International Upper Great Lakes Study
Sub-Product Review of

"Climate Change Analysis Peer Review Submission"

By Thorsten Wagener, PhD, A.M. ASCE

30th July 2011

Manuscript: Climate change analysis peer review submission

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1. Are the objectives of the work clearly stated?	2
2. Are the methods employed valid, appropriate and sufficient to address the questions, hypotheses or the problem?	2-3
3. Are the observations, conclusions and recommendations supported by the material presented in the manuscript (e.g., data, model and analyses)?	2
4. Are the assumptions used valid and are the mathematics presented correct?	2-3
5. Is the manuscript well organized, material precise and to the point, and clearly written using correct grammar and syntax?	2
6. Are all of the figures and tables useful, clear, and necessary?	1-2
7. What is the quality of the overall work?	2

Recommendation

A - acceptable
B - acceptable with suggestions for revision
C - acceptable if adequately revised
D - unacceptable

Rating

100 **90-80** 70 60 50 40 30 20 10 0

Comments

A. What is the best/most unique part of the analysis?

A wide range of climate change impact assessments is currently being undertaken in the US and in the rest of the world. The techniques applied have some similarity (e.g. the general use of GCM output), but also vary greatly in actual methodologies applied. The robustness and reliability/credibility of such assessments is currently difficult to assess and it is likely that future advancements (e.g. in modeling resolution) will alter the results – potentially significantly in some regions. The study report shown here has to be viewed in this context. The authors acknowledge these issues and discuss them in detail. Very positive aspects of the report include the comparative analysis using different approaches to regional climate scenario generation (e.g. 2 RCMs and statistical downscaling), and the discussion of issues related to climate change impact assessment and the inclusion discussion of some of the hydrologic modeling issues (e.g. ET calculation) is very nice. Finally, the iterative process of working backward from the hydrologic situation that is most difficult to manage is a good and practical way to recognize the uncertainties in climate change projections and recommendable for wider practice until the uncertainty in the forward propagation of climate change impacts can be quantified better. The issues discussed in this paper are consistent with those in the current literature (e.g. Wagener et al., 2010, Water Resources Research).

B. What is the most critical aspect of the study/analysis? Why?

There are some general assumptions (typical for any study of this kind) that can cause debate. One issue is the bias correction applied and the assumption that this bias correction will remain constant in the future. This is of course a strong assumption, however, it is an assumption often made in studies of this type and therefore defensible as (more or less) current practice. The limited number of historical runs (in section 2) also somewhat limits the study since such models generate (in each run) one possible version of the past and hence there can be some variability of interest. However, this is again often done due to computational constraints and hence not necessarily a problem for a study of this type.

C. Which aspect of the analysis/modeling is weakest? Why? How can it be improved?

It might be helpful to strengthen or adding some text passages that discuss how the different parts of the study connect (the different chapters of the report). Currently the bigger picture gets sometimes lost in details of the studies technical content. Sections 4 and 5 do not have an explicit conclusions section, which might be good to add to provide easier access to the study results. It would also be helpful to have some overall table/list with abbreviations since one can get a bit lost sometimes unless one is familiar with the terminology.

D. Are there any other suggestions that are related to how this analysis may be used more effectively or the results explicated in a more understandable manner?

The discussion of the link between ET and soil moisture in the GCM and LBRM models is a very current discussion (section 3.1). A recent paper by Hirschi et al. (2011 in Nature Geoscience) showed for example that climate models overestimate the amplification of temperature due to soil moisture for central Europe. See also Alexander (2011, in Nature Geoscience) for a more general discussion of the soil moisture – temperature relationship problem in climate models. So it is important to keep in mind that the GCMs are not ‘correct’ either (even at their scale), though the revised version of the LBRM formulation seems more physically based and hence better suited for the studies purpose. There are also some recent studies that show how model parameters of a conceptual-type model such as LBRM are related to the climatic conditions in which they were calibrated (e.g. Merz et al., 2010 in Water Resources Research; or Singh et al., 2011, HESS-D). Some aspects of this issue are discussed in the report though (e.g. page 50).

References

Alexander, L. 2011. Climate science: Extreme heat rooted in dry soils Nature Geoscience, 4, 12–13, doi:10.1038/ngeo1045.

