

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.

Anonymous Referee #2

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Cornwell et al. (2015) present a distributed evaluation of SWE over the extra-tropical Andes cordillera in Chile and Argentina during the period 2001 - 2014. SWE estimations are obtained retrospectively by using an energy balance model to compute potential melt and MODIS-based fractional snow covered areas to convert potential melt to actual melt. A daily temporal resolution is used and peak SWE is obtained by cumulating daily melt until a reference date. No precipitation input is considered. As a result, the depletion period of the snowpack is reconstructed. Model evaluation is performed by considering both snow pillows data and intensive manual snow sur-

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veys. Results show that modeled and observed peak SWE have an overall coefficient of determination R^2 of 0.61 (values between 0.32 and 0.88). Results variability is explored as a function of elevation, sub-region considered and the position relative to the continental divide. A sensitivity analysis is also performed.

Overall, the topic of this paper is motivated by a clear practical issue. This is the reported lack of distributed estimations of SWE at a high spatial and temporal resolution in an area representing an important source of freshwater for South America. This estimation represents a clear point of originality for this paper. The approach is sound, as it is the evaluation using both automatic and manual data. Authors may consider elaborating on results presentation and discussion in order to highlight the implications of these results in a more effective way. In my opinion, the proposed presentation of results and discussion is very detailed in describing many numerical results that have been already reported in Figures. On the other hand, in my opinion the paper would be improved by including 1) a wider discussion on results implications for final users at the local and regional scale; 2) a comparison with previous estimations of SWE in the same area and 3) a comparison with similar estimations in other areas of the World (see lines 9 – 17 page 8931).

SPECIFIC OR TECHNICAL COMMENTS

- Line 1 page 8929: what do you mean with “precursor”?
- Line 5 – 8 page 8929: may be useful including the temporal resolution of these estimates;
- 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

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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;

- Line 7 – 8 page 8931: are you able to quantify SWE significance in the region?
- Line 9 – 17 page 8931: this part may be enlarged in the discussion. Please define MAE;
- 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.
- Line 6 page 8933: a reference here would be useful;
- Line 11 – 15 page 8933: this statement is not clear to me, please consider rephrasing;
- Line 11 page 8933 and line 12 page 8934: more details on the reasons why these areas are snow-dominated would help here;
- Line 22 page 8934: you may consider including Figure S4 in the paper;
- 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;
- Eq 1: please define M_p here;
- Line 6 page 8936: is 15 August a fixed date, irrespective from year climatology?
- 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;
- Line 1 page 8940: is it Eq. 7?
- Lines 22 – 23 page 8941: reporting existing estimations of this parameter for this area, if known, may help here;

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- Line 7 page 8944: how many measurements were performed within each MODIS pixel?
- 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 on a detailed list of numerical results. Authors may also investigate a possible link between underestimations and the absence of a precipitation input;
- Line 24 page 8947: is it September 15 or 1?
- Line 13 page 8949: I guess it is SWE and not swe;

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