Hydrol. Earth Syst. Sci. Discuss., 5, S1250–S1254, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S1250/2008/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Use of regional climate model simulations as input for hydrological models for the Hindukush–Karakorum–Himalaya region" *by* M. Akhtar et al.

Anonymous Referee #1

Received and published: 25 September 2008

This is an interesting paper and deserves publishing. Its main contribution is to show the feasibility of runoff modelling in large glaciated or partly glaciated catchments in the Himalayan region, even with rather sparse data. However, there are some general points which have not been elaborated or discussed by the authors, but which need to be coped with. So I recommend that this manuscript undergoes revision and will be reviewed again afterwards. I have two main concerns:

1. Firstly, the authors do not distinguish the general differences of the different type of forcing data. They use (very sparse) measurements, they use downscaled reanalysis

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

data and they use downscaled GCM results. The first two data type do have relation to the actual meteorological conditions, i.e. they represent (though with uncertainty) the meteorological situations which actually occurred in the regions during the simulation period. The 3rd type are climate data information, which do NOT HAVE ANY RELATION to the meteorological situations which actually occurred in the simulation period. That means the daily variations of precipitation and temperature derived from these GCM results do not correlate with the actual conditions. In pluvial runoff regimes (as in most of Europe) this would result in discharge simulations which would not coincide with the measurements. The fact that in the HKH-region the simulated runoff driven by downscaled GCM-results actually DO COINCIDE with the measured runoff (though a bit worse compared to the other driving data) shows, that the daily meteorological fluctuation are not very important for the hydrology of that region. This fact should clearly be analysed and discussed by the authors.

2. Secondly, the authors show in Table 3 and Figure 7, that the precipitation data derived with the 3 methods (CRU, ERA, GCM) do differ VERY MUCH from each other. Actually the differences "even on a averaged monthly time scale" is ca. above 200 %, sometimes above 400 % (Table 3). If one believes in the CRU data (but one should discuss their origin and their credibility / uncertainty for that region), the downscaled ERA and GCM precipitation data just can be labelled "terrible";. On the other hand, this phenomena (BIAS in precipitation from global data) is rather typical. So the authors face the problem, others have been encountered before. They should go more in depth here, e.g.:

ů Show, additionally, the data you have from your (very few, but existing) climate stations.

ů Discuss the validity of the CRU data for that region.

ů Why is the BIAS for the ERA even higher (worse) than for the GCM-approach, as shown in table 3? I would expect the opposite.

HESSD

5, S1250–S1254, 2008

Interactive Comment

ů Please discuss the importance of the bias "correction" procedure. To what data do your refer/target your "correction"? The CRU or the measured data? I think that this actually results in a sort of "calibration" of the precipitation data, yielding a precipitation series which looks pretty similar to the reference series. I expect at least that the seasonality looks very similar after correction. You should show the corrected data, both in a figure and a table. If this is the case, what is the real value of the 3 different data sources ? I expect that the correction is (by far) the most important. The differences in the original data might be levelled out? This would also explain (partly) the relatively good performance of the GCM-downscaled (and corrected !!) data (see my first point).

3. Thirdly: in such catchments with extreme altitude differences, the variation of T an P with altitude is essential, both in reality and in modelling. You did hardly mention the procedure, how this is handled in HBV. HBV has a correction function (simpöle regression) for both variables included (which is another important correction of your met input !!). I expect that you also applied this kind of altitude fitting. However, you did not mention this as a calibration factor when describing the HBV calibration. Or did you not fit these factors. In that case: could you derive it from measurements (as it is recommended) or did you use default or literature values ? I feel that this is another important point.

Some specific comments:

ů Page 867, line 27: a 10-50 km resolution is still far from "ideal" for capturing the spatial variability of rainfall and hydrological processes !!

ů Page 868, line 17-20: sentence is difficult to understand. What do you mean with "direct use"? Does this include a bias correction? In that case I would not term it "direct use"; rather than "use without downscaling, but including bias correction" or something like this.

ů Page 869, line8/9: describe the length and periods of the available data series

HESSD

5, S1250–S1254, 2008

Interactive Comment

ů Page 870, line 9: comment on the source and credibility of the CRU data for that region.

ů Page 871: the bias-correction procedure is essential. Discuss its role (see comments above).

ů Section 3.2 could be shortened. It has been described many times elsewhere. But include the altitude correction of T and P and how you applied it in your study.

ů Page 876, figure 4: maybe it would help to ad maps of the differences in P and T.

ů Conclusions: discuss more reasons, for the relatively good performance of the runoff simulations (glacial/nival runoff regime, large size of the catchments, very strong seasonality, over-valuation of lowflow values compared to high runoff values in your long term averaged NS runoff coefficients etc.)

ů Table 4: why did you not (need to) calibrate alpha and beta value ? Ususally (in pluvial catchments) they are most important. ů Figure 1: improve its quality. ů Figure 5: the mentioning of "areas above 4 mm" does not contain much information. It would be much better to ho real precipitation values.

ů Figure 6: You show the huge differences in temperature, in particular in winter. It would help if you could add the measured values/estimates, even though you have to include the altitude dependence. Do you have any in-depth explanations, are there are such high T differences8 $>$10 C!) in winter? Are there problems with the energy balance of the driving GCM and re-analysis?

ů Figure 7: precipitation problems have been mentioned before. Is there any idea, if these problems are mainly caused by (wrong!) boundary values of by insufficient internal dynamics of precipitation generation ?

ů Figure 8: the differences of the 3 hydrographs are hardly detectable, Can you add some detail pictures on floods and low-flow periods ?

HESSD

5, S1250–S1254, 2008

Interactive Comment

ů Figure 9: The averaging of the 3 different driving meteorological data is not really helpful, because you include data (downscaled GCM) which have no relation to the actual met. conditions, see my first general comment. SO it would be better to exclude here the data an modelling results of the downscaled GCM and the GCM calibrated HBV-model ("HBV-Had").

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 865, 2008.