Hydrol. Earth Syst. Sci. Discuss., 9, C250–C252, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C250/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "The implications of climate change scenario selection for future streamflow projection in the Upper Colorado River Basin" *by* B. L. Harding et al.

Anonymous Referee #2

Received and published: 7 March 2012

Summary: The manuscript presents a study that incorporates far more future climate projections, run through a hydrology model, than have been attempted yet for the Colorado basin. Given the importance of this basin, and the controversies surrounding the implications of past work on potential impacts in this stressed basin, the article could be a valuable contribution. Overall it is well written and easy to follow, but I find the interpretation to be missing some important considerations. While there is some solid analysis in the paper, the advantages of using a larger ensemble is not exploited in a way to add quantitative and convincing information to the understanding of uncertainties in projections for the Colorado basin or in how scenarios should be constructed for impacts analysis.

Specific Comments:

1) For regions like this, which have low runoff ratios (from almost zero to 0.3, if basin efficiency, p. 850 line 22, is the same as runoff ratio, which it appears to be), it may be difficult to support the claim that the approximately 5% wettening observed in the BCSD process is negligible (p. 965, line 24). Referring to the simple runoff formulation of Wigley and Jones (Nature, 1985), discounting the direct CO_2 effect, for a runoff ratio of 0.15, a 5% increase in P could result in a runoff increase of 13%, not a negligible number.

2) p. 853, line 16, the use of a large ensemble of 112 projections is presented as an improvement over the recent C07 use of 22. The newer techniques apparently produce equivalent results for the same set of GCMs (p. 864, line 20). However, Table 1 shows that, by using all 112 projections, essentially the ensemble created for this study rewards prolific GCMs with many runs archived. The cited Pierce et al study provides some perspective on this, clearly saying that adding more runs of a single GCM does much less to improve the ability of an ensemble to capture uncertainty than using runs of different GCMs. Furthermore, Pierce et al show that once somewhere around 10-14 GCMs are included in an ensemble, little is gained by adding more runs. Thus, I would not find support for the claims in this paper that a "more realistic weighting" (p. 866, line 1) has been achieved here, or that there has been a "more comprehensive accounting" (p. 866, line 24) of variability. Perhaps a quantitative assessment of this could be done by doing something like using this larger ensemble by randomly constructing ensembles of 10-14 GCMs (not more than 1 member per GCM) and seeing how the variability in projections varies among these ensembles. As it stands, I do not think this large ensemble convincingly provides any better information than the prior studies.

3) The analysis of low frequency variability is interesting, though aside from supporting a warming against using a few projections to inform policy (p. 864, line 14; p. 867, line 26) it seems to be too qualitatively discussed to be conclusive. The Hawkins and Sutton (BAMS 2009) article includes a nice discussion of the internal variability contribution to

projections; their followup article (climate dynamics 2010) shows that for precipitation at 100 years out internal variability is a minor contribution to uncertainty. If the claim here is that it is a much more significant contribution to total uncertainty, then that should be more quantitatively asserted, relative to other sources of uncertainty.

Typos: p. 850, line 18, is -> are p. 856 line 12 a -> an

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 847, 2012.