

Dear authors,

Thanks for uploading a revised manuscript and a file with responses to the reviewers. Although your replies to the reviewer comments were promising, your implementation of changes in the manuscript has a number of major issues that should be addressed.

Firstly, from your analysis it is unclear what the causes are of differences in drought recovery found between the catchments. You mention the importance of soil characteristics, but do not show that the soils are more important than the effect of higher precipitation and lower evapotranspiration (as mentioned by yourself in the paper), the effect of rainfall intensity (as mentioned by reviewer 1), the effect of topography and wetlands (as mentioned by reviewer 2), the effect of differences in vegetation type, antecedent conditions, etc. In the abstract and manuscript text confusing statements can be found, claiming the difference is an effect of soil only (abstract, p.1 & p.23), an effect of soil and vegetation type (p.19), an effect of vegetation & evaporative demand (p.20), an effect of potential evaporation & rainfall (p.20), and effect of soil and evaporative demand (p.23). This should be properly analysed and discussed consistently. The effect of soil is counterintuitive, because larger storage normally results in longer drought recovery. So this also needs more discussion.

Secondly, both reviewers expressed their concern about your use of a model in this study and the validity of the modelling results. In your revised manuscript you did not address these concerns satisfactorily. For example, major point 2 of reviewer 1 has not been answered. Contrary to what you state in reply to that point and in the manuscript, the model does not represent observed flow and soil moisture correctly, especially not during drought. Reviewer 2 recognises that modelling paramo hydrology is difficult, but that does not warrant your claim that the “discrepancy between simulations and observations is low”. Also point 2 of reviewer 2 has not been addressed completely. It is still unclear from the manuscript what the model results add to the observational data analysis. There are valid reasons for using a model, but those have not been explored and discussed in your manuscript. In reply to a point about hydrological drought made by reviewer 1, you state that you used the model to investigate drought propagation and hydrological drought recovery, but if you have observational data of precipitation, soil moisture and discharge, you do not need a model for that. Additionally, contrary to your reply to this point by reviewer 1, your Figure 5 does not provide any analysis of drought propagation or hydrological drought recovery. A quantitative comparison of drought propagation and recovery between the catchments and between soil moisture drought and hydrological drought is a needed addition to the current results. This can also help in providing a better answer to point 1 of reviewer 1, which needs a much more elaborate consideration.

Thirdly, the results are very thin and should be extended. In the Introduction you mention that “the hydrological drought is compared and related to the soil water drought”. This is, however, not the case. Hydrological drought is never quantified in this study. This should be included.

Finally, there is no discussion in the manuscript. Reviewer 2 pointed out a number of topics that should be discussed in the manuscript. Although you added a few lines of text to the results section, the points raised by the second reviewer require an in-depth discussion of the hydrology of the paramo environment.

To solve these issues a few things need to be done:

- Improve the model for the drought periods, for example by calibrating on the Nash-Sutcliffe value of the logarithm of the discharge values.
- Use the model to extend timeseries of soil moisture (Figure 3), so that you can compare the recovery of other drought events.
- Use the model to do sensitivity analysis, by changing the input (e.g. precipitation or potential evaporation during drought recovery) or the soil type.
- Do a proper analysis of drought propagation and hydrological drought recovery, using a drought analysis method and a quantification of drought recovery.
- Quantify the period of vegetation stress in both catchments to calculate vegetation recovery to drought (point 1 reviewer 1).
- Discuss the uncertainties related to the observation, data analysis and modelling of paramo hydrology. This needs to include the selection of the “representative locations for TDR measurement”, the added value of using a model, the difficulties related to modelling, the scaling used for soil moisture, the quantification of drought recovery, the different factors influencing the difference in recovery between the catchments, the significance of the results for understanding paramo hydrology, etc.

If these major issues are not handled satisfactorily in a revised manuscript I will reject the paper for publication in HESS. In addition to these essential modifications, there are a number of other issues that need to be addressed.

- Your answer to point 1 of reviewer 1 and your clarification of the term “resilience” in the manuscript are satisfactory, but from the revised manuscript I do not see the need to use such a complex and confusing term (“resilience”), when there is a more simple and easy to understand term (“recovery”). I would urge you to take out the resilience theory description and every mention of the word resilience from the title and the manuscript. Also because it leads to erroneous statements such as “the páramo vegetation and soil are more *resilient to drought recovery* as compared to the lower grass vegetation and soil”.
- There is a lack of clarity about the time periods of analysis. In the abstract, for example, the periods 2007-2013, 2010-2012 and 2009 & 2010 are mentioned, but it is unclear which time periods were used in which part of the analysis and for which time period the mentioned results are applicable. The same is true for the main text and the Conclusions.
- Similarly, the use of catchment names is confusing. For example, in the Introduction on page 3 the Paute catchment is mentioned, a long time before the catchments are introduced in Section 2.1.
- You should take out the paragraphs about El Nino in the Introduction. As you rightly state, for your study, it is the lack of rainfall that matters and not whether it is caused by El Nino or by something else. The text about El Nino is not relevant.
- As I mentioned before, the Introduction is too long and should be shortened. These suggestions above might help with that.
- The hypothesis mentioned on page 6 does not correspond with the objectives explained in the introduction. Where does this hypothesis come from? Do you need it? Is it new? Can it be proven? Would one of the two methods, experimental monitoring vs. mathematical models, not be sufficient? And what is “drought recovery resilience in land cover and soil systems”? In the Conclusions, “the first aim was to estimate the actual evapotranspiration

based on continuous time series of soil water content measurements". This does not correspond to the Introduction and is not followed by a second aim.

- How is recovery defined? On page 6 you mention that it is "the time needed to recover to its pre drought state of water content once that rainfall has started in a continuous way to exceed the vegetation water demand". So how is that quantified? What is the pre-drought state of water content? When is P-ET positive in "a continuous way"? You analysed the recovery from the 2010 soil moisture drought from observations in Section 4.2. In Section 4.3, you mention recovery for the 2011 drought (Figure 5), but do not quantify it, not for soil moisture drought nor for hydrological drought. You also mention that the "recovery by the vegetation after drought is good". How was this recovery quantified?

Furthermore, a number of textual issues need to be resolved:

- p.10, Qo: how is overland flow measured?
- p.15, lines 21-24: move up to paragraph 3.2
- p.22, line 32: what do you mean with "the proportion of potential water use in the páramo"?
- Check for spelling and grammar errors.