

The authors have done a fair job in addressing most concerns raised, however, there are still a few issues outlined below that should be addressed.

Nss-sulphate

I guess my comment on the inconsistency was not clear; I have not questioned the contribution of different sources to nss-sulphate concentrations in different air masses, which is nicely described in the author's response. However, this is not the case in the MS, where there is no separation between different air masses (e.g. Figure 4 and 5). For the specific examples see below:

The inconsistencies in the manuscript with respect to nss-sulfate have been addressed and the specific corrections are given below.

L293-296 “ During non-dust period long range transport from the northwestern African coast, Europe and secondarily formed PM from the ocean were the main sources of nss-aerosol constituents. However, in the case of sulfate, nss-sulfate from oceanic sources was more important. ”

L295 “However, in the case of sulfate ... important”, has been removed from the manuscript.

L401-409 gives some indication and points to the secondary production, but emphasis is still on the oceanic sources.

The further elaboration of the marine source in this section is to indicate similar observation of nss-sulfate marine sources observed elsewhere and not to further justify that in this region, this source must have same impact.

To clarify this, Lines L407 was ameliorated to show that the arguments are not directly link with this observation, but are just comparative observations made elsewhere.

L407 has been edited to read, “Elsewhere, seasonal trends of”

In addition, lines L423-424 “photochemical production of nss sulfate and increased emission of marine precursors are even more important during the summer in this region”.

Has been changed to

“ ... and increased emission of marine precursors could have also been important during the summer in this region”

I understand this is valid for all air masses during non-dust periods and I read the general conclusion, which states that nss-sulphate concentration was dominated by the marine source.

The sentence “nss-sulfate concentration was dominated by marine sources” has been changed.

The sentence now reads “photochemical production during the summer as well as long range transport dominated the nss-sulfate concentrations.

Lines L801-802 “Marine precursors could explain about 40 ± 20 % of the observed nss-sulfate” has been removed from the general conclusions.

Furthermore, L691 has also been edited to read, "...likely due to the increase in photochemical production activities and the emission of marine nss-sulfate precursors"

While the following paragraph attributes it to the anthropogenic sources: L563-566 " Nitrate and nss-sulfate showed good correlations during the winter and the summer ($r^2 = 0.72$) seasons (Nov-Apr and May-Oct, Figure 5) which could be attributed to their anthropogenic origin due to observation of frequent elevation of these concentrations during long range transport from Europe and Africa."

The same non-dust periods and long range transport from Europe and Africa as before, but here nss-sulphate is dominated by the anthropogenic sources; I understand that figure 5 includes all summer and all winter measurements without separating into different air masses?.

Those are only few examples ...

The discussions on the role of marine sources on nss-sulfate concentration in the manuscript have been revised. Marine sources are now stated as a possible source and no more as a dominant or major source. The details on where these changes have been made on the manuscript have been listed above.

Secondly, I still think that an addition of secondary marine nss-sulphate would distort the correlation (Figure 5), and would not contribute to the slope: biogenic nss-sulphate would not depend on the anthropogenic nitrate; therefore, high marine nss-sulphate contributions would equally occur at low and high nitrate levels. Moreover, adding a random amount of marine nss-sulphate at all levels of nitrate would introduce either scatter or intercept, but it would not affect the slope (Slope could only be affected by the proportional addition of more nss-sulphate for increasing nitrate, which, to my knowledge, could not be the case for marine nss-sulphate).

It should also be pointed out here that the main source of secondary nss-sulphate is assumed to be a cloud processing and condensation of gaseous precursors on the existing particles and not a nucleation. This is still not clear in the MS, and causes confusion.

With respect to the correlation, the reviewer is correct and this aspect has been revised on the manuscript on lines L573-578 and now reads

"However, during summer the photochemical production of particle phase nitrate and sulfate is higher. This may therefore lead to the observed increase in the nss-sulfate concentration reflected by the increase in the slope. The combined effects of increased winter emissions of anthropogenic precursors reduced winter and enhanced summer photochemical conversion might explain the effect depicted in Figure 5".

And finally, a rhetorical question: what did authors mean by gaseous fresh nss-sulphate emitted from marine sources as stated in the reply?:

‘ It is true that the freshly emitted nss-sulfate e.g from marine sources may not have enough time to get internally mixed with freshly emitted sea salt particles, but subsequently depending on the residence time of the sea salt particle these gaseous species may condense on the existing sea salt particles, thus, the term aged sea salt particles. ‘

This was a mistake from the author's side. The sentence should have been, "it is true that freshly formed sulfuric acid from marine precursors may not have enough time to get internally mixed with freshly emitted sea salt particles but subsequently, depending on the residence time of the sea salt particle, it may react with the existing sea salt particles, thus, the term aged sea salt particles.