

Interactive comment on “Transport and build-up of tropospheric trace gases during the MINOS campaign: Comparison of GOME, in situ aircraft measurements and MATCH-MPIC-data” by A. Ladstätter-Weissenmayer et al.

Anonymous Referee #2

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General Comments:

The paper concentrates on two major aspects:

a) to compare (and validate) GOME, and in-situ measurements as well as model results of tropospheric NO₂ and HCHO. b) to investigate the origin, transport and importance of tropospheric pollution in the summer Mediterranean region.

Both aspects are of great interest and significance for the scientific community and the results are convincing in its own. However, the motivation, the presentation, and moreover the logical structure of the presented work has several important shortcomings.

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In some parts of the manuscript it takes the reader (at least me) a lot of time to assess what the authors might have wanted to tell (and why)!

I hope that the authors are willing to spend considerable efforts to improve these aspects, because their interesting results should be made available to the scientific community as soon as possible.

Detailed Comments:

(The page and line numbering refers to the original version)

1. Page 2, lines 1-2: 'trace gases NO₂' Maybe in an earlier version of the manuscript the authors had combined the statements for NO₂ and HCHO. This was possibly a good choice; in the current version this paragraph is a little bit confusing.
2. Page 2, line 30: Tropospheric H₂O might be included in the list
3. Page 2, line 43: Does this mean that a given GOME pixel (320x40km²) is treated as cloud free if the cloud fraction of all three GOME pixels lying side by side is <10%. Why did the authors chose this procedure? Why is then e.g. a GOME pixel without clouds rejected if there are clouds only in the neighbouring pixels?
4. Page 2, line 43: How do the authors define cloud fraction?
5. Page 2, line 43: Do the authors think that a cloud fraction of 10% can be neglected? Since clouds are usually much brighter than the clear part of the GOME pixel even 10% cloud cover can contribute to up to 70% of the total signal received. Do the authors correct for this potential cloud influence?
6. Page 3, line 9: The simultaneous use of 'ppm' and '%' is a little bit confusing here. It might be good to replace the error by an absolute value (0.03 ppm?)
7. Page 4. line 3: for NO₂ and HCHO different days of GOME measurements are analysed and presented (Fig.1). Why?

8. Page 4, line 11: What do the authors mean with 'contribution to the information'? If they mean something like 'sensitivity' I don't agree with this statement (although it is true for many cases). However, the sensitivity depends strongly on the solar zenith angle, the ground albedo and the wavelength. For high ground albedo, the UV spectral range, and low zenith angle the sensitivity (often expressed as AMF) for tropospheric species can be significantly higher than for the stratospheric species.

9. Page 4, lines 15-27: I found this section very confusing! What is at least missing is a clear description (or mentioning) of each step performed in the determination of the tropospheric columns. As an alternative mentioning adequate references might be sufficient.

10. Page 5, line 6: I don't fully understand the statement beginning with 'Ďand additionallyĎ' What do the authors mean with 100% efficiency? Of course, it is clear that especially for low ground albedo the AMF for boundary layer profiles becomes low; however, if an appropriate AMF is applied the true VCD is derived (with 100% 'efficiency').

11. Page 6, section 4. Results and discussion: This kind of information would have been helpful already in the introduction. Please also make clear what is meant with: 'The second aim was to interpret these data with the results of the global detected GOME dataĎ.'

12: Page 8, line 38: What do the authors mean by 'sensitivity regarding to the density of the radianceĎ.?'

13: Page 8, section starting with line 38: What is the motivation to introduce weighting functions here? So far the authors have used AMFs as a measure of sensitivity of GOME to specific trace gas profiles. Essentially, AMFs for box profiles at different layer heights seem to be similar to the weighting functions concept introduced here. I suggest three possible options: a) To totally remove this section because it does not significantly contribute to the findings and conclusions of the paper. b) To replace

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this section by a formulation consistent with the AMF concept. c) To at least explain and motivate why this section was introduced, explain all quantities in a sufficient and consistent way and give appropriate references for the concept.

14. Page 10, line 3: What about trajectories starting at the ground?

15. Page 10, lines 3 to 7: Why are the levels given in two different measures (meter and hPa)?

16. Page 10, line 8: What is 'traj.x'? Give at least a reference. With whom did the authors have their [private communication]?

17. Page 10, line 32: How is the 'Total Totals' index calculated? Give at least an adequate reference.

18. Page 11, line 2: What in the author's view is the difference between 'comparison' (NO₂) and 'validation' (HCHO)? As far as I followed the paper both trace gases have been investigated in a similar way.

19. Page 11, line 10: What do the authors mean with 'based on GOME data'? 20. Figure 1a): Have these excess NO₂ VCDs been calculated using AMFs for 'standard' profiles? And for which ones? What is the reason for the white gaps?

21. Figures 1a), 1b): For HCHO different days are presented compared to NO₂. Why? Also the white gaps are more frequent and at different locations. Why?

22. Figure 3: It seems that the 'average NO₂ profile' is significantly higher than the true average of the individual profiles.

23. Figures 5, 6: Both Figures are a little bit confusing and hard to read. Please increase the font size and remove unnecessary information.

24. Figure 7, 8: I think these Figures are misleading. They display the change in the column density as a function of change in mixing ratio. It is clear that the change in concentration for a given change in mixing ratio depends strongly on altitude. Accord-

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ingly, the sensitivity of GOME decrease with altitude above about 4 km (for a given change in mixing ratio). In reality the sensitivity (for a given change in concentration) is almost constant above 4 km. Also the x-axis do not start at 0 which makes the curves even more misleading.

25. Fig. 9, 10: What was the reason to select the period from 01 - 03 August? What about the other days?

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