Response to Reviewers:

Spatial and seasonal patterns of near-surface humidity in the foothills of the Canadian Rocky Mountains, 2005-2010

https://doi.org/10.5194/essd-2018-62

Wendy H. Wood, Shawn J. Marshall, and Shannon E. Fargey

We thank the reviewer for the unusually thorough read – there are many constructive suggestions here, which have improved the manuscript. Please see our responses in blue.

Reviewer Two

Review of manuscript essd-2018-62 "Spatial and seasonal patterns of near-surface humidity in the foothills of the Canadian Rocky Mountains, 2005-2010" by Wendy H. Wood, Shawn J. Marshall, and Shannon E. Fargey.

This paper presents a novel data set of humidity measurements from a network of near-surface sensors spanning the foothills of the Canadian Rocky Mountains near the city of Calgary. It is a new and significant data set that will be of use to researchers from a broad range of disciplines. Some interesting aspects of the data set are presented which provide some example of the utility of the data set.
While the data set is very much within the domain of ESSD and this special issue, I do have some major concerns that would need to be addressed before the resulting manuscript and accompanying data set is suitable for publication.

Major comments
It appears that an incorrect formula for calculating vapour pressure from RH may have been used. As most RH sensors report RH with respect to water, this equation should be used to calculate vapour pressure and specific humidity from the measurements. The alternative equation for the saturation vapour pressure of ice can be used to rescale RH to be with respect to ice, but this should only be performed after the calculation of vapour pressure. If the sensor used in this study reports RH with respect to water, then the specific humidity data presented here will be underestimated at low temperatures (i.e. at higher elevations and in the winter). This effect is significant - the saturation vapour pressure of ice is lower by 5% @-5C, 8.5% @-10C, 14% at -15C, 17.5% at -20C. This needs to be rectified and may affect the results presented (e.g. lapse rates of specific humidity).

This is a really interesting and subtle insight. It had been our assumption that the Veriteq instruments we use (a capacitance-based humidity sensor) are sensitive to relative humidity, and were then empirically sensitive to RH based on actual saturation conditions – hence, implicitly sensitive to whether vapour pressure was with respect to ice or liquid water. We therefore distinguished between these (using a threshold of 0°C) in our calculations of vapour pressure. We have been in touch with the manufacturers (Veriteq and now Vaisala), and spoke with a former Veriteq calibration engineer for these sensors (the company no longer exists). He could not answer this question. Based on additional reading on capacitance-based humidity measurements and deferring to the reviewer's understanding, however, we assume that the reviewer is correct. We therefore recalculated the full dataset and uploaded a revised dataset to PANGEAE. All figures, tables, and results have been

revised in the manuscript. As the reviewer suggested, this matters to some extent (first decimal place) in winter months, and has little or no noticeable effect from spring through fall. Hence, the results have not qualitatively changed, but the numbers have slight refinements.

There are many ambiguities in the methods used to quality control and derive the daily averages and gap fill the data. For example, it is unclear which daily averages are used in the gap filling and in the summary results presented. These prevent a confident use of the data set.

We revised and reorganized the discussions of quality control, calculation of daily averages, and gap-filling, isolating these in sections 3, 4 and 5 for clarity (in close accord with the suggestion below). We believe this is clear to the point where others could repeat or duplicate this data processing. Gap-filling uses an IDW approach with 18 nearest neighbours (based on the optimal error in cross-validation), and using daily mean specific humidity from this neighbourhood. We restate this more clearly now, p.9, ll.22-24.

The text needs substantial work to remove ambiguities, avoid repetition or redundant text and provide more discussion of the potential uses of the data. To clarify the method used the construct the data set, the manuscript would be better restructured
1 – Introduction
2 – Study area, sensor network and instrumentation (including sensor accuracy and quality control)
3 – Calculation of daily mean values (methods, results and discussion)
4 – Gap filling (method, results and discussion)
5 – Seasonal and spatial humidity variations and applications
6 – Data availability
6 – Summary

We revised the flow of the document in accord with this suggestion, and hope that it now flows more logically and clearly. Section 2 has been separated into two parts, with sensor accuracy and quality control in section 3. We shortened the document by about 1.5 pages (10%), removing much of the repetition, particularly in the summary. Potential uses of the data were outlined in the introduction, e.g. in hydrological and ecological modelling, where applications require a surface energy balance calculation or an estimate of vapour pressure deficit. Wildfire and habitat models. Meteorological analyses of the dryline. We reiterate this with a short comment in the summary, p.13, ll.4-6, but don't elaborate here in order to minimize redundancy and keep the focus on the dataset itself.

