Response to Reviewer-1's main comments

We thank very much the reviewer for his constructive review of our paper. As the reviewer rightfully points out, documentation of coupled water and energy cycles in the vast Sahel region is virtually non-existent. All the more so is a climatological description of these processes, that could serve as a robust reference for many various purposes (e.g., studies of land-atmosphere coupling, of catchment and groundwater dynamics, of vegetation productivity, etc.). Whatever the method used, constructing a reliable climatology requires a substantial observation record, from which long-term, continuous series can be derived. This is what permits the unique dataset available at the Niger study site for two land cover types, and was performed here. Field data alone were not sufficient however, therefore an approach associating data and modeling was necessary. The objective of this paper is (i) to present this field & model -based construction of a comprehensive, consistent 7-year series of water & energy variables, and (ii) to derive a first climatological description of these variables through their mean annual and seasonal characteristics. A forthcoming companion paper is scheduled to further analyze the statistical properties of the generated series, as announced in the manuscript conclusion.

In this comment, we would like to further explain the general objective and method chosen for this paper, by addressing the two major issues we have identified in the reviewer's questioning. Detailed answers to the reviewer's more specific comments will be provided in an upcoming comprehensive response, together with proposed modifications of the manuscript in relation to the reviewer's recommendations.
The fact that both data and model components are important in this methodology, but that it is neither a pure model-development nor a strict data-analysis paper, probably makes it more difficult for the reader to readily identify the paper positioning. We will make every effort in the revision to clarify this positioning in the paper introduction.

Focus: Why concentrate on mean behaviour, rather than show the full 7 year series ?

It seems to us that the reviewer's comments raise the question of the necessarily far-from-exhaustive view that can be provided by a single paper, of the water & energy behaviour of two ecohydrosystems submitted to highly variable conditions over a multi-year period. While we agree that the new data are by themselves very interesting – as they contain a lot of original information, compared to the published first 2-year subset (Ramier et al., 2009; Boulain et al., 2009) –, displaying entire 7-year series, even daily-aggregated, would make it hardly legible, but that's not even the main difficulty. The very high information dimensionality (incl. multiple timescales, from sub-diurnal to interannual x large sample x many variables of interest x 2 systems) makes it unavoidable to restrain a single paper's analysis to some selected, strongly-focused viewpoints that can be derived from the series. In this respect, it seems logical to us that a first paper from this long-term dataset would focus on the first-order behaviour of each of the two systems that can be reliably extracted over this period. Together with annual budgets, this is what is done here with the mean seasonal cycle, which offers a robust picture of the variations in behaviour over the various phases of each season, and of the contrasts in this first-order behaviour between the two systems. Year-to-year variations around the mean cycle represent a second level of information, obviously of great interest, but which does not override the first one and could even largely blur it if considered directly, making conclusions with respect to seasonal variations or systems intercomparison more difficult and fragile. Analyzing the variability around the mean behaviour will be the topic of the second paper, which will build on and extend the results of the present one, to produce comprehensive statistical signatures of local surface fluxes.

As suggested in the manuscript, a field-supported climatology should represent an invaluable reference source for evaluating and improving land surface models or remote sensing algorithms. Indeed, testing algorithms against synthetic statistical field descriptors is very useful for assessing their capabilities (Bellochi et al., 2010; Bennett et al., 2010). For example, evaluation of model/product biases - a primary quality control criterion - requires availability of reference means.

We do believe that extraction of the mean behaviour can also be very useful for purposes of systems understanding, although we agree that many further benefits could be gained in this respect from more detailed/specific analyses of the full series' extensive information content. Considering individual years separately or chronological data directly - as it was only possible so far - rather than a climatology, can be misleading in terms of process interpretation, as briefly illustrated in the paper's discussion. Moreover, analysis of a given time slice often relies on characterizing how the various variables compare with their long-term means (so-called "anomalies"), thus requiring knowledge of those means.

Albeit these justifications of a stepwise climatological approach, we can understand the reviewer's frustration that the paper proposes only a fragment of all could be inferred from this unique dataset. However it is a definite constraint of paper formatting that length should remain standard, keeping in mind that this manuscript is already on the upper side. This is why investigation of variability modes other than seasonal was deferred to a separate paper. The need in this first paper to document the modeling part (construction and evaluation) of the study methodology (see next section), necessarily further limits the amount of analytical results that can be presented in this paper.

