

Interactive comment on “Verification of the new ECMWF ERA-Interim reanalysis over France” by C. Szczypta et al.

Anonymous Referee #1

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Although the evaluation of products like ERA-Interim over well-monitored areas such as France is very valuable and should be documented, this study is not yet mature enough to be published. Apart from a necessary revision of the document structure and language, an attribution of discrepancies between the various data sets and the effects on the various terms in an offline land surface simulation should be included. Some in-depth analysis of the correlation between the bias and a number of suspected environmental conditions should be included, such as bias versus signal, bias versus altitude, distance to coast, surface characteristics, cloud cover, As it is presented here, a list of differences between products and simulation results is presented without any judgment of where these differences come from, and/or how large the differences can be expected to be based on aspects like station representativity, natural variability,

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methodological differences etc. Without this expectation the reported differences have only little sense, and can not be appreciated fully.

In addition, the ISR comparison methodology is not unbiased. In the reference “Brion” data set a portion of SAFRAN data is blended, after which this blended data set is compared to SAFRAN again. The rationale of this blend is not clear to me. It is better to use a raw Brion dataset and attach uncertainty estimates to stations that are suspicious, or remove them from the comparison data set.

Also it is not clear why only two years of data were used. All data sets that are being compared at the level of France contain at least 7 years of data, as far as I can see. It is claimed that the two years are very different, but no attempt is undertaken to use this difference in order to explain the biases.

Also the evaluation of different time slices for the SMOSREX data (1990 – 2005 for the forcing data and trend analysis, 2001 – 2007 for LAI/soil moisture, 2005 – 2007 for turbulent fluxes) is quite unclear and not well justified in the Methods section.

Some specific remarks (I haven't tracked all typos and language corrections – there are many):

7153-26: what is denoted with the “. . .”?

7154-17: delete “The precipitation . . . are described”

7154-21: delete “a long”

7155-2 (and other places): Precipitation is not a “parameter” but a “variable”

7155-7 (and other places): PERSIANN is introduced here but very rapidly discarded because of a large bias (7163-5). Either don't include it at all in your analysis, or try to understand the background of this bias.

7155-1: the section “precipitation data” contains a lot of text about the various data sets (SAFRAN, ERA) not only addressing precipitation. Reorganize as

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“2.1 Structure of data sets”

“2.1.1 SAFRAN”

“2.1.2 ERA-Int”

“2.1.3 Other data products”

“2.2 Precipitation”

“2.3 ISR”

“2.4 T, q and u”

7156-7: as far as I know ERA-Int also contains a 2m temperature and humidity, diagnosed from the 10m level. Why don't you use this, or why don't you mention it at all? By the way, Pitman and Perkins have already evaluated temperature quality in reanalysis data, and should be cited (A. J. Pitman, S. E. Perkins: Global and Regional Comparison of Daily 2-m and 1000-hPa Maximum and Minimum Temperatures in Three Global Reanalyses; Journal of Climate 22, 4667-4681)

7156-13: “This method extracts the information. . .”: a vague phrase. Do you mean that you aggregated all data to the 2.5 degree resolution?

7156-25 and 1757-9: the political justification of GPCC etc can be deleted in this paper

7157-15: “substantially the same” -> “essentially similar”

Section 2.3: why don't you look at nighttime T and q? The diurnal cycle (and biases therein) are very interesting (see also Pitman and Perkins)

7159-11: delete “Although wind speed. . . From SAFRAN to ERA-I.”

2.4.1: too little information is given on the available data and time ranges. Flux measurements are not mentioned here, but used later

2.4.2: a discussion of the comparison metric must be included. How are you going

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to evaluate? When do you conclude the difference in forcing is significant? How do you attribute causes of differences? There are many ways to “compare” data sets. Be specific about your way and the purpose of it.

7161-15: delete “In order to assess . . . too”

7151-20: define a new acronym for “ERA Interim rescaled”, too much to repeat it all the time

7161-20: start new para at “Fig 3”

7162-5: “more important” -> “larger”

7162-13: rephrase as “Fig 5 . . . correlation of monthly values of . . . of 2001.”

7162-25: from a single years evaluation you do not expect a clear seasonal pattern. That's why a multiyear evaluation is to be preferred strongly!

7163-10: “less important” -> “still smaller”

7163-19: the “unbiased” Brion reference data is not unbiased as it contains SAFRAN information.

7163-22: “are very similar”: I don't see this. ERA-I clearly has higher values during Feb-Oct than the others

7164-7: move this part to the “Methods” section

7164-19: “greater” -> “higher”

7163-20: “weak” -> “small”

7165-2: “No correlations found”: what is the explaining variable here? Is it just noise? Please explore more in depth where the differences come from.

7165-6: Suddenly vertical profiles are introduced here, while lines 5-10 should be discussed in section 2. I don't know what is meant here.

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7165-18: the difference in bias between 6 and 12 UTC implies that ERA-I suppresses the diurnal cycle. It is important to check the behaviour at 18 and 00 UTC as well, and try to explain why this diurnal cycle is so off. Further in this section it is explained that the smoother topography can be held responsible for the bias. How does this work? Why would a smoother topography induce a diurnally varying bias?

7166-2: here an overestimation of ERA at 12 UTC in mountainous area is reported, while earlier an overestimation at 6 UTC is stated. This seems inconsistent.

7166-4 to 8: the argumentation is very unclear here, please rephrase

7166-10: why does a difference in altitude affect the humidity bias? Is there such a strong humidity gradient in this lowest atmospheric layer? What is the reason for this gradient? Please provide some more physical reasoning behind the observed biases.

7166-23: strange that in 1990-2007 no temperature trend is observed, while all climate change assessments show a clear positive temperature trend over all European land areas in this period. What significance level was taken?

7167-6: the section should not be called "Discussion" but "Results of offline model simulations"

7167-10: I assume you mean that the ERA-bias is "larger" rather than "more important". Yet, earlier you concluded that ERA has a higher correlation with Brion than SAFFRAN, so this argument is not straightforward to me.

7168-17: it is strange that soil moisture is so sensitive to the precip forcing, but latent heat flux isn't. From the table also H seems to be affected a lot (strong difference in mean bias). What is the physical rationale behind these findings?

7168-22: move this section to "Methods"

7170-25: you conclude that precipitation is a major factor on the biophysical variables. Yet, a comparison between figs 9 and 10 shows that for two simulations with the same

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precipitation the LAI is very different for different ISR/T/q/wind forcings (SAFRAN with ERA-rescaled versus ERA-rescaled). This means that the other forcings are the most important ones.

7170-21: why don't you mention that there is a mean bias at 6 and 12 UTC?

7175: in Table 2 (and in many other tables) too much detail is given, while an aggregated value (annual mean for instance) is lacking.

Table 3: remove "PERSIAN" from the entire analysis

Table 7: there is only one significant digit used in the CO₂-flux column. Is that the maximum allowable accuracy?

Figs 4 and further: the numbers in the legend are too small to be readable. Why is an absolute rather than a relative bias used for precipitation?

Fig 6: R_g is unexplained

Fig 8: swap the upper right and lower left panels to match the layout in fig 4. Is the low bias in the central part of France in the lower left panel due to the data blend between Brion and SAFFRAN? Please explore further in the main text

Fig 9-10: the year numbers are missing on the horizontal axis

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