Also, the data set is currently not openly available on the Pangea webservice, even with a login, so I was unable to check the resulting data set.

Apologies, we were unaware of this – we thought it had been activated. This was our error, we neglected to conduct a final check and approve the activation. The (revised) data are now available.

Line comments (page-line)
Title: this should reflect the presentation of a data set, not the results interpreted from these. E.g "Near-surface humidity from a mesonet in the foothills of the Canadian Rocky Mountains, 2005-2010"

This is a fair point, thank you. We have revised the title accordingly.

1-9: “2” should be in superscript here and 1-10.

This is a fair point, thank you. We revised the title accordingly.

1-20: Please add references to support these statements.

While we know this to be true and a scan of publications presenting temperature vs humidity data would bear this out, we don’t actually have a primary reference for this – we deleted this statement.

1-21: It would be worthwhile defining what you mean by 'humidity' here, as the term is used throughout and its current use is ambiguous.

Understood. In this instance, it is used as a general term referring to any measure of humidity in the atmosphere. Different, specific measurements are described in the next sentence. We went through the manuscript and updated terms to the use the precise variable name where a specific measurement is being described. We believe this is no longer ambiguous anywhere. We now state what we mean by “humidity” on p.1, l.28-p.2, l.2.

1-21: Add “in the atmosphere” after “amount of water vapour"

Sentence revised, p.1, l.21; now n/a.

1-22: Please explain the term vapour pressure as you do for other variables. e.g. "The partial pressure of water vapour in air"

Revised as suggested, p.1, l.21.

1-22: Remove ‘the” before mixing ratio and absolute humidity

Revised as suggested, p.1, l.22.

2-1: RH is defined with respect to the saturation vapour pressure of pure water, so differs from the actual maximum water content - see comment on supersaturation below. Please revise. Also, please introduce the difference between RH with respect to ice and water here. See http://glossary.ametsoc.org/wiki/Relative_humidity
2-1: “Once the air is saturated (RH = 100%), any additional vapour will condense out.” This is not strictly correct, as in certain circumstances air can become super-saturated. Please revise. See http://glossary.ametsoc.org/wiki/Supersaturation

Revised discussion of RH and saturation, as suggested, p.1, ll.24-27. In light of the assumption that measured/recorded RH values are with respect to saturation conditions over water (see above), we now only calculate saturation vapour pressure with respect to water (Eq. 1), and don’t discuss the lower saturation vapour pressure that exists over ice.

2-7: “In operational…” this sentence would be better place alongside the discussion of the spatial variations in the dry line a few paragraphs down.

Relocated as suggested, p.2, l.33.

2-11: remove comma between “data” and “for”

Revised as suggested, p.2, l.8.

2-11: change ‘in’ to ‘of’

Revised as suggested, p.2, l.9.

2-14: This paragraph should be part of the previous paragraph as it is not about human comfort, but how humidity variables are used.

Revised as suggested, p.2, l.9.

2-24: “water vapour in the air”. Water vapour in the atmosphere would be a more appropriate term here.

Revised as suggested, p.2, l.20.

2-25: Please explain the term “rainout” or replace with a description

Replaced with “precipitation”, p.2, l.21.

2-26: “humidity” do you mean RH or water vapour? please be consistent.

We mean both (all humidity measures, as per our definition above). Revised on p.2, l.24 though, where we were not specific enough.

2-31: “while RH does the opposite” you don't explain why RH does the opposite. Please explain.

Now explained, p.2, l.29.

3-2: Please use “dry line” and not “dryline” throughout, as this is how you introduce the term.

Revised to dryline throughout the manuscript.

3-6: “Other systematic spatial humidity structures can be expected as a function of elevation, aspect, surface type, and air mass/frontal interactions.” please give more context for why we would expect these?

