Hydrol. Earth Syst. Sci. Discuss., 8, C66–C73, 2011
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Hydrology and Earth System Sciences Discussions

Interactive comment on "Modelling the hydrologic role of glaciers within a Water Evaluation and Planning System (WEAP): a case study in the Rio Santa watershed (Peru)" *by* T. Condom et al.

Anonymous Referee #1

Received and published: 7 February 2011

General comments

Condom et al. present the implementation of a glacier module within the WEAP model. The new WEAP version is applied to a set of 17 sub-catchments of the Rio Santa basin. The authors' main objective is (1) to demonstrate the value of this model as a tool for water resource management and planning in the Andean countries. Present and future glacier retreat in the Andes and associated consequence on the water resource is a highly topical issue. Nonetheless, few studies tackle this issue at the relevant space and time scales for water resource management. As HESS aims "to serve (. . .) water engineers and water managers", the paper by Condom et al. falls within the scope of

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HESS and is rather unconventional from this aspect.

However, the proposed model is also used (2) to assess glacier vs. groundwater contribution to present-day streamflow and (3) to investigate the effect of climate change on future river discharge.

While the authors strive to meet all these ambitious objectives in a single paper, they tend to neglect methodological issues and results discussion. Moreover, as explained below, several shortcomings undermine the authors' conclusions.

Specific comments

a) Theoretical background

The authors used a "degree-month" model (i.e. melt is proportional to monthly mean temperature if this temperature is above a calibrated threshold, otherwise it is set to zero) to compute glacier and snow melt. Every year, glacier area is updated based on the Bahr volume-area scaling (Bahr et al., 1997). Their degree-month model is a straight transposition to the monthly time step of the widely used degree-day model (actually in its simplified form, i.e. Eq. 2 in Hock 2003).

The degree-day models, or temperature index models, are notoriously inefficient in the Tropical Andes due to the prominent role of sublimation in the glacier energy balance, which is driven by humidity (e.g. Wagnon et al. 1999a, Juen et al. 2007, Sicart et al., 2008).

This point is honestly acknowledged by the authors. They justify this choice against glaciological evidences by the fact that the degree-day model requires few in situ data and few parameters to calibrate. They also make reference to a study by Braithwaite and Zhang (1999), a short paper published in Geografiska Annaler, where the degree-day model was applied to a set of 37 glaciers including 3 tropical glaciers. However, this reference is misleading because:

âĂć No Andean glacier was included in this study. The dataset includes only three

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tropical glaciers, one in Kenya and two in Indonesia; âĂć Even for these 3 glaciers the performance of the degree-day model was not assessed. Moreover, Braithwaite and Zhang (1999) acknowledged that "There is no direct evidence from the field that the degree-day model applies to tropical glaciers"

The only remaining valid reference is Suarez et al. (2008).

This background is important because it gives to the authors the responsibility to demonstrate to the scientific community the value of the degree-month approach to modeling river discharge in the Tropical Andes. In my opinion this aspect should be presented as the main underlying scientific challenge and demonstrated as such.

The authors do not mention that at least one, parsimonious, glacier melt model that meets these requirements and which is not based on temperature-index has been successfully applied by Juen et al. (2007) in the same area.

b) Model description

This part is really confusing and contains some errors that tend to discredit the rigor of the study.

First, the authors adopted a complicated formalism to describe simple mathematical concepts (e.g. some variables have as many as 5 subscripts; the summation operator is used to add a sequence of two numbers see Eq. 3 ...).

Then, the parameters and variables are not given in standard SI units, leading to rather curious equations where unit conversions are mixed with model calculations (e.g. Eq. 6a: "Q/1000 * area*10002 "!, see also Eq. 12, Eq. 13, Eq 15...).

In Eq. 2 the second term should be equal to the last term in Eq. 1 (i.e. z1 instead of z2).

There is confusion about the upper soil storage conductivity: it is written ks in the equations (Eq. 1 and Eq. 2) and kj in the text. I suspect this occurred because some

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portions of Section 2.1 were copied from the paper of Yates, where the second notation is used (Yates et al. 2005, see p.491-492).

Same cause, same effectâĂŤthe authors made another mistake p877, L6: "The second term in Eq. 2 represent the baseflow" (it is actually the first term, but it was the second term in the original equation in Yates et al, 2005, see Eq. 7).

The authors stated "the main modification with respect to Schaefli's formulation of snow and ice melt contribution to runoff was the elimination of the exponential autocorrelation factors". My understanding is that this factor is not an arbitrary autocorrelation factor but results from the resolution of the model's differential equation.

Eq. 6 (a, b, c) are not homogeneous: VQ should be m3/month. Moreover, it took me a little while to realize that VQ was not the product VxQ, but a single variable.

The "RRF" (runoff resistance factor) is a "LAI" factor (leaf area and stem index) in the original description of WEAP (Yates et al., 2005).

The authors did not explain how the model accounts for snow outside the glacier coverage (if any? Unless the seasonal snow cover was neglected?). The WEAP documentation indicates that a temperature index model was already implemented for that purpose. The authors did not indicate if the WEAP melt factor and temperature threshold were also calibrated.

