than emissions from the surface.

Page 24536, line 8: 'Underestimate near the surface…'
Change to 'Underestimate mass near the surface'
Added "mass" as suggested.

Page 24543, line 15: 'BC removal is also dependent on the aging parametrizations,...'
The deviation between sulphate and BC lifetimes could also be driven by primary emission size (Reddington et al., 2013- http://www.atmos-chemphys.net/13/4917/2013/acp-13-4917-2013.html). Your results here are in direct contrast to the recommendations of Bond et al.,(2013). Please discuss further.
This paper is not intended to be a detailed comparison of sulphate and black carbon lifetimes. We refer to BC papers mainly because most recent aerosol lifetime papers seem to address BC. We have added that model results for BC depend on the emitted size distribution and have added the Reddington et al. (2013) reference. We have also added several sentences explaining that our results are probably not representative for BC aerosol and certainly not for freshly emitted BC. Since this study is not focussing on BC we do not go into details about the recommendations of Bond et al. (2013).

Page 24545, line 10: ’The underestimations are largest for the aerosols...’
Change to: ‘The underestimation is largest for aerosol’
Changed

Page 24545, line 11-12: ’deviations to the observations.’
Change to: ‘deviation from observations’
Changed