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Discussions

Interactive comment on "The Budyko framework beyond stationarity" *by* P. Greve et al.

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Greve et al. expand and improve the scope of the Budyko framework by including in a simple way, intra-annual climatic non-stationarity within the Budyko framework, i.e., non-stationarity in intra-annual potential evapotranspiration Ep and precipitation P. They expand the models of Fu, 1981 and Zhang et al. 2001,2004 which accounted for non-stationary conditions (both in the landscape and in time within the year) by means of a parameter (w) which combined with the climatic parameters Ep and P, enabled the estimation of actual evapotranspiration relative to P; ET/P. The authors' new model distinguishes, quantifies and explores in particular intra-annual temporal non-stationarity and incorporates this non-stationarity into the Budyko framework with a new parameter (y0) representing a portion of the always ignored change in water storage of the water budget equation. Nevertheless, the non-stationarity conditions due to landscape changes are mostly unexplored and represented as in Fu, 1981 and Zhang 2001,2004, by a parameter (K) that in reality is similar to (w) but that this time does not include the effects of intra-annual climatic temporal non-stationarity. The reasoning for their new simplified model is clear and direct, and I think the scientific community will make profit out of this scientific product. They show broad knowledge on the nature and interpretation of the Budyko framework and I find the rotation of the supply limit as an intriguing finding.

I think the article should be accepted, but only after the very necessary revisions on concepts, structure and analysis are made. It needs to state from the beginning that it only deals with temporal climatic intra-annual non-stationarity, not with landscape non-stationarity due to water use, land use and land cover change. The authors imply in the beginning that the term "stationarity" only relates to the climate component, but this is not true (See Milly et al., 2008, Science). Landscape non-stationarity is a fact and has been shown to have major implications for E/P at basin scales, I mention some references. I know the authors know this, but they should mention from the beginning what type of non-stationarity they are dealing with. They should mention from the beginning that their advance does not deal with the understanding of landscape non-stationarity.

The methodology is also rather cryptic and confusing. I think they are missing a complete methods section where they explain how they estimate their yo and K parameters at the global scale, boot strapping, resampling, calibration, validation, etc. If they want other scientists to use their model or "framework", they should clearly state what is the procedure to derive yo and K. Importantly, they also need to be more precise in introduction and conclusion on what ways this study differentiates from the former works (e.g., empirical Milly, 1993, Potter and Zhang, 2007, Zhang, 2008; and stochastic Zanardo et al, WWR, 2012) that also tackle climatic intra-annual non-stationarity. In other words why is their work "the robust, theoretical incorporation into the Budyko framework that is missing". Riht now, it is not very clear.

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Their exploration of their model (sensitivity analysis) is very complete and well developed, and the model appears to be robust based on the high correlations at the global scale and for the period 1990-2000. But in order to know really the advance that their models implies for the Budyko framework, it is necessary to show in what way it predicts E and E/P better than Fu 1981 or Zhang 2001,2004 and even better than Budyko's (1956, 1974). They indeed show good correlations between predicted and observed E at the global scale, but how much better than the previous models? For instance, Zhang's model 2004 was able to explain 89% of the variance, how much better is this one then? They should repeat their global analysis with the previous models they mention and compare. I would redo similar Figures 8 and 9 but with the former empirical models and compare.

Please find attached a list with some technical, bibliographical and grammatical suggestions.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C2948/2015/hessd-12-C2948-2015-supplement.pdf

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