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Comment

Interactive comment on “Evaluation of IASI derived dust aerosols characteristics over the tropical belt” by V. Capelle et al.

V. Capelle et al.

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Interactive comment on “Evaluation of IASI derived dust aerosols characteristics over the tropical belt” by V. Capelle et al. Anonymous Referee #2

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The paper by Capelle et al provides information on the evaluation of IASI derived dust

C13608

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aerosol properties. It appears that the IASI data have already been shown to give dust properties as published in Peyridieu et al. 2013. The publication of the data and the retrieval method itself is thus not original here. This present study submitted to ACP does not contain new data on science problems associated with dust, something of typical interest for the readers of ACP. While a documentation of the evaluation is useful, I do not think ACP is the appropriate journal and recommend resubmission to a more technical journal, such as AMT. I recommend thus rejection for ACP.

Furthermore the paper still misses several quantifications, which make the work in the presented detail not very useful. It will require major revision. The authors conclude: "The present results demonstrate the usefulness of IASI data as an additional constraint to a better knowledge of the impact of aerosols on the climate system." Sorry to say, I am not more convinced by the results presented here. However, I believe the authors have the data at hand to quantify the bias and provide an uncertainty estimate for the IASI dust related data. I am sure the data will then become useful.

Smaller comments and suggestions for revision:

Quite some editing of the English, inclusion of more comma, removal of vague statements would help the reader.

@@ Authors to Reviewer: - please see answers to Editor's comments - the time period covered in the paper has been extended to June 2013.

p30145

"disinterest" I don't think this is the right word.. @ changed to : ' lack of interest '

"have a high impact in the infrared when aerosols more typical of pollution or biomass burning usually have smaller size and affect less infrared radiation." please rephrase @ Sentence rephrased : ' Aerosols in the coarse mode much affect infrared radiation contrary to aerosols in the fine mode. Dust and sea-salt particles are the main components of the coarse mode, the latter usually remaining in the bottom of the plan-

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etary boundary layer, to which infrared radiances collected at satellite level are poorly sensitive. ‘

p30146

"the domain remains largely unexplored and is still poorly understood" please omit, a little too general. @ omitted

p30148

"at 9.30 p.m. LT" explain LT @ changed to : ‘ at 9.30 p.m. Local Time (LT)

"except in the presence of a strong, recurrent, local diurnal cycle affecting the free troposphere to which IASI is most sensitive." please rephrase. not clear. @ rephrased : ‘except in the presence of a strong, well established, local diurnal cycle affecting the free troposphere ‘

p30149

"The CALIOP mean altitude is calculated in this way in order to avoid the critical influence of the lidar ratio on the estimation of the extinction coefficient (and the optical depth), which might impact a mean altitude estimation" I do not totally agree. I am not convinced the authors make best use of the CALIOP products. In dust dominated regions it should be possible to compare the extinction profile from CALIOP to the dust occurrence frequency from IASI. I believe this is important to better understand the differences between the IASI and CALIOP profiles. At the very least this discussion needs to be extended to the point that a hypothesis is put forward how the bias would look like. As discussed here and later in the text, it sounds more like an excuse, which leaves the reader with no conclusion. @ Please, see the (extensive) answer to Reviewer 1. Added in the text, p. 30147, line 29 : “and the possible misclassification of dust layers as polluted dust or marine inside the marine boundary layer, which in turns affects the assignment of the lidar ratio. These issues, already discussed in Tsamalis et al. (2013), are related to the fact that CALIOP is an elastic lidar, meaning that it

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needs an assumption about the lidar ratio to retrieve the extinction coefficient. Recent studies further corroborate our choice, by finding significant AOD differences between CALIOP and other instruments (Amiridis et al., 2013; Ma et al., 2013; Omar et al., 2013; Tesche et al., 2013). “

p30150

vary according TO the characteristics of the terrain @ done

"presence of high relief" => relief ?? orography, terrain is probably meant @ changed to : ‘ high orography ‘

"For each site, a month of the period ... " please rephrase @ rephrased : ‘ For each site, all the couples of monthly mean IASI AOD and AERONET AOD available over the period considered are included in the evaluation.’

"on a two dimensional plot" not clear. In contrast: what would be a one or even three dimensional plot. @ ‘on a two dimensional plot‘ has been suppressed.

p30151

"To overcome this difficulty, a fit is done, site by site, including all the available items (monthly IASI-AERONET bins) over the period studied, resulting in an IR 10um AOD/500nm coarse mode AERONET AOD “site ratio”." This procedure removes any bias, if I understand the procedure correctly. Firstly, this should be made more clear, outspelled, explained. Secondly, its not clear how the large scale IASI product shall be used, e.g. by modellers, since one would not know which ratio to apply locally. Any recommendation for users? Can this be translated in an error estimate? @ See answers to the Editor. Because translating infrared AOD into visible coarse mode AOD requires accurate knowledge of variables as the infrared refractive index, or the particle size distribution, quantifying the bias between these two sources of AOD is not straightforward. This problem is now discussed in detail in section 3 (Method) and in the new Appendix to this paper. The problem of the bias is also addressed throughout

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the text (Abstract, Method, Results, and Conclusion). The altitude bias is now given and discussed.

p30152

"Here, the test distance has been chosen so that about 7% of the items are eliminated." How much do the IASI results change if the 7% items are included. If one would use the IASI product one would not have the chance to see which 7% of the cases should be removed. Any recommendation for users? How shall the IASI be used as a constraint by climate modellers? @ As explained in this paragraph, this procedure allows eliminating cases which would otherwise mask the real performance of the evaluation. This is common practice. Obviously, such outliers (7 out of 100) can "contaminate" further use of the data (although methods actually exist to detect them), as it is the case for all retrieval processes. . . Moreover, statistics including the "outliers" are given throughout the text.

p30152

Results, first paragraph: While it is useful to have an explanation of possible errors, the text as such is rather trivial and not very useful. It may be shortened to 2-3 sentences. However, a quantification of the estimated error in AOD and height would be very useful and would certainly deserve more explanation and text.