Hirschi et al. 2011. Observational evidence for soil-moisture impact on hot extremes in southeastern Europe. Nature Geoscience, 4, 17–21, doi:10.1038/ngeo1032.

Merz, R., Parajka, J., and Bloeschl, G. 2011. Time stability of catchment model parameters: Implications for climate impact analyses, Water Resour. Res., 47, W02531, doi:10.1029/2010WR009505.

Wagener, T., Sivapalan, M., Troch, P.A., McGlynn, B.L., Harman, C.J., Gupta, H.V., Kumar, P., Rao, P.S.C., Basu, N.B. and Wilson, J.S. 2010. The future of hydrology: An evolving science for a changing world. Water Resources Research, 46, W05301, doi:10.1029/2009WR008906.

Singh, R., T. Wagener, K. van Werkhoven, M. Mann, and R. Crane 2011. A trading-space-for-time approach to probabilistic continuous streamflow predictions in a changing climate. Hydrol. Earth Syst. Sci. Discuss., 8, 6385-6417.

Signature:

Date: 30th July 2011

Comments for Transmission to Authors

- Are the methods employed by the authors sufficient to answer the questions;
- are they being used correctly;
- are the analyses and tests appropriate for the problem at hand; and
- are the derived conclusions supportable by the model and analyses?
- Are there any other comparable methods or approaches that may/ought to be considered, which would provide more insight for the specific task under review?

GENERAL

- It would be good to have a section that brings all the results together using the figures in section 1 (which introduce the study outline). It would help readers if the authors would summarize their results across the whole document.
- Some of the graphs are missing units, which makes it hard to scan the document and quickly compare results within different chapters.
- There are some general assumptions (typical for any study of this kind) that can cause debate. One issue is the bias correction applied and the assumption that this bias correction will remain constant in the future. This is of course a strong assumption, though an acceptable one, whose potential implications need to be discussed.
- The authors could utilize some the recent literature discussing outstanding research questions and issues to support their critical discussion of the current state of climate change impact science (e.g. Wagener et al., 2011, and the references therein).

SECTION 1

- This section could include a brief executive summary of the whole report (incl. results).

SECTION 2

- Page 12: How are parameters a and b estimated for future conditions? Are they assumed constant?
- Page 12: The constant bias assumption made here is often made in many studies. Would the available historical database allow for testing this assumption?
- Page 13: In how far could the problem of lack of correlation change if multiple simulations were analyzed? Is each simulation just one potential historical series of events and hence of limited explanatory value? See also pages 19-20.
- Page 32: Are the changes in NBS attributable to changes in the input for the calculation of NBS?

SECTION 3

- The discussion of the link between ET and soil moisture in the GCM and LBRM models is a very current discussion (section 3.1). A recent paper by Hirschi et al. (2011 in Nature Geoscience) showed for example that climate models overestimate the amplification of temperature due to soil moisture for central Europe. See also Alexander (2011, in Nature Geoscience) for a more general discussion of the soil moisture – temperature relationship problem in climate models. So it is important to keep in mind that the GCMs are not ‘correct’ either (even at their scale), though the revised version of the LBRM formulation seems more physically based and hence better suited for the studies purpose.
- There are also some recent studies that show how model parameters of a conceptual-type model such as LBRM are related to the climatic conditions in which they were calibrated (e.g. Merz et al., 2010 in Water Resources Research; or Singh et al., 2011, HESS-D). Some aspects of this issue are discussed in the report though (e.g. page 50).
- Page 32, Fig. 22: Is the solution to the phase difference problem problematic for projections of future time periods?

SECTION 4

- Page 79: Is there an issue due to the scale difference between the current results and the results of Angel and Kunkel (2010).
- Page 82: The results shown in Table 2 seem very close. Could the percent difference be added to strengthen this point?
- Pages 84 and 85: Why is the model having difficulties in the same time period (figures for NBS)?
- It would be good to add a conclusions section to the chapter.

SECTION 5

- It would be good to add a conclusions section to the chapter.

References

Alexander, L. 2011. Climate science: Extreme heat rooted in dry soils Nature Geoscience, 4, 12–13, doi:10.1038/ngeo1045.

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