

**We are grateful to Dr. Ceburnis for the valuable comments and references. Our responses to the comments and details of the changes made to the manuscript are given below.**

*Comment 1:* I am somewhat confused over the terms “open oceans” and “coastal oceans”. By definition the ocean beyond a continental shelf should be considered an open ocean. The continental shelf in the reported region R3 does not extend beyond 100 km from the coast and the shelf sea occupied only a part of R3 region. In my opinion the difference between open ocean and coastal ocean in this study was in the abundance of chlorophyll in surface waters. However, chlorophyll rich waters can be found thousands of kilometres from the coast as in the North Atlantic & Arctic (O’Dowd et al., 2004; Yoon et al., 2007; Russell et al., 2010) or the Southern Ocean (Sciare et al., 2009). Is it not better referring to “high/rich” and “low/poor” chlorophyll regions?

***Reply 1:* We agree. In the revised manuscript, we have used the term “high and low Chl *a* regions” to refer to R3 and R1/R2, respectively.**

*Comment 2:* I suggest using the term “sea spray” when referring to primary marine particles as sea salt is only one component of sea spray (Section 3.1., line 31). Major sea salt ions should not be expected to correlate linearly with wind speed as there is a power law relationship between sea salt(spray) mass and wind speed, e.g. (Ovadnevaite et al., 2012), hence, moderate linear correlation observed. Cl depletion during certain periods was rightly attributed to acidifying species where sulfate (sulfuric acid) should have been dominant component contrary to nitrate (nitric acid) which significant presence could not be corroborated by stable carbon values (significant nitrate must be anthropogenic and consequently would have driven C13 to more depleted values). It is very likely that if sulfate was plotted against Cl/Na ratio an apparent anti-correlation was clearly revealed. Cl depletion by sulfuric acid is rarely implicated due to the ever-present nitrate, but in clean oceanic regions sulfuric acid must dominate the observed depletion.

***Reply 2:* As suggested, the term “sea salt particles” has been replaced by “sea spray” in section 3.1 (P.4, L.31). In addition, we have added the following sentence to explain the moderate linear correlation between sea spray mass and local wind speed (P.5, L.1–3): “This is consistent with the wind-driven production of primary marine aerosol particles, whereas the moderate linear correlation can be explained by a power law relationship between sea spray mass and local wind speed (e.g., Ovadnevaite et al., 2012).”**

**Regarding the chloride depletion, the  $\text{Cl}^-/\text{Na}^+$  ratio tended to decrease with increasing**

sulfate, whereas this is not apparent for nitrate. In the revised manuscript, the following sentence has been added: (P.5, L.29–30) *“In fact, the  $Cl^-/Na^+$  ratio tended to decrease with increasing sulfate concentration (not shown in the figure), whereas this trend is not apparent for nitrate.”*

*Comment 3:* Despite anthropogenic sources contributed negligibly to the TC mass, its contribution cannot be totally discounted in the Northern Hemisphere and it would have resulted in slightly more negative stable carbon ratio of TC as it was measured (Section 3.5). Very small contribution of anthropogenic carbonaceous matter would not compromise the results of this study as the region was predominantly clean based on cumulative evidence from all the tracers. It is hard to discount small amount of anthropogenic tracers without concurrent BC measurements as the Shank et al. study was conducted during different season and meteorological conditions. Lastly, even small amount of BC is always accompanied by a small corresponding OC mass.

*Reply 3:* We did not intend to imply that the contribution of anthropogenic sources is negligible. Both the  $\delta^{13}C$  and Lagrangian trajectory analysis indicated that marine-derived carbon accounted for the majority of submicron WSOC. These results contradict those of Shank et al. (2012), who suggested that there is little to no marine source of submicron OA to the atmosphere. Therefore we wish to emphasize that DOC substantially contributes to the submicron WSOC mass across the study region. In the revised manuscript, we have added/modified the following sentences taking into account the comment: (P. 10, L28–31) *“It is noted that the contribution of anthropogenic sources cannot be negligible although this is indicated by the isotopic analysis. Nevertheless, the present study demonstrates that DOC is closely correlated with submicron WSOC aerosols and implies that it may characterize background OA in the MBL over the study region.”*

We have also added the two references of Chesselet et al. (1981) and Ceburnis et al. (2011) to the Introduction section.

#### References

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