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Hydrology and Earth System Sciences Discussions

Interactive comment on "Modelling the hydrologic response of a mesoscale Andean watershed to changes in land use patterns for environmental planning" *by* A. Stehr et al.

Anonymous Referee #2

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General Comments:

The authors present an interesting case study about the hydrologic response of an Andean watershed to land use change. While the model application, as reported in the text, is not very innovative from the scientific point of view, the number of SWAT modelling case studies from the Andean region is rather low compared to other regions. So there might be an interested audience. The topic of the paper is suitable for HESS, but the scientific quality of the paper is not yet sufficient for publication. In its current state the paper should be rejected. Based on an assumed general interest on

the reported work, the authors are encouraged to resubmit the paper, after they have rethought some aspects of their simulation experiment, and after they have significantly expanded the presentation of the method and the discussion of their results.

Specific Comments:

1. At p. 3076, l. 3-5 you notice a lack of case studies for the southern hemisphere. For the scientific readers it is very interesting to get information about possible problems of the SWAT model structure for your region. Recommended ranges for parameters, which have not yet been reported elsewhere, are also worth reporting. We do not get much information in the paper. Some possible questions to address:

a. Applicability of the SCS CN approach for your region. Your model was very sensitive against the CN values (table 4). This needs more consideration.

b. Vegetation parameters. Your study is about land use change, respectively change in vegetation. Can you work with the (mainly US based) database of the original SWAT model, or did you expand the vegetation/parameter databases?

c. How good are the incorporated evaporation/evapotranspiration approaches for your region, which one did you use?

2. The land use change from 1979 to 1994 is rather dramatic compared to other regions. From 1979 to 1994 it is 15 years, and now you might even have data for the next 15 years (-2009)? Can you evaluate your scenarios against recent observation data? Which of the five scenarios is closest to reality so far?

3. Scenarios 1 and 5 seem to have a logical background, while scenarios 2 to 4 seem to be more a kind of sensitivity analysis. These three are not realistic, are they? The way how you developed the scenarios, mainly scenario 5, should be documented in more detail. Please describe the regression model (e.g. formulae) and document your assumptions. The generation of the scenarios should be reproducible.

4. In table 8 you show results for the scenarios 1 to 4, but not 5 – why? Scenario 5 is

very interesting. The caption of fig. 10 is incomplete: is f) scenario 5?

5. The description of the SWAT model should be carefully revised. Interception is not computed with the CN method, you mention surface runoff twice, but I think you mean generation and concentration of surface runoff etc.

6. The deep aquifer in SWAT should be handled with care. You can remove a lot of water from your watershed when you set a high value for the rchrg_dp parameter – as you did (0.5 – 1 as reported, that means up to 100% of the percolating water!). Does this correspond with the local hydrogeologic situation? When removing so much water (violating the continuity equation for your watershed), it should be documented where the water is transferred and why it is. Reading such parameter values for rchrg_dp and a relatively high bias of your model results gives cause for serious concern.

7. The results of the sensitivity analysis (Section 6.1) are not discussed. How do they compare with the results of others? Did you observe a specific behavior of the model, which can be related to the local situation? Is there a recommendation to use different parameter ranges for PARASOL in your region?

8. In section 6.2 you discuss the calibration results for 2000-2002. You report that the model "satisfactorily reproduced the order of magnitude of the observed discharges". This does not sound convincing. Model and data uncertainty should be analyzed in more detail. For example: you report that the model subestimates (underestimates) peak discharge. I am not surprised when I see the availability of rainfall data: only one station within the mountain area, and that station has a lot of missing values even within the calibration period. Which method did you apply to interpolate (?) rainfall data?

9. In 6.2 and 6.3 you mention "changes tendency in time" – what do you mean with that? Are you able to say anything about trends in runoff, based on the short calibration/validation periods?

10. P. 3082, l. 20: "calibration period" – isn't it validation period here?

11. Table 2: what do the numbers mean? I'm not sure if I understood the table. It needs some explanation in the capture, also provide units (ha?)

12. Only three of the five gauging stations have been operated in 1977, so are the data presented in table 3 really related to the period 1977-2002? Why do you include the two stations Rehue and Renaico in your study, could they even be left out?

13. P. 3083, l. 2: From your model results, you assume that the model can be applied to analyze the impact of land use changes on the hydrologic response. Table 5: In my opinion the model bias is rather high. The percentage of change caused by the different land use scenarios is within a similar magnitude (table 8). This needs to be discussed in detail.

Technical Corrections: Language is understandable, but grammar and spelling need a revision to meet publication standards. Asking a native speaker is recommended.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3073, 2010.