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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Precipitation characteristics and associated weather conditions on the eastern slopes of the Rocky Mountains during March–April 2015" *by* Julie M. Thériault et al.

Anonymous Referee #1

Received and published: 3 May 2018

Overview

The study's authors present a thorough and wide-ranging study of the precipitation characteristics during a filed campaign. The manuscript is well written and organized, and the subject material is interesting. The analyses are detailed and robust. The manuscript will, however, benefit by addressing several key concerns identified by the reviewer. My recommendation is publication once the concerns listed below have been appropriately addressed.

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Discussion paper

Main Concerns

1) The authors state the importance of major summertime floods in the region and that no dedicated atmospheric study of such events have been made. While true, this claim is problematic when framed in the context of this particular study, which concerns low-intensity precipitation events observed during the spring months. My suggestion would be for the authors to frame the context of this work as it pertains to winter and spring precipitation, and how the characteristics of the observed precipitation/hydrometeor might affect i) the snowpack in terms of SWE, and ii) the spring freshet. On a related note, one could easily justify the need for a similar study in June, when heavy precipitation events typically affect this region.

2) The authors correctly point out that the climate in this region is changing. However, they do not discuss how the climate has changed in the spring months (i.e., the time period when the observations were made). Also, no connection is made between the long-term trends in temperature and precipitation and the objectives of this study. If they spring climate has been warming and drying, for example, how does that relate to this particular research initiative? That link needs to be clearly stated and substantiated with references to the literature.

3) The reasoning behind the motivation provided on lines 7 through 9 is not clear to me. Mention of made of the importance of precipitation, trends and future occurrence, but no specific discussion on these points is made in the preceding text. The connection between these aspects and this research needs to be clearly laid out.

4) A major focus of this research is how precipitation differs between westerly and easterly flow regimes. In my opinion, while logical, this classification may be too simple, especially considering the location of the study site and change of wind direction with height. For example, oftentimes winds in this region veer markedly with height, so an upslope flow (i.e., easterly) in the low levels may transition to westerly or southwesterly near the barrier height. The absence of winds from the radiosonde soundings, or dis-

cussion of vertical wind profiles in general, make correctly differentiating between “true” westerly and easterly events problematic. This could have important implications for the interpretation of results and conclusions drawn in this study. I strongly suggest that the authors use either data from the HRDPS or a higher resolution reanalysis product (e.g., NARR) to generate time-height diagrams of winds above the study site. These data, even with their limitations, can be used to draw more nuanced interpretations of how the winds vary between events and how the vertical profiles of winds affect precipitation.

5) Another important consideration is the impact of the local orography on the winds, as this could have important implications on the flow relative to the orography, vertical motions and ultimately precipitation. For example, according to Fig. 1, the study site was located on the western flank of a pronounced ridge on the front range having heights exceeding 2500 m. Whereas an easterly flow would be upslope on the eastern flanks of this ridge, is not clear what happens after the air passes over the ridge-line. It could be that the easterly flow actually results in a localized area of downslope flow on the western flank of the ridge. Similarly, a large-scale westerly flow could become upslope on the western flank of the ridge, resulting in localized vertical lift.

6) Precipitation amountsâĂŤthe manuscript would benefit from a brief discussion of the precipitation amounts observed for the different flow regimes. Also, are heavier amounts typically favoured by a particular flow regime, and if so, why?

7) The manuscript would also benefit from a discussion on the impact of hydrometeor drift, as non-negligible precipitation amounts can arise because of hydrometeor drift from upslope flow on windward slopes.

8) Data from a sonde mounted on a vehicle (i.e., car-sonde) is referred to in the text. Details concerning these data need to be providedâĂŤmaybe as an Appendix? Also, reference in the text is made to data gathered whilst ascending Fortress Mountain; please indicate the route and location of Fortress Mountain in Fig. 1.

Technical Comments

1) Page 2, lines 13-14: Make sure that this pertains to spring or cold season only.

2) Page 2, lines 26-27: The reference to FOPEX seems to be an afterthought. Reasons why the findings from FOPEX are, or are not, relevant to this study need to be clearly substantiated. FOPEX also considered the impact of altitude on precipitation amounts.

3) Page 3, line 7: It is not clear what is meant by "...taken in a systematic manner". Please reword for clarity.

4) Page 3 lines 19 and 29: Given the large instrument suite, please make sure that it is clear what instrument is being spoken to in the text. On line 19 are the authors referring to the Geonor gauge? On line 29, what sondes were used? Vaisala RS92s?

5) Page 4, line 3: Is the mean observed RH of 63% really anomalous compared to the average of 65%? Is the difference even statistically significant?

6) Page 4, line 14: Referring to Figure 2, this figure could be another useful contribution from this paper. Although similar, there are noteworthy shifts in the boundaries for these data compared to those from Matsuo et al. There are data points to warrant shifting the demarcation lines between categories. This would be useful to others.

7) Page 4, lines 15-16: The discussion of the wet-bulb temperature comes out of the blue. The importance and relevance of this parameter needs to be introduced first. What do "low values of relative humidity" translate to in percent?

8) Page 4, line 31: This is not a big issue, but using 250-mb to describe synoptic systems is atypical. Was there any particular reason of using data at this level at not 250-mb? Also, it would also be interesting to look at data from 700-mb as this coincides closely to the height of the mountain barrier.

9) Page 5, line 4: That these synoptic setups corresponded with "all" of the westerly and easterly flow regimes seems too definitive. Unless this can be quantified, maybe

more conservative language is warranted.

10) Page 5, lines 6-8: This may be pedantic, but the others refer to “dry” several times. It would be more appropriate to say “drier” given that we are referring to precipitation events, albeit with unsaturated conditions below cloud base. Please check text throughout. Also, the wording used to describe the impact of the flow regime on the relative humidity makes it sound like conjecture. Maybe rewording would address this, although using in-situ data that substantiate the claims would be best.

