

# ***Interactive comment on “Limitations of WRF land surface models for simulating land use and land cover change in Sub-Saharan Africa and development of an improved model (CLM-AF v. 1.0)” by Timothy Glotfelty et al.***

## **Anonymous Referee #2**

Received and published: 10 February 2021

In this study, Glotfelty et al. assess the ability of several land surface models to simulate the surface characteristics of sub-Saharan Africa, within the WRF regional numerical weather prediction framework. They find that the default models do a rather poor job in the region, in terms of, for instance, albedo and leaf area index. Consequently, they develop a new CLM land surface model variant, wherein they have improved the representation of such surface properties. Following, they perform a land use/land cover change experiment to highlight the applicability of the new variant, since they show how the meteorological proficiency of the default models hides problems in surface pro-

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cesses, which can lead to flawed conclusions in such experiments. I agree with RC1 that the topic is within the scope of GMD and that this work is relevant not only for the WRF modelling community, but also for land use/land cover change studies focusing on Africa and the Southern Hemisphere. I also find the manuscript to be well outlined, with a clear motivation, sufficient methods, and an in-depth analysis. However, I think it could be improved by addressing the following specific comments. Additionally, I list some technical details that I think should be corrected.

### Specific comments

1. After reading the major comment 1) in RC1, it is clear to me that authors need to improve a few things in Section 4 to better explain their experimental setup. I agree in Section 4.1 they should address RC1 comments, and justify why a single-year validation is enough for the scope of the study, and why it is desirable it is not during an El Niño–Southern Oscillation phase. Maybe a reference to a similar study could help, or stating explicitly the proof-of-concept nature of the validation. Perhaps avoid use the word "validation", in case other studies have validated these LSMs in Africa.

2. Then I think Sections 4.2 and 4.3 are failing to explain the time period simulated. The comment in RC1 confuses me, because what I understand is that simulations are for boundary conditions of period 2010–2015, all of them, only that in LU01 land use maps are from MODIS 2001, and in LUD land use maps are from Dinamica EGO results. Is this right? If so, then comparison between LU01 and LUD is done subtracting 6-year (2010–2015) averages. Is this right? Authors should explain why this time span was chosen. I think the different time periods in Section 4.3, to explain the Dinamica EGO methods, may confuse the reader. Why mention year 2050? Why add in line 317: "and to 2050 for simulation purposes"?

3. Minor comment 2) of RC1: does it perhaps have to do with the fact that SWDOWN

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is compared with CERES and CF with MODIS?

4. I think also a table with the data used from ERA-Interim in the Supplementary Material could be helpful.

5. I agree with RC1 that the role of Noah-Sat in the study is not explicitly stated. I understand it is used as a proxy for observations in Section 5, and that in Section 6 it helps assess the parameterizations in Noah and Noah-MP, since the surface properties should be ok. But I wonder if Section 2.1.1 could be improved to better explain these reasons why Noah-Sat is included.

6. I agree with RC1 that it is easiest to assess performance with maps of differences (e.g., Figs. 2–5, and Figs. 7–9).

7. Is this the first assessment of all these LSMs in Africa using WRF? If so, state it somewhere. If not, include references.

8. In the end, it was not clear why the regionalization of Africa occurred. In Section 4.3 I learn that it is for the Dinamica EGO predictions, however in Section 3.2 I was told it was for the LAI monthly profiles. Is it both? If so, connect the two ideas.

9. The study prefers to use Dinamica EGO output instead of MODIS directly to reduce "noisiness". Is this a usual practice? Is there some previous reference?

10. How different is the "default" land use in the validation and the MODIS 2001? Is "validation 2013" very different from "year 2013 of LU01"?

11. Is Fig. 10 similar to other results of LULCC studies in Africa?

12. I do not know how to interpret Table 4. Please explain what is "First Region" and "Second Region".

## Technical details

1. As pointed out in RC1, some acronyms are not explained. I point out RL. Also in Table 9 it says WS10, but in text it is used as WSP10.
2. Abstract: "climate signals" is used too often. I wonder if "illogical" is the right word.
3. Text sections in supplementary material should be labelled, to be referred to from the main text easily (e.g., ST1, ST2).
4. Check the use of "en dash" for ranges of numbers (e.g., domain ranges).
5. I would stick to "DJF" for winter season, rather than "JFD".
6. Authors prefer the use of block titles for referring to panels in figures, instead of using labels (e.g., (a), (b)). It is ok, but I think the font size could be reduced. Also figures should use the same font types (titles are serif, but coordinates are sans-serif).
7. Tables are missing punctuation signs in captions and footnotes. I also think that tables with a lot of text are more readable if left-aligned (raggedright).
8. Sometimes too many references are used. For instance, the number of applications of Dinamica EGO has 12 references.
9. Figure captions in supplementary material say "verses" instead of "versus".
10. I wonder if the inset boxes in all maps with extreme and mean values are really being used. I like the idea, but they are not being used in the text or discussion.
11. Is Table S3 really necessary? Maybe only referencing Friedl, et al. 2002 is enough. By the way, this reference is nowhere in the references list.

## Line comments

- L38: through A land surface model
- L39: different SIMULATED climate responses
- L42: and IN the PRESCRIPTION OF
- L47: with WIDESPREAD surface heterogeneity
- L62: Air Force AND Hydrology Lab (Noah)
- L63: reanalyses (maybe plural?)
- L86: parametrization or parameterization?
- L95: each year, EVERY
- L96: model year is SIMULATED
- L115: why "In"? Maybe only: "Noah and Noah-Sat are"
- L127: also? why also?
- L153: "(Sellers 1985)", check citation format
- L173: why not "PFT distributions" instead of full name?
- L174: "3 arc minute": a magnitude and a unit
- L180: why not: categories 2, 4–10, 12 and 14
- L183: "100%" instead of "one hundred percent"
- L203: spacing: "Fig. 1", and "). These"
- L204: parametrizations or parameterizations?
- L210: "80%" instead of "eighty percent"

- L218: "60%"
- L219: "60%"
- L222: "satellite-derived" (hyphen)
- L276: why not LULC?
- L277: experiment name is LULCC (double C)
- L283: remove "simple or complex"
- L289: "500 m" (why say "meter")
- L292: I wonder if "ingested by" is the right word.
- L327: "(Hagen 2003)", check citation format
- L336: performance IS shown ("a list", singular)
- L355: Figures 2–4 (because it starts a sentence)
- L378: I would not reference Fig. 10 so far in advance.
- L387: This leads TO an overall
- L427: dataset INDICATES that (closer inspection, third person)
- L442: "LSM, but provides" why but? maybe "and"
- L443: wettest?
- L448: "a more muted" maybe use "reduced" or synonym
- L450: remove "select" and "very"
- L459: wet instead of "wetter"
- L478: Dinamica EGO
- L524: patterns

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Figs. 5, 7 and 9: I would add "Year 2013" to start the caption.

Fig. S11: has low text quality.

Table 9: It says Dowelling, instead of Downwelling

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-193>, 2020.

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