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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Emission of biogenic volatile organic compounds from warm and oligotrophic seawater at the Eastern Mediterranean" *by* Chen Dayan et al.

Anonymous Referee #2

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The authors report BVOC measurements at a site close to the Mediterranean sea. They suggest that a large amount of the BVOCs originate from the sea. This is a result that merrits publication. However, the evidence presented is not very consistent and/or robust in several aspects (see specific comments). The authors use (maybe too) many tools (PMF, Hysplitt, MEGAN, WRF) to substantiate their case, however, for most of these methods insufficient information is given to judge their appropriate application. The paper needs a substantial revision before publication. I recommend to focus on a careful and clear presentation of the strongest evidence. I wonder why the authors did not use the eddy covariance technique to constrain local emissions.

Specific comments:

line 78: "... are ESTIMATED to be substantially smaller ..." I guess nobody really knows the marine source strength.

Fig 1: remove the red triangle that separates panel a and b. This is confusing.

Table S1: what is meant by poor quality and failed calibration?

There is no Figure 2.

Caption of Fig S4 can be improved. What is the color code?

Why are benzene, toluene, and acetonitrile not reported? These compounds should be valuable tracers to constrain traffic emissions (2 highways between the site and the sea!) and biomass burning.

The evidence shown in table 1 is insufficient to classify the period September 14-17 as irregular conditions. PAR is less than 20% lower than in the other period in September. All other parameters are similar.

Lines 277-280: I don't see extreme meteorological conditions and I don't see extreme concentrations in Fig 3. The modelling exercise in the supplement is not convincing because there is no reference period. It could be convincing, for example, if much higher boundary layers would be calculated for the second period in September.

Lines 281-283: Please explain why the shape suggests a biogenic source. Also, it would be useful to see the data for DMS and other BVOCs to show their difference!

Line 303: would be good to see hexenal and hexanal in Fig 4, even if these compounds are not calibrated!

309-312: the fact that MEGAN does not predict local isoprene emissions is no convincing argument. Surely not all species (including invasive species) are included in the MEGAN model.

315: I cannot follow all details of the kinetic analysis in the supplement, but I doubt that this can rule out the possibility of local emissions. I do not understand why the authors do not process their data with the eddy covariance technique. This would give a clear answer on whether there are local emissions or not.

324: I do not agree that this has been demonstrated...

327:Figure S12: it would be informative to see how the scatter plot looks like for other VOCs.

328: Figure S10 shows data for one day. This is insufficient to make general claims.

334: Fig 5 caption: There is no "regression" you just show a scatter plot temp vs iso+MBO

369-370: Given the fact that you measured at high E/N (140 Td) I am not so sure that such high fractions of MBO are expected at 87 Th. Maybe you can prove this by showing calibration measurements.

445-447: I think that it would be interesting to estimate daytime isoprene production from these lifetime values.

475-478: what was the sea surface temperature in 1995 as compared to 2015?

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