Review of "Modelling of Multi-Frequency Microwave Backscatter and Emission of Land Surface by a Community Land Active Passive Microwave Radiative Transfer Modelling Platform (CLAP), by Zhao et al.

Reviewer: David Chaparro

This paper presents a multifrequency active-passive transfer modelling, named CLAP, for simulating backscatter and emission signals of a vegetated (herbaceous) surface in the Maqu area in Tibet. Results are compared with ground-based radiometer and scatterometer measurements, and with AMSR2 data. The authors explore how simulations using different frequencies (L-, S-, C- and X-bands), vegetation configurations (cylinders vs. discs), seasons (winter vs. summer) and soil moisture sources (*in situ* vs modelled) track radiometer and scatterometer observations. With this, they aim at enhancing our understanding of vegetation, soil, and temperature impacts on daily variations in emission and backscatter.

The paper is well written and encompasses a wide range of characteristics influencing model outputs, thus being complete research. The work improves previous papers estimating microwave emissions by including the backscatter component in the outputs. Overall, it is research with good potential, but I have relevant major concerns that must be thoroughly addressed through the manuscript, especially regarding that: (i) the metrics used in the results should be extended, (ii) it is needed an extended discussion based on literature previously mentioned by the authors in the introduction, and (iii) the paper needs putting the results obtained in the context of the current and future missions and retrievals (e.g., which could be the impact of the reported RMSE and Biases in SM retrievals?). The third point is especially important, as the authors emphasize the applicability of the model stating that it can mimic observations and that it could be applied to global scales. None of these are fully justified if the reader cannot understand how CLAP could be applied: how can it be transferred to global scale? Which could be the impact of model errors if it was to be applied in moisture estimates?

I detail my comments hereafter.

Major comments

- The results presented are based on RMSE and Bias metrics between the estimates and the in situ, as well as on time-series plots. However, as the authors focus on the study of the daily variability, correlation metrics are needed. Also, optionally, scatters between *in situ* and estimates can be plotted next to the time-series (instead of in separate figures; note that the number of figures in the paper is very large and should not be increased).

- Lines 120-132, and 142-145, in p. 5, provide an interesting state of the art which is used as the basis for this paper. The manuscript will improve a lot if the authors extend the discussion explaining how the current manuscript improves previous literature. For instance (among others) the authors could address the question: "which are the improvements if compared to Zheng et al. (2021) and Dente et al. (2014)?"

- The authors conclude that the model is able to track well the observations in many cases (especially for cylinders and in summer). Still, the results show some differences between estimates and observations. In that sense:
    - In Figures 2 to 5 and Table 3, even in the best cases (cylinders and X to S frequencies) the errors reach RMSEs between 1 and 4. As the authors are presenting this model as potentially applicable for future missions, how would an error like this impact soil moisture and optical depth retrievals? Based on either literature or observations, the authors should discuss which is the impact of this error and if it is small enough to allow the applicability of the algorithm.
    - Table 4: similar to above. RMSE minimum values are 5.9 and 2.4 at H and V polarizations. Which would be the impact in soil moisture simulations? Is the conclusion of CLAP "mimicking the observations" consistent according to these results? Why? Maybe, the problem is that affirming that the model reproduces well the observations is subjective if there is no reference for what is "good" and what is "bad" (in terms of amount of error). Can the authors provide reference or thresholds of errors to justify why their affirmation of CLAP mimicking the observations is true enough to allow the model applicability?
    - Similar problems arise in Figures 7 to 14, but in this case they are well justified in the discussion.
    - Similarly, the following sentence would need justification answering the question: "why are the errors low enough to affirm that the CLAP is reproducing or close enough to observations?":
        - P. 29, l. 508-509: "In short, the observed co-polar σ_{pq} and its diurnal variations especially at VV polarization during the winter period can be reproduced…".
    - L. 515-516: Figure 15 shows differences between disc/cylinder and observations of around 50K in H and 25K in V. Based on this, at L-band, the affirmation that the model is reproducing the observations is not true. Please review the sentence and derived conclusions through the paper.