We removed this sentence as this is now discussed in Section 6, with proper context.

3-11: “humidity” do you mean RH or specific humidity?

Relative - revised, p.3, l.8.

3-11: Please reword to “…2010. The dataset of temperature is described…”

Revised to “the temperature data are described”, p.3, l.8.

3-15: Please use a full stop between the description of each section.

Revised as suggested, p.3, ll.14-18.

3-21: Please change section title to “Study area, sensor network and instrumentation”

Revised as suggested, p.3, l.20.

4-7: “Temperature and relative humidity…” This sentence is confusing - do you mean " Instantaneous measurements of T and RH were recorded every hour using SP-2000..." please revise.

Revised as suggested, p.4, l.6.

4-15: “reliable data”. please point readers to the quality control section below.

Pointer to section 3.2 as suggested, p.4, l.13.

4-17: “pole extensions were added”, please clarify if they were added and removed each season or if the sensors were just permanently mounted higher.

The pole extensions were in place year-round, as we visited only once per year. Clarified as suggested, p.4, l.16.

4-25: please change section title to “Calculation of vapour pressure and specific humidity and daily means” or similar

Revised to ‘Water Vapour Calculations’ and organized into two new sub-sections following the QC discussion (section 3), p.6, l.8.

4-27: “actual humidity”, please avoid using this term as you don’t define it and is ambiguous. Do you mean the “amount of water vapour in the atmosphere”?

That is what we meant, but we now avoid this throughout and refer to this as water vapour.

4-27: “We take different approaches to this calculation.” please explain what you mean here - i.e. you take two different approaches compare them.

Rewritten for clarity, and differences clearly discussed, p.7.

4-29: please explain why the pressure dependency of the saturation vapour pressure was omitted?

Is there a pressure dependency on saturation vapour pressure, at ambient atmospheric pressures? Antoine’s equations, WMO equations, etc. all give approximate curve-fits to the Clausius-Clapeyron equation to parameterize saturation vapour pressure e_s purely as a function of temperature, i.e. the energy of molecules in the air parcel. Unless there are interactions with other atmospheric gases (i.e., under highly pressurized situations), it is our understanding that the balance between condensation and evaporation is only a function of temperature. See, e.g., Tsonis (2002, p.83). Or do you mean something else?

5-1: does the instrument report RH with respect to ice? Most instruments report RH with respect to water, so that vapour pressure should be calculated using 1a. 1b can be used to transform the sensor RH to RH with respect to ice after the calculation of vapour pressure. Please address this.

Please see the discussion above (p.1); we have recalculated all of the data using Eq. (1a) (now Eq. 1) and report RH only with respect to saturation over water.

5-5: this is the equation for mixing ratio. Please explain that it is used here to approximate specific humidity.

Thank you, careless of us: clarified and justified, p.6, l.15-17.

5-17: please describe the approximate error you expect from using equation 3 vs actual pressure measurements.

Good question, as these pressure estimates can have some error. Comparison to *in situ* air pressure data from a higher-elevation site in our study region (2350 m altitude) indicates that our estimated air pressures can be off by up to 1 kPa in some weather systems. For cases with e_v = 1 hPa and 10 hPa (representative of winter and summer values in our study region), at a location with P = 800 hPa (typical of our upper sites where pressure errors will be greatest), an error of 10 hPa means a difference of 0.1% in each case. Specifically, for P = 800 hPa, e_v = 1 and 10 hPa give q_v = 0.7785 and 7.873 g/kg. If P = 810 hPa, the associated values are q_v = 0.7775 and 7.863 g/kg. Hence there are differences in the second or decimal, but these are negligible relative to our estimated confidence of ±7% in specific humidity. We make a brief comment on this, p.6, ll.29-31.

5-25: please avoid using "actual humidity"- see earlier comment

Removed as part of the rewrite of this section. See first paragraph on p.7.

5-26: "For applications…" this sentence is very confusing. Please reorder this section by, 1. defining the equations used to calculate vapour pressure and specific humidity 2. defining (and preferably naming) the two methods used to calculate daily means of each variable 3. describing which will be most appropriate for different applications.

