



Interactive comment on “Spatio-temporal variability of snow water equivalent in the extra-tropical Andes cordillera from a distributed energy balance modeling and remotely sensed snow cover” by E. Cornwell et al.

E. Cornwell et al.

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We are grateful for the insightful and constructive comments issued by the Dr. Parajka and one anonymous reviewer. Below we list replies addressing these reviews.

Reviewer #1 (Dr. Parajka)

General comments: we agree with the reviewer, and in the revised version of the manuscript will re-structure the discussion section in order to provide further insights

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on model performance. The model domain, which includes a majority of un-forested areas and displays a strong seasonality in precipitation, provides unique opportunities for assessing sources of error in isolation, including conceptual model and input data errors.

Specific comments:

Comment Response 1 How are the results sensitive to the selection of sub-regions?

Response: We thank the reviewer for an interesting question. The main impact of sub-region delineation involves the selection of index meteorological stations for extrapolating input data at the domain level. Thus, for example, two adjacent pixels that are part of different sub-regions may be assigned input data derived from two different meteorological stations that are many kilometers apart. It would be preferable to use distributed inputs only, but these are not always readily available for this domain and we opted for focusing on other aspects of the modeling application. In the revised text, we will add a more thorough treatment of this topic in the discussion section.

2 Approach to test and justify the regional consistency of river flow data is not clear.

Response: We pre-selected river flow data leaving out series that showed too many blanks and verified regional consistency through the double mass curve method. We will provide a clearer explanation in the revised manuscript.

3 Results: I would strongly suggest to show some time series (i.e. snow pillow/survey data vs. model simulations).

Response: We agree with the reviewer that a time-series comparison with snow pillow data would be useful. We will present such data in the revised version, either on the main manuscript or in the online supplementary material.

4 Please consider to elaborate more on why? is the model over/underestimating snow pillow and snow survey data?

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Response: Yes. We will add more discussion on the reasons for model performance in the revised Discussion section. Given the scope of our research, we expect to provide informed hypothesis on the factors more likely to affect model performance, and suggest future work in order to test these hypotheses.

5 p.8947, l.1-23: Does this part refer to model validation (as the title indicates)?

Response: In our opinion, comparison with river flows should not be thought of as model validation, because of the many other hydrological processes that interact with SWE accumulation in order to yield river flow. On the other hand, comparison with river flows is valuable in order to evaluate the predictive power of distributed SWE estimated relative to, for example, snow-pillow data. In the revised manuscript we could consider moving this analysis to the “results” section.

6 p.8947, l.24: September 15? not 1?

Response: Our preliminary analyses showed that peak SWE at snow pillow sites is reached on average on September 1st for the western sites (Chile) and on Oct 1st on the eastern sites (Argentina). For the sake of simplicity, we decided to report estimates for one date only, and adopted the September 15th time stamp accordingly. The caption in figure 7 should state so, and will be corrected in the revised version.

7 regional SWE estimates - how do the values above 1500mm represent reality? the comparison on Figure 5 indicates that some model estimates strongly overestimate observations for larger peak SWE.

Response: Based on the snow pillow data comparison, we believe that most severe overestimation occurs at the northern sub-region in Chile (C1). Here, above-1500 mm estimates may not represent reality. In this region, sublimation may represent a significant portion of the annual mass balance, and model performance should be affected by the fact that the simplified EB calculation does not account for this energy loss. For other sub-regions we believe it is possible to observe such values of end-

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of-winter accumulation at the 500-m pixel resolution. We will provide a more thorough discussion of this issue in the revised manuscript.

8 Please consider to move the comparison of results with the literature (SWE reconstruction in other parts of the world) from the conclusions to discussion and to elaborate more about the similarities and differences of the findings. What can be learned from the current results?

Response: Yes, we will improve the discussion section following these suggestions.

9 Fig.2: caption - hidro-climatology

Response: Will correct in the revised version.

10 Fig.5: Plots are very small. Please consider to use 3x4 panels arrangement. Why are the units in m? Please consider to make them consistent with other figures.

Response: We used a panel organization that attempted to replicate the geographical location of the sub-regions. We improved these figures by enlarging the panels and adding a sketch of the model domain, in order to relate each panel with the corresponding location. These improved figures will be included in the revised manuscript. Please see the figure at the end of this file for a sample of the new proposed figure format.

