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14, C7569–C7583, 2014

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.

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Response to Referee #1 comments.

Authors response to referee comments are labeled as such.

General Comments Referee #1 comment 1: The manuscript presents results from field measurements obtained from two structurally different terrain: Big Southern Butte which is about 800 m tall, and a steep river canyon in Idaho. I commend the authors for undertaking this work as there is a need for observational data for complex terrain wind models. These two cases significantly differ from existing complex terrain studies.

Based on their observations, authors also make a valid point regarding the use of numerical weather models with insufficient resolution for complex terrain regions. The manuscript is written clearly and data is presented in a way that can be used for model evaluation. Therefore, I am in favor of its publication in this journal after the authors address the following issues in a revised version.

–Author response –The authors thank this reviewer for the positive and encouraging comment. Of course we feel the same and are excited to get this data out for use by others.

Specific Comments 1) Line 5 on page 16823: mention wind forecasting and resource assessment in addition to wind turbine siting.

–Author response –These additional examples will be added to this sentence. See lines 32-54 of attached manuscript pdf.

2) Line 15 on page 16824: Askervein Hill study should be cited and mentioned.

–Author response –The Taylor and Teunissen study referenced here is the Askervein Hill study; however, we will explicitly include the name "Askervein Hill study" in this sentence as well. See lines 95 of the attached manuscript pdf.

3) Although the information is available in the main text, figure captions should convey more information.

–Author response –Additional information will be added to figure captions to ensure they are stand-alone. For example, BSB and SRC will be spelled out as Big Southern Butte and Salmon River Canyon, the time zone will be included for all reported times, and data averaging periods will be included where appropriate.

4) Provide a table for measurement coordinates. Abbreviations for sensor locations need to be spelled out in atable (R, TSW, etc) It gets confusing after a while.

–Author response –This type of table was not originally included in an attempt reduce

ACPD
14, C7569–C7583, 2014

the length of the manuscript (the table will be large due to the large number of sensor locations, 53 at BSB and 27 at SRC); This information is available in the database referenced in the manuscript and all sensor locations are shown on the map in Figure 1 which we will enlarge for the final published version. However, if the editor recommends we can include such a table.

5) Some of the figures are too small in the printer friendly version of the manuscript. Fig 1b-d, Figs 4,6,7,8,9, 10, 12

–Author response –We will enlarge these figures to ensure they are readable in the printer-friendly version.

6) Authors collected wind profiles upstream of the BSB. Those vertical profiles should be presented and discussed for each of the regimes in light of theoretically expected profiles.

–Author response –Yes, the near-surface wind observations were part of a larger field campaign in which radar profiler, sodar, sonic anemometer, and radiosonde measurements were also made for selected time periods. It is beyond the scope of this paper, however, to present all data collected from the various instruments operated during the larger field campaign. The goals of this paper are to (1) give an overview of the measurements made during the larger field campaign (as an introduction to the field campaign – this is the first paper resulting from the study), (2) describe how others can access these data, and (3) to present the near-surface wind fields measured over the terrain obstacles at each site in order to investigate the spatial effects of the terrain on the flow. These goals are stated in lines 7-15 on p. 16825. We chose to focus on the surface wind measurements in this paper because the very high spatial resolution of the surface wind measurements made during these field campaigns is perhaps the most unique contribution of this work, as essentially no datasets exist in the literature with this level of resolution. These high-resolution wind data are crucial for developing and evaluating high-resolution wind models. The vertical profile data are available in the database as described in the text. We will review the vertical provide data in the context of determining if they provide further information relevant to the primary discussion in this manuscript.

7) Provide information on the limitations of the instrumentation (e.g. threshold speeds)

–Author response –Yes, details on instrument limitations will be added in the text. For example additional discussion to the following effect will be added on page 16827. The cup and vane has a measurement range of 0 to 44 m/s, accuracy of +- 0.5 m/s and +-5 degrees with resolution of 0.19 m/s and 1.4 degrees.

The Campbell Scientific CSAT3 sonic anemometers have a measurement rate of 1 to 60 hz, with resolution of 1mm/s, 0.5 mm/s and 15mm/s for uy uz and c respectively, with a direction resolution of 0.06 degrees rms. The SATI/3Vx has measurement range of 0 to 20 m/s, with resolution of 10 mm/s and 0.1 degrees.