Rewritten, and more clear now. These topics are split into sections 4.1 and 4.2, and the discussion of (sample) applications is rewritten and limited to p.7, ll.26-33.

6-3: please briefly explain how instruments were deployed and calibrated i.e. were different instruments used at each site, how often were instruments changed, were instruments pre and post calibrated? Were any adjustments to the raw data made?

We added a couple of sentences to clarify these points. Instruments were calibrated before deployment, on occasions where they were swapped out during the study, and after being taken down; p.4, ll24-26. We made no adjustments to the raw data, noted on p.5, ll.8-9.

6-12: "RH difference". do you mean the mean bias difference? or mean absolute difference? over what time period (i.e. hourly or daily). Please clarify.

We mean the mean absolute *RH* difference over the calibration study (one to two weeks). Rewritten, p.5, ll.4-5.

6-12: '20%" do you mean 20% RH or 20% of the mean RH value (i.e. 50% RH +/- 10% RH)?

See the explanation above. No more like 50% vs. 70%, for the actual *RH* values.

6-15: "read lower" by how much, and what do you think the cause of this is?

By 1-2%, but this sentence has been removed as we do not have an explanation for it; not all sensors were tested every year or across seasons, making it difficult to assess seasonal differences rigorously.

6-16: "biases of several %" your method would indicate that individual bias can be as large as 20%- please revise.

Sensors with the biases of 20% (or more) were removed from the analysis, and we resort to gap-filled data for these sites. We have revised this statement on p.5, l.8 to reflect the mean absolute errors of the sensors that we retain: typical a few %, with a maximum MAE of almost 10%.

6-17: "although this may depend on the season of the sensor validation tests" please expand on what you mean by this?

Removed as per comment 6-15.

6-17: "Uncertainties in RH are estimated" is this based on the standard deviation of the differences between WRS and Veriteq instruments?

Yes, that is what we meant. Clarified, p.5, l.10.

6-20: "we estimate an accuracy of ±7% in mean daily ev and qv." but this is assuming that the WRS RH is a true representation of the RH and the actual uncertainty is higher. Please revise.

Yes, that is a fair point – revised as suggested, p.5, l.14.

6-24: "relative humidity" please be consistent in the use of either "RH" or "relative humidity" throughout the manuscript.

Revised as suggested throughout. Where it begins a sentence we use proper english; otherwise we now use RH in most instances, with the exception of the abstract and the table captions.

6-31: "exclude days" one presumes this is indicative of sensor malfunction, but please state the reason here.

Correct, this is a form of sensor malfunction; sentence added to clarify this, p.5, ll.24-25.

7-10: change to "control steps result in"

Revised, p.6, l.2.

7-15: "89% of site-days with valid data," do you mean that 89% of days have complete data (i.e. 24 valid measurements)? Please clarify.

Yes, that is essentially it. 232 sites over 1826 days gives 423,362 site-days. Of these, 89% have complete data. Rewritten for clarity, p.6, ll.3-4.

8-1: change "is" to "was".

Revised as suggested, p.8, l.20.

8-22: section "4.3.1 Kriging" is not needed as the results are not presented.

Kriging is considered the more sophisticated interpolation method, so we retain mention that we tested this – otherwise it might be expected or recommended. We found that it did not perform better for our data. We briefly discuss (but don't plot) the results, p.9, l.13. This is a good example of a null result, something that needed to be tested, and it is worth a brief explanation of our methods and results in case this is helpful to others doing similar work. We minimize this though; kriging no longer has its own subsection.

8-32: you do not present the kriging results so this sentence need revised.

Per above, results are not presented in detail for the different parameters and variables tested for either IDW or kriging, but we think that it is worth briefly describing the methods we explored and the core results (i.e., the best model).

8-33: please give the range of parameter values you tested for the IDW.

This was given in the paragraph beginning on p.8, l.15. Now at p.8, l.25.

9-3: "<ean daily….saturates at 100%" This paragraph should be contained in section 3.

Rewritten and moved as suggested, to top of section 4.2, p.7, l.3.

9-7: "all site-days" is this just days with good data or does it also include the gap filled data?

