

## ***Interactive comment on “Modelling LAI at a regional scale with ISBA-A-gs: comparison with satellite-derived LAI over southwestern France” by A. Brut et al.***

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This manuscript inspects the land-surface model used by Météo France as a diagnostic model driven by atmospheric forcing fields from numerical weather prediction (NWP) analysis.

Testing model performance is an important component in development and improvement of complex models, although the pure scientific merit of such activities can be debated. My view as an invited reviewer for this paper is, that the primary goal of sci-

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entific publication is to make one's own new insight into a scientific issue available to others. Whether this is a big step or a small step in overall scientific knowledge gain, and whether it is of global, national or local importance is in my view not the key question. The scientific correctness of the work however is. Based on such considerations, I suggest acceptance of the paper with the following modifications:

1. It is not quite clear how the ISBA-A-gs is used in operational mode by Météo France. From the manuscript it looks like a one-way top-down approach without feedback in the reverse direction (from surface to atmospheric model). This may be my interpretation based on the lack of information in this respect. Suggestion: add a paragraph about this to make clear whether the model is intended to be an **add-on** to derive CO<sub>2</sub> fluxes without feedback mechanisms in the NWP model, or whether the default mode is that it is an interactive component of the NWP system used by Météo France, including the feedback mechanisms (probably not CO<sub>2</sub>, but energy budget?).
2. One major shortcoming of this paper is that it compares one model output (from ISBA-A-gs) with other model outputs (MODIS, CYCLOPES), whereas only one single surface site is used for ground-truthing (Section 4), and it appears that only eddy covariance fluxes have been measured there, but not LAI or surface phenology. Especially with respect to the importance of this aspect (Figures 6–8) it should stress out the importance of the lack of surface phenological observations for this model validation. A difference between ISBA-A-gs and MODIS (Figure 8) does not provide any information on which of the models (satellite image driven as in MODIS or NWP output driven as in ISBA-A-gs) is closer to reality. Both could be way off, and the reality in fact need not necessarily be between the two values.

I have also checked how the technical issues were resolved by the authors (the reviewer feedback before the paper is published in the discussion section). I found that

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the relevant issues have been rectified, namely the use of consistent terminology for the land surface classes.

Minor issues that I still recommend to revise:

In Figure 8 the zero-axis is identical with the boundary of the graph which obscures the conditions around zero. A certain margin (some modern statistical programs use something like 4% of the axis) should be added to see the zero values.

Moreover, the line thickness in the boxplot is quite thin for the print version. Suggestion: just use black color (not blue for boxes and red for outliers).

A third issue that I do not feel very happy about is the two different map projections used by the authors. In Figure 1 the upper map uses an azimuthal projection (or similar), whereas the lower map uses a Mercator projection (at least I assume this, since it looks similar to Fig. 7). Thus, the margins of the domain in the lower map shown in the upper map with the azimuthal projection is incorrect. The sides should be parallel to the meridians, and the longitudinal margins should be curved in the same way as the  $40^\circ$  and  $45^\circ$  lines.

If the authors have the possibility to display all maps with a more reasonable projection than the Mercator projection (which enlarges the cells in the North relative to the grid cells in the South of the domain), then I would encourage them to do so.

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