

## ***Interactive comment on “Atmospheric deposition of organic matter at a remote site in the Central Mediterranean Sea: implications for marine ecosystem” by Yuri Galletti et al.***

### **Anonymous Referee #1**

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The manuscript addresses the atmospheric deposition of organic matter in the Mediterranean, for which there is little data available. It quantifies such deposition in the small island of Lampedusa in the Central Mediterranean, in terms of carbon, nitrogen and phosphorus. It also tries to untangle possible sources of such organic matter. In this aspect the manuscript is less conclusive as there is no good relationship to aerosol origin or type of deposition. The conclusion is that the OM is mainly coming from sea-spray that the different air masses pick up and transport to wind up depositing. It could be in large part but really it is just a hypothesis that needs further exploration. Also, I was surprised not to consider wind direction properties when analyzing deposited material. Lampedusa is a small island but I would not be surprised that when wind blows

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from directions other than due East, and especially when it blows over the island from the West, substantial OM could be picked up from the island itself. A third aspect of the manuscript deals with estimating the local and Mediterranean basin-wide importance of such deposition estimates for the biogeochemical functioning of the Mediterranean. I like this part myself but I have to admit it is the least elaborated since it is based on assumptions that will be hardly met. For instance, calculations based on the extension to the whole Mediterranean of the measured OM deposition at Lampedusa. Given it is so variable and without a clear reason, I would expect variability to increase when other locations are taken into account. Also, the lability of the deposited organic matter is an unknown, so the final role of the marine biota is also unknown. But anyhow, I like these exercises.

Thus, to me the main value of the manuscript is to provide a much needed data series of OM deposition measurements. The methods are standard within the field and thus assure quality control. Maybe I am not clear whether monthly data were calculated and how or whether just sample data was provided always? or in what cases? That is, how were data treated when more than 1 sample per month was available? How was the data split when covering periods from two consecutive months?, etc. I understand that sample data is clearly reported in Fig. 5, but how were the rest treated is a bit mysterious, especially since bars have unequal width within and between figures.

In line 150 it is also important to know the flow rate of the low-volume sampler. Also, I guess that because of physical flow rate constraints a 1  $\mu\text{m}$  filter could not be used. That would have been much more desirable since there tend to be organic rich particles at the very fine particle ranges, and they would have been missed, not a minor issue in this paper on OM. I would like the authors to comment on the choice of a 2  $\mu\text{m}$  filter to collect particles.

The manuscript is well structured and balanced. The title is informative of the contents. The language is proficient. Figures should be uniformized or clarified in aspects such as the x-axis but are otherwise well done.

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Other than that, I have no major concerns publishing the manuscript pretty much as it is.

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