Sentence added to indicate that this is for good data only, p.7, l.5. Hourly data are required for the second method used to calculate daily means; gap-filling was only done for daily means.

9-9: please move the definition of delta q to the start of the paragraph.

Moved as suggested, p.7, l.13.

9-11: please change to "than that during"

Revised as suggested, p.7, l.7.

9-13: "average daily specific humidity calculated from method (ii)" please use the terms you define and refer to qvh

Revised as suggested, p.7, l.18.

9-13: "generally positive" the figure only indicates this is true for temperatures greater than -5C. please revise.

Revised to say greater than 0°C, p.7, l.19.

9-30: "jackknife" please the term you introduce earlier "leave-one-out"

Revised, now stick with "leave-one-out" cross-validation, p.8, l.19.

9-31: "tested difference interpolation methods" please make a comment on how the kriging results compared or exclude these from your methods.

Per comments 8-22 and 8-32 above, we retain a brief summary that the kriging results were not as strong (higher average MAE) than our optimal IDW results. We do clarify the basis for model evaluation in this paragraph, p.8, ll.18-23.

9-32: "qv" is this qvd or qvh? you discuss the differences and the need for thermodynamic constrain on one but not the other, so it is an important distinction.

We mean q_{vd} here, as these are daily interpolations. Clarified, p.8, ll.15-16.

10-7: “Relative (percentage) error” please clarify you mean in qv, not RH here

We mean q_v here – now stated, p.9, ll.30-31.

10-8: it would be worth adding another line to table 1 showing the statistics for qv as a percentage

Added to Table 1 as suggested.

10-9: “Interpolation errors in RH” what statistic is used here?

Mean absolute errors again, now noted, p.9, l.32.

10-12: “7% of the mean annual values” why don't you list the average percentage error listed above?

This discussion has been removed, as it was redundant – now just cited above and in Table 1.

10-13: “, respectively” do you mean in summer and winter or for qv and RH?

This discussion has been removed, as it was redundant.

10-16: “actual humidity” please avoid this ambiguous term

Revised, now say “vapour pressure”, p.9, l.16.

10-17: please add “from the leave-one-out validation” after “RH values”.

Revised, new sentence added to explain this, p.9, ll.18-19.

10-19: please replace both instances of “values” with “distributions”

Revised as suggested, p.9, l.20.

10-19: “RH values differ” is this due to the 100% limit imposed during the gap filling or due to the differences in thermodynamic constraint in daily vs hourly data? Please discuss.

We cannot tell – both of these influences likely come in. The statistics don’t enable us to infer causality. Briefly discussed on p.9, ll.22-23.

10-19: “These differences” please describe the specific differences in the figure you are referring to.

Referring to the differences in distribution, in general, in particular the higher mode of 100% in the interpolate data. Briefly noted, p.9, l.21.

10-20: “This is the approach” it is still not clear how data were gap-filled. Are days with missing station data filled with daily mean qvh or qvd? Is qv this is transformed to ev and further transformed into RH using thermodynamic constraints. If qvh is used does this mean that the filled values in the qvh dataset will be biased with respect to the actual data values (keeping in mind figure 2)?

Apologies that this was not clear. Revised for clarity, p.9, ll.23-24. It is q_{vd} that is used for the IDW interpolations, as we are working with daily means of T and q_v for the interpolations and prefer the thermodynamic consistency of daily mean values of T, RH, qv and ev.

10-21: “qv” again, you need to clarify if this is qvh or qvd

Based on q_{vd} again. Clarified, p.9, l.25.

10-24: what ev and qv are used in Table 2 - qvh or qvd? Please clarify in the text and the table caption

This uses the gap-filled data (q_{vd}) to give means over all site-days – now noted, p.10, l.3.

11-2: please add "(Table 2)" after "in the summer"

Revised as suggested, p.10, l.15.

11-7: "other three-seasons" there is a lot of spatial structure in the mountains during most seasons that it would be worth mentioning.

Noted as suggested, p.10, l.23.

11-11: "running eastwards along FCA lines 3 and 4, where line 1 is the northernmost line (see Figure 1)." This description is rather awkward - it would be much clearer if the figures could be annotated with the bow valley and the approximate transition from the mountains to prairie sites.

