Hydrol. Earth Syst. Sci. Discuss., 3, S1481–S1484, 2006
www.hydrol-earth-syst-sci-discuss.net/3/S1481/2006/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Uncertainties in river basin data at various support scales – Example from Odense Pilot River Basin" *by* J. C. Refsgaard et al.

J. C. Refsgaard et al.

Received and published: 16 November 2006

Anonymous Referee #1

1. *Discussion and Conclusions*. The referee recommends that we rewrite this chapter and present things in a more compact and informative style. As an example the referee mentions "what is the real role and value of statistics here?" and continues to state that "the authors appear to be arguing that the large uncertainty associated with assumptions should be neglected, because they have no numerical values for this uncertainty, so it cannot be processed by their statistical methods".

→ We did not intend to argue in this direction. The message we tried to present was not that the uncertainty that we acknowledge our crude assumptions introduce *should be neglected*, but rather that it in practise (if you are not working in a catchment with research instrumentation and similar data quality) *cannot* be assessed by direct, comprehensive statistical methods requiring a lot of data. Thus, we do not agree fully to the sequence of arguments by the referee. However, we acknowledge from the referee comments that our text obviously is not very clearly written. We have therefore rewritten parts of the 'Discussion and Conclusions' chapter.

2. *"There appears to be nothing in the paper about how the rescaled uncertainty estimates could be tested"*. This is almost correct. The only exception to this is the checks on the upscaled precipitation uncertainties from daily to monthly support – see last paragraph in the section on Precipitation Data. We have added a brief note on this in the discussion in the revised manuscript.

3. *p1957, line 23 – odd with a factor two*. The referee mentions that he/she find a likely uncertainty interval for N-concentrations in rivers to be [-50%; +100%] instead of [-10%; +10%] as argued in our paper. It is difficult to comment much on this as the referee does not justify the [-50%; +100%] range. However, if we assume that the uncertainty related to instrument + laboratory analysis is 5%, which is well documented by Rode and Suhr (2007, this issue) then the factor should be 10-20 instead of 2. This appears to us much too high and not at all in accordance with the literature values and recommendations given by Rode and Suhr (2007, this issue). We have therefore not modified our manuscript on this issue.

Review by A. Bardossy

1. *Include description of methods from Journal and Huijbregts (1978)*. We agree

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that the spatial upscaling could have been done in this way instead of using the simpler formulas we have used. We would then have had to adapt similar formulas for temporal upscaling. We have addressed this in a new section 'Comparison with other approaches' in the chapter on 'Discussion and Conclusions'.

2. *Uncertainty of discharge data exceeding 10% for high and low flows*. We agree that discharge usually has larger uncertainty for high and low flows than for medium size flows. Similarly, the precipitation uncertainty depends on the weather type (orographic rainfall has smaller length scale and more uncertainty on an areal basis than frontal type rainfall). Thus in principle, the data uncertainty model should vary with time and/or with the values of the variables in question. We have addressed this issue in the chapter 'Discussion and Conclusions' in the modified version of our manuscript.

3. *Errors of mixed character*. The referee notes correctly that the errors are of mixed character. However, most often these error terms are lumped, and we believe this will most often be the case for studies in catchments that are not instrumented for research purposes. The example given by the referee on evaporation losses and wind losses for precipitation is a good example for this. We have used state-of-the-art research publications from the meteorological society as a basis for our correction of point rainfall data, but these publications (Allerup and Madsen, 1979 and follow up publications) and similar from other countries (e.g. Richter D, 1995. Ergebnisse methodischer Untersuchungen zur Korrektur des systematischen messfehlers des Hellmann-Niederschlagsmessers. Berichte des Deutchen Wetterdienstes 194) actually lump these two error sources in their recommended correction procedures.

4. *Normal assumption*. The referee notes that it would be no problem to extend the methodology to lognormal variables. We agree, and actually the basic methodology (Brown et al., 2005; Brown and Heuvelink, 2006) is applicable for a large

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variety of statistical distributions. We have added a brief note on this in the discussion in the new version of the manuscript.

12.11.2006

Jens Christian Refsgaard

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1943, 2006.