Atmos. Chem. Phys. Discuss., 13, C5045–C5052, 2013
www.atmos-chem-phys-discuss.net/13/C5045/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of the diurnal development of the Ora del Garda wind in the Alps from airborne and surface measurements" *by* L. Laiti et al.

Anonymous Referee #1

Received and published: 23 July 2013

(A) General comments:

A cases study is presented for a thermally driven wind in an Alpine valley based on airborne and surface observations. The study is similar to a previous paper published in another journal. Although the data is unique and deserves to be published, the analysis needs to be better focused an new aspects not treated in the previous paper. At least a critical comparison of current and previous results is necessary. Connected to this critique, the research goals need to be more specific. The structure of the manuscript (sections and content) need to be improved. Questions arise on the representativity of the observed cross-valley features in data-sparse regions close to the terrain. Some

of the cross-valley variations are rather small and might be blurred by noise. I suggest major revisions.

(B) Specific comments:

1. It is not clear how this manuscript differs from the cited paper Laiti et al. (2013b; published in Atmospheric Research). The titles of these two papers are very similar and the results mentioned in the abstracts are nearly identical. From a reader's perspective it appears that the two case studies only differ in the analyzed dataset (different cases) but not in the general results. In the conclusions on page 19148 (line 3) the authors state that "[a]ll the above findings are consistent with previous results contained in Laiti et al. (2013a,b)". Hence, the reader will ask why we need another paper on the same subject. In the manuscript, there are only a few references to Laiti et al. (2013b) from which the reader cannot deduce the additional value of the current manuscript. A critical comparison of the results of these two papers in some discussion section and a summary of the key findings of the first study in the introductory part is missing. The authors should better focus their manuscript on new aspects not treated in their previous paper(s) or better highlight differences in the flow characteristics observed in these two case studies.

2. The abstract is long and discursive. Only about 20% of the lines contain results. The last paragraph fits better to the section "conclusions".

3. Similarly, the conclusion is long and discursive. I suggest to split this section into two: a discussion (e.g., also containing a comparison to previous literature) and a discrete, concise conclusion containing the main findings. See the suggestions of Geerts (1999) concerning the form of the abstract and thes conclusions and see also Schultz (2009) for the difference between discussion and conclusion.

4. Introduction and section 2, page 19123-19128: The authors should provide more specific goals or research questions (e.g., see so-called SMART goals). They should also highlight differences in these goals/questions compared to the ones in Laiti et al.

(2013b). Usually these goals are formulated at the end of the literature review (section 1). In the present manuscript, however, the aims are mentioned in section 1 between two parts of literature review. Moreover, section 2 is partly a repetition of section 1 as it also contains a literature review. I suggest to combine these two sections followed by the goals of the study. The literature review could also be reduced by referring to Laiti et al. (2013b). It could also be focused on new aspects relevant in the present study but not treated in the previous paper (e.g. propagation of the lake-breeze front).

5. The authors mention on page 19127 a number of small lakes located north of Lake Garda. I am wondering how these lakes influence the valley wind and the boundary layer structure observed by surface stations and the aircraft. A discussion of this aspect in the results section is missing. This could also be a specific research question.

6. Introduction, results and conclusions: The references and the interpretation of results regarding the heating of the valley atmosphere are somewhat biased. Heating is explained as a result of compensating subsidence in the center of the valley (e.g., page 19138, 19139, 19146, 19148) and papers are cited that propose this mechanism. These papers are mainly based on the analysis of vertical profiles (observed and modeled). However, in recent years studies have been published that propose another approach based on the heat budget analysis for the whole valley volume. They tried to clarify the role of the volume effect. I strongly suggest to integrate and discuss ideas of both perspectives. These two different perspectives are not necessarily contradictory and data gained by one or the other method are not wrong. It is often a matter of the right interpretation.

7. The structure of the manuscript is not ideal: Section 3.3 “Weather conditions” should not be part of section 3 “Experimental dataset”. It should be rather part of section 5 “Results”, first explaining the synoptic background conditions before focusing on the regional and local scales. The section title “discussion of results” is somewhat misleading. Usually the section “discussion” follows the section “results”. The devision of the section 5 in various subsections with partly the same title (e.g., “Lower Sarca

Valley ...”) causes repetitions and, hence, is somewhat tedious. I suggest to introduce a new structure by combining different datasets (weather stations and aircraft data) in order to draft a comprehensive picture of wind and boundary layer structure for each subregion.

8. Section 4.2 on page 19132-19134: On one hand the section is to short for actually understanding the details of the kriging technique. On the other hand most of it is presumably already mentioned in more detail in Laiti et al. (2013a,b). Hence, I suggest to reduce this section and the previous one to a minimum by referring to the former paper or to expand it (or at least the critical parts) to explain the technique in more detail.

9. Section 5.1.1. on page 19134 and Figs. 3-4: Explain the weak southeasterly winds at Monte Terlago between about 8030 and 1030 LST already before the onset of up-valley winds at Lake Garda (RDG). Is this pattern a cross-gap circulation before the actual up-valley flow establishes at Monte Terlago? Explain the earlier decay of the up-valley flow at RDG in comparison to Monte Terlago. Discuss the contradictory feature of a nighttime down-valley flow at the shoreline (RDG) and a lake temperature that is cooler throughout the whole day than the air temperature at RDG (which would favor an up-valley flow).