11 Fig. 6.: Again, plots are too small, please consider some other arrangement to make the message out of this figure more clear and attractive.

Response: Please see reply above.

12 Fig.9, 10: Please add x labels. What is the meaning of (a), (b),...(h)?

Response: We will improve this figure in the revised manuscript. Letter indices refer to each sub-region, and we will modify the graphics in order to make this clearer.

Reviewer 2

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General comments: these comments complement those made by reviewer 1, and we will improve our discussion of the results and implications in the revised manuscript. In particular, we will further elaborate on the predictive power of the distributed SWE estimation relative to site-based snow pillow observations, with respect to river flow forecasting/estimation.

Specific comments:

Comment Response 1 - Line 1 page 8929: what do you mean with “precursor”?

Response: We wanted to mean “source”. We will revise the wording in the updated manuscript.

2 - Line 5 – 8 page 8929: may be useful including the temporal resolution of these estimates;

Response: We agree. We will include the temporal resolution in the revised manuscript.

3 - Line 20 – 21 page 8930, lines 20 – 25 page 8934, Eq. 4 and 5: independence from precipitation data is a key point of this approach. In this perspective, the paper would benefit from a wider discussion on this point. Including precipitation in the simulation of SWE is conceptually easy, as it represents a model input. On the other hand, I understand that precipitation data in mountains region are usually sparse and noisy. However, it would be interesting to mention existing (or available) data, their quality and completeness, and reasons why existing strategies to correct errors in precipitation data were not considered. This may be done here, or in a specific paragraph in the Discussion;

Response: We agree with this comment. Our model domain is particularly scarce in precipitation data. Although some reanalysis products do exist, these are usually strongly biased and their spatial resolution is not readily amenable to that appropriate for hydrological applications. An added value of this type of reconstruction is that it may

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be used as an independent validation dataset for precipitation analysis in this region. In the revised version of the manuscript, we will add a paragraph regarding the strengths of our approach in the context of precipitation uncertainty wherein we will cite other works which have estimated precipitation in the region.

4 - Line 7 – 8 page 8931: are you able to quantify SWE significance in the region?

Response: All rivers in this region show regimes typical of snow/glacier dominated hydrology (high spring and summer flows). For some glacier-free basins, quantification is relatively straightforward. For basins with glacier presence, the distinction between seasonal SWE and glacier ice contribution to river flow is more complicated, although it has been attempted in the past. We will provide some examples of these estimations, obtained from the literature, in the revised manuscript.

5 - Line 9 – 17 page 8931: this part may be enlarged in the discussion. Please define MAE;

Response: Yes. We will expand on the significance of the statistics on the revised manuscript, and define all terms.

6 - Figure 1: in this Figure, clusters C1 to C8 are reported, but their determination is explained later in the text. This should be specified in a better way to avoid confusion. A representation of the topography of this area would be useful.

Response: We will improve the consistency of text and figures in the revised manuscript. Although Figure 1 does show a topographic map, we will include hypsographic curves in the supplementary online material for the revised manuscript.

7 - Line 6 page 8933: a reference here would be useful ;

Response: Agree. We will add in the revised manuscript.

8 - Line 11 – 15 page 8933: this statement is not clear to me, please consider rephrasing;

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Response: Yes we can do a better job in explaining ourselves. Basically we wanted to state that the somewhat gentler topography and relative position of streamgages with respect to the water divide in the eastern slope results in larger (in area) watersheds.

9 - Line 11 page 8933 and line 12 page 8934: more details on the reasons why these areas are snow-dominated would help here;

Response: Figure 2 includes the temperature; precipitation and streamflow climatologies for the study region, where the distinct seasonality patterns associated with snow-dominated regimes can be seen. We will strive to provide a better explanation of this figure in the revised manuscript.

10 - Line 22 page 8934: you may consider including Figure S4 in the paper;

Response: We appreciate the suggestion. We considered very carefully which figures to include in the supplementary material, and wanted to keep the story in the main manuscript straightforward in terms of the graphical support, in order not to lose focus. We believe that although figure S4 is relevant, a mention to its main message in the text (about the date of peak SWE) should be sufficient for the purposes of this manuscript.

