

# ***Interactive comment on “Atmospheric Water-Soluble Organic Nitrogen (WSO<sub>N</sub>) in the Eastern Mediterranean: Origin and Ramifications Regarding Marine Productivity” by Münevver Nehir and Mustafa Koçak***

## **Anonymous Referee #2**

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Review of the article entitled “Atmospheric Water-Soluble Organic Nitrogen (WSO<sub>N</sub>) in the Eastern Mediterranean: Origin and Ramifications Regarding Marine Productivity” by Münevver Nehir and Mustafa Koçak.

An interesting article dealing with occurrence, size distribution and impact of WSO<sub>N</sub>, a class of compounds not well studied up to date. The manuscript is well written and deserves publication once the authors addressing the following points. Abstract: - Aerosol WSO<sub>N</sub> concentrations exhibited large temporal variations mainly due to rain and the origin of air mass flow. Rain scavenges all compounds why preferably WSO<sub>N</sub>?

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the authors should be more clear, e.g better say meteorology instead on rain -Line 27: agricultural activities (43 %), secondary aerosol (20 %), nitrate (22 %), crustal (10 %) and sea-salt (5 %). NO<sub>3</sub> is a secondary aerosol? Please be more specific. See also comments below regarding sources attribution. -Line 29: Considering the Cilician Basin, the atmospheric water soluble nitrogen flux would sustain 33 % and 76 % of the new production in the associated coastal and open waters, respectively. Cilician Basin as part of the E. Mediterranean is mainly P limited. Thus explaining productivity on the basis of N only could lead to erroneous results. I suggest to remove this part or to be more cautious. Experimental section. -Line 122: The observational coverage of the aerosol sampling period was 80. Did the samples were uniformly distributed all over seasons? -Line 164: Did the authors perform recovery experiments with well-known mixtures of organic and inorganic compounds? It is reported that High Temperature catalytic oxidation is not fully recovering WSON from mixtures with inorganic substances. Results: -Please also report median in addition to average. Also how mean and median values changes if all data are considered (sensitivity test). -What is the seasonality of coarse/fine ratio of WSON? Also how this ratio varies as a function of air masses origin or better in dust vs non-dust samples? -At Table 4, the number of samples reported is only 216, whereas at line 120 they report analysis of 337 results. Why this difference? They correspond to samples with precision larger than 0,3? If yes what is the seasonal representativity of these 216 samples? -In rain water how many values have been omitted with precision lower than 0,3? What is the temporal variability of the omitted data?

Sources attribution (PMF). Using only IC data, source attribution of WSON is highly subjective. No ancillary data such as metals or OC/EC, NO<sub>x</sub> are available? Why no NH<sub>4</sub> is found in agricultural factor? Given the compounds associated with this factor better assign it to soil re-suspension. Also factor A should better attributed to long-range transport (regional sources) due to the presence of NH<sub>4</sub> and SO<sub>4</sub>. Similarly factor B with the presence mainly of NO<sub>3</sub> could be better attributed to Anthropogenic sources (combustion) Line 487, better replace water-soluble nitrogen by WSTN.

Finally as said before, I strongly disagree with the utility of paragraph 3.7. The Eastern Mediterranean area is P limited thus all primary marine calculations should be based on P availability or better P and N availability. The conclusions presented at 3.7 are thus questionable.

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