Revised for clarity; the Bow River valley is apparent in Figs 1b and 1c, and the cities of Banff and Calgary are labelled (the river runs through them), so we now reference with respect to these sites in Figure 1, p.10, ll.25-26.

11-17: "is a significant relationship from November through January" the R2 for this relationship must be very low - please list the R2 for each month and the annual value in the table and consider revising this statement.

By significant we mean statistically significant, per conventional regression statistics, at the 99% confidence level ($p < 0.01$), as indicated in the caption for Table 2. We inserted the word "statistically" to clarify this p.11, l.5. Because it shows up as statistically significant, there are objective ground to retain this conclusion. That said, the relationship is indeed weak – R2 values can be less than 0.1. Noted in the text and included in Table 2, as suggested, p.11, ll.6-9.

11-29: "8.5% km^{-1}" is this the average of the two months? why not just list each month? It gets confusing when different time periods are introduced in this section.

Yes the average of two months – it seems more compact than listing each month separately; monthly values are in Table 2, so rather than repeat this, we try to synthesize a bit here. Inserted "average here" and we have rewritten for clarity. And given the comment above and the weak R^2, we no longer explicitly discuss the winter months. We hope this is more straightforward now, p.11, ll.6-12.

11-31: "both specific and relative humidity" from what I can tell, the relationship with elevation for RH must only explain a small amount of the relationship (less than 40%), so this result is tenuous. Please revise.

We go back to statistical significance here. Perhaps it is due to the relatively large dataset, but we commonly get statistically significant (at 99% confidence) regression lines that have low R^2 values. So there are two different things here: the reviewer is correct that elevation explains only a small part of the variance for *RH* (16% for these summer months, compared with 89% for *qv*), but there is still a significant linear trend with elevation. There is a lot of variability overlying a general decline with elevation. We would argue that this is not tenuous, objectively, but perhaps would be if one wished to develop a predictive model for spatial variations in RH. That is not our goal here; rather, just to

report whether there are statistically significant relations with elevation or not. We discuss this a little further now, p.11, ll.6-8 and retain a modified discussion of the summer lapse rates, p.11, ll.11-13.

11-33: “Figure 5b also support this” please define what you mean by 'this'. The increase in qv with longitude can be explained by increasing temperature, so doesn't necessarily support decreased RH at higher elevations. The mechanisms behind the observed differences between April-May and July-August would be worth investigating further but need further analysis that is beyond the scope of ESSD. Please revise this section.

Revised – no more ‘this’. We agree that the qv trend can have two components: higher temperatures at low elevations and moisture sources at low elevations. We discuss both, p.11, ll.11-17.

12-4: This paragraph (and figure 6) would be better placed at the start of the section, to give context for spatial and elevation patterns presented.

Revised as suggested, now Figure 4, discussed on p.10, ll.5-7.

12-11: it would be more intuitive for the reader if the order of these examples aligned with the order in figure 7.

Good point, re-ordered as suggested, p.11, ll.20-25.

12-24: “high humidity levels” but this is just for RH, indicating that temperature is warmer but that the air mass has a similar water vapour content. Please revise.

Revised for clarity, p.12, ll.1-2.

12-25: “Daily mean …” this sentence seems very out of context is not needed.

Deleted as suggested.

12-30: “inversion” you don't explain this term or present data to show elevation gradients, so this sentence is confusing. Please remove the term and revise.

Apologies, we had considered inversion to be well-understood in a meteorological context, and the change in sign vs. elevation is apparent in Figure 7, without giving the gradients. We have revised this sentence for clarity, p.12, ll.16-7.

13-4: “common structure” common with what? Please revise.

Reworded and no longer compared with the system above, for brevity, p.12, l.l114-15.

13-26: “estimated (interpolated)” it is still unclear if the interpolated data are based on qvh or qvd?

q_{vd}, as per points 10-20, 10-21 and explained on p.11. We don’t repeat it here, to minimize redundancy.

14-2: “complicated” “complex” is a more common term to describe terrain – consider changing.

Revised as suggested, p.12, l.31.

15-2: “15 sites” this differs from the 18 sites mentioned in the methods. Please revise.