The Scintech MFAS samples velocities from 0 to 50 m/s up to 1000 m agl over 1 to 60 min averaging intervals, with horizontal wind speed uncertainty of 0.3 m/s and vertical wind speed accuracy of 0.1 m/s and directional uncertainty less than 1.5 degrees.

The Imet-1 system has a maximum range of 250 km to altitude of 30 km and measures air pressure, temperature, and humidity. Wind speed is calculated from onboard GPS measurements. Accuracy is 0.5 hPa in pressure, 0.2 C in temperature, and 5% in RH. Wind speed is accurate to within 1 m/s and is updated at 1 Hz. Altitude is accurate to within 15 m.

The Vaisala WXT520 measures air temperature to 60C with +-0.3 C accuracy and 0.1C resolution, Wind speed is measured from 0 to 60 m/s with 0.25 s response time and +-3% accuracy in speed and 0.1 degree accuracy in direction.

See lines 186-210 of the attached manuscript pdf for proposed edits.

8) As the authors state in the Instrumentation section, they have collected data to quantify turbulence, friction velocity, sensible heat flux, temperature and relative hu-

ACPD
14, C7569–C7583, 2014

midity. These quantities need to be presented, and discussed in a way that can help modelers.

–Author response –We appreciate this recommendation. However, as outlined in #6 above, presenting all data from all instruments is beyond the scope of the paper and would render the paper much too long.

9) Figure 3. I understand that the threshold was chosen after a visual inspection. However authors can still provide a percentile for this threshold (What percentage of data is below this value?)

–Author response –Yes, the percent of data falling below the thresholds at each site will be included in the revised manuscript.

Author's reply to Referee #2 comments

-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.

ACPD

14, C7569–C7583, 2014

–Author response –We respectfully disagree with the reviewer on the point that binning of the datasets into synoptically forced and thermally forced regimes led to exceptions which render the analysis unpublishable. In reality, synoptic effects and local thermal effects are nearly always combined to some extent; the goal of the partitioning scheme was to separate these effects to the best extent possible in order to focus on the predominant driving mechanism at a given time period. Had this type of binning not been used, we would not have been able to identify the average flow characteristics during the monitoring period (months of observations at each site). The goal was not to evaluate only one or two specific events, but to provide a description of the general flows over the study period – this could only be done by using some type of data partitioning and averaging schemes.

–Author response –We believe that the methods used revealed interesting characteristics of the flow at the two sites. For example, analysis at Big Southern Butte showed that under periods when most locations on the butte were experiencing diurnal flows, ridgetop locations were experiencing higher wind speeds, suggesting that ridgetop locations were decoupled from other locations on and around the butte. These types of findings have important implications for surface wind flow modeling.

–Author response –The approach we used is a logical one since high-wind events overpower the local thermal effects which dominate during the diurnal flow regime. There were obviously times when synoptically-forced flows occurred and times when diurnal flows dominated. The goal of partitioning the data was to bin the data into discrete periods during which the flow was predominantly driven by a common force (i.e. synoptic or local thermal effects). We believe the partitioning scheme used is a reasonable attempt to bin the data into interpretable time periods appropriate for the purposes and scope of this study. Additional specific comments on this topic are provided below.

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.

–Author response –We regret that the reviewer did not capture the objective of this manuscript. We attempted to clearly state in lines 17-23 of page 16823 the objective was to describe a research program and associated datasets from two different terrain features. We also attempted to state that the intent was to provide these high resolution data to the larger meteorological modeling community for comparison against simulations. It is our intent that these data inform the development of high resolution near surface wind flow models. A secondary objective was to provide the foundation for future papers that will explore causal factors related to flows in complex terrain. An example is the manuscript 'Diurnal Late Spring and Summertime Wind Patterns on the Snake River Plain and the Influence of Complex Terrain Factors' which examines larger scale flow patterns in the boundary layer over the Snake River Plain. It is currently in review at JAMC. The analysis presented for Big Southern Butte links the effects of the larger Snake River Plain flow patterns to their expression at smaller scales in highly complex flow situations. It is these smaller scale effects, either unique to the geography or common to these geographical features that might have implications to near surface wind modeling for specific application to wildland fire management support.

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.

