Answer to Anonymous Referee # 3

We would like to thank Reviewer #3 for his very careful reading of our manuscript, raising questions that needed to be raised. Our answers are given below : in black bold fonts are the Reviewer comments, in blue our answers, and in green the description of the corresponding changes in the manuscript.

I do not understand why the authors have chosen to use WRF model at resolution of 60 km to carry out a study using observations in Lampedusa island, which cannot be represented in the model at such a coarse resolution. Figure 1 clearly shows that the diurnal cycle of surface temperature is not correctly simulated because of the absence of land surface. The authors should justify their choice, and explain if results would be changed with an explicit representation of the island. Moreover, it would be interesting to know if this impact of the island on temperature can be seen at higher altitudes.

The coarse resolution (60x60 km) has been chosen because many companion simulations of the reference (REF) simulation had to be performed for this and other studies : three simulations for the present study, also 4 other simulation in order to test the forecast skills of the model in Menut et al. (2015) as well as other simulations for quantifying the importance of the various aerosol sources in Rea (2015). Therefore, and also due to the need of simulating a huge domain in order to include the dust sources in Africa and the Arabian Peninsula as well as substantial portion of the Atlantic ocean for dust advection, and northern Europe for anthropogenic sources, it was difficult to perform simulations with a finer resolution.

As observed (and also stated explicitly in the article), at such a coarse scale, the Lampedusa island is a subgrid-scale feature of the model. As for the impact of the island on the temperature at higher altitudes, we have no data that could allow us to answer this question : it depends on the altitude of the boundary layer which, regarding an island surrounded by open sea, is a delicate question.

I find that the authors are too optimistic with regards to the performance of the CHIMERE model to reproduce AOD and ozone variations. For ozone, it would be better to justify why the smaller variability in the model compared to observations can be attributed to the use of a climatological value for the stratospheric ozone column. For AOD, figures 3 and 5 show that CHIMERE has some deficiencies that should be better pointed out. For example, during the dust peak from 21 to 24 of June in Lampedusa mentioned page 7601 (line 14), the overestimation by the model is about 40%.

According to the state-of-the-art measumement values from Ziemke et al. (2011) (their Fig.~9b), for the area of Lampedusa, the climatological values for the stratospheric ozone column should be of 280 DU for June and 260 DU for July at Lampedusa, much stronger than the Mc Peters (1997) values used here (respectively 248.6 DU and 236.4 DU for June and July). Therefore, the low bias of about 30 DU in our total ozone columns relative to observed values can be attributed mostly or entirely to the use of the climatology used here for stratospheric ozone values. This insufficient stratospheric ozone column is expected to have a significant impact on the modelled J(O1d) photolytic rates.

Figure 2 also shows that the variability of the total ozone column is much smaller in the model than in the observed values, most likely also due to use of climatological stratospheric ozone colums, because the observed extreme variations of the ozone column (from 360 DU to 290 DU) are too strong to be due to the variability of the tropospheric ozone column. In fact, the ozone column simulated by CHIMERE from the ground to 300 hPa varies around 25 DU, with relatively small variations. This value of tropospheric ozone column is smaller than climatological value from the

Ziemke et al. (2011) results, which is around 40 DU for June-July in the Lampedusa area, but this is consistent with the fact that the atmospheric layer from the ground to 300\hpa{} simulated by CHIMERE does not include the ozone-rich layers of the upper troposphere.

p. 7587, l. 22 : please define J(O1d) and J(NO2)

The definition of these two quantities has been added in the Abstract as requested as well as in the introduction.

p. 7588, line 17 : the effects of aerosols on meteorology and climate should be mentioned, in addition to their effects on the radiative budget

These effects are now briefly mentioned in the introdustion as required, referring to two recent papers on the climate effects on the aerosols and on cloud-aerosols interactions.

p. 7589, l. 24 : could the authors give more details about this climatology for stratospheric ozone (how it has been built, evaluation etc.) ?
We have contacted the developers of the Fast-JX model on this issue before submitting, and had the following answer :

« *The data were sent as personal communications by Labow and Nagatani (the latter dates back to the 1993 Models & Measurements Workshop (3 volumes), but the best ref for the ozone would be:*

McPeters, R. D., G. J. Labow, and B. J. Johnson (1997), A satellite-derived ozone climatology for balloonsonde estimation of total column ozone, J. Geophys. Res., 102, 8875– 8885.

