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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Summertime tropospheric ozone assessment over the Mediterranean region using the thermal infrared IASI/MetOp sounder and the WRF-Chem model" *by* S. Safieddine et al.

Anonymous Referee #2

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This paper reports on the summer ozone maximum in the Mediterranean region using the thermal infrared space-borne instrument IASI and the model WRF-Chem results with additionally ground based EMEP stations. Authors investigate the 0-10km range within the 2008-2013 summer periods with a focus in 2010. They conclude on an ozone maximum, which is greater in the eastern basin (∼30°E) than westward (∼15°E). From WRF-Chem, they point out, the anthropogenic emissions strongly contribute to the maximum within the 0-1km altitude, whereas above 4km the transport from outside the domain is predominant. They investigate hypothesis on stratosphere to troposphere

exchanges to explain the ozone enhancement in the east compared to the central basin around 15°E.

This paper reports on the summer ozone maximum in the Mediterranean region using the thermal infrared space-borne instrument IASI and the model WRF-Chem results with additionally ground based EMEP stations. Authors investigate the 0-10km range within the 2008-2013 summer periods with a focus in 2010. They conclude on an ozone maximum, which is greater in the eastern basin (∼30°E) than westward (∼15°E). From WRF-Chem, they point out, the anthropogenic emissions strongly contribute to the maximum within the 0-1km altitude, whereas above 4km the transport from outside the domain is predominant. They investigate hypothesis on stratosphere to troposphere exchanges to explain the ozone enhancement in the east compared to the central basin around 15°E.

The paper is well in the scope of ACP on a very interesting topic. It should be accepted if the paper is revised and improved. Material and methodology are well appropriated. Nevertheless, text should be more accurate and justifications are sometime insufficient. Results supported by fig 10, 11 and discussions on STE in particular are not enough convincing. Revised the conclusions and do not forget to provide recommendations.

General comments:

In the paper, could you explain the reason why you investigated 2010 as an example? Is it anomalous? Is it better documented... In figure 1 you qualify "it is representative"... the June-July 2010 difference at 30°E is atypical and you mention Russian forest fires in 2010. Justify more clearly and rigorously, please, that would help.

Could you also modify the text in order to provide the exact years used for IASI. It is specified too late in the text, only in your figure 4, 5 and conclusions.

Description of your model should be given in the section 2.2 and not lately. The regional

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context of the study is unclear to me and should be improved. For example, the Figure 2 should provide : the exact model domain with the Mediterranean region you are studying (model and IASI) and you refer to in the text. Take care your text agree with the description and keep it constant. the EMEP stations you include in the study (what you did). highlight the two regions you point out is the abstract : the eastern part of the basin and the middle of the basin. Note “middle of the basin” is not specified or mentioned elsewhere in the text. The two transects provided in figure 4 are too difficult to see.

I listed below some points to clarify or revised and suggestions.

P12379, L2 : add “Thermal” before “infrared”.

P12379, L6 : Specify exact period, “Six years (2008-2013)”. . .

P12380, L2 : Cairo, Istanbul and Athens you cited are located in the eastern part of the Mediterranean basin. Thus “surrounded” is not appropriated.

P12380, L7 : Would it be better to replace “region” by “circulation”???

P12380, L10 : The heat wave induces conditions in favour of severe fires. . . and the fires causes high O3 precursor emissions. The link between the Rossby wave and the climate extreme events with the Russian 2010 heat wave example remains in the context of your paper unclear to me. . . Please clarify.

P12380, L12 : Could be better to replace “in Europe” by “for the European Union”.

P12381, L12 : Expression “Mediterranean atmosphere” should be more accurate, from X to X altitude or hPa, because your title is on tropospheric O3 and definition of troposphere is not given. . ..

P12381, L24-26, the text “most of Europe” is inaccurate. . . Please modify the whole sentence. I expect the model domain is over Europe and the study focus on . . . It would be also interesting to specify the number of model levels relevant to the [0-10km] layer

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you studied.

P12382, L22 : cite “Stratospheric impact on tropospheric ozone variability and trends: 1990–2009” by Hess and Zbinden, acp 2013. From this reference, you could see that the transport from lateral boundary conditions is an important term and that should be evaluated individually. As far as I understood your O3inflow include this term, Linox and STE. Please revised the text and indicate in your conclusions that to refine with more accuracy your hypothesis on STE, this evaluation would be an interesting point to investigate and solve.

P12382, L25, “inflow” is here too imprecise, check with reference.

P12383, L12 : “Given that their contribution to the total budget in comparison with INFLOW and ANTHRO (please keep constant your labelling and modify here!) tracers is very small, they are not analysed in this study.”. . . Could you justify or at least evaluate the range of this “very small” contribution before you conclude on the O3 from fires discard from your study. Is seems to contradict to what you said P12380, line 10 on Russian fires in 2010. . . Note that they take place in late July and your figure 5 shows a great June-July enhancement on that specific year for the 30°E transect.

P12383, L16 : Please specify the EMEP instrumentation used.

P12383, L18 : In the legend the red line is not legible, adapt the colour to the red line used and your black symbol is also difficult to see. Please mention you EMEP stations are within 78m-1332m height and show up on each panel after the station name. That makes a great difference if you compare O3 at the surface with the O3 at ∼1000m height. It would it be interesting to show the six stations separately to improve the comparison (a table with the six stations would be convenient).

