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Interactive Comment

Interactive comment on "High resolution observations of the near-surface wind field over an isolated mountain and in a steep river canyon" *by* B. W. Butler et al.

Anonymous Referee #2

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General comments:

The authors give an overview of two very unique new datasets collected in two types of complex terrain. In two separate summer field campaigns, near-surface wind data at 3.3 m agl at 50+ locations was collected (1) on and around an isolated mountain (Big Southern Butte, 800 m relief) and (2) in the 550-m deep Salmon River Canyon.

The methodology of binning the dataset in synoptically forced and thermally driven regimes based on a threshold wind speed at one single site has caveats that become obvious from the results but are not thoroughly discussed. These problems lead to exceptions from the expected results (such as 'downslope winds' of 12 m/s on top of BSB; even the 7.5 m/s wind speeds are doubtful (Fig5b)) that are then discussed and excluded. See more details in specific comments below. The failure of this method casts doubt on the presented results. Maybe a case study approach would be more useful and could better test and improve the current concepts of thermally driven flows in complex terrain.

Other than comparing trends of down- and upslope flows with distance up and down topography gradients, however, the article does not provide any significant scientific results. The goal of this article remains somewhat unclear, other than reporting on a new dataset.

The authors have a unique new dataset to analyze which mirrors the complex interplay of thermally driven flows on different scales. The rather crude approach, however, leads to a confusing picture and no clear results. This analysis, in my opinion, needs more work is not publishable in its present form.

Specific comments:

1) Thermally driven flows in complex topography are a key topic in mountain meteorology. The manuscript lacks references to some relevant articles and reviews such as Defant (1949), Whiteman (2000) and Zardi and Whiteman (2013).

2) "Upvalley drainage winds" are listed as a mechanism to couple the surface flow to the synoptic flow. Drainage winds are usually related to the fact that denser air drains down a topographic gradient. It is not clear what process the authors are referring to.

3) A paragraph describing the surface flow field that is expected in the current state of knowledge at each the two study sites under the 'diurnal wind regime' could be included to set the stage for the findings.

4) Binning into synoptically forced regime: The authors chose to use one single representative site for each experiment for which threshold wind speeds are determined that

will separate thermally driven and synoptically driven regimes. What are the caveats of this methodology? For example, a "reference station" on the plain surrounding BSB was chosen (R2) to distinguish between the two regimes. How likely is it that this station will be dominated by nocturnal thermally driven flows in the evening while the flow on the butte is not? On the other hand, NM1 was chosen as "reference station" for the Salmon River Canyon site, which is ∼500 m (?) above the canyon bottom. How likely are thermally driven flows still dominating the river gorge when a synoptic influence is seen at the reference site? A thorough discussion of the implications of this filtering method is needed. Furthermore, the methodology seems to fail, and while extreme events such as drainage flows on top of BSB of 12 m/s are discussed as outliers, speeds of 7.3 m/s are reported as valid data points (Fig 4b).

5) BSB: The "afternoon regime" vector map (Fig 4) could be interpreted as a flow field based purely on daytime thermally driven circulations where upslope and upvalley flows interact. How is the distinction made between a purely thermally driven flow regime and a situation with a synoptic influence? R2 shows only a weak flow (maybe 4 m/s?; see comment on presentation) around the obstacle.

6) Figure 12 includes a site (NM2) that was in an earlier thorough discussion characterized as an outlier. It therefore should be omitted and not presented as part of a elevation transect.

7) Standard times should be used instead of daylight savings time.

8) What is the role of terrain shading at the SRC site? What are its implications on the timing of the transitions between thermally driven flow regimes?

9) The manuscript unnecessarily describes sodar and radiosonde observations and deployment schedules. This should be omitted, as none of the data is presented or used in the presented analysis.

10) Presentation:

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Overall graphic presentation is fair and could be substantially improved:

a) Maps: The article lacks bigger and clearly readable maps for the two field sites. Instead of several subfigures covering different geographic extents, a full-page figure is needed with readable labels of the sites and elevation contours. A distance scale is needed; different symbols could be used for the different instrumentation. Transects later referred to could be marked and labeled.

b) Wind vector graphics: Color bar could be extended; a reference-length vector could be included. Two bigger figures would be better than 4 small sub-figures. Key locations referred to in the text discussing these figures should be labeled. A cross reference with the initial maps is extremely tedious for the interested reader. Figures could be formatted to fill the space available on a page.

c) Contour graphics: Color scales could be kept constant for all sub-figures. Otherwise a comparison is not possible.

d) All subfigures should be labeled, i.e. Fig 4a through 4f.

11) SRC: How could the available, but not presented, temperature data help to evaluate different regimes?

12) Wind speed trends presented in Fig 10 are rather small. How do they compare to the uncertainty of the anemometers?

13) Correlations with gradient level winds are mentioned in the conclusions. How were gradient level winds determined for the period of observations? They should be presented earlier in the manuscript. Could they be used to filter the dataset, rather than selected surface observations?

Technical corrections:

- Decapitalize "s" in "radiosonde" (i.e. page 16829, line 3) - p 16828 l 2 Table 2 does not list AWS - Reduce number of digits in GPS readings - p 16826 l 5 ; change "down-

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drainage" flows to "down-valley" flows - p 16830 l 18: could be clarified by expanding to "... into the forth, synoptically forced, regime." - Fig 6: Label subfigures with site elevations. Mention filtering (Thermally driven regime) at the beginning of caption. Label key directions (upvalley & downvalley, upslope and downslope) in figures.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 16821, 2014.

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