

Interactive comment on “Impact of wildfires on particulate matter in the Euro-Mediterranean in 2007: sensitivity to the parameterization of emissions in air quality models” by Marwa Majdi et al.

Anonymous Referee #1

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General comments

The manuscript addresses an interesting subject relevant with the impact of wildfire emissions on air quality with a focus on the atmospheric PM levels. The underestimation of PM concentrations by air quality models is a well-known issue becoming even more evident in extreme events like those of biomass burning and wildfires, considering that the estimation of amounts of pollutants emitted during these events has many uncertainties while the physical phenomena to describe their evolution (e.g. pyroconvection to determine injection height) are poorly described in the air quality models.

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The submitted paper can only be considered for publication after major revisions. The main general comment for the manuscript is that several of the results presented can be considered as tentative since they are provided for and refer to a single site (GR0039A). In addition, in the manuscript more results are presented for the first summer period wildfire event (20-31 July 2007) although the maximum of emissions is during the second summer period wildfire event (24-30 August 2007) as shown in Figure 2.

Specific comments

1. Page 6, lines 20-22: Instead of primary aerosol oxidation, are you probably referring to the oxidation of I/S/L-VOCs by OH in the gas phase?
2. Page 8, lines 8-9: Please use a reference and explain why POA emissions taken into account are modelled as LVOCs.
3. Pages 11-12, Section 3.3: The overall performance of CHIMERE model is shown to be slightly better than Polyphemus model considering also the statistics in Tables 3 and 4. Under this view, you have to present results of CHIMERE model in Figures 3, 4 and 11 additional to those of Polyphemus model for comparison.
4. Pages 12, Figure 3: Presenting similar maps using the data of MODIS is necessary to support better the comments in the manuscript about Figure 3.
5. Page 13, Section 3.4.1: Figure 4 reveals several parts of the Mediterranean area being affected by the wildfire plumes for example Central and South Italy, Sicily, Bulgaria, almost the whole country of Greece etc. In the manuscript you are referring to only 2 stations being affected by the wildfire emissions during the first event and 3 stations during the second one. Aren't there additional AIRBASE or AERONET stations that could be used to support your analysis? (for example GR0035A is also a background suburban station where PM_{2.5} concentrations are being measured; is there a reason for not using measured data from this station?). Is the performance of the model good also for the remaining areas that are not affected by the fires?

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6. Page 14-15, Section 3.4.2: The results presented in this section refer only to a single station and only to the fire event of the first period. Under this view, they are of very limited reference and cannot be used to characterise the aerosol composition in the Mediterranean areas affected by the fire plumes. Similar results with those presented in Figure 5 have to be presented for different stations and wider areas affected by the fire emissions and separately for both fire peak events in order possible differences to be identified.

7. Page 16, lines 4-8: Comments for Figure 8 is better to be written at the end of the section 3.4.3 after the comments for Figures 6 and 7.

8. Page 16, Figure 8: Please present and comment on similar maps based on the models and MODIS results for the second fire event in August.

9. Page 23, Section 4.1: In lines 10-12, it is commented that according to the Polyphemus model results the number of days, when the WHO air quality limit for the daily PM_{2.5} values is exceeded in GR0039A station, is 7 while the corresponding number based on measurements is 14. This corresponds to a -50% bias by the model. Is it acceptable? No data are provided for additional stations in the study area rendering the results presenting in this section very tentative. Further evidence is necessary to prove the acceptable performance of Poly-ref runs (and CHIMERE-ref runs, please see a previous comment) in the estimation of the PM_{2.5} air quality exceedences supporting better the necessity for the maps in the Figure 11 to be presented.

10. Page 23, Figure 11: Figure 11 is not clearly explained in the manuscript. What is presented in Figure 11 (left panel)? Is it the additional days with exceedences due to fires (i.e. (days with exceedences Poly-ref) minus (days with exceedences Poly-Nofires))?

11. Page 23, lines 16-18: It is commented by the authors that PM_{2.5} concentrations are composed mainly of organic matter. However adding the percentages in the parentheses (i.e. 7.3% for POA and 19.1% for SOA), a percentage total contribution of 26.1%

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is estimated. Is this contribution to PM_{2.5} the main? Please clarify.

12. Page 24, lines 9-10: The percentages have been estimated for one site and for one fire event and have a very limited reference (spatial and temporal). Please see a previous comment of mine so as to update the conclusions.

13. Page 24, lines 10-12: Which Figure or Table supports these high correlation values and where in the manuscript are these results commented? Is it Figure 7?

14. Page 24, lines 14-16: This conclusion is not straightforward and requires further clarifications not only in conclusions but also in the manuscript. In addition, it is not clear how overestimations in simulated results may indicate missing emissions.

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