Response to Reviewer #1: Jean-Jacques Morcrette

The authors would like to firstly thank the reviewer for taking the time to review this paper and for the constructive comments received. We have tried to address all comments below:

My only concern is the length of period over which this study has been conducted. Whereas I don't doubt that the results would likely stand over longer periods of time, the five weeks considered (17 June-24 July) in this study seem quite short, as only Northern Hemisphere summer is actually studied. However, given the high horizontal and vertical resolutions of the model used (□40 km, 70 levels), I can accept the argument about the additional computational expense in the experiments using prognostic aerosols.

The length of the period was limited, as the reviewer mentions, by computational costs of running the model with prognostic aerosols. We are fully aware of the limitations of such a short period, but still ascertain that the results in this short study are interesting and worthy of publication. We are currently carrying out further work looking at the impacts of aerosols during different periods and for different events such as biomass burning events during the SAMBBA campaign and aerosol impacts on the Asian monsoon as part of on-going projects which will be published in the future.

More detailed comments and typos: ——————————————
l.20: propagate - CORRECTED

l.113: 50 or 30 Wm-2 as quoted in l.1094? l.120: (Allan et al., 2011)

The maximum bias in clear-sky OLR in West Africa shown in Figure 2 of Haywood et al (2005) is 50-55 W m-2 (we have changed the text to reflect this more accurately). The Allan et al. (2011) paper highlights biases of up to 30 Wm-2 in clear-sky OLR based on a climatological mean of an ensemble of 8 climate models. The above line (L113) to which the review refers correctly reflects the bias as related to the MetUM model without any representation of dust. With regards to L.1094 this refers to the resulting negative bias found in this current study using the current dust climatology shown in Figure 6(b). The text has been modified to make this clearer to the reader.

l.214-216: the comments about tropospheric and stratospheric levels should be revised.

We have revised the text removing the references to tropospheric and stratospheric levels.

l.229: which is persisted - CORRECTED
l.263: hydrophilic - CORRECTED
l.297: FFBC or BCFF (cf. l.384) - CORRECTED
l.357 June/July 2009 is quoted here, whereas June/July 2010is quoted in l.218 - CORRECTED (should be June - July 2009
l.362: was necessary - CORRECTED

l.367: tropospheric - CORRECTED
l.384: see l.297 - CORRECTED
l.439: Angstrom (with some marks on A and o?) - CORRECTED
l.442: Sentence starting "AOD measurements from MODIS" is likely to be wrong. I would think the Giovanni server allows to get much more than that.

The text was meant to reflect the starting years of data availability from the satellite datasets (2000 and 2002 for Terrra and Aqua respectively) and not that the data was only available for those stated years - the text has been modified to make this clear to the reader as follows:
"AOD measurements from MODIS are available from the year 1999 and 2002 from the Terra and Aqua platforms respectively." → *"AOD measurements from MODIS have been available since 1999 for the Terra platform and since 2002 from the Aqua platform."*

l.462: Same comment as above also applies to MISR data. http://disc.sci.gsfc.nasa.gov/giovanni has daily servings of MODIS and MISR. As of 20131129, the following data appear to be available for download: MODIS Terra between 20000301 and 20131127 including DeepBlue MODIS Aqua between 20020704 and 20131126 including DeepBlue MISR between 20000225 and 20130831

See response to comment above - text modified to make more clear:
"Measurements of AOD are available from the year 2000." --> *"Measurements of AOD have been available since the year 2000."*

l.484: it's? its? - should be "its" - CORRECTED

l.504: Strictly speaking the areas covered by observations and model simulations are not the same, making a comparison of "global means" difficult.

We recognise that the model and satellite observations are not exactly sampled the same. This is why we describe this first part of the aerosol evaluation as "qualitative" in L.505. We have also removed the sentence referring to global mean biases on L.541

l.553: Caribbean - CORRECTED

l.611 and l.618: Sentences starting on these lines are long and could be cut into smaller chunks. – CORRECTED as follows:

L611: "In producing these mean values both model and observations have been cloud-screened by removing values where both the observations and model report cloud amounts greater than 50\%, however it is likely that some residual cloud contamination remains along the edge of the 50\% contour line in Figure \ref{fig:gerb_OLR}." → "*Both model and observations have been cloud-screened by removing values where both the observations and model report cloud amounts greater than 50%. However, it is likely that some residual cloud contamination remains along the edge of the 50% contour line in Figure 6.*"

L618: "Evaluating the radiation biases early in the forecast minimises the role of other model errors such as general circulation and temperature errors and therefore the role of the different aerosol representations on these radiation biases can be more easily assessed." → *"The impact of aerosols on the model radiation biases is evaluated early in the forecast in order to minimise the role of other model errors such as general circulation and temperature errors."*

l.657: Figures 7 are rather tough to read!