@ paragraph shortened. Now reads : 'A few remarks are necessary to a better understanding of the following analysis. First, the signal induced on IASI observations by each variable of interest, here AOD or altitude, depends on the intensity of the variable. This is however less trivial for the altitude but, generally, the higher the altitude the larger the signal. This is due to the decreasing thermal contrast between the surface and the atmosphere when approaching the surface. For that reason, infrared sounders show a limited sensitivity to the boundary layer. Second, the signal induced by altitude is intrinsically smaller than that induced by AOD: retrieving accurate altitude is more difficult, even more for low AOD. Third, IASI, a remarkably accurate and stable instru-

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ment, has a drawback with the larger noise of its short wavelength channels used for a good disentangling of the AOD and altitude respective signals; this difficulty has more impact on the altitude than on the AOD.

p30162

"These parameters varying from one site to another (and, often, from one day to the next), there is no one common factor reconciling the two observation metrics." There is no reason to believe that the ratio is stable at a given site. Dust properties will change with time considerably even at one site. Utilizing one ratio for all sites would be better, since it would be simpler and more reproducible. Unless more sophisticated modelling would be involved. @ As the reviewer knows, in the infrared as in the visible, the variability of aerosol optical properties depends on that of their microphysical properties. In the infrared, the largest source of variability is that of the refractive index, followed by the size distribution. The shape of the particles has a minor influence. This is different in the visible. In the two cases, assumptions are necessary. Here, the lack of measurements of the refractive index in the infrared, for a variety of representative aerosol types, is the dominant problem. Using one ratio for all sites would mask this problem. In the Appendix, we now show that the IR/Vis coarse-mode AOD ratio may vary from 0.9, using the MITR refractive index model, to 0.5, using the so-called "Fouquart" model. A very large range of variation which forbids taking one unique value for the ratio. Results presented in the paper roughly follow the theory: mean ratios are different over sea, far from the sources (larger) and over land (smaller); over sea, ratios for sites far from the sources are (more or less...) different from the ratios for sites close to sources. New measurements should be available in a foreseeable future and will undoubtedly improve the situation.

p30163

"The box and whiskers results (Fig. 5) are significantly degraded / The Taylor diagram for the altitude over sea" please rephrase @ sorry, we do not clearly understand what

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the Reviewer wants. The first part of the sentence quoted is end of §5.1(AOD) and the second part concerns §5.2 (altitude), line 6. . .

AOD discussion: The site ratio used, removes the bias between aeronet and iasi. However, it would be interesting where bias exists, if a best guess ratio of AOD@IR and AOD@500nm would be applied. How big is the bias and thus error in the IASI AOD estimate. Which part of the bias may be attributed to the coarse mode AOD from Aeronet. This has to be discussed quantitatively to make the work more useful. @ The AOD bias issue is now addressed in Section 3 (Method) and detailed in the new Appendix to this paper. Please see also answers to the Editor.

Altitude discussion: It is not clear which altitude differences exist between the two datasets. Correlation and amplitude do not inform about bias, which is a very basic description for a comparison. How much are IASI and CALIOP disagreeing? Which part of the bias may be attributed as error to both methods. Or ff the two datasets describe different properties, why are they compared at all? I believe the authors can be a bit more quantitative. How does the mean height distribution look like in the two datasets, displayed in a histogram? @ In the revised Figure 7 (Box-and-Whiskers plot; formerly Figure 8), the difference between the median and zero is the bias observed between IASI and CALIOP. An overall systematic bias of -0.4 km (IASI-CALIOP) has been observed; site by site biases are shown. These differences are discussed. Section 5.2 has been modified accordingly.

p30164

refractive index discussion: Please be more quantitative. @ This paragraph has been modified and completed.

tables 2-4 :

If the authors believe that only correlation is of interest, then the correlation columns could be integrated in table 1, saving space. However, one could also argue for more

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statistical info on the comparison, such as mean, median values from aeronet and from iasi, rmse, mean normalised bias, std. The correlation numbers given are not reproducible, so they are not very informative for further work. Scatterplot for land and ocean data might be useful. @ Indeed, Taylor diagrams give more infos than the Tables. Clearly, the normalized standard deviation of IASI (“amplitude”) is as important as the correlation: it has been added in Tables 2-4 and is discussed in the text.

Fig 3,5,7:

It would be more readable if the station identifiers are used on the taylor diagrams. @ we tried this with a poor result due to overlapping.

Fig 4,6,8:

The figure caption is not sufficient. What is really shown here? Ratios? bias in percent? The plot title does not need to explain the plots as box-and-whisker plot. @ Figure captions (now Figs. 3,5,7) changed to : “Box-and-Whiskers plot (ends of the whiskers exclude the outliers) for the difference between IASI and AERONET 500 nm coarse mode AOD (scaled by the site-ratio as explained in 3.) over sea. “ fig 10:

the amplitude curve is not well explained in the caption. It might be useful to mention in the caption that this is for stations over land. @ The caption has been clarified : “Differences, site by site and total, found between the “MITR” and “Revisited” evaluations (see text) for the AOD (IASI versus AERONET) and for the sites over land (see Table 3). Differences in correlation are shown in red; differences in amplitude (normalized standard deviation) are shown in blue. Positive (resp. negative) values mean better correlation and amplitude closer to 1 (amplitude of the AERONET reference) for “MITR” (resp. “Revisited”). “

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 30143, 2013.

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