

Interactive comment on “Building a field- and model-based climatology of local water and energy cycles in the cultivated Sahel – annual budgets and seasonality” by C. Velluet et al.

Anonymous Referee #2

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The authors describe a novel approach that combines data mining and modeling in order to represent water and energy dynamics in southwestern Niger. In the manuscript, the authors manage to present a multi-year climatology for water and energy in this region, with the fundamental help of the SiSPAT model.

The paper is generally well written and structured. The authors explain fairly well the aim of new approach, which can serve as a basis for further simulations in different environments. On the other hand, the extensive length of the paper and the very detailed description of methods and results make somehow the paper lose focus in the long Result section, which in my opinion should remain on the very interesting

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procedure of combining model and field observations.

My only general remark is therefore that there are some Subsections of the paper, which are nevertheless quite interesting and do allow the reader to get a deeper understanding of the procedure, that could be rather inserted in Supporting Materials, or an Appendix. In my opinion this would improve readability and help the authors to focus the paper to the novel approach that merges the model and observational data. For example, the sections regarding Water and Energy could be incorporated in a single one.

Specific comments: Page 4755, Line 13 and further: The sentence is too long, and not very clear. It might be useful to split the sentence in two parts.

Page 4756, Line 11-12: In order to make the acronym clearer, it should read: Ground-Atmosphere Interface.

Page 4758, Line 6: It is the first time the authors name the SiSPAT model, and the acronym should be defined. I know that it will be explained later, but it would be useful to introduce it here.

Page 4763, Line 18: It should read “Neumann”

Page 4764, Line 10: It would help to know how deep is the modeled soil column.

Page 4765, Line 12: How is the dry heat capacity estimated? Maybe a reference to a method would help

Page 4767 Lines 1-14: These first lines are not results, and should be moved to the methods section.

Page 4768 Line 10: why must we expect a certain particular uncertainty from these observation?

Pages 4773 and 4774: As I mentioned, these sections could be shortened and incorporated in a single one. The Subsections lack numbers.

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Page 4773, Line 22 and further: maybe the authors forgot a noun near to “absolute high”, “relative high”, and “relative low”?

Page 4777 Lines 25 and further: the first part of this paragraph is a summary of the motivation, in my opinion, and the authors already discussed it.

Questions:

What is the spatial scale at which the model is applied? It is clear how the model deals with vertical heterogeneities in the soil column, but what about possible spatial surface heterogeneities? Are they completely neglected?

Are there possible developments of this approach to not-managed ecosystems in the same region?

One last suggestion is to underline once again in the Conclusions the novelty of the combined "model and data"-based approach, and why it is essential in the construction of the 7-years climatology in the region.

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