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Interactive comment on "Analysis of the drought resilience of Andosols on southern Ecuadorian Andean páramos" *by* V. I niguez et al.

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1 General comments

This is a well-written manuscript that gives some interesting insights in the hydrological behaviour of an exceptional geographical region providing important ecosystem services. The applied methods are sound, and the fact that the monitoring data cover an exceptional drought period is a strong asset.

However, I share the first reviewer's concerns about the use of the term resilience, especially because it is not always clear whether the perspective is the ecosystem itself or the downstream users. The introduction and rationale of the project is very

much written from the downstream users' perspective, because of the exceptionally high runoff ratio, and an equally high buffering capacity (i.e. very high base flows and comparatively low peak flows) of the paramo region. As the authors correctly point out, this is indeed related to the extreme soil water retention capacity of the paramo soils.

But the obtained results suggest that the ecosystem itself may not be so resilient. The fact that the critical water content of the paramo is found to be exceptionally high suggests that the vegetation may be quite prone to water stress. This is again good from the viewpoint of downstream users, because that means that ET reduces quickly under drought conditions, which reduces soil water depletion and increases the recovery rate. But it may well mean that the paramo itself is quite sensitive to drought, depending on the plants' physiological reaction to water stress (see also the specific comment below). One potentially interesting piece of information that is lacking here is the wilting point, which determines the total available water. Especially since the water retention curve is estimated (11457/4-9), and the wilting point used in the analysis, it would be useful to show the data themselves and discuss them in a bit more detail.

Elaboration of the resilience concept in the above-mentioned context may even be an opportunity for a more thorough discussion on the hydrological behaviour of paramo catchments and how to interpret the response in the context of drought. For instance, another aspect that is worth discussing further is the extremely high wilting point of many paramo soils, including those in the studied region, and what this may mean for the hydrological response and base flows in particular. At least theoretically, water retained beyond wilting point is hydrologically inactive, and therefore not relevant for water resources. Instead of the large water retention of the soils, there is increasing evidence that the paramo catchments' extreme water buffering and streamflow dampening response is rather related to the topography, and the resulting extensive occurrence of wetlands with a high storage capacity, which effectively create extreme variability in the contributing area. All this is very compatible with the findings of the study, but also gives an opportunity for better contextualisation and I would strongly encourage the authors to take this further than the current discussion.

One minor point of the study is the relation between the observations and the modelling. I don't think that the GLUE analysis provides much added value, and I suggest to take that out. As also pointed out by reviewer 1, neither would I rate the performance of the model as particularly good. There are good reasons why modelling paramo catchments is very challenging, in particular the large spatial gradients of precipitation, which leads to large input errors. This is not a criticism of the model implementation, but warrants a more in-depth discussion of the added value of implementing a model in addition to the more direct analysis of the results.

Nevertheless, an interesting and well-presented paper that surely merits publication.

2 Specific comments

11451/11: "WRB": needs to be written in full (World Reference Base for Soil Resources) and referenced (as is done further down but not here). Also, it is probably good to point out that these are the dominant but not the only classifications. For instance, there are also umbrisols and regosols, among others.

11452/20: "the first three years have been classified as el Niño years": needs a reference. According to what criterion? If they were, they surely weren't very pronounced.

11452/29 -> "resilience or resistance": concurring with reviewer 1, I find the term resilience a bit problematic here, and would prefer resistance. But it may be worth trying to be more precise as to what kind of soil behaviour would be preferable from a water resources perspective (see above).

11453/23: "the main hydropower projects": which ones?

11453/26: "hydrological capacity": needs a more precise formulation. What capacity?

This relates to my comments above about regulation and buffering.

11453/26: "resides on" -> resides in

11454/9: "characterized as" -> "characterized by" or "classified as"

11454/10: The large difference in catchment area is not ideal, but I understand that it is caused by data scarcity and topographical constraints. It is important to keep the potential consequences in mind when comparing. For instance, may the fact that a higher spatial variability of soil moisture is found for the (small) paramo catchment (11467/15-21) have to do with the fact that Cumbe is much larger, and therefore the hydrological response is longer, which may reduce the sensitivity of parameter b. A dotty plot of parameter b would be useful in that regard. The appendix contains some dotty plots, but apparently not of parameter b.

11457/1: "soil samples": what type of measurement? Gravimetric?

11457/eq.1: As the time interval is non-infinitesimal, it is better to express the storage variation as $\Delta S/\Delta t$ instead of dS/dt, which is the differential.

11458/5: "in páramos, the": remove "the"

11465/4-14: I think that this section can be formulated more sharply. Essentially, what happens is that the xerophytic properties and other adaptations to a high-radiation environment such as the dead leaves increase stomatal resistance, reduces the root suction capacity, and in general increases the plants' sensitivity to soil water potential. The question is of course to which extent this makes the plants more resilient to drought. The low transpiration rate may make them survive longer during drought, but it may also mean that their wilting point is much lower than the typically assumed 15 bar. This generate an interesting trade-off. It is probably not possible to determine with the available data which is the dominant process, I think that this merits some more discussion.

11467/6-9: the relevance of the robustness of the model as evaluated by the GLUE method is not really clear, and is a bit disconnected from the rest of the paper. I suggest to leave it out to make the paper more focused.

11469/12: "reached values": better "dropped to values" as I assume that these are extreme minima? Also, use "unprecedented values", or "not previously observed" instead of "values never seen before"

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 11449, 2015.