–Author response –Clearly, the reviewer agrees that the data are unique in that they characterize thermally and mechanically driven flow at a very high spatial resoluiton. Part of the difficulty in evaluating a dataset like this is that the flow is driven by a complex interplay between thermal and synoptic processes that are varying in time and space. Thus our "crude" attempt at differentiating the data into different flow regimes. As stated above the primary objective was to present the data in a quantitative format to give an overview of the surface flow characteristics. Thus the logic for the “binning” methods. We argue that the primary point is not the partitioning method, but rather the high resolution data themselves. We leave it to future users of the data to select whatever partitioning schema seems best for their particular needs. The point of the analysis discussed in the manuscript was not to investigate specifically upslope or downslope winds, but rather to assess the actual surface observations under the range of flow regimes experienced at these sites under summer time conditions.

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).

–Author response –We appreciate this suggestion and will include these additional references in the section referencing other work, for example modifying lines 25 to 30 on page 16824 to read “Fine-scale (i.e., ∼1-100 m) variations in topography and vegetation substantially alter the near-surface flow field through mechanical effects, such as flow separation around obstacles, enhanced turbulence from increased surface roughness and speed-up over ridges, and through thermally-driven flows induced by local differential surface heating in steep terrain (Defant, 1949; Banta, 1984; Banta and Cotton 1982; Whiteman, 2000; Zardi and Whiteman, 2013; Chrust, et al., 2013)." See also lines 75-77 of attached manuscript pdf.

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.

–Author response – The referee has not provided sufficient context for us to clearly determine where his/her concern lies. If the concern is with the term "drainage" we argue that drainage does not imply whether the winds are flowing up the drainage or down it, thus the adjective "upvalley". In retrospect the phrases "upvalley drainage winds" or "downvalley drainainge winds" could be modified to the simpler terms "upvalley and

ACPD
14, C7569–C7583, 2014

downvalley winds" respectively.

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.

–Author response –Thank you, this will be included in the revised manuscript. We propose to include a discussion of the current state of knowledge regarding diurnal flows for each type of terrain feature. For example, discussion regarding the downslope flow regime will include: "Downslope winds develop under nighttime conditions when the air adjacent to the surface cools and becomes more dense than the free air at the same elevation. The denser air near the surface flows downslope and is channeled by the underlying terrain to form large scale drainage winds."This discussion will be included at the beginning of section 5.1.1 for Big Southern Butte and section 5.2.1 for the Salmon River Canyon. A background discussion will also be provided for the current state of knowledge regarding the synoptically forced regime at each site at the beginning of section 5.1.2 for Big Southern Butte and the beginning of section 5.2.2 for the Salmon River Canyon.

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,

ACPD
14, C7569–C7583, 2014

speeds of 7.3 m/s are reported as valid data points (Fig 4b).

–Author response – The overall goal was to present the average flow fields at each site in a context useful for surface wind flow modeling applications. We chose to present average flows for the four wind regimes described in section 4.1. The regimes listed in 4.1 are widely recognized in the mountain meteorology literature (e.g., Banta and Cotton, 1982; Whiteman, 2000). In order to summarize months of wind data at each site in terms of these flow regimes, we had to choose a partitioning scheme to bin the data. Many different partitioning schemes could have been used. We believe our choice of selecting a single representative sensor at each site to partition the flow was a reasonable approach for the purpose and scope of this study.

–Author response –It is possible that our selected reference station at the butte, for example, could "be dominated by nocturnal thermally driven flows in the evening while flow at some locations on the butte is not." In fact, this is precisely the type of unique flow features we would like to uncover in this work, as this is the type of information that is lacking in the literature, but could be very useful to surface wind model developers. We will add a more thorough discussion of the results related to the data partitioning/averaging, for example what may have been happening stability-wise in the approach flow to set up the average flows we observed. We will also employ wind data from the INL mesonet station (already described in the text) on the summit of Big Southern Butte as a measure of the gradient level winds to facilitate the discussion around lines 4-16 on page 16835. We do not report the observed ridgetop high winds (e.g., 12 m/s) during the diurnal regimes as thermally driven winds, but rather point out that these ridgetop locations appear to be decoupled from the diurnally driven flows at other locations on and around the butte and appear to often be more closely coupled with the gradient level winds. Inclusion of the mesonet data from the summit of Big Southern Butte (as described above) will help to demonstrate this point. Flows described in this paper as "upslope" and "downslope" fall within the range of slope flows reported in the literature (e.g., see discussion on p 13-14).

ACPD
14, C7569–C7583, 2014

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.