- In some sentences, the authors derive conclusions or potential applicability at global scale, which is not demonstrated by the results. These sentences should be rephrased:
    - L. 686-687: even if it is shown in the results that combination of frequencies is not enough for constructing a homogeneous time-series of τ, it cannot be concluded that this would happen globally. It could be said that this is the case for the study area and maybe in other grasslands.
    - L. 760-761: suggesting that CLAP can be applied at a global scale is maybe premature. The model has potential, but I suggest saying that larger scales (up to global) should be studied in the future. Let it open as a future work rather than an affirmation.

<u>Minor comments</u>

- Title: can you explain briefly in the introduction why the word "Community" is used as part of the name of the algorithm?

- L. 26: "simulate both ground-based and space-borne". This sentence may lead the reader to think that two different simulations (one for ground-based and the other for space-borne) are built. I think that the differentiation in these two types is more appropriate when you talk about the sensors used for validation.

- Abstract & introduction: I suggest being specific from the beginning of the paper stating that this analysis is conducted in soils covered by herbaceous vegetation.

- L. 45 – 47: following my major comments, reevaluate if this conclusion can be driven from the results. It should be further justified.

- L. 55: instrument → instrument**s**

- L. 59-60: maybe add the soil texture here?

- L. 75-77: in addition to the Steele-Dunne paper maybe you want to add further references referring specifically to agriculture. Some suggestions: Patton & Hornbuckle (2012), Hornbuckle et al. (2016), Chaparro et al. (2018), Mateo-Sanchís et al. (2019); Weiss et al. (2020).

- L. 87-89: with some exceptions, such as the Multi-temporal Dual Channel Algorithm (MT-DCA).

- L. 94-95: I cannot understand clearly what you mean with the first sentence of the paragraph. Please rephrase or remove.

- L. 141: after "Wang, 1987), a comma should follow instead of a full stop.

- L. 153: "temprature" → "temperature".

- L. 216: temperature → VWC

- L. 225-226: "the simulated data during the winter period is focused, as we assume…". Review the structure of the sentence.

- L. 226-227 & Fig. S2: at 2 cm, the simulated soil moisture fluctuates much more than the observed SM. Even if we do not have 1 mm observations, it is reasonable to think that the same "excess of fluctuations" could happen at the skin layer. How can this impact the results?

- Fig. 1: do you mean "Rayleigh-Jeans" instead of "Ryleigh-Gans"?

- Fig. 2b: the dashed line is not in the legend.

- Fig. 4b: the high variability of the observations between 31-7-18 and 5-8-18 is not captured by the model, which in general (also in other figures) shows a very regular, sinusoidal behavior, not capturing extreme deviations such as these ones. Could you discuss why this happens and its implications, please?

- L. 457: Fig. 8 → Fig. 9
- L. 458: Fig. 13 → Fig. 10.
- L. 458: "simulated those" → "those simulated"
- L. 480 and 482: Table 5 → Table 6.
- L. 540: for further comparison of the albedo values, Baur et al. (2021) could be an interesting reference.
- L. 545: "suppressed". Instead, I would say "reduced".
- Figure 16: for readers who are used to see VOD (τ) satellite retrievals which are not polarization dependent, values of up to 5 in τ for a grassland are absolutely out of the expected range. Is this expected in the vertical polarization? Why?
- L. 670: wavelenth → wavelength
- L. 689-690: is this sentence incomplete?
- L. 694-695: you could add the reference Jagdhuber et al. (2022) as an example, optionally.
- L. 702: in line with my major comments, affirming that CLAP has been proven as a key tool for understanding is too much if no impact of the errors in the potential retrievals is assessed.
- L. 706-708 and 758: SCOPE has not been presented or explained before in the paper. I suggest removing it or explaining previously.
- L. 717-725: review specifically this paragraph according to the major comments.

References

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