Interactive comment on "Uncertainty analysis of a spatially-explicit annual water-balance model: case study of the Cape Fear catchment, NC" *by* P. Hamel and A. J. Guswa

Anonymous Referee #2

I read this paper with interest, because of my recent extensive work involving the Budyko curve.

The goal of the paper is relatively straightforward: applying the Budyko curve in a spatially distributed manner. Strictly speaking, Budyko curve only applies at the catchment scale (in space) and long time scale, because the validity of the Budyko curve arises from the holistic nature of the interactions between climate and landscape properties and their co-evolution and co-dependence. Clearly this is an intriguing question. In the past there has been suggestions that Budyko may even work at the annual scale (to capture between year variability as well) suggesting that vegetation may adapt to the

inter-annual variability of rainfall. Some authors have tried to extend the applicability of Budyko to monthly variations by explicitly including storage change. So, trying to extend the applicability to within-catchment spatial variability is a worthwhile challenge.

Given this background, and my general curiosity about the results, I have some serious concerns about the manuscript/discussion paper.

1. The first concern is one of presentation. While there is no problem with the language, the manuscript is unfocused. I could illustrate it with many examples.

In the abstract it is only half way through the paragraph before the key issue of the paper is even mentioned, and even then it is mentioned in passing. Even after that, it talks about related other issues but never addresses the main issue directly. I would have expected to go straight to the issue and mainly discuss the methods used to test the validity of Budyko and then the results obtained.

If this is indeed the main issue, then this (unintended) obfuscation of the main focus of the study continues in the introduction. I am not against providing some background to the study (so the application of the InVest model to assess ecosystem services is fine), but the reader should be attracted to the main aim of the paper, which is the testing of the Budyko theory for distributed predictions of annual water balance. In this respect, the paper confuses the reader with all this discussion about sensitivity (structural uncertainty, parameter uncertainty etc). They do have their place, but they have be put in the context of the main issue, which is the question whether Budyko applies at the individual patch level (as opposed to the catchment level).

Indeed this confusion continues to the end of the paper. This calls for complete restructuring of the presentation of the paper. Once again, I reiterate that I am not against what is being attempted here, but the authors have an opportunity to bring out in a better some novel aspects of the Budyko theory if they managed to reorganize the presentation.

2. A minor technical point: on page 11005, in lines 20-25, the description of Budyko in the form of $E/E_p = f(P/E_p)$ is certainly correct (it is one way), but it is inconsistent with Equation 1, which presents Budyko as $E/P = f(E_p/P)$. Please make them consistent – if you are using Equation 1 in the rest of the paper, then define Budyko in lines 20-25 in a consistent way.

3. Another technical point: The original equation by Budyko had no parameter, same as the Schreiber, and Turc. However, subsequently several authors introduced 1-parameter formulations, and these include Zhang et al. (2001), Yang et al. (2008). However, to my knowledge Zhang et al. introduced the w parameter to capture the effect of vegetation cover (including human induced changes to vegetation cover). I found the general description between lines 5-15 on page 11006 a bit loose and imprecise. I would ask the authors to correct this for completeness.

4. I can understand the model formulation for distributed predictions, presented in Equations 2 and 3. However this has to be tested in an appropriate way (distributed prediction is easy using these equations, but testing is hard, because there are no distributed observations). What is the strategy that you have adopted? If you have one (you may have one), but it is not presented in a straightforward manner. There is a lot of obfuscation in the methods section and in the results sections – this issue is not addressed head on. I would like a more direct summary of the method in this respect, and a more direct presentation of the results. This is a major weakness of the paper

5. Also, in order to attract the interest of the reader, why is there no presentation of the distributed predictions in one or more of your study catchments? Even if the results are not testable, I would be curious to know what sort of patterns arise, and what the main drivers of these spatial patterns. It seems like you are deliberately under-selling the most interesting aspects of your work, and instead distract the reader by the more technical (boring, but still essential) details of uncertainty. I would like to see some patterns, because your paper is about spatial patterns.

6. Finally, I am genuinely interested to know how in the world you test the validity of the spatial patterns, without the benefit of data. Or may be you do have data? I am not sure – but this aspect is unclear in my mind, after reading your paper a couple of times. Hope you can clarify and justify, directly and not indirectly.

In summary, I do like this paper, and want it published eventually. However, my concerns about must be addressed before publication - they call for better presentation (no major new analysis). The changes, however, are fairly major as it requires for a complete revamping of the presentation, including possibly new figures.