We will make every effort to make this figure more readable in the final manuscript.

l.771 and Figures 12: Top figure is somewhat difficult to read. What about the ARM curve, and the differences to the ARM curve below? Bottom figure does not reference the UM model in the same way as the figures above.

We will make every effort to make this figure larger and more easily readable, making the comparison between model and observations clear and correct labelling.

l.820: Each set of experiments has its own analysis. Would it be possible to get a comment on how these analyses differ (not much, I imagine)? The main improvements described in 6.1 therefore come from the forecasts, not from a potential change in analysis. I suspect that in Figure 13, there is a "story" hidden in the better results for T and RH obtained by AER_DIR and AER_CLIM wrt the other four around 700 hPa for the Northern hemisphere. Have you seen any change in convective precipitation, convective clouds? In discussion of Figure 14., I would point specifically to the areas where an improvement is to be seen or provide a figure showing DIR+INDIR-Analysis.

The verifying analyses used to calculate the mean errors shown in Figure 13 differ by very little between the model simulations (< 0.03K, 0.1m, and 0.2% for temperature, height and RH NH profiles respectively). The results found at 700hPa for T and RH is most likely caused by a cooling above the aerosol layer in the direct only simulations. The Cusack aerosol climatology (used in the CNTRL simulation) has a constant vertical profile in the boundary layer (defined by a set number of model levels up to approximately 700-750hPa (Cusack et al., 1998)) before reducing sharply in the free troposphere. The NH cooling seen in Figure 13 is concentrated over the NH continents and is consistent with a shift in the vertical profile of continental aerosol type of Cusack which is quite absorbing in the boundary layer. Above this Cusack has less aerosol than either the revised climatologies (AER_CLIM) or the prognostic aerosol (AER_DIR) which results in a cooling due the enhanced scattering. The improved aerosol representations in the form of the aerosol climatologies and prognostic aerosols leads to a more realistic aerosol vertical distribution and subsequent beneficial impacts on temperature and RH forecasts. Inclusion of the aerosol indirect effects leads to an overall warming and drying in the troposphere in the NH due to impacts on the cloud fields in this region. This is predominantly found over the NH land regions of northern Canada, Scandinavia and Siberia (where the change in CDNC is largest as shown in Figure 10) and we will refer to the exact areas where these impacts are found in the revised manuscript.

l.864: The increase in Sc by 20% in AER_DIR_INDIR and INIT_DIR_INDIR off-coast Chile and Namibia is not so obvious from Figure 15d and f?

We will improve the readability and clarity of this figure in the final manuscript.

l.921L over across? - CORRECTED

l.951: sentence improperly linked - Sentence removed
l.956: CNTRL - CORRECTED
l.959: negligible - CORRECTED
l.982: Could you put explicitly where previously it has been addressed. - CORRECTED (discussed in Section 6.2 (Clouds and precipitation))

l.1062: Here or in the conclusions, might be a place to stress that not all dusts have the same optical properties with for example various imaginary part of their refractive indices. In this respect, modelling aerosols for NWP will continue to be tough given the constraints of computer costs.
Have added the following text: *"Dust optical properties vary depending on the particle size, chemical composition and distance from source (Ryder et al. 2013b). Using a single set of dust optical properties to model the global radiative impacts is therefore a known limitation in global high resolution dust modelling but is currently necessary given the computational requirements of using regionally varying optical properties as well as the uncertainties in the characterization of dust physical and optical properties."*

l.1070: This is what - CORRECTED

l.1094: 30 or 50 Wm-2 as quoted in l.113? –

Please see earlier response to comment on L. 113. We have amended the text as follows: "However, comparisons of model OLR against GERB observations suggest that the model reduces the OLR by too much in West Africa leading to a significant negative bias of up to 30 W m-2. This is most likely due to inaccuracies in the representation of mineral dust in the climatology." → *"However, the comparison of model OLR against GERB observations in this study suggests that the dust climatology leads to a significant reduction in the OLR over West Africa resulting in a negative bias of up to 30 W m-2. This is most likely due to inaccuracies in the representation of mineral dust in the climatology."*

l.1122: Apart from this (?) - CORRECTED
l.1128: The potential of using = the potential use of the global NWP ... - CORRECTED
l.1212: from NWP to climate, an objective in the development ... (?) - CORRECTED
l.1216-1221: This paragraph sounds a bit too much as "Ten-year plan gobbledygook". Any possibly to say the same thing with more than one sentence?

