Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2017-79-RC1, 2017

Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Atmospheric pollution concentrations over the Eastern Mediterranean during summer – A review" *by* Uri Dayan et al.

Anonymous Referee #3

Received and published: 28 April 2017

This manuscript intends to present a review of atmospheric pollution over the Eastern Mediterranean during summer. There is a lot of information provided from others work but often this information is fragmented, not well connected and generally the manuscript lacks of coherence. Furthermore there is strong self-citation and the vast majority of the discussed articles refer to a specific region of eastern Mediterranean thus leading to an unbalanced discussion especially in some sections. There is no distinction in the discussion of the short-lived species and the long-lived species. Generally, I think that the manuscript needs to be restructured in order to provide all this interesting information in a more coherent way. Please find below a number of major comments that have to be taken into consideration for restructuring the manuscript.

Comments 1) Section 2.1, page 4, line 15: The authors state that dry north Etesian winds are generated by the Persian Trough. It is actually generated by the east–west

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pressure gradient manifested by large scale circulation features, low pressures over eastern Mediterranean/Middle East as an extension of the PT and the high pressure over central and southeastern Europe .

2) Section 2.1, page 4, lines 16-21: The discussion for the eastern Mediterranean subsidence during summertime needs elaboration in connection to the discussion in page 5 (lines 16-19) based on the paper of Rodwell and Hoskins. The current consensus view recognises the importance of the interaction with the mid-latitude westerlies of an equatorially trapped Rossby wave to its west induced by the South Asian monsoon heating as well as an enhancement of the descent due to diabatic radiative cooling under clear sky conditions (Rodwell and Hoskins 1996, 2001; Tyrlis et al. 2012).

3) Section 2.1, page 4, line 25: The discussion for the eastward progression of the subtropical high needs clarification. Which subtropical high do the authors mean? During summer the Azores High moves westward toward Bermuda (when it is known as the Bermuda high). Furthermore a number of studies point out the differences between the acticylonic center over central and southeastern Europe causing the Etesians and the Azores permanent Anticyclone (Prezerakos, 1984; Tyrlis and Lelieveld, 2013; Anagnostopoulou et al., 2014). The acticylonic center over central and southeastern Europe causing the Etesians is related primarily with anticyclonic vorticity advection from Northwestern Africa and secondly with diabatic cooling under clear skies.

4) Section 2.2, page 6, line 11: The authors state " Since this turbulent layer is mainly governed by synoptic-scale circulation patterns ..." . What exactly do the authors mean? Please clarify. Is this a general comment or a comment associated with the specific cited study of Dayan et al., 1988?

5) Section 2.2: There is extensive description of the link between synoptic patterns and the structure of the mixing layer in Israel within this session. As a reader I am rather confused and I do not really see the scope of such extended description of this link for a specific region in the frame of an overview paper for the regional baseline atmospheric

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pollution concentrations over Eastern Mediterranean.

6) Sections 2.2: The majority of the discussed articles refer to studies at the coast of Israel which leads to an unbalanced discussion for Eastern Mediterranean boundary layer. There are a number of boundary layer studies from other coastal regions in Eastern Mediterranean and their links to atmospheric pollution (e.g. Melas and Enger, 1993; Kallos et al., 1993; Svensson, 1996; Kostopoulos and Helmis, 2014; Tombrou et al., 2015).

7) The connection of the Sections 2.2, 2.3 and 2.4 with the discussion of the manuscript in later sections is fragile. The Sections 2.2, 2.3 and 2.4 could be merged into one broader in scope section for the role of the atmospheric boundary layer for atmospheric pollution over Eastern Mediterranean and make a stronger link with the core part of the review paper which is the atmospheric pollution concentration distribution during summer.

8) Section 2.5 in its current form provides basically information from a single study for a receptor site at Israel. It does not provide an overview over Eastern Mediterranean. I am not sure what is the added value of this Section in its current form.

9) Page 13, lines 17-19: The authors state that "in general , mineral dust does not affect the EM during summer". This is a rather strong statement. Consider that there a number of observational and modeling studies indicating a contribution of 25-30 % of dust aerosols on the total aerosol optical depth during summer over land and see in Eastern Mediterranean (Gerasopoulos et al., 2011; Georgoulias et al., 2016; Tsikerdekis et al., 2017; Marinou et al., 2017).

10) Page 15, lines 7-14: This is not exactly the finding of the study by Tyrlis and Lelieveld (2013). The various components observed over the Eastern Mediterranean that include the Etesians, subsidence, tropopause folds, stratospheric intrusions, and the summer ozone pool are dynamically interwoven manifestations of the influences induced by the South Asian monsoon and the midlatitudes. Tropopause folds and the

subsidence are the key components yielding high ozone concentrations in the middle and lower free troposphere over the region (see e.g. the recent publications on the topic by Tyrlis et al., 2014 and Akritidis et al., 2016).

11) Page 18, lines 19-20: " ... controlled by the strength of Azores High and the PT" . See my comment 3. There are a number of studies showing the differences between the acticylonic center over central and southeastern Europe causing the Etesians and the Azores High.

12) Section 3.2: The majority of the discussed articles refer to studies at the coast of Israel which leads to an unbalanced discussion for Eastern Mediterranean sulfate aerosols. Consider that there are some other earlier and recent studies for sulfate aerosols, SO_2 and their transport over Eastern Mediterranean during summer (e.g. Mihalopoulos et al., 1997; Kouvarakis and Mihalopoulos; Zerefos et al., 2000; Kubilay et al., 2002; Karnieli et al., 2009; Georgoulias et al., 2009; 2016).

13) Section 3.3.: The discussion of the NO_y species is fragmented, with lack of coherency and it does not provide a thorough overview over the Eastern Mediterranean regional baseline. In the begging there is some reference to baseline observational studies , then there is a sudden shift to a more extensive discussion of NO_y and NO_x species at urban sites at Israel and in the end there is a short discussion of satellite studies of tropospheric NO_2 columnar densities.

14) Page 24, line 14: It is written that " NO_y, the total reactive nitrogen (NO + NO_2 + HNO_3)..." . The NO_y includes also PAN along with HNO_4, N_2O_5, NO_3 and other PAN homologues (PANs) and organic nitrates (Emmons et al., 1997).

15) Page 24, line 23: It is written that NO of 20 pptv were observed at Finokalia. This is rather low and not typical for Finokalia station. For example Kouvarakis et al., (2002) reports that NO concentrations ranged between the detection limit of 50 pptv (most of the time) and 100 pptv and NO_x' between 0.1 and 4 ppbv. Also Gerasopoulos et al (2006) reports average day-time values of NO up to 80 pptv and respective NO_z

values up to 1.6 ppbv. See also related articles for NOy measurements at Eastern Mediterranean from the MINOS campaign (Traub et al., 2003; Heland et al., 2003).

16) Section 3.4: The section focuses on CO sources and pathways but I think it is essential to give in the beginning also an overview of the CO baseline levels at Eastern Mediterranean based on observational studies. Furthermore the discussion is not balanced e.g. from page 27 (line 8) to page 29 (line 29) there is extending discussion on the results of a single article (Drori eta l., 2012)

17) Section 3.4: Methane is a long-lived species in contrast to all others species discussed earlier. I think the authors should make a distinction in the discussion of the short-lived pollutants versus the long-lived pollutants.

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