



EGUsphere, referee comment RC1  
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## **Comment on egusphere-2022-415**

Anonymous Referee #1

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Referee comment on "Potential impact of shipping on air pollution in the Mediterranean region – a multimodel evaluation: comparison of photooxidants NO<sub>2</sub> and O<sub>3</sub>" by Lea Fink et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-415-RC1>, 2022

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### **General comment**

The paper reports a modelling study of the contribution of shipping to NO<sub>2</sub> and O<sub>3</sub> concentrations in the Mediterranean area. In addition, deposition is included in the analysis. Results of different models are compared among themselves and with measurements in some specific stations. The topic is interesting and the paper generally well written. It has elements of novelty and I believe that it could be published after a revision step necessary to clarify some aspects and put results in a better perspective, see my specific comments.

### **Specific comments**

Lines 31-32. It is not clear this sentence. The other two models use different meteorological input?

Lines 41-43. I suggest to mention the recent work of Contini et al (Atmosphere 2021, 12, 92.) that gives a global overview of the effects of shipping on air quality and health.

Line 140. Please remove etc. if authors want to add something it is better to do it explicitly.

Line 243. Actually, looking at the map in Fig. 1 it seems that it is included also the major part of eastern Mediterranean.

Section 2.5. It should be mentioned how these stations have been chosen and if a certain threshold of distance from the coast or from the main routes of ships. This because it is known that the impact of the emissions from ships to air quality is strongly depending from the distance from the harbours/routes and I see some stations that are quite inland, especially in Northern Italy. A discussion on this should be provided even because I believe that the impact of shipping on such stations would be really small.

Another aspect that should be clarified and it is partially correlated to the previous point is if the emission dataset used include emissions of ships at berth. Several studies indicated that in EU harbours the emission at berth lead to the majority of the impact on local air quality in port cities, see for example Merico et al. (Atmospheric Environment 139 (2016) 1e10). Considering the use of low sulphur fuels at berth, this phase is particularly relevant for nitrogen oxides and could also lead to local exceedances of air quality standards. If neglected it could be present an underestimation of the impacts.

Line 336. A correlation with  $R=0.06$  is not weak, rather it is a total absence of correlation.

Lines 410-415. I believe that the results here are also comparable with those obtained with CAMx in the central/eastern part of Mediterranean area reported in Merico et al (Transportation Research Part D 50 (2017) 431–445).

Lines 420-421. I would add or near the harbours.

Figure 6. Please use the apex for  $m^3$  as in the other figures.

Line 446. The absence of negative values in the tagging method is a consequence of how the method is formulated rather than a relevant result. Could this lead to problems in evaluation titration of  $O_3$ ?

Section 3.1.5. Regarding  $O_3$ . There are several experimental evidences, some of them also in the papers that I already mentioned in my previous points, that emission of NO from ships could lead to a local reduction of  $O_3$  concentrations, especially in the spring/summer period. At larger distances instead there could be an increase. There are also some hypothesis that models could catch this behaviour more or less efficiently according to the spatial resolution of simulations. Could this be an issue in your results considering that outcomes ranging from negative to positive impacts for  $O_3$  were observed? Also Figure 16 shows that relevant differences are observed especially in the negative part.

Line 656. I would not say air pollution considering that only NO<sub>2</sub> and O<sub>3</sub> are considered in this work.

Lines 714-719. This part is a little vague. It could be useful to understand if there is any possibility to understand if one of the model performs better than the other. In addition, it should be mentioned how to use results from the different models with different resolution, averaging the results?