P12384, L16 : Why do you provide the averaging kernel for the specific June 2010, your are studying the 2008-2013 period with a focus on summer 2010. Explain why.

P12384, L26 add partial before “tropospheric O3 column”.

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P12384, L21 “on several pollutants”: which one?

P12385, L1-2: A bit too trivial here, revise and condense using the informations on season and month from lines 5-6.

P12385, L9: please refer also to the “A Lagrangian “1-year climatology of (deep) cross-tropopause exchange in the extratropical Northern Hemisphere“ study from H. Wernli and M. Bourqui, JGR 2002 who wrote : “Generally, the cross-tropopause mass fluxes are largest in winter and smallest in summer. The most pronounced seasonal variability occurs in the southern part of the midlatitudes (30°–45°N) where the STE winter values are 2–3 times larger than the summer ones. . . Clarify.

P12385, L15-17 : Is that your definition of the eastern and middle Mediterranean region? That should have been provided before. . .

P12385, L18-22: Should be specified that this result is a 0-8km partial O3 column and may be valuable to clarify in the introduction (P12381, L12-18) the partial layers you will investigate?

Figure 6 (a) : I could not find the CY2 station, should it be GR02?. . . Please check and modify.

P12386, L11 : The model underestimates . . . Yes, from your fig 6b. But from 6a, I have the feeling the E06 (at 78m) and ES10 (at 23m) is in better agreement with an overestimate in July for these two stations, which stations are the closest to the surface. I noticed the Stations ES07 and MK 07 are above 1km. Furthermore, your Fig 8 highlight the difference between the modelled WRF-Chem O3 concentrations at the surface and 1km. Therefore may be interesting to provide the six individual results in a table instead of Fig 6b as said previously.

P12386, L14 : Suppress “which is around 0.5° by 0.5°” as already given in the section 2.2 (and it is but in km) and just mention the ground resolution difference contribute to the discrepancy with the other possible reasons. . .

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P12386, L18-20 : It is unclear what your correlation refer to (range and mean value). Is that for individual rural EMEP sites vs WRF-Chem??? Clarify

P12387, L9-11 : As you provide on fig 7 the IASI and WRF-Chem results in DU for the 0-4 O3 partial column, why this summer mean bias is provided in ppbv? Is the difference less over land than over sea??? More comments are expected on that difference (line 7).

P12387, L17-20 : Please clarify "We analyzed the IASI columnar total error relative to measurement for the [4–10]km integrated partial column", I do not understand what you meant here... Revised and clarify the whole lines 17-20.

P12387, L25 : Please note that your summer maximum occurs at 1km and careful take it into account in your EMEP comparison at ∼1km with WRF-Chem surface.

P12388, L3 : "the entire model study ... 10 hPa" : this should be removed from this section and included in section 2.2. What "the entire model study" means. Keep the labelling steady or explain the difference.

P12388, L4 : To what I understood, these residuals plots should reveal the O3 from biogenic sources and O3 from fires... This seems in contradiction with what is said on P12383 L 12-14.

P12388, L5 : Provide evaluation in the text instead of "most", too imprecise.

P 12389, L 13-14 : the extended domain ... includes the Russian fires ??? Still the same, your domain (here extended) is not rigorously defined. You must define this clearly and rigorously in section 2.

Figure 10 : Replace "along" which is confusing by "at" and add after "15° E" "(left)" and "30° E" "(right)". Remove from the figure caption : "The Eastern part of the basin ... events" Could you add in the text or figure the altitude of your seasonally-averaged cold point, quartiles tropopause or the dynamical tropopause at 2pvu with statistics on the quartiles to be more meaningful... I have the feeling your 100ppbv is more above 8km

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than between 6-8km (also on fig 9c).

Undoubtedly you have more ozone at 30°E than at 15°E.

Nevertheless, I am not convinced by your concluding remark because below 8km I can't see any typical STE features as described in the figure 5a and 5 from Zanis et al. 2014 showing O3 values much higher that 100ppbv in July-August within 1998-2009. Moreover your 2010 JJA-average include a June low O3 anomaly at 30°E. The highest PV values you cite 0.8 to 1.4 pvu in the east-basin and the lowest are 0.6-0.9 in the mid-basin (15°E) appears as typical tropospheric PV values. STE events should provide higher values, is that the effect of the 3-month averaging?... To me the June 2010 doesn't seem to be so typical and probably lessen you result in 10 b and 10 c... Your O3inflow is not an exclusive tracer on O3 from STE and you mentioned from Pfister al al (2013) the transported plumes over large distance might not be well resolved from the model. To improve may be valuable to provide if possible where, when and how frequent the maximum PV-values are occurring in your model after the tropopause position has been clarified. STE events are not shown so clearly on fig 10 at least it could be transients or shallow events. I recommend concluding more carefully on the impact of STE and using "suggest" which seems to me more appropriated.

Figure 11 and Page 12289 L 21 : Provide the longitudes in Fig 11a-b. After the figure 10, this figure doesn't help much to conclude on STE. The layer you investigate is ∼8-10km whilst the fig 10 is within 0-10km...

References to add :

Liu et al., JGR 2011: "Influence of interannual variations in transport on summertime abundances of ozone over the Middle East".

A large number of useful references are provided by Zanis et al. 2014...

Take into account and refer to a study submitted recently to acpd by Doche et al, 2014 on "Summertime tropospheric ozone variability over the Mediterranean basin observed

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with IASI”

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