From my understanding, Eq. 10 implies that rainfall over glaciated areas can contribute to glacier accumulation (in addition to snowfall), which I doubt is realistic.

There is no information about the aquifer representation, although WEAP allows the inclusion of a conceptual alluvial aquifer. The aquifer representation should be described because the authors have used the model to assess the historical contribution of groundwater to river flows: in Section 4.2. p. 895, it is written that simulated groundwater flow is low in some catchments because "aquifers are of small lateral extent".

I am not a specialist in numerical analysis; however, I would like to understand how the equation describing glacier evolution can be solved independently, i.e. outside the WEAP Runge-Kutta algorithm, although it involves common variables?

c) Parameters

The optimal degree-month factor for ice is: aice=600 mm/°C/month, i.e the maximum value of the investigated parameter set. This result should encourage the authors to broaden the initial parameter space to check if a better calibration can be achieved with higher values. Note that the authors obtained the prior parameter ranges for aice and asnow (degree-month factors) by multiplying by 30 days the min and max values of the degree-day factors reported in the literature, which is not necessarily adequate as the temperature index model is nonlinear ought to the temperature threshold (Hock 2003, explains this in the case of the hourly to daily scaling “Use of daily temperature mean can be misleading during times of temperature fluctuations around the freezing point. Mean temperature may be negative indicating no melt, whereas melt conditions may have prevailed during part of the day. Hence, the degree-day factor will be overestimated”).

d) Calibration

The model was calibrated using observed discharge as the “main” criterion. The authors should clarify which other criteria were used, in particular if glacier extent was used as a criterion, and how (which objective function?). This is crucial as glacier extent is used for model assessment in Section 4.3.

e) Climatic settings

The authors mentioned a weighted average of precipitation. Weighted by what? It is not a minor detail as precipitation interpolation is a recurrent issue in mountain areas (see further comment below).

Question: How can one distinguish longitudinal from altitudinal climatic gradients in the study area where topographic gradients are roughly parallel to longitude?

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f) Input data

The authors aimed to simulate the monthly river flow over a long period (30 yrs), for a considerable number of gauged catchments, which was a good approach to validate their model. The lack of reliable data is understandable in this context (only one temperature record).

P890, L.4, the authors made reference to a dataset of glacier cover for the period 1969-1999, but the dataset was published in . . . 1989.

Per capita water use was set to 300 l/d. The authors did not provide any reference for this figure, neither they discuss its importance in the water balance. It is unfortunate because few hydrological studies account for the human and agriculture consumption in a catchment area.

The assessment of the precipitation data is insufficient:

- The authors mentioned a “data quality analysis” but did not report the result of this analysis. - The authors asserted that the interpolation scheme by inverse distance squared is “well suited to maximize utility of the available data”. This statement needs a reference. - “39 stations are evenly distributed”. Since the stations were not indicated on the map of the study area, there is no way to verify this. - I suppose that the high-elevation areas are poorly monitored (in spite of their greater contribution to the regional water balance), as in many other Andean countries. The authors elude this question.

g) Model calibration

Again, this section is unclear. The optimal WEAP standard parameter set was obtained from a separate model application to a glacier-free catchment; on the other hand, the parameters of the glacier module were calibrated separately in a glaciated catchment. Then, the authors performed a split sample calibration-validation exercise to make some “small adjustments of the glacier parameters”. I do not understand the

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rationale of this strategy. If the parameters were adjusted, then the final parameter set should be provided in Table 2 instead of the preliminary parameter set.

h) Model results

Looking at annual streamflow simulation for La Balsa station (Fig.5, top panel and not bottom as indicated in the legend) I seriously doubt the bias is only 1% as indicated in Table 1.

Fig. 6 shows that the simulated glacier discharge is almost constant over the year. This is rather in contradiction with the glacier runoff seasonality described by the glaciologists in the tropical Andes (Wagnon et al. 1999b, Juen et al. 2007 among others). I think this may reveal the shortcoming of using degree-month modeling in this area.

i) Discussion

The assumption of a constant melt factor is not discussed (see Hock 2003, p.109 "Results must be interpreted with caution as the inherent assumption that degree-day factors remain constant under a different climate may not be true")

Neither do the authors discuss how sublimation losses are handled in the water balance: e.g. are they compensated by the melt factor, or by evaporation from the non-glacierized areas? This question should be addressed in view of a climate change impact study as future meteorological conditions may affect the sublimation/melting partition.

The climate change impact study in the last part of Sect. 4 is too superficial to be properly reviewed.

In conclusion, I think that the authors should not pretend to present in the same paper: - A model for water management, which requires robustness and simplicity - A model for process analysis, which requires multi-objective validation etc. In my opinion the authors should focus on one of the two aspects and address all the aforementioned methodological shortcomings before publication.

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Technical corrections

What is the first month of the hydrologic year?

Pearson's coefficient was used (e.g. p.892) but not included in Section 2.2.5 "Efficiency criterions"

Sect. 3.2.: mean annual temperature values are given in °C y-1 !! (it would be a trend).

P873, L10 "associated such" P885, L3 "based on" P887, L2 "although..." finish sentence P888, L2-6 repetitive P888, L19 and P890 L5: latter P893, L16: "capture improve" 894, L7: "difficulty"

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