Atmos. Chem. Phys. Discuss., 14, C8778–C8782, 2014
www.atmos-chem-phys-discuss.net/14/C8778/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dynamical analysis of sea-breeze hodograph rotation in Sardinia" *by* N. Moisseeva and D. G. Steyn

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Received and published: 4 November 2014

We would like to thank the reviewer for his feedback and suggestions. Please find the in-text responses to comments below.

The authors conducted a modeling study to understand the terms controlling the clockwise and anticlockwise rotation of seabreezes. The terrain seems to have a significant role in determining SB hodograph rotation, which may have been fairly important in determining the different patterns of SB hodograph rotation around the island. Moreover, how those 12 stations around the island are positioned relative to the synoptic circulation pattern in that case study and hence how that might've affected SB hodograph rotation is worth looking into.

Response:
The 12 stations around the island were used primarily for model evaluation, rather than to understand the dynamics underlying hodograph rotation. As we were predominantly interested in larger mesoscale effects the dynamical analysis was performed in regions away from the coast, which were, hence, less sensitive to local influences of topography, land use and cover.

A case in point is pages 22891 lines 9-10, where the authors stated, "The synoptic gradient acts largely in opposition to the surface gradients, likely due to the formation of SB return flow near 850mb level". Wouldn't it be easier to illustrate the synoptic circulation pattern over the island and be quantitative about it, which they did in Fig. 5, so as to be more definitive than "likely"?

Response:
Thank you for highlighting the lack of clarity in both: describing the synoptic conditions, as well as explaining the meaning of synoptic gradient. The issue has been raised by a number of reviewers and we agree strongly that further clarifications are needed. Please find a detailed response to the issue as well as proposed additions to the manuscript in our Comments for Reviewer 1.
Regarding the apparent ambiguity in the wording: it is generally impossible to quantitatively separate true synoptic effects from the SB return flow. As per Melas (2000), "the return flow merges with the prevailing synoptic flow and is difficult to distinguish".

BTW, what did the authors mean by "surface gradient"? If they were referring surface pressure gradient, they should stick to the term. The presentation of their analysis can be more quantitative than it is.

Response:
Thank you for pointing out inconsistencies in the use of the term throughout the paper. We will revise this in the next version of the manuscript. Similarly to the previous comment, please find the proposed additions with term descriptions in Comments for

Reviewer 1.

Another comment is it is not clear why the authors conducted the idealized case study. The island has three mountain ranges. In the idealized case, they reduced it to one among a few other assumptions. If they aimed to narrow down the causes for those CR and ACR patterns by comparing the idealized with the real case, it seems to be pretty difficult, as there were a few other factors that were also different.

Response:
The primary reason for performing an idealized study is to attempt to generalize the findings and extrapolate them to islands other than Sardinia. On this matter, we tend to agree with Short Comments by Reviewer 2, that an idealized experiment is of interest as it allows to experiment with alternative geophysical settings. The use of idealized topography in mesoscale modelling has a substantial history, and many precedents (for example, Gaberšek and Durran, 2006 and Pathirana et al 2003). In such studies, the idealized topography allows one to draw conclusions not specific to a particular instance of a phenomenon, but to similar phenomena in general. The approach also allows conclusions about processes independent of small-scale variability in topography, coastline, roughness length and surface energy budgets.

Page 22894 lines 2-3: the authors stated, "Regions of CR and ACR are arranged on opposite coasts to that of the real Sardinia, and similarly to Corsica from the real simulation and the Attic Peninsula from Steyn and Kallos (1992)." Again, wouldn't it be helpful to show the synoptic system for that day? It seems to work in opposite directions on the west and east coast SBs. Comparing this result with Steryn and Kallos (1992) is not really that meaningful, unless they had also pointed out the terrain there was similar to the idealized terrain in this study and the synoptic flow was quite the same etc.

Response:
Thank you for pointing out the disconnection in the argument. Attic Peninsula does, in

fact, carry more topographic similarities with the idealized simulation than Sardinia. It is dominated by a single mountain range with similar elevation to that of our idealized island. Similarly to our results, the synoptic flow in Steyn and Kallos (1992) was also found to be of little importance in the Attic Peninsula, simply due to the nature of conditions typically associated with sea-breeze days. To summarize, we will include the following clarification following the quoted sentence in the revised manuscript:
This can likely be attributed to similarities in the shape and elevation of topography of the Attic Peninsula to that of the idealized simulation.

On the same page, lines 12 -14: "This may be an indication of a model response to the morning switch in the direction of surface heat flux, which in some cases produces a spike in model fields." Again, this can easily be quantitative by showing the diurnal cycle of modeled surface heat flux to back up this point, instead of leaving it qualitative and speculative. Overall, this paper can use some revision to make their analysis more quantitative and detailed.

Response:
While we have certainly verified that the timing of the switch in the direction of surface heat flux does in fact coincide with the occurrence of the spike in model fields (in exactly the way suggested by the reviewer), we are wary of drawing such firm conclusions. Though the events occur simultaneously, the complexity of interactions between land, surface and boundary routines in the WRF model makes the relationship difficult to quantify further.

References
Gaberšek, Saša, and Dale R. Durran. "Gap flows through idealized topography. Part II: Effects of rotation and surface friction." Journal of the atmospheric sciences 63.11 (2006): 2720-2739.
Pathirana, Assela, Masafumi Yamaguchi, and Tadashi Yamada. "Idealized simulation of airflow over a mountain ridge using a mesoscale atmospheric model." Annual Journal of Hydraulic Engineering, JSCE 47 (2003): 31-36.

Melas, D., Lavagnini, A., and Sempreviva, A.: An Investigation of the Boundary Layer Dynamics of Sardinia Island under Sea- Breeze Conditions, J. Appl. Meteorol., 39, 516–524, 2000.
Steyn, D. and Kallos, G.: A study of the dynamics of hodograph rotation in the sea breezes of Attica, Greece, Bound-Lay. Meteo- rol., 58, 215–228, 1992.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22881, 2014.