We thank the reviewer for the insightful comments and questions. Please find our responses to questions below.

Q. *p. 28839, line 5-25: This introduction is a bit too general. Instead, you could concentrate to better explain the EAKF algorithm, which is used in the manuscript.*

Response: We have not included the details of the EAKF algorithm to keep the length of the manuscript manageable. However, if the reviewer feels that it is necessary then we can include an appendix describing he EAKF algorithm.

We found far many references in the literature for the application of EnKF with real data compared to EAKF. If the reviewer knows of EAKF references especially in context of aerosol estimation we would like to include them. The following reference is inserted:

Karspeck, A. R. & Anderson, J. L., 2007. Experimental implementation of an Ensemble Adjustment Filter for an intermediate ENSO model. J. Climate, Volume 20, pp. 4638-4658

Q. *p. 28845, line 22: "ensemble based estimation" → ensemble Kalman filter based estimation*

Response: This is corrected.

Q. *p. 28846, line 13: "meteorology" → atmospheric state- p. 28847, line 10: "initial guess" → define in the context of EAKF (usually "prior state", or "background").*

Response: We have used atmospheric state to mean the meteorological state plus the dust variables (dust concentration and AOD). The meteorological state includes only T, U, V, W and humidity at all grid points.

We have modified the sentence to include *prior/background*.

Q. *p. 28847, line 17: Explain the lateral boundary condition for dust in the forecast model.*

Response: We have inserted the explanation in the paragraph 2, section 3.
"For the lateral dust boundary conditions we have assumed that dust does not enter the domain, which is quite large. For the period of our study there is no dust storm east of the Arabian peninsula. So these dust boundary conditions approximately hold. This approximation may impact the estimation results in the real data experiments but it does not impact the OSSE results in any way."

Q. *p. 28847, line 24: Before c_m, write "dust concentration"*

Response: We have inserted "dust concentration"

Q. *p. 28847, line 28, and forwards: You state (here and in many places in the manuscript) that for parameters there is no dynamical equation. And yet you use one: dalpha/dt = 0. In fact, for a well tuned forecast model this is an exact equation. Re-write here, and elsewhere, that this equation is applied although it is not*

exact for an imperfect (un-tuned) system.

Response: We mean to say that for the untuned model we don't know what the dynamical equation should be. We have modified the text in section 3.1 and the last paragraph before section 3.1, to implement the suggestion by the reviewer.

Q. *p. 28848, line 5: "The state variables (T, V, etc.). . ." Please be more accurate here. What exactly are your state variables because the atmospheric state is given (not a state variable)!*

Response: We have modified the sentence for more accuracy. The meteorological state does have a spread because of the ensemble boundary conditions and hence T,U, etc are state variables. But we are not estimating them.

Q. *p. 28848, line 13: "Theory states .." Which theory? Be more specific.*

Response: We have changed it to "theory of data assimilation".

Q. *p. 28849, line 13: A comment: 24-h interval is quote long implying low ratio of observations vs. parameters to be estimated.*

Response: A 24 hour interval was chosen to match the availability of the MODIS satellite data.

Q. *p. 28851, line 11: "is the innovation" is repeated.*

Response: We have deleted this paragraph.

Q. *p. 28853, line 11: "A cutoff radius is not imposed in the vertical". I do not understand, please clarify.*

Response: We have deleted this sentence because it is not relevant. In this problem the fields involved are two dimensional. So the vertical localization does not matter.

Q. *p. 28853, line 22: "Arabia" →Arabian peninsula-*

Response: We have corrected this.

Q. *p. 28853: Figure captions are very long, and at places, duplicate the main text. Reduce captions for better readability.*

Response: We have shortened the captions.

Q. *p. 28854-55: Experimentation provides material for clear understanding of parameters l and c on parameter recovery.*

Response: Ok.

Q. *p. 28857: You could say that spatial correlation in alpha effectively means reduction of the problem size (fewer degrees of freedom to resolve by estimation, and thus better parameter*

recovery).

Response: We have inserted this comment in paragraph 5, section 5.

Q. *p. 28858,line6and7: Fig.5iand5g→Fig. 1iand1g*

Response: We have corrected this.

Q. *p. 28861, line 4: remove "in"*

Response: We have corrected this.

Q. *p. 28861, line 13: Interestingly, "bad" values are near the boundaries.*

Response: This could be because the dust boundary conditions do not allow dust to enter the domain. The assumption that dust does not enter from the boundaries holds true only approximately.

Q. *p. 28861, line 19: You could discuss the realism of the recovered parameter values here.*

Response: We have added paragraphs 3 & 4, section 6.1 to discuss the shortcomings of the real data estimation. Also section 7 is modified to discuss the simplifying assumptions made in this work.

Q. *p. 28864, line 21: Figure 12 is too small, and thus unreadable. Therefore, I have not been able to review any text text between p. 28864, line 21 and p. 28867, line 9.*

Response: We have deleted several panels from this figure and retained only two of them. They are bigger.

Q. *p. 28867, line 11: I wonder whether the term "trend" is adequate here?*

Response: We have replace "trend" with "variations".

Q. *p. 28868, line 20-25: State clearly that in the OSSE, the alpha-map is the only* model error.

Response: We have inserted a sentence clarifying this.

Q. *p. 28869, line 11: Yes, but with the additional complication that the temporal correlation time-scales need to be determined simultaneously.*

Response: That is true.

Q. *p. 28869, line 26: Is this realistic knowing the surface type over there?*

Response: This may not be realistic. The values do look too low. This might be because the estimation is adjusting erodibility to correct a bias in friction velocity or some other model error. The assumption of a perfect model except for error in erodibility is too simplistic. We have inserted texts at several places to highlight this. Please see paragraphs 3, 4 section 6.1 and last four paragraphs of section 7. Particularly we have inserted the following text in paragraph 4, section 6.1.

"Considering the tuned map Figure 8(a), on an average in west Sahara and Arabian peninsula the parameter estimation results in lower values of α compared to the operational values (Figure 1(a)). It is possible that the estimation decreases the erodibility in these areas to correct for a positive bias in the friction velocity."