–Author response –This is also true and it is probably not possible to say for sure which mechanism is at play. Wind speeds would not need to be high in order for convective mixing to play a role. Prevailing gradient-level winds were often from the southwest, which is in alignment with upvalley flow on the snake river plain in the vicinity of the butte. We describe the flows in the “afternoon regime” as developing from convective mixing of gradient-level wind into the growing boundary layer, as described by Banta and Cotton (1982). We argue that this is a reasonable explanation that is supported in other reported studies, although as pointed out, there could potentially be other mechanisms driving this “afternoon regime.” Ultimately, it is the observed surface wind field that we are interested in presenting and we clearly observed a unique “afternoon regime.” We can really only speculate on the forcings which may have set up this afternoon flow field (it is beyond the scope of this study to investigate the larger scale forcings). Convective mixing is one likely mechanism.

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.

–Author response –The term outlier is not used anywhere in the manuscript and we apologize if we implied that NM2 should be considered so. We did not intend to imply or describe NM2 as an outlier. We speculate that this sensor may have been in a zone of recirculation, but we have no reason to suspect that the data are not good. There is no reason to omit the data from this sensor.

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

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–Author response – Our logic for presenting time as local daylight time was that it does not require the reader to do any conversions to estimate solar position at a given time. We prefer to remain with the original time format.

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?

–Author response –Terrain shading is a likely contributor to the local surface flows at both sites, particularly under the diurnal wind regime, however, we did not investigate it in this version of the manuscript. We recommend that it be considered in detail in a future subsequent analysis and manuscript separate from this study that is primarily focused on summarizing the data.

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.

–Author response –One of the goals of the paper is to introduce the larger field campaigns, which included these additional measurements. We would like to describe the methods used and how to access these data, although it is beyond the scope of the paper to provide analyses of these data in this manuscript (the focus here is on the near-surface wind observations). As this is the first paper to stem from this larger field campaign, we feel it is appropriate to describe the full dataset here, although in-depth analyses of some of the data is saved for future work.

10) Presentation: 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.

ACPD
14, C7569–C7583, 2014

–Author response –The overview maps will be enlarged to a full-figure page. Lat/lons are currently provided, but a distance scale will be included. Elevation contours will be considered within the context of overall figure clarity.

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.

–Author response –The color bar will be extended. The figures will be enlarged so that two figures are used at the full extent, rather than a zoomed-in version and the full-extent version. Key locations will be marked. Final formatting to fit the page/text will be handled by the journal.

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

–Author response –Yes, thank you for this observation. The contour intervals vary slightly; this will be fixed in the subsequent version of the manuscript.

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

–Author response –Yes, all subfigures will be labeled in the revised manuscript.

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

–Author response –We have temperature data for one ridgetop location and one valley bottom location for select time periods during the field campaign. We considered looking into these temperature data to determine if there is any information to add to the discussion. However, the partitioning method based solely on time of day and threshold wind speed appears to work well for binning the data into various flow regimes (as evidenced by the vector plots), thus at this point we will not explore the temperature

ACPD

14, C7569–C7583, 2014

data further.

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

–Author response –Thank you for this observation, in response to an earlier comment we included additional discussion of sensor accuracy. We will add some discussion regarding the uncertainty of the anemometers within the context of the reported trends.

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?

–Author response –In the current version of the manuscript, actual measures of gradient level winds are not reported. We described some ridgetop observations as being correlated with gradient level winds when ridgetop observed speeds were much higher than other nearby observed surface speeds during the diurnal flow regime. These are qualitative statements based on the assumption that the gradient level wind speeds are likely higher than the speeds measured by our non-ridgetop surface sensors. In the revised manuscript we will present data from the INL mesonet station at the summit of Big Southern Butte as a measure of the gradient level wind at this site for comparison against our surface observations. We will investigate the sodar and radiosonde data for gradient level winds at the Salmon River Canyon site. For time periods during which we do not have sodar or radiosonde data, we will look at archived mesoscale forecast data as an estimate of the gradient level winds at this site. This proposed presentation of measured gradient level winds will strengthen the discussion, especially on the topic of ridgetop wind decoupling from the rest of the surface flow.

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-drainage" flows to "down-valley" flows - p 16830 l 18: could

ACPD
14, C7569–C7583, 2014

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.

–Author response –Technical corrections: all of these suggestions will be incorporated.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C7569/2014/acpd-14-C7569-2014-supplement.pdf

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

ACPD

14, C7569–C7583, 2014

Interactive Comment