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Atmospheric Chemistry and Physics Discussions

# *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 #2**

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This paper tries to evaluate model estimates of fire-related PM concetrations through evaluation with data from surface networks and satellite remote sensing. However, I think this work is flawed due to the decision to use only PM measurements. For some reason they do not make use of the rather extensive measurements which are available (Airbase,EMEP, or even AMS data) of sulfate, nitrate, ammonium, or other inorganic components.

This is a serious problem I think. For example, Fountoukis et al (2011) suggested significant contributions of sulfate, nitrate and ammonium to PM1 mass, and backed



up their analysis with comparison to measurements. Van Damme et al. (2014) showed enormous levels of NH3 from biomass burning, which presumably caused high PM2.5 also.

Further, on p10 they claim that by focusing on PM2.5 they avoid contributions from dust, but this simply isn't the case. The low-diameter tail of coarse-mode dust (and sea-salt) contributes of course to PM2.5, and measurements in southern Europe have shown that this can be a significant fraction even over long-periods (Putaud et al., 2010). (This same publication also demonstrates clearly the importance of non-organic compounds in contributing to PM2.5.)

I see no reason to believe that changes in model performance attributed here to various assumptions on OA emissions could not be masked or mistaken for problems with inorganic pollutants. As well as attending to the points raised below, I would encourage the authors to do a thorough analysis of the model performance for all the major compounds, and across many sites. I am afraid I cannot recommend this paper for publication without major revision.

Other points

0. The title is actually quite misleading. The paper only addresses some aspects of OM emissions, not emissions in general.

1. The word dispersion, used in eg line 17 of the abstract, is usually associated with turbulent mixing in the air pollution context. Please use another term to make clear that here you mean statistical spread.

2. p2, L20. Give reference for statements about anthropogenic origin.

3. p3, L8. Give reference for the volatility limits and definitions. Also, do you really mean from 1e4, or from 0.32e4 (see eg Table 4 of Murphy et al., 2014) ?

4. Units (e.g. p3. L8) should not be in italics.

5. p3, L23. Change 'current CTMs' to 'two CTMs'. Your study says little about the many other CTMs used in Europe.

6. Table 1 is very vague and uses many acronyms that haven't been explained yet. What is ECMWF, which version? What does 'from nesting simulations' mean? Which version of MEGAN? Which version of EMEP emissions? Give more specific details and references here.

7. P6, L3. The nesting factor is here given as 2:1, but usually one uses an odd ratio to avoid interpolation errors (e.g. https://earthscience.stackexchange.com/questions/8067/why-is-wrf-most-often-configured-at-31-nesting-ratio). Have the authors checked if their results are robust?

8. p6, L9. Biogenic emissions of what? I assume isoprene and terpenes, but which?

9. p6, L17-24. The explanation of the VBS scheme used is unclear. Firstly, where do the numbers -0.04, 1.93 and 3.5 come from? They are not suggested by May et al. 2013, and not obvious to the reader. Secondly, L21 suggests that OH+POA leads to SOA compounds in the same VBS bin, but L22-23 suggests a reduction in volatility equivalent by a factor of 100. Which is it?

10. p6, L17. The use of 'log' is ambiguous. In most VBS papers it means log10, but in other papers (especially mathematical) and common programming languages (eg python) it means the natural logarithm, log_e (or ln). I suggest using log10 to be clear.

11. p6, L33-34. Akagi provides emission factors for several ecosystems, but non are a close match to Mediterranean landscapes. And some of these emitted isoprene and terpenes, which also form SOA. Which emissions were in fact used?

12. Also, L33 suggests that only carbon emissions were used, but many studies suggest that fires are a major and sometimes huge source of gases including NH3 (eg van der Werf et al., 2017, Van Damme et al., 2014), and hence of inorganic PM. Were such



emissions examined for their impact on PM? Were they checked?

13. p7, on. The notation PPMfine is very confusing. This is usually used to represent emissions of any primary particulate matter including OM, BC and inorganics. Am I right in saying that your PPMfine is PM2.5 - OC - BC - SIA? In which case I would expect most of be essentially OM-OC - is that what you mean? To further the confusion with this PPMfine notation, you introduce PPMcoarse which is more traditional, just PM10-PM2.5. So, PPMfine excludes OC, BC and SIA, and consists largely of O and H, whereas PPMcoarse consists of OM+BC+SIA. As I said, confusing.

14. p7, Figure 1. It is difficult to see the yellow fire labels on top of the yellow background. Change the colors.

15. p9. Table 2.

- The Poly-ref emissions are said to be introduced between 1km and the PBL. I assume you mean between ground level and min(PBL,1km)? If not, I have a serious problem with your base-case!

- What is PB?

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