

Interactive comment on “An investigation on the origin of regional spring time ozone episodes in the Western Mediterranean and Central Europe” by Pavlos Kalabokas et al.

Anonymous Referee #2

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The paper highlights one of the important factors, i.e. synoptic meteorological system, controlling high ozone concentration episodes over Western Mediterranean (W-MED) and Central Europe (C-EU), during spring. The text is generally well written and clear. The time series plots of the observations clearly confirm there were two episodes in which ozone builds up during late April and early May in 2008 over most of the Mediterranean countries. However, when it comes to the proof of the argument, there are a number of statements and images repeating the same messages. The analysis of various parameters that are generated from different types of observations and models is a great idea, but unfortunately they are not always univocal. Therefore I think this paper needs a major revision. Particularly, I would encourage the authors to avoid

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misinterpreting the results of multiple sources.

The major concern I have with this paper, particularly in the result and discussion section, is that there is no clear focus on the two regions which are mentioned in the title (i.e. W-MED and C-EU). The NCEP/NCAR reanalysis maps (geopotential height, etc.) include enough evidences to confirm the existence of subsidence over those (two) regions during late April and early May, respectively. Also, there is a positive signal in all selected meteorological parameters on episode days; however the signal is not as strong as expected for some of the parameters. I cannot understand why the authors avoid focusing on them. Furthermore, the mechanisms leading to ozone enhancement as a consequence of high pressure systems should be explained in more detail. One mechanism could be the accumulation of surface ozone which is produced through chemical reactions due to the stagnant air flow. Another one could be linked to ozone flux from the upper troposphere to the surface, which the authors have already tried to prove but without sufficient arguments.

Detailed comments:

1) Abstract: “ozone measurement from countries surrounding Western Mediterranean...”_The promising title indicates that the study is focused on two different regions, W-MED and C-EU. Then in the abstract the focus changes to only one of them i.e. W-MED. Even the time series are plotted just for W-MED, why? According to the EEA-AirBase maps, Fig.18, the second episode is located over C-EU, isn't it? Wouldn't it be better to select a few stations from C-EU and plot their time series for them (as it has been done for W-MED)?

2) Abstract: “the results show that high ozone ...”_I think here you mean the results of the observation, don't you?

3) Abstract: “over these areas, strong...”_I think it is too much detail for the abstract section.

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- 4) Introduction: in general the strongest focus of this part is over E-MED region during summer.
- 5) Introduction: “transport times are typically shorter...”_wouldn’t it be better to rephrase this sentences to something like “they can be transported over longer distances than that in the boundary layer”?
- 6) Introduction: are there any references regarding the frequent existence of anticyclone condition over MED during spring?
- 7) Data and methodology: I think it would be nice to add the path (precise address) of the data center or website which the data are taken from.
- 8) Data and methodology: what is the horizontal and vertical resolution of the CHIMERE model?
- 9) Data and methodology: the description of methodology is insufficient.
- 10) Results and discussion, section 3.1: as it has been mentioned in this part, Fig. 1 (lower panel) shows a very good agreement between the results of different stations. So I cannot see the reason of having two different time series plots in Fig. 1?
- 11) Results and discussion, section 3.1: the title of this section is “. . . over Western Mediterranean”, but the description mixes both episodes together. I would strongly recommend the authors to create a separate time series for each episode in each region as it has been done by airbase maps (lowest panel of Fig. 9 and Fig. 18). These maps and time series are enough proof for the confirmation of the existence of two episodes over these regions.
- 12) Results and discussion, section 3.2: since there are too many plots, I would suggest the authors to keep the plots related to the episode days in the main paper and move the others (i.e. the plots which they are related to the a few days before episodes) to the supplementary.

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13) Results and discussion, section 3.2, a): is there any necessity to explain the low pressure system over another region i.e. E. Europe? How this system leads to high ozone concentration over W-MED?

14) Results and discussion, section 3.2, b): why do the authors describe negative specific humidity over Atlantic or etc.? The main focus of this part must be over W-MED and there is strong signal of negative specific humidity over this region (in the lowest panel of Fig. 2), why don't the authors concentrate on that?

15) Results and discussion, section 3.2, c): do we really need different maps of omega and its anomaly? In fact, both of them have the same messages.

16) Results and discussion, section 3.2, d): Yes, indeed there is a strong westerly wind toward W-MED a few days before the episode. {It may transfer ozone and its precursors from other places such as eastern US, etc. towards this region (via long-range transport), but there is not enough evidence for that through this maps.} However, on the 26th and 27th of April (in the lowest panel of the left column in Fig. 4), there is a weak wind flow over W-MED due to the existence of a high pressure system.

17) Results and discussion, section 3.2, e): in those episode days, there is a positive temperature anomaly over W-MED due to high pressure system. It leads to even more ozone production through photochemical reactions, doesn't it?

18) Results and discussion, section 3.2: "overall, over the same area of subsidence . . ."_how is it possible to have strong subsidence and strong wind together at the same time? Strong wind may even lead to a reduction of ozone by transferring them to the other regions.

19) Results and discussion, section 3.2: "In figure 5, the composite ozone IASI . . ."_ as the authors have already mentioned in this section, there is a signal of high ozone at free troposphere over C-MED and Atlantic. There is no explanation of how these are connected to high surface ozone over W-MED.

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20) Results and discussion, section 3.2, page 9, first paragraph: I think, adding Fig. 7 is just overemphasizing the same messages which have been already explained in Fig. 1, 2, and 3.

21) Results and discussion, section 3.2, page 9, second paragraph: the CHIMERE simulation shows more or less the same results as IASI satellite data. There is high ozone in the free troposphere over C-MED. I cannot understand how this information is connected to high surface ozone over W-MED? I do not recommend the authors to apply a model simulation in this study without any evaluation of that.

22) More or less the same recommendations as above are valid for section 3.3.

23) Conclusions: “in this paper, the investigation of the regional ...”_what does ‘surrounding countries’ mean? Does it mean C-EU?

24) Conclusions: paragraph 4: how do negative temperature anomaly and strong wind contribute to the high ozone level?

25) I would strongly recommend the authors to use a larger size for labels, title, etc. for all figures to make them readable.

Figure 1: The lower time series plot clearly shows the episodes. There is no need to keep both plots. Furthermore, the unit of ozone in the legend should be “ppb” instead of “ug/m3”, shouldn't it?

Figure 2: The right column is specific humidity anomaly, but in the legend it is written specific humidity. It would be recommendable to add the unit of this parameter.

Figure 3: Both omega and omega anomaly have the same messages; I would recommend the authors to keep only one of them. Units are missing.

Figure 4: Again, adding units would be recommendable.

Figure 9: It is hard to see the white contour over CHIMERE maps. In the legend the color of contours is labeled black instead of white. I do not know what the aim is of

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putting surface ozone (from EEA-AirBase) maps separately below CHIMERE simulation maps.

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