Hydrol. Earth Syst. Sci. Discuss., 9, C6643–C6648, 2013
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Hydrology and Earth System Sciences Discussions

Interactive comment on "Climate change impact on groundwater levels: ensemble modelling of extreme values" *by* J. Kidmose et al.

J. Kidmose et al.

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Received and published: 8 February 2013

Reply to comments from referee (#1) T. Green to Climate change impact on groundwater levels: Ensemble modelling of extreme values

1 General comments

We thank the reviewer for an insightful review of our manuscript with several good recommendations for improving the manuscript.

2 Specific comments

1) The reviewer notes that the term evapotranspiration is used loosely throughout the

manuscript and in some cases the abbreviation EP is used to most likely denote potential evapotranspiration. We agree with the reviewer that the formulation of evapotranspiration throughout the manuscript is not clear. In the study we operate with three kinds of evapotranspiration. The term "evapotranspiration" is used to denote actual evapotranspiration, the sum of water evaporating as a result of interception loss, evaporation from soil surface and transpiration from vegetation. The term "reference evapotranspiration denotes evapotranspiration from a reference surface (well-watered grass). This is the term used for input to the hydrological models and is wrongly named EP in some sections and figures, for example in Fig. 7. This should instead be termed "Er" or "reference evapotranspiration". Thirdly, the term potential evapotranspiration denotes maximum evapotranspiration from a defined land cover with a specific crop. The applied hydrological model (MikeShe) calculates potential evapotranspiration by multiplying reference evapotranspiration with a defined crop coefficient (Kc) for the specific vegetation type. We would like to correct the manuscript with this terminology. The reviewer notes that we haven't accounted for CO2 effects on plant transpiration in estimation of future evapotranspiration. This is correct. Increased CO2 concentrations will decrease the evapotranspiration, but longer growing seasons with longer periods with high leaf area index on the vegetation will have the opposite effect. This has been discussed by van Roosmalen et al. (2009) in a study covering the same geographical region and they concluded that the CO2 effect on evapotranspiration should not be included without also including other effects. As our study focusses on extreme groundwater head events that are linked to extreme precipitation periods rather than to evapotranspiration, we find it justified not to include CO2 effects on evapotranspiration. We nevertheless agree that a paragraph about this issue should be included in the discussion section of the manuscript.

2) It is pointed out by the reviewer that groundwater head extremes have an upper limit in a natural aquifer system (the land surface). If groundwater extremes under any possible climatic conditions could rise to this level, then a bounded solution for the extreme value analysis would be necessary. We agree that this would be the case

under many conditions. However, this is not the situation at the Silkeborg case because groundwater levels are several meters below surface at all of the analyses zones. The reason that high groundwater levels are critical is that the stretches of the motorway is planned to be 6 meters below terrain. Furthermore we have tested different drain-levels and seen that drain flow are negligible even with drains placed 150 cm below surface simple because the saturated zone never rises to this level. The reviewer mentions that the analyses refer to a 95% confidence interval of assumed two-tailed distributions even though the minima are not of interest. It is not quite clear to us what the reviewer means by "minima" in this context. It is correct that we have not analysed annual minimum extremes, which would have required a different EVA analyses – see also the discussion to comments from Reviewer #2. However, this has nothing to do with the 95% confidence intervals for extreme values of the annual maxima heads. Here, we believe that both the lower (5%) and the upper (95%) confidence intervals are relevant to characterise the uncertainty on the estimated maximum events for different return periods.

3) Structural uncertainty of the used model is not addressed in the presented study as pointed out by the reviewer. We agree with the reviewer that this source of uncertainty is significant and would like to detail this untouched area further by discussing its implications on the study.

4) The Gumbel distribution is applied to a dataset because we only have a limited period of data available and because we would like to know the size of a statistical calculated 21, 50 or 100 yr event. Together with estimates of the respective T events, an estimate of the inherent uncertainty of the distribution is presented. This uncertainty, as seen in Fig. 8 and 9 in the manuscript, is significantly larger for estimation of a 100 yr event than for 21 yr event. This is because the dataset used to formulate the distribution support the 21 yr estimate with more confidence than the 100 yr estimate. The uncertainty formulation (equation 3) also accounts for the number of data points used to derive the Gumbel distribution, i.e. if the number of years (n) increases the

uncertainty decreases. We acknowledge that the manuscript must be clearer on the Gumbel uncertainty formulation stated in section 3.4 and would like to update and clarify this section.

3 Technical corrections

7836, 2: Done

7836, 7: Done, following comment 1

7836, 11: Done

7836, 17: Done

7836, 19-21: Done

7837, 12: Allen is replaces by Green

7838, 3: Done

7839, 12: Done, abbreviation changed to MAMSL (meters above mean sea level)

7839, 14 and 15: Done

7839, 15: Rephrased

7839, 19: Done

7839, 23: Done

7840, 2: Done

7840, 4: Done

7840, 20: Both corrections done

7842, 6: Done

7842, 8: Rephrased

7842, 11: Done

7843, 27: We are not sure what is meant by neglecting run-on?

7844, 17-23: New objective function described with better specified parameters. Section 3.1.3. is rewritten.

7845, 4: Section 3.1.3. is rewritten for clarification. 7845, 20: Done

7845, 24: It is correct that the current study do not discusses the effect of different CO_2 scenarios.

7846, 28: Seaby et al. (2012) are now under final revisions so we believe the paper is accepted in time to cite it in the present manuscript. 7849, 2: Done

7849, 17: Done. Pre-Quaternary refers to the mica sand and clay which is deposited before the Quaternary units, or before the glacial units (glacial=Quaternary).

7849, 24: It is certainly correct that clay parameters could be affected by macropores etc., especially the upper formations. Cite specific evidence of this is unfortunately not available.

7849, 27: Corrected (to reference evapotranspiration.

7850, 10-11. It is correct that the ARPEGE model shows results that are quite different from the other models. But this does not in itself make it an outlier. It was not questioned by the reviewers of the manuscript of Seaby et al. (under revision, and we consider the ARPEGE model a plausible outcome

7851, 17: Yes the focus is on the h maxima. All of the uncertainty estimates are also based on the uncertainty of the h maxima estimates. For example, an error bound for the Gumbel uncertainty is calculated. In practice this means an upper and a lower 95% confidence value of the T100 estimate representing the Gumbel uncertainty performed on the 20 annual h maxima values.

7854, 9-12: Corrected

7855, 18: Done

7855, 23: Section rewritten

7856, 7: Done

7856, 20: Done

4 References

van Roosmalen, L, Sonnenborg, T O, Jensen, K H. 2009. Impact of climate and land use change on the hydrology of a large-scale agricultural catchment WATER RESOURCES RESEARCH, VOL. 45, W00A15, doi:10.1029/2007WR006760.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7835, 2012.