

Interactive comment on “Precursors and formation of secondary organic aerosols from wildfires in the Euro-Mediterranean region” by Marwa Majdi et al.

Anonymous Referee #1

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General comments

This study presents a new SOA formation mechanism developed to quantify the relative contribution of SOA precursors from wildfires in summer 2007 to organic aerosols in the Mediterranean region. The mechanism is an extension of an existing one by inclusion of some aromatic volatile organic compounds (VOC) emitted from wildfires. Since the wildfires have significant effects on the chemical composition of the atmosphere, a realistic representation of their emissions as well as their chemical fate in the models is important. Although I think such efforts might be valuable, this manuscript needs a major revision if accepted.

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One of the weaknesses is that the manuscript presents simulations using an extended mechanism to quantify the relative contribution of precursors from wildfires to OA formation, but it does not provide any attempt to show how realistic the results are. It makes more sense to perform such studies during periods where detailed measurements –especially OA- are available to support the results, at least to a certain extent.

A general model evaluation (for both gaseous and particulate pollutants) to provide some confidence on the model performance during the selected period of time is the basis for all kind of modeling studies. Without such confidence it is impossible to get reasonable conclusions out of the simulations. In introduction, authors mention “a general good performance for PM2.5” citing another manuscript which is still under review.

Specific comments

1) The title indicates that the study is for the “Euro-Mediterranean” region. Results, however, mainly focus on a sub-region over Italian peninsula, Greece and some Balkan countries, named awkwardly as *MedReg”. I assume the name comes from the definition of different regions in the Mediterranean used in Majdi et al. (2018) as MedReg1, MedReg2, etc., but it sounds strange when it stands alone in this manuscript. Authors might consider changing it, for example simply as “sub-region”.

2) Page 1, line 23: Last sentence makes no sense.

3) Page 2, line 10-11: please replace “intermediate organic compounds” with “intermediate volatility organic compounds”

4) Page 12, Section 3: This section is too short. For the model set up authors cite Majdi et al. (2018) which is still under review. Even if that manuscript is accepted for publication, the modeling methods and detailed information about the model inputs must be described in this manuscript as well (meteorological parameters, anthropogenic and biogenic emissions (inventories, model, version), boundary conditions, deposition,

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etc). The model domain covers an area where desert dust is very important. Some studies show significant dust contribution in the Mediterranean even below 2.5 micrometer (Fernandes et al., 2015; Denjean et al., 2016). Was dust included in the model simulations, in boundary conditions, how was dust distributed in model size fractions?

5) Page 12, line 27: Authors need to explain the reason of using CB05 mechanism by including additional compounds and reactions leading to SOA formation instead of using more advanced CB6 mechanisms (Yarwood et al., 2010) which have already some of these compounds.

6) Page 15, Section 5.1: Uncertainties in VOC emissions are probably very high. As I understand, authors considered only two types of vegetation (crop residue and chaparral) of which the emission factors were available and also assumed that temperate forest and savanna have the same EF as chaparral for cresol, catechol, guaiacol, syringol, naphthalene and methyl naphthalene. Additional discussion about the variability of emissions from different vegetation with references is needed to justify this assumption.

7) It is known that terpenoid emissions –especially monoterpenes- increase during forest fires (Ciccioli et al., 2014). Since monoterpenes are the essential precursors for SOA formation, their contribution to OA during wildfires might be larger when taken into account in addition to their natural emissions. Although not mentioned in the manuscript, I assume MEGAN model was used to estimate the BVOC emissions. At least some discussion about the contribution of the increased BVOC emissions to SOA formation during wildfires might be useful.

8) Page 13, Fig. 5: I assume that the MedReg is not another nested domain (same resolution as the red dotted domain). It has to be explained better in the text the choice of the green box named as MedReg -which is odd

9) Page 25, Conclusions are very short, based only on model calculations without any supporting material or discussions. This section needs a revision.

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10) Please change Giancarlo et al., 2017 to Ciarelli et al., 2017 in page 3, line 16, 29, 33, page 12, line 13, page 13, line 15, page 16, line 30, page 33, line 2

11) Page 37, line 25: Please correct “Lowik, J.” as “Slowik J.”

12) Page 38, line 27: Please correct the following reference: “Giancarlo, C. and El Hadad, I., Bruns, E., Aksoyoglu, S., Mohler, O., Baltensperger, U., and Prevot, A.: Constraining a hybrid volatility basis set model for aging wood burning emissions using smog chamber experiments, *Geosci. Model Dev.*, 10, 2303–2320, <https://doi.org/10.5194/gmd-10-2303-2017>, 2017” as “Ciarelli, G., El Haddad, I., Bruns, E., Aksoyoglu, S., Möhler, O., Baltensperger, U., and Prévôt, A. S. H.: Constraining a hybrid volatility basis-set model for aging of wood-burning emissions using smog chamber experiments: a box-model study based on the VBS scheme of the CAMx model (v5.40), *Geosci. Model Dev.*, 10, 2303-2320, 10.5194/gmd-10-2303-2017, 2017”

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