The Fast-JX developers also state that this climatology needs to be updated.. »

Therefore, we unfortunately do not have any exact information on this ozone climatology, which is the one distributed with Fast-JX and recommended by its developers.

p. 7591, l. 8 : what is the resolution used for NCEP/GFS ? I suppose that it is not very different from the 60 km used in the WRF simulation. Has a nudging method inside the domain been used ?

The resolution for the NCEP/GFS analysis used here is of about 75 km at the considered latitude. We added the following paragraph in order to describe more the meteorological configuration :

« The meteorological model is forced at its boundaries by the global hourly fields of NCEP/GFS, and inside the domain the main atmospheric variables (pressure, temperature, humidity and wind) are nudged towards the NCEP/GFS hourly fields using spectral nudging Vopn Storch et al. (2000) for wavenumbers up to 3 in latitude and longitude, corresponding to wavelengths about 2000 km. Nudging is not performed below 850 hPa in order to allow the regional model co create its own structures within the boundary layer. Meteorological input fields have been produced for the same domain as the CHIMERE simulation domain, which is shown on Fig.~3. »

p. 7892, l. 3 : is it really necessary to use a climatology (I suppose that it is a monthly climatology) for dust aerosols in the boundary conditions ? The domain seems to be large enough to include all the dust sources that affect the Lampedusa island. It could even mitigate the performance of the model if climatological dust plumes come from remote places.

The standard configuration of CHIMERE includes the monthly GOCART climatology for dust at the domain boundaries. It is true that, for a small domain such as, e.g., continental France, this method has the advantage of reducing model bias by including a background dust level. However, the domain used here is very large, and Lampedusa is very far away from domain boundaries, so that this background level due to dust imported from domain boundaries will be at insignificant levels at that location, particularly when compared to the substantial dust plumes that have been simulated and observed at Lampedusa and other locations, as described in the manuscript.

p. 7893, l. 12 : Are these values of radiative indices specific to Saharan dust ?

Variability in the radiative indices of dust exist due to the different mineralogies of the source areas : clay, quartz, etc., as well as the content of iron and other minerals. The values used here are from a large global sampling based on AERONET measurements (Kinne et al. 2003), therefore based on global averages on dust properties based on the inversion of AERONET measurements.

In a study published in 2009 in Tellus (*Saharan dust absorption and refractive index from aircraft-based observations during SAMUM 2006*) Petzold et al. give measured values of the refractive indices of Saharan dust in Morocco for three episodes. At 450 nm, the given values values are between 1.549 and 1.559 for the real part, and 2.7*10^-3 and 6.1*10^-3 for the imaginary part . These values are slightly higher than the value we used for the real part (1.53), but the difference is very small. Regarding the imaginary part, the values given by Petzold et al. are of 2.7*10^-3 and 6.1*10^-3 (2.7*10^-3 at 400 nm and 8.9*10^-3 at 600 nm in our study). They also show that the variations of the imaginary part are very strong depending on the mineralogy and source area of the dust (their Fig. 8) so that the values used here, even they are global averages, are within the uncertainty on the refractive indices in the current state of the art.

p. 7599, l. 16 : How could these missing dust emissions be explained ? Is it due to a poor characterization of the soil characteristics in some regions ? Is the soil humidity taken into account in the calculation of dust emissions ?

Many explanations for the missing dust emissions are plausible, including misrepresentation of the wind fields, and of the humidity. Humidity of the soil is taken into account in theis version of CHIMERE, and may therefore be over- or underestimated over some regions.

p. 7600, l. 10is it 600 nm ?

The comparison in the initial paper was indeed between MODIS measured values at 550 nm and CHIMERE values at 600 nm. However, in the revised version, this has been changed by calculating the CHIMERE AOD at 550 nm by exponential interpolation between the value at 400 nm and at 600 nm.

p. 7600, l. 22 : To evaluate more precisely this plume of strong AOD, it would bee nice to have AERONET stations in northern France or in the british isles. Because in Fig. 3, only the points where MODIS data are available are taken into account, so that this plume of strong AOD is not evaluated.

