Hydrology and Earth System Sciences Discussions

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Comment

Interactive comment on "Impacts of climate change on Blue Nile flows using bias-corrected GCM scenarios" *by* M. E. Elshamy et al.

Anonymous Referee #1

Received and published: 4 July 2008

In this manuscript, a climate-scenario exercise is performed to analyze how the runoff regime of the Blue Nile may change given the state of the art climate scenarios (from the 4th IPCC assessment report). The authors bias-corrected and statistically down-scaled rainfall fields from all climate scenarios to match the temporal scale of the hydro-logical model used and analyzed what their effect would be on discharge at the outlet of this subcatchment of the Nile basin. According to the presentation of their results, the aim was to show the possible impact of climate change on time scales of months or years, without considering interannual or sub-monthly variability (e.g. dry-year persis-tence or likely increase of length of the dry season or dry spells, the latter maybe not so interesting at the whole basin-scale). Although the approach is not unique, it is for decision making very relevant to see how unpredictable the future is given the climate

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scenarios. This is especially due to the extremely non-linear character of both climate models and the hydrology of semi-arid areas. Although I think that the methodology is quite thorough, I have quite some comments on the way results are presented. I hope my comments will result in a valuable revision of the manuscript and I'm eager to further discuss of course. Below I first describe my general comments and afterwards some details in the manuscript that relate to clarity, quality of English, relevance, etc.

The problem of the methodology as such (as it is nearly always with climate scenarios in hydrology) is the fact that a non-linear hydrological model is forced by a highly non-linear output of another model (i.e. the circulation models used in this study). This accumulation of non-linearities in the approach is amplified by the fact that we are dealing with a semi-arid area (i.e. $E_p >> P$ on yearly averages), which results in small runoff coefficients. In the end, this means that the uncertainties in the original input, i.e. the circulation pattern and dynamics of the atmospheric model underlying the climate scenario, is seriously amplified by the precipitation generating modules and consequently the hydrological model itself. I believe that it is unfortunately extremely hard to identify how large these uncertainties are. So the authors cannot do much more than what they already did. As long as the model is more or less calibrated and the scenarios are bias corrected, the exercise is straightforward.

The largest difficulties I have with this manuscript is the way the results are presented. To be honest, I found it very difficult to see how significant the changes in discharge were with respect to the total water balance. My main comment is therefore that the authors should do everything in their power to show how the outputs of the model in the climate scenarios relate to the water balance, rather than plotting individual outputs. This will make it much much clearer for the reader (although not quantified as such), how meaningful the results are. An example: if I make a plot of yearly discharges and yearly rainfall in a semi-arid catchment and draw a straight line through it, I will notice that a certain threshold of rainfall is needed to generate any discharge at all, which means that at this point, the catchment would be infinitely sensitive to climate change,

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which makes the results less meaningful for a decision maker. In semi-arid areas, you are often very close to that point. I thought of a practical approach to tackle this: since the authors are not really focusing on inter-annual variability, I thought it might be practical to plot all results in the way it is done by Budyko (1974). He showed with data of several catchments that over long periods (years) there is a meaningful relation between E_p/P and the part of rainfall that evaporates (E_a/P), which reads as:

$$\frac{E_a}{P} = \left[\frac{E_p}{P} \tanh\left(1/(E_p/P)\right) \left(1 - \exp\left(-\frac{E_p}{P}\right)\right) \right]^{0.5}$$

Many catchments do not follow this curve exactly due to for instance phase differences between E_p and P or land use practices, but at least the shape of this curve should be the same as the formulation of Budyko (and other researchers that derived similar empirical curves). If the authors present their results in a Budyko diagram (plotting E_p/P on the x-axis and E_a/E_p on the y-axis, the results become much better readable and the uncertainties of the results in an absolute sense (although not quantified) can be judged from a water-balance viewpoint (the y-axis being a way of displaying the water balance). Every scenario (including the base line) can be plotted on this curve to see in which regime the result is (left-side of the graph would be energy constrained and right side moisture constrained). On the extreme right side of the graph, the runoff coefficient becomes extremely low, so that results become highly uncertain given the uncertainty of the scenarios used as input. E_p/P is in your case about 1 in the current situation which means that the blue Nile should be exactly on the edge between going to a moisture constrained regime and an energy constrained regime (just plot the equation above and you'll see what I mean), which makes this a much more informative way of showing your results on annual time scales.

The authors could do the same for the sensitivity study of the hydrological model. Make 2 Budyko graphs within one figure, one with P varied and one with E_p varied. You can then nicely see if such changes correspond with changes from moisture constrained to energy constrained and vice-versa and you can see if E_p changes have a significantly different effect than P changes with respect to the hydro-climatological regime instead

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of with respect to just discharge. The hydrological model should more or less follow the shape of the Budyko curve, otherwise it is simply not producing hydrologically meaningful results. The same goes for your derived relationships between temperature, rainfall, PET and flow. It looks as if these relations are unique while in fact they may be very similar to the ones proposed by Budyko and others. Before concluding that you have found a new way of quickly assessing impact of climate change on water resources, please first investigate how your method compares with established empirical research.

Related to this, show all fluxes in the same unit, this makes it all much easier to read and much easier to estimate how large a flux is with respect to the other. For instance fig. 2. Why display discharge on a separate axis in BCM? Just do it in mm/year as well and give it the same scale so that everybody can immediately see how large this flux is with respect to rainfall. Another thing is the units themselves. On the y-axis of fig. 2 it says 'mm'. Is rainfall suddenly a state instead of a flux? Is it 'mm/day'? 'mm/month', well I guess it is 'mm/year'. Please change that in all graphs (fig. 2, 4, 6, 8, 9).