Method : Why not use the model just to fill in the data gaps ?

To derive synthetic/generic features of the studied systems, continuous long-term series of consistent, complementary variables are necessary, which the field data do not provide directly: the raw data are incomplete over time (10-35% of data missing, depending on variables; highest for turbulent fluxes), do not cover all variables of interest, and are subject to estimation errors and scale discrepancies leading to inconsistencies between variables.

At least two alternative general approaches could be considered:

- the data-oriented approach, i.e. using the observed series to construct a continuous series as directly as possible (the reviewer's suggestion would fall under this category);

- the mixed data/model –based approach, where the data is used to tightly drive/control a model that produces the desired series (includes the paper's approach).

The first approach may seem to offer the advantage of looking both simpler and more reliable, in the sense that it should remain in tighter connection with the observations. However, the above limitations of the field data call for major additional processing steps, that make the approach both less simple and reliable than would be expected, while not providing access to unobserved variables as modeling allows.

In particular, lack of energy balance closure in the data (typically 10-35% of available energy) makes some kind of transformation of the observed series necessary to ensure minimum consistency between variables, but with little rational ground for the modification rules. Given the intrinsic uncertainty in field estimation of flux variables (e.g., turbulent fluxes, which require complex processing including substantial multiple correction steps, or ground heat flux) and their widely differing footprints, observations can hardly be considered as unquestionable field truth for our analysis.

With the data-oriented approach, incompleteness of field estimates over time would make it necessary for our purposes to fill in the gaps with synthetic values. Standard, "black-box" gap-filling techniques (Moffat et al., 2007) can be considered for sparse, short and low-density gaps, but this is hardly the case for long-term series as we are interested in here. The reviewer advocates that the physically-based model we developed in this study be used to replace the missing data only. We do agree that physically-based modelling is undoubtedly and by far the most reliable approach to the missing data substitution problem. However, given the magnitude of gap rates and the non-random distribution of gap occurrences, associating into a single series data of two different origins – field estimation and model – would produce a heterogeneous sample that could be a source of significant biases in statistical inferences from that sample. Relative weights of the two data sources would vary strongly over the average year, leading to a mean seasonal signal potentially distorted by the hidden uneven abundance/lack of observations.

Our opinion is that truly meaningful and interpretable results can only be obtained with the second, mixed data/model approach, that recognizes the value of both sources of information into a time-homogeneous procedure ensuring unbiased statistical estimation. Like A.D. Richardson (http://www.oeb.harvard.edu/faculty/richardson/data_fusion.html) and many others, we believe that by merging models and observational data constraints we can improve upon model-only or data-only analyses. In our study, the tight model constraining by the data was achieved both through calibration and through model application only to years for which posterior control was possible as validation against available data – showing excellent agreement as underlined by the reviewer. Compared to the first approach, this more explicit, homogeneous procedure (even if model-constraining by the data may not be totally uniform over time, in relation to data availability) avoids arbitrary data correction and substitution steps. It makes all sources of information (different types of observations, physical laws, process knowledge, …) work together synergistically in an integrative framework, rather than in a fragmented way. It gives access to fully-consistent series of observed AND non-observed variables, to apprehend the whole water and energy cycles. Unlike an essentially model-oriented approach, observations really are central to our approach, as only intensively monitored years are considered, thus avoiding any potentially hazardous model extrapolation.

What determined the choice of the SiSPAT model for this purpose is its capacity to represent the main processes governing the studied systems, as testified by cited previous experiments in the Sahel context (ex.: Braud et al., 1997), as well as its ability to integrate the various available data types, for model forcing and evaluation. The very high model skill confirms this choice was appropriate. Note that the importance of the modeling component in this study methodology justifies in our view devoting sufficient space in the paper to specifically document this part of the work, in particular with a subsection of the Results section dedicated to model calibration and validation results.

Because these methodological aspects were viewed as important issues, they were included in the paper discussion (section 4.1.2 "Model vs. data").

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