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Comment

Interactive comment on “Water vapour variability in the high-latitude upper troposphere – Part 2: Impact of volcanic emissions” by C. E. Sioris et al.

Anonymous Referee #3

Received and published: 28 October 2015

The authors investigate vertical profiles of water vapour using data from satellite instruments after three recent volcanic eruptions. Their objective is to show that volcanic water vapour emissions lead to an enhancement of upper tropospheric humidity in high latitudes during several months after the eruptions. I am not convinced by this analysis. I think the authors address an interesting question but the presentation is confusing and the physical argumentation unclear, such that I could not really follow the line of thoughts. In the present form the paper cannot be published in ACP. I recommend a complete rewriting of the paper after the authors have carefully reconsidered how they think that volcanic emission can impact upper tropospheric humidity in remote areas on time scales of several weeks.

Comments (most of them are major, some are minor):

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1) General: I found nowhere a good explanation of why this study focuses on water vapour in high latitudes. Nabro is close to the equator and Puyehue at 40degS - would it not be much more intuitive to first look at water vapour profiles close to the eruptions? What is your argumentation that water vapour emitted near the equator should reach the polar regions (see also comment 2)? The authors should explain how many profiles would be available to look at the surroundings of the volcanoes and why they decided to not look at them (except for one profile in Fig. 7).

2) General: It is not trivial for water vapour emitted in the tropical/midlatitude troposphere to reach the polar upper troposphere. As long as the air is not saturated transport is along isentropes which slope upward towards the pole. Therefore air parcels moving poleward from Nabro or Puyehue are expected to experience adiabatic cooling, leading to cloud formation and rainout. Since I assume that the emitted air from the volcanoes is humid, it requires only a minor lifting to reach saturation and cloud/rain formation. In other words, water vapour cannot be transported easily from the tropics to the polar upper troposphere without being deposited at the ground via precipitation. Therefore studies on the typical tropospheric residence of water estimate values of a few days (e.g., Trenberth, K. E. (1998). Atmospheric moisture residence times and cycling: Implications for rainfall rates and climate change. *Climatic Change*, 39(4), 667-694, and several other/more recent studies on this topic). Your statement on p. 25879/80 "most of the water emitted ... will tend to remain in the vapour phase as it is advected to the southern high-latitude upper troposphere" is most likely wrong. I think with a simple parcel model, lifting a moist air mass to the upper troposphere, you could show that saturation would occur rather quickly. Of course an alternative pathway of water vapour transport is via the stratosphere. If the volcano injects water (most likely in form of ice particles) into the lower stratosphere, then this vapour can "survive" much longer without being trapped by clouds and could maybe make it to the polar regions. But the paper remains very fuzzy about which transport pathway occurred, and I find it irritating that the aspect of saturation and cloud formation associated with poleward transport in the troposphere is never mentioned.

3) General and in line with comment 2: the authors sometimes compare aerosol signals with water vapour signals, and they seem to conclude that when the volcanic aerosol plume reaches the high latitudes, that then an observed water vapour enhancement is also due to the volcanic plume. Again, water vapour is rather short-lived in the troposphere and responds differently to cloud formation and rainout than aerosols. Therefore I would be much more careful with linking volcanic aerosol plumes to water vapour signals.

4) p. 25874 line 15: for most readers of ACP the volcanic explosivity index is not known. Therefore mentioning the index value for one eruption (but not for the others) and without a more general context is not useful in the abstract.

5) p. 25875 line 4: what do you mean by "in theory"? I don't think that there is a theory about this topic.

6) p. 25875 line 15: here you mention an indirect effect: volcanic eruption → temperature change → humidity change. What I am missing here, is a systematic summary of different processes of how volcanic eruptions may influence tropospheric humidity and on what time scales (direct emission, transport, indirect effects via temperature, pathway via the stratosphere, ...).

7) p. 25875 line 17: what do you mean by "remain in the ... data": is it persistent feature over many years?

8) p. 25875 line 23: this sentence is very long, contains different things and is confusing. Please try to write in a clearer way.

9) General: I find it strange the the coordinates of the volcanoes are never given. This is important information.

10) p. 25876 line 14: cf. comment 1): Why do you mention here only high latitudes?

11) p. 25876 line 19: Bernath et al. is not in the list of references.

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12) Figures 1 and 2: the caption of Fig. 1 mentions VMR (of what?). What should the reader learn from Fig. 2? I was confused by the many lines, instruments, errors ... please help the reader to understand what is relevant for this study. It is also irritating that only the caption of Fig. 2 mentions the vertical resolution of the data. I never found this discussed in the text!

13) General: It never became clear to me how many profile observations are available from the two satellite instrument. I suggest that for every figure it should be clearly indicated whether this is a monthly mean or climatological profile calculated from 10, 100 or 1000 individual profiles.

14) p. 25878 line 8: I am not sure that your course analysis of the tropopause height is relevant. Also Fig. 12 does not contain very interesting information. I think it would be sufficient to mention that the tropopause height varies between X and Y km.

15) p. 25878 lines 13ff: I don't understand this paragraph. "20 observations per altitude bin per month": is this at a particular point or somewhere in the 60-90deg latitude band? In case of the latter, then I doubt that 20 observations are enough to obtain representative monthly mean, high-latitude averaged profiles.

16) Figure 4 is an important figure, but I am not sure that it is consistent with Fig. 3. Figure 3 shows an enormous peak in spring 2007 at 7.5 and 8.5 km, but this is not seen in Fig. 4, which I find very irritating. Since the scale in Fig. 3 is a log-scale, this peak should lead to a very prominent anomaly in Fig. 4(?).

17) Section 3.1: I found it very difficult to understand the presentation and discussion of the results in this section (which is the core part of the paper). The discussion jumps from high latitudes (60-90S) to the band from 40-60S, from aerosols to water vapour, from a single profile (Fig. 7) to monthly means, from VMR to relative humidity ... this really did not help to understand the story and to find the story convincing. Please help the reader much better to follow your line of thoughts.

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18) p. 25880 line 18: I don't understand why there is this sentence about cooling rates at the surface in the paper - also the appendix does not help to understand what has been done and why.

19) General: I find the quality of the figures rather low. For instance, there are often no axis ticks and therefore it is not clear, e.g., in Fig. 3 where 20, 30, ... ppm are. Also in Fig. 3 some vertical lines would help a lot to attribute the values to a particular month. Some figure captions are specific about the region, others are not. I think every figure caption showing a profile should indicate how many profiles have been averaged to produce the profile shown.

20) P. 25882: here I am completely lost; why do you discuss here data quality issues?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 25873, 2015.

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