We have amended the text as follows:

"Near real-time verification of aerosol forecasts produced using short-range high resolution forecasting systems with the wealth of near real-time aerosol observations would feed directly back into the aerosol model development and subsequently would lead to improved predictions of aerosol forcing on climate." → "*Furthermore, aerosol forecasts produced using short-range high resolution forecasting systems can be more easily evaluated against a wide range of near real-time aerosol observations. Findings from such routine evaluations would feedback into aerosol model development and lead to improved aerosol predictions on both NWP and climate timescales and subsequently improve our estimates of the direct and indirect aerosol forcing on climate.*"

l.1227: was? - CORRECTED

l.1301: upper level tropical ... - CORRECTED

Response to Reviewer #2:Georg Grell

The authors would like to firstly thank the reviewer for taking the time to review this paper and for the constructive comments received. We have tried to address all comments below:

"the results for indirect aerosol effects maybe somewhat tentative"

While the results maybe somewhat tentative in terms of the way the aerosol-cloud interactions are currently modelled, this work does highlight the importance of including a more realistic representation of aerosol-cloud interactions than currently used in most global NWP models. We have included text to further highlight these uncertainties (see your further comments on this below).

Include a description of how the interactions are handled in the model - extend description of direct and indirect interactions currently in Section 2. Include how the optical properties are calculated. Details of aerosol removal processes. Details of microphysics scheme etc.

We have included a more detailed description of aerosol interactions in Section 2 (see response to comment on Pg 30459, line 1-4)

Abstract: If you make a point out of improving the simulations in tropical regions, maybe you should mention that the aerosol indirect effects are not included in the convective parameterization.

The abstract does not explicitly highlight improvements in the tropics. We highlight improvements in the northern hemisphere and outline the response found due to the indirect effects in the tropics. We have added text in the Conclusions section to further highlight uncertainties in our representation of the aerosol indirect effect in the model. However, we do not feel we need to also add this to the Abstract, as this is just giving a brief overview of key findings in paper.

Pg. 30457, line 11 – 14: That paper was not looking at aerosol interactions versus no aerosol interactions, but only at the impact of a very strong signal from biomass burning (versus no biomass burning). This was done with cloud resolving simulations that included complex chemistry. And the only improvement we could show was in the lower troposphere temperature, moisture and wind forecast. There was no comparison to CAPE with observations. So I would phrase it more like "in high resolution weather forecasting models MAY improve forecasts. . ..", and take the CAPE out.

Text revised: "They and other studies (e.g., Grell et al. 2011) show that coupling aerosols to radiation and microphysics schemes in high resolution weather forecasting models improves forecasts of temperature, wind and also convective available potential energy (CAPE) during a significant wild fire event in Alaska" → *"They and other studies (e.g., Grell et al. 2011) show that coupling aerosols to radiation and microphysics schemes in high resolution weather forecasting models may improve forecasts of temperature, moisture and wind during a significant wild fire event in Alaska."*

Pg 30459, line 1-4: Is wet scavenging included in the parameterization?

Yes, aerosols are removed via dry and wet deposition processes. Dry deposition is parameterized in a manner analogous to electrical resistance (Seinfeld and Pandis, 2006). The mechanisms for wet removal of hydrophilic aerosols are via in-cloud scavenging by large-scale and convective precipitation and below-cloud (washout) large-scale and convective scavenging of hydrophobic dust aerosol. Re-evaporation of in-cloud aerosol to the accumulation mode is also included when all of the precipitated water does not reach the surface but is re-evaporated. We have modified the text in Section 2.1.1 (CLASSIC aerosol scheme) to include a more detailed description of these processes:

"Aerosols are removed by wet and dry deposition processes. Dry deposition is parameterized analogous to electrical resistance (Seinfeld and Pandis, 2006). The mechanisms for wet removal of hydrophilic aerosols is via in-cloud scavenging by large-scale and convective precipitation and below-cloud (washout) large-scale and convective scavenging of hydrophobic dust aerosol. Re-evaporation of in-cloud aerosol is also included in cases where all of the precipitated water does not reach the surface but is re-evaporated. CLASSIC aerosols have prescribed size distributions and refractive indices as detailed in Table A1 of Bellouin et al. 2011. Aerosol optical properties are calculated offline using Mie calculations and hygroscopic growth is parameterized as a function of relative humidity (Fitzgerald et al. (1975), Haywood et al. (2003)) for the hygroscopic aerosols. The results are stored in look-up tables for use during the model integration."