Furthermore, the structure of the paper is not completely clear. In the section "datasets" all used data is described without much information about what the information is used for. Then, in a later section "Methodology" some description of the NFS hydrological model is given without paying much attention to what datasets actually were used in relation to this model. My suggestion is to come up with a section "models and datasets" in which the used models are described in relation to their spatial/temporal scale, the applied input/output datasets and model structure. The same for scenario and ancillary data. The section "methodology" will become better readable if a small flow diagram of the undertaken steps is presented. This should include the downscaling procedure, the scenario computation and post-processing of results. I leave it to the authors if they find this a relevant comment or not.

Finally, I find the description of the downscaling approach somewhat short. I am not so familiar with the method of Ines and Hansen, but from your description it should

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at least become clear of what you estimate the distribution (nr of rain days within the month? intensities in the month?) how the distribution was matched (simple shifting of the mean? Or was also the 2nd and-or 3rd moment varied?) and finally, what, if you only correct for intensity, in practice the difference is between fitting a distribution and a simple multiplication factor.

Although I believe that no additional computations are required, there's still quite some work to be done on this manuscript. I hope the authors and the editor agree with me on my comments and I wish you luck with the revisions.

Below you can find some more detailed comments on the text itself.

p. 1407/1408. l. 1. 'depict different pictures', a bit strange English. l. 3. 'downscaled', suggests that the authors have spatially downscaled the output fields of GCMs. I cannot recall I read anything about it. Remove this or show how you've done the downscaling l. 15-16. 'The increase in PET ... reduced by 15%'. This is not clear and I don't even believe it is true. Something should be added on the significance of the results with respect to the magnitude of the terms in the water balance (i.e. if discharge becomes really a small relative flux, then results are not really significant)

p. 1409 l. 18 '...increase in evaporative demand would offset the increase in basin rainfall...' what do you mean by this sentence (same as in abstract)? Please rephrase.

p. 1410. l. 16 '...and for the first time, daily GCM rainfall ....'. How did you downscale it? I didn't read anything about this in the manuscript. This is crucial because it can cause a lot more revealing of non-linear behaviour (exceeding of threshold locally in the model) than pixel averaged rainfall. You have to explain this somewhere!!!

l. 22. Typo 'Penman-Monteithmethod'

p. 1411: l. 6.'democratic Congo' I'd rephrase this into 'democratic republic of Congo'.

l. 11. '...total Nile yield', do you mean discharge? If so, please just say 'discharge'. l. 15, 18 and 24. The area and rainfall depth numbers have a great amount of significant

numbers. Are they so certain?

p. 1412. l. 6. Typo: 'has been also obtained', should be 'also has been obtained' l. 13/14: How where these weights fixed?

p. 1413. l. 3-11. Am I right if I say that you compute an inverse distance weighted field of the residuals of station rainfall and a climatology? Is this gauge based field used to bias-correct the satellite estimates? Please explain.

p. 1414 l. 10. Maybe not a good sentence to start with. Now it looks as if the study is all about bias-correcting GCM precipitation. I also suggest to add a line about what the assumptions of the distribution are and make it very clear that this is a temporal distribution, not spatial. For instance, there is an assumption of stationarity of the distribution of the variable and this stationarity persists in the future scenario. I do not understand by the way how you get a fine-scale precipitation field from this approach, it is temporal downscaling, not spatial.

p. 1417. '...because AET is generally satisfied during the wet season', rephrase to 'AET is generally energy constrained during the wet season'. same for line 14. l. 15-16. You increased PET by 10%. Why? Is this not simply due to poor calibration of the hydrological model. You have to justify why this is necessary. And if so, why should you not also do it with the PET values of climate scenarios? l. 27. 'Fig. 4 shows the mean distributions of rainfall', why not 'Fig. 4 shows the climatology of rainfall...'.

p. 1418 l. 22. typo 'except of..' should be ' except for..' l. 26. 'in case of...the ensemble is packed...'. Strange wording, please rephrase, don't use the word 'pack'.

p. 1419. l. 2. 'handled with more care'. They are in the paper not handled differently from other results, so better say 'suspicious' instead. l. 9-12. This sentence is not clear. You test the sensitivity, right? But you already do that in the sensitivity analysis right? l. 17. typo: 'the PET increase in' l. 27-28. Isn't this a justifiable reason to simply exclude these models from your analysis?

p. 1420. l. 17-19. This is not significant.

p.1421. l. 1-2. Do you really mean feedback? Not just relation? l. 18-28. This relation is probably very similar to the moisture constrained part of the Budyko curve. I would try to relate it to that.

p. 1422. l. 1-16. Seems a bit irrelevant. Can you not leave this out?

l. 20-21. Use the term 'energy constrained'.

p. 1423. l. 1. typo 'hydrological model were...' l. 25-29. Put this into perspective of climate - water balance indices such as Budyko.

Figures: Fig. 1. I cannot read the text in the figure. Maybe it is a missing font. Please embed the fonts.Please add a small globe or picture of Africa with the Nile basin indicated on a larger scale. Fig. 2. I already mentioned the use of wrong different units per flux. Please correct this.

Fig.3 This can be plotted much more informative with A Budyko graph.

Fig. 4. Maybe just give all scenarios the same color. It looks a bit chaotic this way and the legend is not very well readable.

Fig. 5. The x-axis is incorrect I think. It looks as if all scenarios produce an enormous increase in rainfall. Is it a multiplication factor you plot here?

Fig. 6. Same as Fig. 4, give all scenarios the same color, except for potential evapo and flow observations. Put them in the same units.

Fig. 8. Dark blue is not visible. Please select a different color.

Fig. 9. Same as Fig. 8.

Fig. 10. What exactly is the difference between the solid and dashed line? The annually averaged numbers can be plotted on a Budyko graph.

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