Amended, it is 18 – now consistent, although n/a here as we have trimmed the summary.

15-6: “High summer humidity … particularly in the spring and autumn.” These sentences are more speculative and should be more removed from the summary.

Removed from here as suggested.

15-10: “also creates a general increase in RH with altitude, with an average gradient of +3% km−1.” but this result explains only a small fraction of the variation, so cannot be considered robust result. please revise.

Paragraph revised, p.13, ll.21-28, but we keep this result as it is statistically significant. See the response to points 11-17 and 11-31. We do now note that elevation explains only a small portion of the variance in *RH*, but there are valid and interesting seasonal variations. One would not have a skillful spatial model of RH variations using only elevation as a predictor variable, but if estimating or extrapolating RH values in the mountains from low-elevation station data, one would do better to adopt an RH lapse rates (save in winter) than to assume no elevational relationship.

15-13: This last paragraph belongs in the discussion section.

We removed most of this and kept only the summary numbers, extracting any other speculations or discussion.

Table 1: please explain how the standard error was derived in section 4.

This is just the usual standard error – the standard deviation in the distribution of errors for all estimated sites (using the leave-one-out cross-validation). Briefly explained in the Table caption.

Table 2: please place the standard deviations in parentheses as the use of +/- is not appropriate.

Revised as suggested.

Table 2: $R_2 > 0.4$ seems like a very low benchmark. It would be better to list the R_2 values in parentheses after the lapse rates to indicate the strength of the relationship in each month.

Revised as suggested.

Figure 1: please explain what the different symbols (dots or crosses, grey and black) mean?

Revised as suggested to explain the dots and crosses (prairie and mountain sites, as defined in the text).

Figure 2: what is the red line in these plots? if it is a best fit line, please describe in the text and provide some statistics.

The line has been removed for clarity.

Figure 2: what is the justification for the single vertical line at 10C? either add regularly spaced vertical grids or remove

The line has been removed for clarity.

Figure 2: this figure would be better suited to a kernel density plot or to having smaller circles to allow the reader to see how frequently the points overlap. it is currently hard to tell.

Retained as is, as we believe that it effectively makes the point: the increasing discrepancy (Δq_v) at greater temperatures and diurnal temperature ranges.

Figure 2: The caption needs reworded to “Relation between Δqv and (a, c) temperature and (b, d) daily temperature range. Δqv is the difference in specific humidity calculated from daily mean T and RH (qvd) and from hourly data (qvh): Δqv = qvd − qvh. The top panels are actual differences, in g kg−1, and the lower panels are normalized by mean daily qvx, expressed as the % difference.” Also the caption needs to clarify is they are normalised by qvh or qvd

Reworded as suggested.

Figure 3: please clarify the “interpolated” data interpolated using the leave-one-out method. Also, the last sentence - “Specific humidity distributions are statistically equivalent, but the RH distributions differ” - should be in the main body text, not the caption.

Revised as suggested to explain the origin of the interpolated data and to move the discussion of distributions to the main text.

Figure 4: the order of these panels is confusing. Please place in consecutive order, perhaps starting with winter as this is the first season discussed in the text.
Also, the colour scale is confusing and would ideally be consistent between each panel. Consider using a perceptually uniform colour palette such as 'parula' in MATLAB or 'virdis' in Python

Now Figure 5. Reordered as suggested and we have applied a different colour scale here.

Figure 6: It would be much more useful to plot the seasonal mean (as in figure 5) as well as plot the saturation vapour pressure and specific humidity curves on panel a to give context for the results in figure 5

Now Figure 4. We have kept this with the annual means in this context, as an introduction to the data and its relation with temperature. Seasonal plots are interesting but make the same point while adding extra content and discussion that might be distracting to this point.

Figure 7: it would be much better for the labels in each panel to describe the conditions rather than the date. e.g. 'summer cyclonic', 'summer convective ', 'winter polar airmass', 'winter chinook',
Also, the colour scale is confusing and would ideally be consistent between each panel. Consider using a perceptually uniform colour palette such as 'parula' in MATLAB or 'virdis' in

Caption revised and colour palette revised. Now gives the weather type (as well as date).