Pg. 30462, line 13: Aerosol ice interactions are not included. Would you expect this to make a large difference? Could it influence the results that you got for the summertime polar region?

See response to Pg. 30483, line 22-25 below.

Pg. 30477, line 15-19: Here is the only place you bring up the possible deficiency with the aerosol indirect effect. This should also be mentioned in the conclusions and should also have an impact on the abstract.

We have added to following to PP30487, L1: *"Representation of aerosol indirect effects remains one of the largest uncertainties in estimates of aerosol forcing on climate (IPCC, 2013). The lack of a coupling between aerosols and convective parameterization in the present study could potentially lead to inaccuracies in the findings particularly for the tropics."*

Pg. 30482, line 20: If some type of aerosols (even simple versions) are included, volcanic ash can also impact NWP and can easily be added. It would almost have to be added, since otherwise any assimilation of AOD may lead to really strange aerosol concentrations.

We have now referenced volcanic ash to highlight its importance as correctly pointed out by reviewer.

Line 20 now reads: *"The potential use of the global NWP configuration of the MetUM to predict significant aerosol events such as large dust storms, volcanic ash events and an increasing number of wild fire episodes would be extremely important due to the high impact these events have on daily lives and health of the general public."*

Pg. 30483, line 22-25: The results for the polar regions are really interesting and revealing. Do you think that inclusion of interaction between aerosols and ice will lead to any qualitative differences?

The authors believe the primary response we are finding in the polar region is the change from an incorrect land-sea split representation of CDNC to more realistic values for the polar region. As a result CDNC values change from values more representative of polluted industrial regions to pristine marine values with a subsequent reduction in low level cloud amounts and a SW warming. The interactions between aerosols and ice are very much outside the scope of this paper and we can therefore only speculate on the impact they would have on the current findings. In Arctic mixed phase cloud ice lies below the super-cooled liquid (Morrison et al. (2012). Aerosols acting as ice nuclei (IN) will impact the ice / liquid relative contributions in these mixed phase cloud types. While this could impact the LW downward impact at the surface (by increasing LW downward component), the dominant impact is in the SW. Xie et al. (2013) show how inclusion of aerosol-ice interactions in the CAM5 models significantly reduces IN concentrations in the Arctic, reducing low level cloud by approximately 20% and increasing mid-level cloud. The smaller IN concentrations lead to an increased liquid water path and overall brighter clouds, leading to a TOA cooling of about 3 Wm-2. Therefore it is possible that including such interactions would offset the TOA warming found in this study and might impact the LW downward response found at Barrow. However, we believe that this response would be small compared to the magnitude of the response caused by the CNTRL simulation having a totally incorrect representation of CDNC (~30 Wm-2). Furthermore, inclusion of aerosol scavenging by ice-phase clouds will impact aerosol concentrations in the Arctic region. While scavenging in this region during the NH summer (covered by this study) will be dominated by warm liquid cloud scavenging, inclusion of ice-phase aerosol removal would be important for getting NH winter aerosol concentrations and CCN correct (Browse et al. 2012).

Fig. 13: It is hard to see (for me) what really is better where. Fig. 17: I cannot read the winds on these figures.

We will make every effort to increase the size and improve the readability of these figures in the final manuscript.

References:

Browse, J. et al. (2012), The scavenging processes controlling the seasonal cycle in Arctic sulphate and black carbon aerosol, ACP, 12, 675-6798

Morrison, H. et al. (2012), Resilience of persistent Arctic mixed-phase clouds, Nature Geosci., 5, 11-17, doi:10.1038/NGEO1332

Xie, S. et al. (2013) Sensitivity of CAM5-simulated Arctic clouds and radiation to ice nucleation parameterization, J. Clim., 26, 5981-5999, doi:10.1175/JCLI-D-12-00517.1

Fitzgerald, J. W. (1975), Approximation formulas for the equilibrium size of an aerosol particle as a function of its dry size and composition and the ambient relative humidity, J. Appl. Meteorol., 14, 1044-1049.
Haywood, J. M. et al. (2003), The mean physical and optical properties of regional haze dominated by biomass burning aerosol measured from the C-130 aircraft during SAFARI 2000, J. Geophys. Res., 108(D13), 8473,doi:10.1029/2002JD002226