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Comment on egusphere-2022-415

Anonymous Referee #2

Referee comment on "Potential impact of shipping on air pollution in the Mediterranean region – a multimodel evaluation: comparison of photooxidants NO_2 and O_3" by Lea Fink et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-415-RC2, 2022

The manuscript of Fink et al., 2022, investigates the effect of shipping emissions on NO2 and O3 in the Mediterranean Sea. Results of five different models are compared which used the zero-out method. In addition results of the tagging method in LOTOS-EUROS for NO2 are presented.

The topic of the manuscript is of high relevance as shipping emissions are an important source of pollution in the Mediterranean Sea. In the current state, however, I can’t recommend a publication in ACP. To my opinion the following major points need to be adapted:

1) The biggest issue of the manuscript is the used terminology. By definition, contributions can not be calculated by the zero-out method (at least not for non-linear species), but by the LOTOS-EUROS Tagging method. I recommend to use the terminology (e.g. potential impacts and contributions) of the „Source apportionment to support air quality management practices‟ from FAIRMODE (https://fairmode.jrc.ec.europa.eu/document/fairmode/WG3/European%20guide%20SA_3.1_online.pdf). The clear terminology is important as the different methods (tagging, zero-out) focus on two different scientific questions. Zero-Out shows the change of e.g. ozone in case of an emission reduction. Tagging gives the contribution to ozone for the ‘reference state’. Due to the different aspects of the two methods it is important to have a clear terminology to avoid any misunderstandings. A lot of literature exists on this topic for further reading, e.g.:

Wang et al., 2009, Grewe et al., 2010, Emmons et al., 2012, Butler et al., 2017, Clappier et al., 2017, Mertens et al., 2020, Belis et al., 2021, Thürkow et al, 2021, Rieger & Grewe, 2022

2) The paper is very long. The authors tried to explain some of the (large) differences of the model results, but many differences remain unclear. As an example, the model results for Ox look very different (e.g. compare EMEP with e.g. CAMx). Also the results for deposition differ largely (as noted by the authors), but there are no further analyses. On p39l606 the authors note that this could be due to different dry deposition velocities, but the velocities itself are not analyzed. I understand that due to the multitude of effects and the differences between the models itself it is almost impossible to find a reason for the large spread between the models. However, in this case I suggest to reduce the amount of information (e.g. also the length) of the paper by presenting the most important findings only. This could be for example the impacts of the shipping on NO2 and O3 as simulated by the different models (and a short chapter to deposition). Also, for example, the time-series of the different models at the different stations (e.g. Figs 2 – 4) are interesting, but also very lengthy. The figures and their discussion could be moved to the supplement and 'only' the summarizing evaluation could be presented in detail.

In addition, I suggest to better highlight/focus on what we can learn from a policy point from this study? Where are open questions? What did you lean from the multi-model study which should be considered in follow up studies? Should more things be harmonized? Where do models need to be improved?

3) I like the idea that the different models were applied in their 'default configuration' and only resolution and anthropogenic emissions are prescribed. However, the description of the models differ strongly in their level of detail. Some examples:

- For CAMx no information about the biogenic emissions are given;

- Information about sea salt emissions are only given for CMAQ, EMEP and CAMx;

- Information about dust emissions are only given for CMAQ and EMEP.

Similarly, the description of dry- and wet-deposition differs (for example EMEP in Sect 2.1, for all other in Sect 2.4). Information about lightning NOx are missing completely.

I suggest to give the same amount of information for all models in the same level of detail. I would also suggest to expand Table 1 with details about the chemical mechanism, the used dry deposition scheme, biogenic emissions etc. Finally, I further suggest to add tables with total emissions (especially for biogenic sources) for each model to the supplement. It would also be nice to see all computational domains in the supplement.

In addition, I noticed that for all models the figures for NO2, O3 etc. (e.g. Fig. 7) show slightly different geographical regions. This contradicts with Fig. 1 and the information about a common domain. As example, Fig 7 (e) does only partly show the Po Valley while Fig. 7 (b), (c) and (d) show the Po Valley completely. For better comparability the same geographical region should be displayed for all models (and of course should be used for calculating mean values, frequency distribution etc.).

4) I suggest to replace the color scales. The rainbow color scale can be misleading. In addition it is problematic for people who are colorblind. You can check your plots for example with a 'CV Simulator' on you phone. Also some of the labels at the figures are very small. I suggest to use at leas the same font size as in the figure caption.

Minor comments:

p6l155: Is there something missing in this sentence (boundary conditions from Mozart44 output were activated?). But more importantly, if CAMx OSAT output is available why not discuss this in the manuscript? To my opinion the paper would benefit from including OSAT results.

P8l233: I am not familiar with LOTOS-EUROS, but does this mean that the model time step is 1 hour or should it read 'hourly model output' ?

p10l257: This does mean that the NMVOC split was not adjusted to the chemical mechanisms of the individual models, right? No lumping of species were performed?

P10l264f: The part about the VOC emissions is unclear to me. Please rephrase. Thanks!

P20l402: You mention the longer lifetime of NO_2 for CAMx and CHIMERE. I wondered if HNO_3 mixing ratios of the models differ. Please add figures in the supplement and discuss them shortly.

Fig 6: Please don't use the tagging results for calculating mean impacts. Tagging and zero out give something different (see main point 1).

p24l448: See also (1) – To my opinion the main reason zero-out gives different results (and results from different sensitivity simulations do not add up) is the non-linearity of the chemistry. Of course other factors also lead to differences.

P26l495: Please provide figures of the different boundary conditions in the Supplement.

P32l516ff: I don’t understand this sentence. How should a split of the emissions lead to high concentrations over sea and low concentrations over land? I guess the main reason is the low dry deposition over sea, right? (as well as the overall higher land emissions).

Figure 18: The label for the subplot should be contribution frequency distribution? Please check also for all other figures.

P46l679ff: Please see main point (1) above. There is also a lot of literature discussing zero out vs. tagging which could be cited here.

P47l691: Is there any answer on the question of and how the different dry deposition can explain the model differences?

Technical comments:

I found some typos and missing spaces etc. Please double check the manuscript. Some examples:

p3l88 differences, p4l103 % by

p13l334 – Should be Table 3?

p36l580ff (and throughout the whole manuscript): I suggest to replace ‘output’ with model results or similar

p46l661: The output was quantified? I guess it should read the differences of the model results was quantified or the impact of shipping simulated by the different models was quantified.

P46l673: the maps display – In my opinion the model results display (please check also the manuscript for similar wording as the term 'maps' have been used quite often)

Literature:

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