

In general, the revised version demonstrates better structure and provides a clearer explanation for biases between modeling and observations. I recommend applying the ‘tracer’ method, to explicitly incorporate species in the MONARCH model. This would help address uncertainties that remain unresolved in the revised manuscript. For instance, it may shed light on the speculated effect of deposition velocity on biases between modeling and observation. While the authors have adequately addressed most of my comments, I provide in the following additional feedback on some of my original comments that require further attention.

Lines 26-27 - The sentence should be clearer regarding the contribution of VOCs to SOA via those oxidants.

Answer: In response to a similar concern raised by another reviewer, we have further clarified the sentence as follows: “Additionally, it is important to note that human-induced atmospheric changes through land use management increase oxidant levels which can also boost natural aerosol production like biogenic SOA (Kanakidou et al., 2000)”

Response:

I'm uncertain why land use management would necessarily lead to increased oxidant levels. Isn't oxidant formation primarily associated with anthropogenic activities rather than land use changes?

Lines 58-70 – Not clear to me which information given here specifically refers to the UK.

Answer: To improve the readability, as also pointed out by another reviewer, we removed “in the literature” from the sentence.

Response: It is unclear whether the reasons mentioned after 'This is due to several reasons' specifically refer to the example provided for the UK or if their relevance is broader.

Line 249 – “Urban and suburban industrial stations were also aggregated” – what is the rationale for this aggregation?

Answer: The authors changed the sentence to include the rationale as follows: “Urban, suburban and rural industrial stations, when available, were also aggregated due to their similar observed range values and trends. This consolidation was relevant as there is only 1 urban station and 1 rural industrial station.”

Response: Given that there is only 1 urban station and 1 rural industrial station, wouldn't it be logical to mention that only suburban stations were aggregated?

Lines 312-313 – “Notably, underestimations are more pronounced during winter, suggesting a potential underestimation of road traffic cold start emissions” – For traffic? I don't see that this winter trend is significant when looking at Fig. 7 and Table 2

Answer: The authors agree that when evaluating the average of all the traffic stations, as presented in Fig.7 and Table 2, this might not be so clear to the reader. Despite this, in Table 2 we can see that the lowest MB values are in summer and the biggest in winter. This effect is more evident when looking at specific stations, e.g. stations located in Barcelona and Valencia, as presented in Figure 16.

Response: I cannot locate Figure 16. Could you please direct the reader to the table or figure where this trend can be observed? If this minor trend is only evident in a few stations, it would be advisable to restrict the discussion to those specific stations.

Line 363 – “chemical processes affecting VOCs” - This is not clear to me. Can you specify what kind of chemical reaction could lead to an earlier VOCs morning build-up compared to the measurements? Do you imply that benzene and toluene are formed by chemical reactions which occur in the morning? Could meteorological effects/stratification of the atmosphere play a role here too?

Answer: While uncertainties in the meteorology and emissions could play a role, the authors believe that one of the main processes affecting these results is the dry deposition. This is mainly because, for the same stations, for NO₂ the model is performing well.

Response: The provided explanation is not convincing. Here, the discussion addresses the time of the peak rather than its magnitude. Different deposition values would likely affect the amplitude rather than the timing. Additionally, the agreement between simulated NO₂ and observations does not support this conclusion. Additionally, referring to 'chemical processes' in the context of dry deposition is problematic, while PBL is not a meteorological parameter."

Line 409 – “Indicating an issue related to VOC chemistry in MONARCH’ – Can you explain why you necessarily attribute this issue to VOC chemistry? The same comment is relevant for toluene and benzene.

Answer: The authors gave a detailed explanation regarding this point in the previous comment regarding L363. To specify what we believe to be the main issue, we added in line 409 the reference to the dry deposition as follows: “...indicating a possible issue related to VOC processes in MONARCH (i.e. dry deposition).”

Response: Please refer to my response concerning the original comment on line 363.

Lines 612-613 – “suggesting that some sources are either not accurately represented in our model or are unaccounted for” - What about atmospheric chemistry effects and/or meteorological effects?

Answer: The authors agree with the reviewer that besides the emissions, there are several other sources of uncertainty affecting the results, e.g., the methods used in the measurements and the model performance reproducing the meteorological and chemical parameters. Therefore, this was revised in the manuscript and a paragraph was added as mentioned in the previous comment.

Response: I don't see that you addressed atmospheric chemistry and/or meteorological effects in the revised paragraph.

Line 643: “this could in the model could lead to further improvements” – please revise.