This has been done in the new version by performing some statistical comparison with the AOD at two AERONET stations in northern Europe, Palaiseau (close to Paris, France) and Mainz (Germany). Unfortunately, data availability on these and other stations in northern Europe for this period was rather poor (see the number of available data points in Tab. 3 of the revised version, 202 / 961 in Palaiseau and 250/961 in Mainz), and did not cover the period when the dust peak was simulated so that we are not able to evaluate the presence of a dust layer over northern Europe at that time.

p. 7601, l. 27For the peak in Oujda between 12 and 17 June, it seems that CHIMERE simulates a second peak after the first one which is not the case in observations.

This is true, but given model uncertainties and missing data during nighttime, it can only be stated here that both in model and observations there is a period with significant dust AOD from June 12 to June 17 at Oujda. The expression « peak in AOD » in the initial manuscript was misleading and not describing exactly the plot. We replaced it by « period of strong AOD » in the revised version.

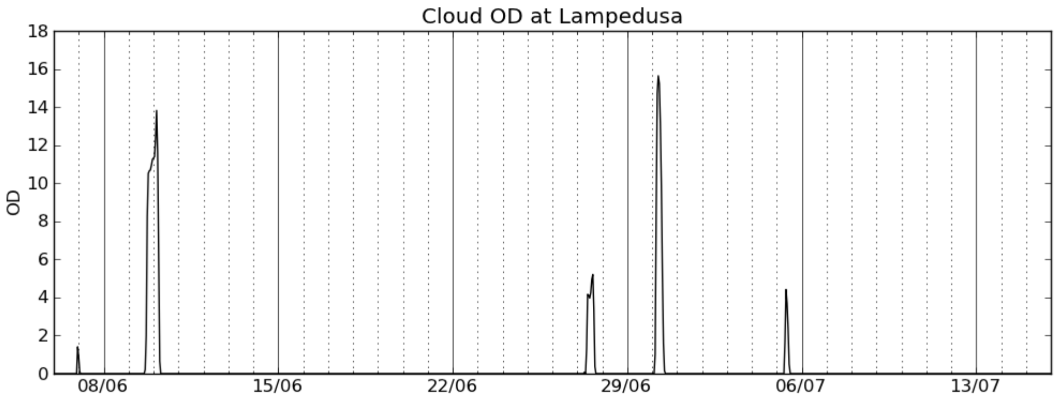
p. 7601, l. 10 : could the meteorological conditions explain that CHIMERE has missed the peak between 25 and 30 June ?

Errors in the transport due to misrepresentation of the meteorological conditions are among the possible causes for this missing peak in the model. Other possible contributing cause can include the lack of dust emissions over continental Europe (which are not taken into account in this model version). We agree that, from a model's developers point of view, these kind of observations are very useful and interesting. However, since we do not have more precise quantitative data to analyze this peak, we prefer not to engage at that point in the manuscript into developments which would turn out to be too speculative.

p. 7604, l. 29 : I don't agree that the overestimation of the wind during periods of weak winds can explain the excessive background SSA, as even when wind is not overestimated, SSA concentration is overestimated (For example on 8 June)
It is true that this interpretation is too speculative. We therefore suppressed the following sentence from the manucript :
« a period of very weak wind during which the model tends to overestimate the wind. This overestimation of the wind during periods of weak winds can be a factor explaining the excessive backgroung sea-salt concentration »

The question why the model tends to overestimate the sea-salt content is therefore left open in this study. This could be due to a misrepresentation of the low-level wind, as it was first assumed, but also to deficiencies in the sea-salt emission scheme or in in the transport, scavenging and/or deposition of sea-salt, as well as problems of the meteorological model to represent adequately the marine boundary layer.

p. 7607, l. 25 : As the presence of clouds is discussed in this paragraph, it would be interesting to add an estimation of the cloud cover simulated by the model in Fig . 1 in addition of temperature and wind.



The cloud optical depth simulated at Lampedusa is shown above. We do not think that this figures brings a lot of additional information, since the statement « In the model, cloud cover was present over Lampedusa in daytime only on June 27, June 30 and July 5. » is already present in the article. Further comparison of the measured vs. simulated cloud cover is beyond the scope of the present study.