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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Climate changes of hydrometeorological and hydrological extremes in the Paute basin, Ecuadorean Andes" *by* D. E. Mora et al.

Anonymous Referee #1

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Review of Mora et al., Climate changes of hydrometeorological and hydrological extremes in the Paute basin, Ecuadorean Andes

General comments

This manuscript reports on an assessment of the potential impacts of climate change on the precipitation patterns and hydrological response of the Paute basin in the Ecuadorian Andes. It focuses mostly on aspects of the statistical downscaling, but also reports on hydrological model performance, spatial variability and local hydrological process understanding.

The manuscript surely contains relevant data and some very interesting ideas on the hydrometeorology of a poorly known region. It also asks some pertinent questions on methodological elements of statistical downscaling which are worth considering. Nevertheless, the approach taken in the paper strikes me as rather odd, and I am not convinced that it allows for the data analysis and inference that the authors claim it does. As a result, I do not think that most of the conclusions are valid, and several of are direct artifacts of the rather complex setup of the scientific experiment, which results in circular reasoning, and conclusions, which are direct effects of the assumptions made.

First, I am rather confused by the evaluation of the downscaling method. In essence, the statistical properties of observed time series of precipitation and temperature are perturbed with some change factors obtained from GCMs, and then evaluated by calculating different change factors on these statistical properties. I do not understand the rationale behind this approach. If one is interested in understanding the potential change of climatic variables, it seems much more logical to separately report on the changes projected by the GCMs and on the local precipitation characteristics, and then discuss how they might interact. This would seem much more transparent and straightforward than the obfuscated approach used now. It would also avoid several of the false conclusions that plague the current manuscript. For instance:

"More significant changes in temperature are observed in sites with higher elevation, whereas sites that are allocated in lower elevations show a lower increase." (P6457 l7)

This conclusion seems to be a direct artifact from the procedure of applying absolute deltas change values to observed temperature series, and then evaluating this impact in terms of relative changes. Of course, this automatically results in stations with lower temperatures showing bigger relative changes! Given the inverse relation between temperature and altitude, this also means that stations at higher altitude will show a higher relative change. This has nothing to do with any physical process (such as the change in lapse rate hypothesized by Bradley et al. (2006)).

"highest monthly temperature values are experimenting lower changes than the average ones and, opposite, the minimum average temperatures meet higher changes." (p6457/12).

As explained above, this conclusion is also a direct result from the mathematical procedure and has no physical meaning. The same absolute change applied to higher values will inevitable result in lower relative changes.

"When yearly, monthly and daily rainfall changes are compared, the dependency of the changes on temporal scale concludes that higher changes are observed for higher temporal resolutions." (p6457/20)

I am not sure I fully understand this sentence, but again it seems that much of the obtained results are a direct consequence of assumptions made in the precipitation downscaling routine. For instance, one of the downscaling methods uses a combination of absolute and relative changes, as visualized in Fig. 3. Fig. 5 then shows the impact of this procedure in terms of relative change (%). Again, the spatial patterns observed in Fig. 5 are a direct consequence of the choices of thresholds and change values applied in the downscaling method.

For instance, the rationale behind the application of absolute change factors below a certain threshold is that relative changes would result in a very small absolute change (p6451, l2). So this in essence "boosts" the change in low precipitation values. As a direct consequence, stations with a dominance of low values (either stations in dry regions or in regions with a well distributed and low-intensity precipitation regime such as the paramo) will of course show a larger relative change than stations with a dominance of high precipitation values.

The impact of downscaling assumptions on the calculated changes in precipitation more difficult to assess compared to that those of temperature, because the procedure is more complex. It also includes a compensation for changes in frequency. But again, it is very likely that this part of the procedure introduces biases that are picked up in the

evaluation criteria (e.g. a bias towards adding or removing high precipitation values).

As a result, there is a lot of circular reasoning in the method, which largely invalidates the conclusions made or at least makes them almost impossible to interpret.

Instead, I suggest a different approach. First, it would be very useful to report on the projected changes in precipitation and temperature properties as extracted from the GCMs. For instance, changes in magnitude, frequency, variation in changes in quantiles etc. This in itself is very useful information and will allow the reader to assess whether any "added value" can be generated by the spatiotemporal patterns observed in the local stations.

Subsequently I suggest a detailed discussion of the spatiotemporal patterns observed in local precipitation and temperature stations. For instance, variations in intensity, amount, frequency etc. This, again, is necessary to understand how applying change factors from GCMs will interact with the statistical properties of the local stations.

Only then may it make sense to combine both and report on any spatiotemporal patterns observed in the downscaled projects. At this stage, it should be possible to discuss whether these patterns are simply the result of the assumptions of the downscaling method, or whether they actually provide insight in the future behaviour of the local weather.

The manuscript also reports on the performance of the hydrological model, e.g., (p6456 l7): "numerical optimization can achieve better overall results, but that it does not guarantee accurate submodels". First, it is not clear what is meant with submodels, and in general not enough information is given to properly assess the modelling approach. This may not be a problem if the model is only used to convert meteorological variables into streamflow (conditional on the model being described in other publications), but in that case I think the authors should refrain from making conclusions about model performance that cannot be substantiated. On the other hand, there may be scope to go further in terms of discussing the impact of climate change on streamflow. When discussion peak flows, it seems rather superfluous to report that changes in precipitation dominate changes in temperature, given the limited impact of evapotranspiration on peak flow. For instance, reporting on total impact of ET on the water balance on longer time scales (e.g., yearly) would give very interesting insights in the potential impact of ET on water availability and drought risk.

Lastly, the paper needs a further revision on language and accuracy of the description of the procedures. While not exhaustive, here are some specific comments:

p6446/1: "despicably": whatever word may be meant here, this surely is not the correct one.

p6447/15: "produce inappropriate results compared with GCMs": this is a rather liberal interpretation of the citation. Buytaert et al. (2010) showed that RCMs do not necessarily give better simulations of precipitation during the historical run, especially in complex regions such as the Andes. But that definitely is not a reason to simply discard them as inappropriate.

p6448/21: interspersed: wrong word?

p6449/1: páramo: explain, for instance as "tropical alpine grasslands (páramo)"

p6451/21: to be add: to be added

p6452/22 - 23: this needs more elaboration: what data were used to calculate ET? From what and how many stations? Where future ETs calculated by keeping all these data constant except T? While I think this is a reasonable simplification, it is often criticized because it is relatively straightforward to extract changes of other factors (e.g. humidity) from GCMs too. Perhaps the potential impacts of this simplification should be discussed briefly.

p6455/28: "by Pacific drivers considered in the climate models.": Can you be more specific? Which drivers? How?

p6456/10: "in unitary runoffs, this is relative to its area.": typically referred to as runoff depth.

p6457/13: experimenting -> experiencing

p6458/5: founded -> found

p6458/16: data feasibility: wrong word. Perhaps “data access”?

p6465: explain the abbreviations for the performance measures: NS eff Ob, NS eff BF etc.

All in all, in my opinion this calls for a very major revision, which should not be limited to adding some paragraphs and figures, but in which a rethinking of the concept and structure of the paper, and subsequently rewriting of major sections will be necessary.

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