10. Section 5.3, page 19141, line 12 and elsewhere in the manuscript: A standard deviation of the interpolated (residual) values of 0.00-0.25 K raises the question if the cross-valley structure in terms of variations of the heights of the isentropes shown in Figs. 8-12 are significant. The precision of the temperature measurement (not the theoretical one in the laboratory but in the real one in the atmosphere) together with the instrument’s time lag in air and the artificial heating due to air impinging on the sensor (which is not constant as the air speed varies) may introduce noise and obscure the true structure (e.g., the maximum variation in potential temperatures in Fig. 8 at a certain altitude is only about 0.5 to 0.75 K). Further, in several of the cross-valley transects shown in Figs. 8-12 the interpolated potential temperature field is extended

to the slopes. However, due to aircraft safety reasons the horizontal distance between the slope and the nearest data point is in the order of 500 to 1000 m. Hence, the slope wind layer is not captured and the interpolated (or rather extrapolated) fields close to the terrain do not represent the reality.

11. Figure 8 and page 19142, line 9-14: I do not see the described asymmetry. For example, the isentrope near 1500 m (302.25 K) is about at the same altitude on the eastern valley side as on the western valley side. Instead of an asymmetry it appears that the central part of the valley atmosphere is slightly cooler than about 1-1.5 km east and west of the center.

12. A comprehensive discussion and interpretation of Fig. 9 (along-valley transect) is missing.

13. Figure 12 and page 19143, line 23-28 and following page: How significant are the features represented by extrapolated data close to the slopes? The closest data point is about 500 to 1000 m located horizontally from the slope. See also comment further above.

(C) Minor and technical comments:

1. Title: “Analysis . . . based on airborne ...” would be better than “... from airborne ...”

2. Section 1, page 19124, line 16: “thermotopographically-driven” is a misleading expression

3. Section 1, page 19124, line 28-29: “Extended Sea Breeze” is explained/mentioned twice.

4. Section 2, page 19128, line 17-21: I assume that the times of the flow reversal and the outbreak into the Adige Valley strongly depend on the season. Information on this dependency is needed, or at least information of the season for the indicated times.

5. Table 1: Add date of flight in the caption. Explain symbol “[-]” in the caption.

6. Table 2, 3 and elsewhere in the manuscript: Use MSL and AGL instead of m.s.l. and a.g.l. The former two are used in many axis labels.

7. Section 4, page 19131, line 21-25: The temporal variability between the first and last transect of the morning might be substantical (about 2.5 hours between E1a and E1b). This should be mentioned and discussed. Figure 6b shows a significant change in the PBL structure between E1a and E1b.

8. Section 4, page 19132, line 28: Better explain “moving-window vertical average”. Are data within this window equally weighted or weighted according to their distance?

9. Section 4, page 19133, line 1-4: I do not understand this sentence. Explain better or skip and refer to technical paper Laiti et al.(2003a).

10. Figure 3 and corresponding text on page 19133-19134: I suggest to indicate the times of onset and decay of up-valley winds at different stations. Also describe gray shaded area in the caption of Fig. 3.

11. Figure 4: Part of the dashed/dotted lines are rather hard to distinguish. Further, I suggest to use potential temperature instead of air temperature in order to facilitate a comparison of stations at different altitudes.

12. Page 19134, line 2: Is this 6 m/s wind speed a one-hour average? Be careful when relating this wind speed to wind speeds at other stations with a shorter averaging period

13. Page 19134, line 24-26: Provide an appropriate reference for the “standard” diurnal cycle.

14. Page 19136, line 3-7: Is there any effect of the “anomalous southward channeling” on the temperature shown in Fig. 4 (e.g., an abrupt decrease)? If not, why?

15. Section 5.2, page 19137: Udine (LIPD) is mentioned in the text but not shown in Figs. 6-7. Are the soundings shown in these figures the ones used in the kriging

algorithm (“vertical drift”)? I suggest to mention this again.

16. Section 5.2.1, page 19138, line 14 and several other places in the manuscript: “lapse rate” is the rate of decrease of height of some parameter. As potential temperature increases on average with height, the expression “lapse rate” is misleading. I suggest to use “vertical gradient”.

17. Page 19142, line 25: instead of “the local cross-section is very narrow” rather “the valley is very narrow”.

18. Page 19142, line 27: Notice that rock usually has a higher albedo than forest and, hence, less net shortwave downward radiation which might reduce the heating of the air. However, there is less (or no) latent heat flux above rock compared to vegetation and hence more energy available for sensible heat flux at the surface.

19. Page 19144, line 2 and page 19147, line 21: Explain “turbulent recirculation”. Is this a sort of wave breaking or turbulence in a hydraulic jump-like feature?

20. Page 19144, line 20: I do not understand “obstruction exerted by ...”.

21. Page 19148: Line 14-23: Do we really need this long list of references at the very end of the paper? Some of the papers should be rather cited in the introductory part as a motivation.

22. Figures 8-12: Ticks on x- and y-axis are hidden by the gray area.

(D) References:

Geerts, B., 1999: Trends in Atmospheric Science Journals: A Reader’s Perspective Bull. Amer. Meteor. Soc., 80, 639-651

Laiti, L., Zardi, D., de Franceschi, M., and Rampanelli, G., 2013a: Residual Kriging analysis of airborne measurements: application to the mapping of atmospheric boundary-layer thermal structures in a mountain valley, Atmos. Sci. Lett., 14, 79–85

Laiti, L. Zardi, D. de Franceschi, M. and Rampanelli, G., 2013b: Atmospheric boundary-layer structures associated with the Ora del Garda wind in the Alps as revealed from airborne and surfae measurements. Atmos. Res., in press, doi:10.1016/j.atmosres.2013.07.006

Schultz, D. M., 2009: Eloquent Science: A Practical Guide to Becoming a Better Writer, Speaker, and Atmospheric Scientist, American Meteorological Society, 412 pp.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 19121, 2013.