



Interactive comment on “On the importance of appropriate rain-gauge catch correction for hydrological modelling at mid to high latitudes” by S. Stisen et al.

Anonymous Referee #2

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REVIEW – Manuscript: Stisen et al., 2012, HESS

On the importance of appropriate rain-gauge catch correction for hydrological modeling at mid to high latitudes

Overall Review

The manuscript presents an analysis of how applying precipitation correction factors affects simulations of hydrological dynamics. The authors applied two specific correction methodologies suited for Scandinavian regions: (i) mean monthly correction factors (standard correction) and (ii) dynamic factors (time evolving and daily time step). These

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corrections were applied to the rainfall fields used to force a hydrological model. Better performances and more plausible values of model parameters were obtained in the case of a dynamic correction. With this study the authors highlighted the importance of having “a correct” input of precipitation, and that automatic calibration of hydrological model are very prone to lead to unrealistic parameters just to compensate for wrong inputs. Despite the fact that the paper is well written and clear, and the results are well presented, I have major concerns about the novelty and generality of the presented study (see below).

MAJOR COMMENTS

1) The introduction is lacking a broader picture of the consequences and importance of the study. How the results obtained for a specific correction method and for a particular geographic area can be of general interest? How other correction techniques compare with the ones used by the authors? A wider discussion referring also to previous literature and stressing the importance of having a, as precise as possible, meteorological input, rather than a very detailed calibration procedure might make the paper of interest to a larger audience. This discussion is only partially presented in the conclusion but it might be highly significant for most of the reader that are less interested in the specific Danish case.

2) Another major comment regards the novelty of the presented study. The fact that the methodology presented by the authors has been already evaluated at the catchment scale (Page 3610. Line 26-28, see Stisen et al., 2011b) in Denmark and that the main contribution of this study is to extend it at the national level is of great concern to me. The Stisen et al., 2011 (VZJ) paper has a very similar structure to the paper submitted to HESS. They use the same precipitation correction methodologies (standard and dynamics), the same calibration procedure of the hydrological model, and they have very similar conclusions. Now, I'm not fully sure the extension of the analysis from one catchment to the entire Denmark is enough for considering this study as “new”.

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MINOR COMMENTS

Section 1.

Page 3609. Line 8. I would suggest the author to refer also to the work of Nespor and Sevruk (1999) that extensively discuss the problem of wind induced undercatch.

Page 3610. Line 16. I would suggest the author to revise the sentence because recent literature (Ryu et al., 2011) suggests that relatively high-resolution estimates of regional and national evapotranspiration might indeed become available in the near future.

Section 2.

Page 3612. Line 2. I would invite the author to separate λ_{solid} from λ_{liquid} as done in the equations.

Page 3613. Line 3-15. The authors made several assumptions for applying the dynamic correction of precipitation, especially as far as concern wind speed. I understand that this is unavoidable for obtaining final results on the basis of available data, however, could the author provide some sensitivity or test on how these assumptions are affecting the correction factor. The authors quote other papers (Vejen 2005; Allerup et al., 2000) but I suppose that it would be possible to provide more explanations on the reasons why these assumptions are justifiable.

Section 3.

I would state upfront that all the simulations and modeling are done at the daily time scale.

Page 3616 Line 5. I think that more than “large model setups” should be “simulations of large areas”, I’m not sure what a large setup is.

Page 3616. Line 19. I think “contract” should be “contrast”.

Page 3617. Line 5-17. Maybe a map of the geological units used in the model will help

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the reader.

Reference

Nespor and Sevruk 1999, Estimation of Wind-Induced Error of Rainfall Gauge Measurements Using a Numerical Simulation Journal of Atmospheric and oceanic technology 16 450-464.

Ryu et al., 2011. Integration of MODIS land and atmosphere products with a coupled-process model to estimate gross primary productivity and evapotranspiration from 1 km to global scales GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 25, GB4017, doi:10.1029/2011GB004053.

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