Hydrol. Earth Syst. Sci. Discuss., 7, C733–C735, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C733/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Sustainability of water resources management in the Indus Basin under changing climatic and socio economic conditions" *by* D. R. Archer et al.

Anonymous Referee #2

Received and published: 1 May 2010

1. General comments

I find this a timely and well-written position paper, with a good discussion of the issues facing the sustainability of water resources in a highly important basin. In my view, it is a good example of how we need to balance the importance of different global and local change stressors on water resources for a good assessment. In this case, putting the climate change factors in a scale where there is enough data as to sustain that socio-economic factors prevail. Or at least there is sufficient base so as to push for more and better data gathering and analysis so as to sustain the claim that climate is the key stressor, at least in the Indus Basin.

The authors give a good account of the pressing needs, the context related to hydrology and water use and increasing pressures on the resource from differing demands. And give a comprehensive look at the challenges. I see this paper as a good example of a preliminary assessment to be used as a template in other under-gauged basins of the world.

However, I do find that there is a general lack of novel scientific ideas, methods or data, which needs to be clarified as per this journal's objectives. I would suggest exploring the idea that, with further review and references, it has potential as an excellent position paper, comment or review paper.

2. Specific comments

Regarding 3 Water Stress, the authors present a reasonable analysis of annual water availability alternative assessments, with advantages and shortcomings. Also, possibly an additional method to consider might be the water footprint approach, which has increasingly been applied also in research.

On the issue of reservoir sedimentation, some mention should be made on the increasingly applied approaches to manage the issue, and whether these are applicable in the IB - see Morris GL & J Fan. 1997. Reservoir sedimentation Handbook. McGraw-Hill, now online, and Palmieri, Farhed, Annandale, Dinar. 2003. Reservoir Conservation, Vol. I, the RESCON approach. The World Bank.

In 4., some referencing is needed, for example on the claims that "locally aquifers are already being drawn down rapidly" (4.2) and "in the Punjab where abstraction exceeds the rate of recharge" (4.4) - no data is provided to back these.

On climate change (5), the comments by Berthier need to be further addressed. Particularly, issues that need to be revisited include: using glacier areal change as a proxy for volume change; glaciers not retreating as signifying no mass loss; and glacier surges in relation to mass balance.

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Also, I would be weary of using a correlation as the only measure of cause-effect if there is no further supporting evidence. This might well be the only data needed, and authors have mentioned drawbacks, but in my view not in a sufficient manner. The authors I am sure are well aware (reading their previous papers) that also temperature is just a proxy for the reason of (snow or ice) melt (and high temp), namely radiation. [BTW if we use temperature, what temperature?: a sustained high period of several days/weeks; over a threshold; over the key melt month(s) or entire summer?]

Additionally, the claim supported in analysis of previous papers that a valley station is representative of mountain weather is debatable regarding extrapolation towards the future (the authors acknowledge this problem as I understand too). This needs to be clarified, since it can be the source of uncertainty not necessarily because of the data not being conclusive but on how it is analysed.

In my view, reference to IPCC predictions needs to differentiate between GCM coarse grid projections in an area with steep physical gradients, and the need of downscaling and regional models, and the degree of confidence on these broad estimates (eg see Buytaert et al 2010 HESS 7: 1821-1848).

Notwithstanding, and actually given the above points and the authors presentation of arguments, it all advocates for more data quantity and quality, approaches for data analysis (eg Lafreniere & Sharp 2003 HP 17: 1093-1118) and hydrological modelling (eg Konz et al 2007 HESS 11: 1323-1339), to link snow and glacier melt to streamflow, above what can be concluded from the data available, which this paper nevertheless makes a valuable synthesis of. As such, it is a welcome contribution.

3. Technical corrections (minor)

p1886, line25: change "over 5000m" to "above 5000m" p1887, line 2: bcm is billion cubic metres I gather

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 1883, 2010.

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