11 - Line 1 – 6 page 8935: please define fSCA here, as it is the first point where it is introduced. “Under certain conditions” should be better specified;

Response: We agree with this comment. The phrase “under certain conditions” mainly refers to snow regimes with distinct snow accumulation and snow ablation seasons; the Central Andes exemplify this type of system given that very little precipitation occurs during the spring snowmelt period and summer months. We will clarify this in the revised manuscript.

12 - Eq 1: please define M_p here;

Response: Will correct in the revised manuscript

- Line 6 page 8936: is 15 August a fixed date, irrespective from year climatology?

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Response: We chose August 15 as a date before which little melt can be expected, and applied it to all years in the simulation. This is a conservative assumption based on our experience in the region and given the radiation, temperature and precipitation climatology.

13 - Sections 3.3 and 3.4: authors may consider merging these two Sections. I think this would help their readability. Figure S3 would be appropriate in the main text as well;

Response: We will consider merging sections 3.3 and 3.4. We agree in that figure S3 could be a welcome addition to the main manuscript, although it refers to an intermediate step in the process of data extrapolation.

14 - Line 1 page 8940: is it Eq. 7?

Response: Indeed.

15 - Lines 22 – 23 page 8941: reporting existing estimations of this parameter for this area, if known, may help here;

Response: Actually we meant that few systematic observations of the variables required to estimate this parameter are available. To our best knowledge, no estimations of this parameter have been published for this domain.

16 - Line 7 page 8944: how many measurements were performed within each MODIS pixel?

Response: On average, 120 measurements spaced at 50 m were obtained within each MODIS pixel. We will report this value in the revised manuscript.

17 - Figure 3 and 4, Section 4.1: including a wide set of point measurements of SWE is very interesting, as it shows how comparing gridded estimations of SWE with point data is difficult. I think it would be probably more effective to focus on this discussion and on reasons why SWE predictions are overestimated or underestimated rather than

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on a detailed list of numerical results. Authors may also investigate a possible link between underestimations and the absence of a precipitation input;

Response: We agree. In the revised manuscript we will expand the discussion section in order to better comment the reasons for variations in model performance. Please see replies to reviewer #1.

18 - Line 24 page 8947: is it September 15 or 1?

Response: It is Sep 15. We will revise all figure captions and descriptions in the revised text.

19 - Line 13 page 8949: I guess it is SWE and not swe;

Response: Yes.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/12/C4716/2015/hessd-12-C4716-2015-supplement.pdf>

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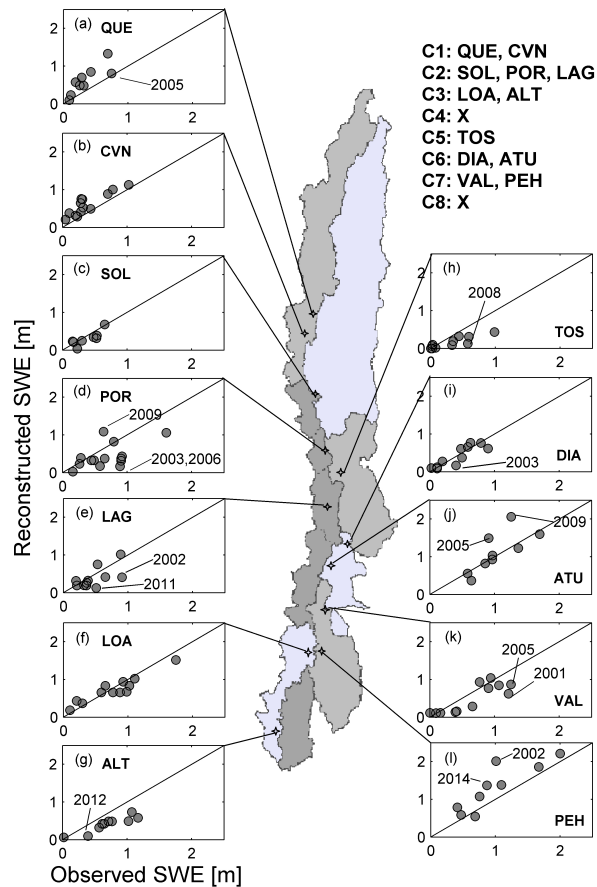


Fig. 1.