<u>Review of the manuscript "SMOS L-VOD shows that post-fire recovery of dense vegetation is slower than what is depicted with X- and C-VOD and optical indices" (second review)</u>

I thank the authors for addressing part of my questions in the first revision round. I still have some major concerns with some of their answers, which should be addressed to improve the paper. The main issue is that the study is probably biased towards more humid regions (with drier ones such as the Sahel, the Mediterranean and Australia being under- or not represented). This is important because the consistency of the main conclusion (that recovery is slower as seen by L-band) is likely to be dependent on the climate and vegetation types (i.e., wetter regions have denser vegetation and require applying L-band VOD instead of C/X-VOD or VIS-NIR indices because L-VOD has greater penetration capacity). I detail my comments hereafter:

<u>Major comments</u>

1. As the authors say, the number of fires is still low in the Sahel (due to the thresholds chosen). I also think that it is still low in the Mediterranean regions. It is important to include these regions in order to check whether your main conclusion holds in all cases or only in dense vegetation and/or mesic/humid sites. I think that this should be addressed in the manuscript. Can the authors provide a sensitivity analysis of the results according to these thresholds? In other words, how the changes in these thresholds impact the results? This could be addressed globally, or at least for the Sahel and Mediterranean regions. Should the authors adapt these thresholds in these regions? This should enable the inclusion of Sahel and Mediterranean regions. As the fire regimes are different around the globe, in some regions it may make not sense to keep the same threshold than in other regions.

2. The results that the authors present comparing with and without Australia are interesting and show potential to improve the manuscript. I understand the exclusion of Australia, which is justified by (i) impossibility of a postfire study after 2019-2020 fires in forests, and (ii) the inclusion of the Outback fires unbalances the distribution. In that regard, point (i) has not potential solution and I agree that we need to wait until enough data is available. Regarding point (ii), I suggest two possible solutions:

   a. The authors could choose a random subset of Australian shrub fires (with a balanced sample in comparison with the rest of fire events in the same land cover worldwide). Then include this subset in the global analysis.
   b. The authors may leave the global analysis without Australia, but at least they should include a specific analysis of Australia in the supplementary and suggest possible interpretations. Not only because of completeness, but also because the results shown in the revision are interesting! I suggest some ideas hereafter:
      i. The anomaly of temperature before fires when including Australia is lower. A possible interpretation of this is that, in dry regions and seasons (e.g., Australian summers), the occurrence of a large number of fires is not strongly linked to a positive temperature anomaly. Dry and warm conditions are normal for the season in that region, which means that standard summer conditions in the region are prone to wildfire ignitions most of the years. We found a similar result for summers in the Iberian Peninsula in Chaparro et al., 2016, where positive

temperature anomalies were helpful to predict fire ignitions mainly out of the summer season (i.e., we do not need anomalously warm summers in the Iberian Peninsula to be under high risk of fire ignition; the same should be expected in Australia, so I think that your results are consistent).

ii. The increase of the C- and X-band anomaly before fires could be caused by an accumulation of fuel (more biomass in shrublands). This is consistent with the fact that C- and X-bands could be more sensitive to biomass in open and small shrublands than L-band (due to lower penetration and lesser soil contamination of the VOD signal). But this should be further explored, maybe also using absolute values (not only anomalies) of P, T and EVI in the region, to understand if this interpretation is consistent.
iii. The authors find slower recovery in Australia. Is this due to a drier and warmer climate (i.e., more difficulty for recovery) than in other regions analyzed in the study? Considering that the Sahel and the Mediterranean are under-represented, this may also indicate that the study is shifted towards more humid climates, which is a reasonable conclusion looking at the fires' distribution map. This reinforces my argument in comment 1 (i.e., drier regions such as the Sahel and the Mediterranean should be included, as well as Australia).

Minor comments

- The authors' answer to my comment in lines 311-312 is very interesting and very well justified. I suggest that you include this explanation briefly in the paper.

References

Chaparro, D., Piles, M., Vall-Llossera, M., & Camps, A. (2016). Surface moisture and temperature trends anticipate drought conditions linked to wildfire activity in the Iberian Peninsula. European Journal of Remote Sensing, 49(1), 955-971.