This paper has gone through substantial revisions and, based on apparently very critical earlier-round referee comments, the EC part of the analysis has been taken out of the paper. The current version of the paper is in a relatively good shape. I have a few, mostly minor, comments that should be considered before accepting this paper for publication.

Thank you. Minor comments were addressed, Figure fonts unified and resolution increased to 1200dpi.

Scientific comments

One thing that the authors did not consider when discussion the result was the potential influence of the coarse/submicron partitioning of a compound. This definitely affects the relations between two compounds (Figure 3) and might also affect concentration gradients. This could, for example, be one explanation for the odd relation between MSA and SS in Fig. 3. MSA is rather non-volatile, so the great majority of it is expected to be in the aerosol-phase in the marine boundary layer. When there is lot of sea-salt, a bigger fraction of MSA is expected be in coarse particles, lowering its lifetime and thereby its concentration. This might explain the relatively low MSA concentrations at high SS levels. Likewise, when SS concentration are moderate to low, one would expect more MSA in submicron particles with higher lifetimes, and thereby a higher MSA concentration. This is exactly what is seen in Figure 3. The authors might think of this partitioning issue and perhaps add something to the manuscript along with it.

We agree with the reviewers considerations and we discussed following exact same considerations. However, the reviewer missed or misinterpreted MSA gradient which is not as monotonic as the nitrate or oxalate gradient, but more similar to WSOC or WSON gradients and grouped accordingly. The partitioning issue has been discussed in detail by Rinaldi et al. (2011) who presented long term size-segregated measurements of MSA, nitrate and oxalate in clean marine air masses at Mace Head. Nitrate was distinctly present in super-micron particles while MSA and oxalate were relatively equally partitioned between sub and super-micron particles. We believe that the issue is adequately addressed and the above study is referenced several times in the text as quoted below.

"The relationship of sea salt and oxalate (top right plot of Figure 3) was slightly different from nitrate and somewhat similar to MSA. While oxalate (and MSA) can indeed condense on pre-existing sea salt particles, its chemical pathways of secondary production are different and more diverse than that of nitrate as were detailed by Rinaldi et al. (2011)."

The authors should be more careful when using statistical expressions. For example, what is statistical basis of stating "…were not significantly different" on line 359? What does "correlated at a significant level" on line 444 mean? The strength and significance of a correlation are two different things. On lines 511-522, should one say inversely related to… than inversely correlating with…?

We are fully aware of the fact that the strength and significance of a correlation are two different things and that the strength is not important without stating its significance. Significance of a correlation is reported at P<0.01 level and now noted in brackets. Regarding statement on line 359 it is based on overlapping error bars which is now noted in brackets.

Influence of partition between coarse and submicron particles 435

Addressed with above comment.

Technical comments

The first paragraph of introduction is very long. I suggest slitting it into 2-3 parts, e.g. on lines 46 and 62.

Done

Section 2 starts with a general background of the approach and then introduces an instrumental set up. To make this clearer, I suggest adding 2 sub-section titles to this part before the current sub-section 2.1.

Done

Sections 2.2 and 2.3, and elsewhere. It is a bit confusion that the authors use the same notation for both the name of compound and its concentrations. The authors could think of using e.g. brackets (i.e. [WIOC]) when meaning concentration (like in eq. 3) or writing explicitly WIOC concentration in the text. Compound names should not be italicized like on lines 239-249.

All notation have been correctly introduced in previous chapter with [WIOC] meaning water insoluble organic carbon, not concentration. Therefore, WIOC concentration is a valid expression. Italicized notation appeared when linking to formula and has been changed now to regular.

The same symbol should be used: H_BL in eq. 1 vs. H in eq. 2

Done

line 139: 30-m height

Done

lines 269, 271, 278, 579 and 642: there should be a space between the variable and unit

Done

line 310: should it be oxalate rather than oxalic acid?

Done

line 321: section 3.4 rather than chapter (the same on line 513)

Done

line 366: here, one may get the wrong exression that ammonium is neutralized by sulphate, even though it is the other way around

Done

Finally, the figures should be understandable by themselves. Therefore, I recommend moving some explanation given on calculated NH4 concentration explained in the main text (lines 364-370) to the caption of Figure 2 (By the way, the yellow circles in Figure 2 do not show up clearly, maybe some other colour would be better). In the caption of Figure 4, it could be added: …contribution of organic matter in sea salt particles (OMss).

Done

Language issues (mainly suggestions)

line 144: …any particular wind speed

line 171: “did not have” correct tense?

line 184: sufficient at or sufficient for?

line 337, comma before “it should”

line 359, comma before “preventing”

lines 400-401: …were mostly below detection limit…

line 427: comma before “its”

lines 474-479: this is a complicated sentence, please reword.

line 515: comma after “range”

line 539: the comma should be after, rather than before, “that”

line 542: in the submicron particle range

lines 557-560: this is a complicated sentence, please reword.

line 604: the occurrence of

line 617: comma before following

All corrected