

Interactive comment on “Cold air outbreaks and their signature in the ozonometric data at the mountain station near Kislovodsk, Russia” by N. P. Chakina et al.

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In their study, the authors present detailed case studies of cold air outbreaks and their impact on ozone measurements. The study is well prepared, the number of figures is adapted to the length of the manuscript and the figures are reasonably selected to illustrate the contents of the study. Nevertheless, some comments might be taken into account by the authors in order to improve the understanding and to help the reader.

At the end of each major subsection, the authors present a short summary of the main results. I very much appreciated this kind of intermediate summary, and think that it should be kept.

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1. The authors mention right at the end of the introduction what the aim of the study is. Hence, it remains to the reader unclear for a very long time what the aim is. Certainly, it would be much better if the reader learns much earlier about it. Furthermore, the authors should inform the reader of the new aspects that the study brings. Perhaps, this can be done by stating some explicit hypotheses, which are then tested within the study. I had sometimes the impression (not only for the introduction) that such a small sample of hypotheses would give the reader a very helpful guideline throughout the paper, such that he does not get lost in the details of the described measurements or upper-level structures. So, please consider to restructure the introduction and to give the reader an explicit guideline throughout the paper.

2. Some comments and clarifications concerning the "data and diagnostics" section are:

(a) The authors state that the large-scale atmospheric conditions are diagnosed from objective analysis data. What is the source of these analysis data?

(b) The discussion of the different PV values as tropopause markers is quite long. Perhaps, a reference to a suitable review article might be better (for instance, Stohl et. al). Furthermore, a value of 1 pvu as a marker for the lower dynamical tropopause seems to be quite low. I know of no other study which takes such a low value. But I agree that 1 pvu might be taken as a marker to highlight stratospheric intrusions into the troposphere.

(c) If the frontal parameter has been introduced in Chanika et al., there is no need to reintroduce it here. On the other hand, it might be difficult to get access to the mentioned study. To solve the dilemma, and since the definition of the parameter is not crucial for the further discussion, I suggest either to skip the definition of the frontal parameter or to place it into an appendix. In the latter case, it should be somewhat extended, since in its present form the definition is rather technical and difficult to understand.

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(d) In section 7 (vertical cross sections) the data source of the surface topography files is described. This description should be placed in section 2.

3. The following discussion is splitted into too many different subsections. Here, the reader is somewhat in danger to get lost in too many details. Here, the guiding hypotheses (see above, comment 1) might be very helpful. Furthermore, I would prefer a start with the large-scale dynamics instead of with the surface observations. Having the large-scale in dynamics in mind, it might be easier for the reader to follow the description of the surface measurements. Since the subsections 5 and 6 are very short, it might also be reasonable to incorporate them into subsections 3 and 4. Thereby, the discussion would be nicely splitted into a surface subsection (now 3 and 5), a tropopause subsection now 4 and 6) and a link section which illustrates the vertical structure (now 7).

4. Some clarifications in sections 3-6 are:

(a) In section 6 reference is made to a threshold value for the tropospheric jet stream. What is meant by that statement?

(b) In section 4, reference is made to a hydrodynamical instability. What kind of instability is it? Please be somewhat more specific? Furthermore, I would prefer the term "flow instability" to "hydrodynamic instability", since the latter is strongly associated with water flows. In the final discussion, the instability is referred to as baroclinic in its origin. Is this obvious?

(c) At several places in subsection 4, reference is made to a streamer. In what sense are these features streamers? Isn't it the case that the meaning of streamer becomes clear only after the introduction of Fig. 5? Care should be taken here, since different meanings of streamers might be in use.

(d) In the third paragraph of section 3, an intermediate band is described. Does this intermediate band have any dynamical significance? Has it any influence on the mea-

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surements?

5. Finally, the authors use isentropic trajectories to elucidate the upper-tropospheric or lower-stratospheric origin of the measured air masses. Why are isentropic trajectories used? Wouldn't it be better to use full 3d trajectories? And if not, please justify why the trajectories are isentropic and what the deviation between these isentropic and full 3d trajectories might be. Furthermore, the authors state that the elevation of the points vary from 370 to 3700 m asl. I did not understand this statement in the context? Please be somewhat more specific.

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