11) Page 5, line 8: In Fig. 4b the profile looks near-saturated from just above the surface.

12) Figures 5 and 6: Panels c and d are swapped in the figure caption. No units are provided for the surface winds, and no explanation of what speeds the barbs represent. It is not clear what sensor1, sensor2 and sensor3 representâĂŤplease clarify. Is there a particular reason why accumulation estimates from sensor2 are lower than the other sensors for both events?

13) Section 3.3. It is not obvious to me why the authors chose to plot the spectral width in Figs. 5,6 and not the Doppler-derived vertical motions. Spectral width data are typically used as a proxy for turbulent motion. Sound reasoning needs to be provided for the choice of spectral width data when speaking to vertical motions of hydrometeors. The choice of spectral width is also confusing because on line 16 on page 5, reference is made to “the vertical particle motion”, which suggests that the authors are referring to radial velocity data.

14) Page 6, lines 21-22: Regarding “. . .but also due to more regions of upward motions”. It is not clear on which data this assertion is made. Please clarify and substantiate this comment and also how it possibly explains the wider range of fall velocities.

15) Page 6, line 26: I can’t find the empirical relationship that is provided in Appendix B.

16) Page 6, line 27: Did the authors mean to say “less humid”? And, is reference being made to the difference in conditions near the surface or aloft in the source/growth region?

17) Page 7, lines 20-22: This sentence needs to be improved for clarity. Does the presence of denser (rimed) particles mean that they have higher terminal velocities, so spend less time in the warmer air and are therefore more likely to succeed in reaching the surface? Reference is made to the wet-bulb temperature, but no actual values are provided. Some readers may also not be familiar with how the wet-bulb temperature affects the evolution of particles below cloud base. A short explanation and citation would be helpful both here and set the stage for discussion that includes the wet-bulb temperature in the subsequent section. The last two sentences at the end of section 4.4 could be moved forward and expanded as part of addressing this.

18) Page 8, lines 8-9: No information is provided on how the melting layer and warm layers changed later in the event, or how these changes relate to the observed changes in precipitation type.

19) Page 8, lines 11-12: I do not think one can claim that the simulations demonstrated the impact of dry conditions on precipitation type reaching the surface, because no simulations were undertaken (or at least included in the text) using relatively moist conditions below cloud base.

20) Page 8, line21: Are the authors referring to 5 dBZ representing a substantial difference between the two profiles of vertical velocity? A return of 5 dBZ is equivalent to a very low precipitation rate.

21) Page 9, lines 4-6: This portion of the text reads awkwardly. I suggest rewording and elaborating for clarity. Also, is there a more important take-away message here about the broader implications of these observations?

Editorial Comments

1) Page 1, line 7: Maybe say, “weather balloons were released” instead of “soundings were launched”.

2) Page 1, line 11: Maybe say, “63% showed signs of riming”?

3) Page 1, lines 13-15. Sentence starting with “Radar structure aloft. . .” lacks clarity and reads awkwardly, I’d suggest rewording.

4) Throughout text: Authors use “m/s”. Is this a HESS format? If not, I’d suggest using the conventional ms-1.

5) Page 1, line 17. Suggest replacing “generally” with “relatively”.

6) Page 2, line 9: Remove “nonetheless”.

7) Page 2, lines 23-24: Suggest removing “. . .and they can sometimes lead to major disasters such as the 2013 flooding”. This has already been mentioned twice before

8) Page 3, lines 9-11: Current wording is awkward. Maybe try “Observations of precipitation type, cloud cover, temperature,10 relative humidity, wind speed, wind direction and surface pressure were typically recorded at 10 minute intervals;. . .”

9) Page 4, line 15: Please quantify what is meant by “low values of relative humidity”.

10) Page 4, line 24: Missing parenthesis after “...Area Model”.

11) Page 5, line 2: Wording is not quite right. Specifically, “border northern Alberta”.

12) Page 5, lines 6-8: Suggest rewording, “The westerly flow events were generally associated with drier conditions near the surface (Figure 4a) because of adiabatic heating associated from the downslope flow. The easterly flow events produced. . ..”.

13) Page 5, line 11: Note sure that “systematically” is the most suitable word here.

14) Page 5, line 16: Replace “events” with “event”.

15) Page 5, line 27: Maybe say, “formed in different growth environments”? Are there

any other data that support this claim?

16) Page 5, line32: Suggest removing “...at these temperatures or at higher ones”.

17) Page 6, line 8: Remove “solid” before “precipitation”.

18) Page 6, line 12: Insert “a”. “. . .associated with a varying flow field”.

19) Page 6, line 17: Remove “But”.

20) Page 6, line 20. Suggest replacing “. . .for both types of events” with “. . .for events during both flow patterns,. . .”.

21) Page 7, line 13: Suggest saying, “Due to the relatively dry. . .”

22) Page 7, line 14: Suggest, “. . .been found in this region by Harder and Pomeroy (2013).”

23) Page 7, line 18: Suggest saying, “. . .at which ice particles. . .”

24) Page 8, line1: Include the dates and times in parentheses for the rain/mixed precipitation and for the light snow.

25) Page 8, line 1: Suggest following, “These data are interpolated to over 100. . .”

26) Page 8, line 4: Replace “though” with “through”.

27) Page 8, line 18: Make sure that reference is consistently made to either MRR or MRR2 throughout the text.

28) Page 8, line 19: Remove “region”.

29) Page 9, line 8: Suggest saying, “. . .even under relatively dry surface conditions”.

30) Page 9, line 25: Suggest following punctuation, “. . .was observed, or inferred to occur, with many. . .”.

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