Dear Associate Editor, dear Reviewers,
Thank you for your additional comments and suggestions. You were right to state that this study was not global. We reframed our objectives and included many of your suggestions to improve the manuscript.
Please find below in blue font our answers, and in red font the changes against the previous version.
Sincerely,
Emma Bousquet et al.

Associate Editor decision: Reconsider after major revisions
by Alexandra Konings

Dear authors,

Thank you for your detailed responses to the reviews. As you can see, both reviewers are still concerned that your manuscript claims to be a global scale fire study but leaves out major fire hotspots like the Sahel, Australia, and, to a lesser but still significant extent, the European Mediterranean region. I agree with them. I do not fully understand why you believe that using monthly averages means you can't analyze the recovery at monthly timescales, which would enable you to add these regions in a relatively straightforward manner. Nevertheless, I understand the authors do not want to undertake this in the present manuscript. However, it is still necessary for a global study of fire recovery to actually consider global fire hotspots.

The two reviewers provide two different solutions to this mismatch between the papers' claimed coverage and its actual coverage. I agree with Reviewer 1 that the thresholds you use to exclude seasonal fires seem somewhat arbitrary, and you should check you can't include more regions by changing them locally. This would be the best solution. If this is not possible, I suggest following Reviewer 2's recommendation and reframing objective, abstract, introduction, and title to be clear that this study applies only in more humid sites, or at least in only a limited number of locations across the globe and why. This would require extensive rewriting.

We fully agree that calling the paper as if it was a global-scale study was probably inadequate, so we have modified it accordingly to address the Reviewers' comments. Consequently, we decided to: i) include Australia in the ecosystem-scale study; ii) reduce the time range for the studied fire events; iii) exclude areas prone to seasonal fires; iv) increase the second threshold in order to include more fire events. These choices are further explained below, in our answers to the Reviewers.

Regardless of which of these solutions you choose, I also agree with Reviewer 2's concern that the current title is highly technical and will turn away a large number of potential interested Biogeosciences readers who are not experts in microwave radiometry, not to mention underselling the novelty of the analyses. I suggest reframing as suggested.

We also agree with you and Reviewer 2, and decided to change the title as follows : "*Monitoring post-fire recovery of various vegetation biomes using multi-wavelength satellite remote sensing*".

Lastly, per journal policy, please ensure your figures are colorblind-friendly.

For that matter, two colorblind-friendly palettes designed by Paul Tol (2021) were used.

Best regards,
Alexandra Konings

References

Tol, Paul. 2021. “Colour Schemes.” Technical note SRON/EPS/TN/09-002 3.2. SRON. https://personal.sron.nl/~pault/data/colourschemes.pdf.

Referee #1: David Chaparro

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)

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:

Major comments
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.

We fully agree with the Reviewer that our study is not global. Our goal was not to provide global results but to extend the analysis to a larger scale, which is the ecosystem scale. We decided to monitor only the major (i.e., large) and unique fires during the time period, in order to both be observed with the SMOS satellite (broad spatial resolution), and to avoid other perturbations (i.e., secondary fires) which can bias the analysis. With respect to seasonal fires, we consider that the vegetation cannot fully recover before the following fire event, that's why we decided not to keep them in the dataset. Indeed, in the savannas and shrublands of the Sahel (Fig. R1), we can observe a regular yearly decrease in the vegetation variables corresponding to the seasonal fires. The signals are very noisy and no major conclusion can be drawn from the observation of the major fire. If we mixed these time series with time series over areas showing a unique and major fire event, it would strongly dampen our observations. A sensitivity study was conducted to set the threshold values. Fig. 8 of the manuscript was also plotted for other biomes and led to a choice of 5 for the first threshold. Indeed, a compromise must be reached between a threshold high enough to observe a significant impact of fire; and low enough to keep a significant number of fire events in the sub-dataset. The second threshold enables to exclude areas subject to other major fires during the time period, and was arbitrarily fixed to 2. A similar trade off must be made in order to eliminate seasonal fires while keeping enough fire events. We agree to increase this threshold to 2.5,

which is half the first threshold, and which enables us to add several points to the dataset, in particular in areas affected by seasonal fires.
Moreover, we believe that it would be arbitrary to set different threshold values according to the biome, and we prefered to fix the same values for all biomes.

The text was modified in order to clarify our choices, and to remove the term “global”:

L. 13: *“we investigated pre and post fire vegetation anomalies ~~at the global scale~~ over different biomes”*

L. 80: *“To evaluate the long-term impact and recovery, the study focused on areas with unique fire events, thereby excluding areas with regular seasonal fires where the vegetation cannot fully recover before the following fire event. Fire-prone areas are then excluded from this study. We first observed three particular cases of large fires and then extended the analysis for different biomes.”*

L. 187: “*The rationale was to capture significant and unique events occurring over an area large enough to be observed with the SMOS satellite without any ambiguity.”*

L. 253, 299, and 396: *“Extension to the ~~global~~ ecosystem scale”*

L. 254: *“Fires were then studied at the ~~global~~ ecosystem scale to assess the general factors and impacts of fire according to the specific features of each biome.”*

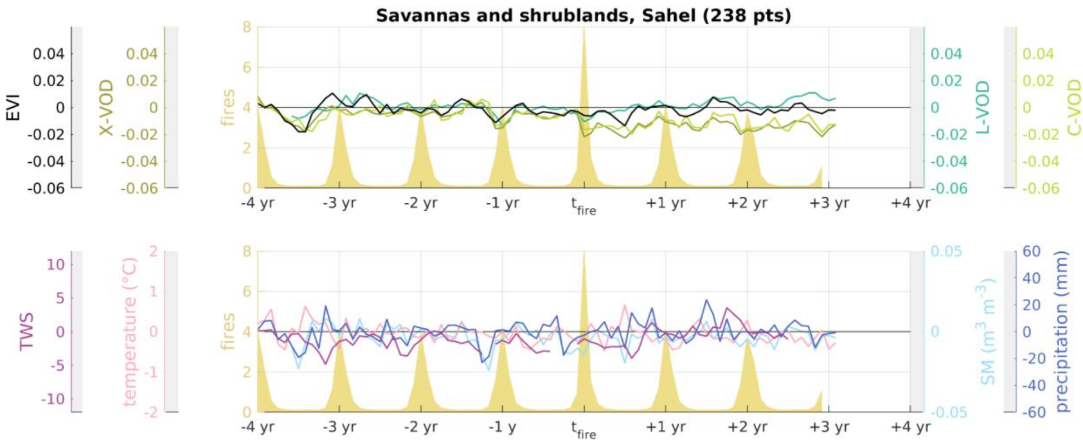


Fig. R1 - Anomaly time series over the Sahel region, without applying a second threshold on the secondary fires => the anomaly of vegetation variables is quite stable, with a light decrease each year during the fires.

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).

Thank you for these comprehensive comments and suggestions.
Regarding option a, we believe that selecting a random subset of Australian shrub fires would be arbitrary compared to other regions. Option b is interesting, but we propose another way to include Australia to our study. Indeed, we stated that the main fires occurred in Australia at the beginning and at the end of the time period 2012-2020, which prevented a pre- and post-fire study. To circumvent this problem, we propose to reduce the time range of the studied fire events, by removing 14 months at the beginning and at the end of the period. The chosen time range for fire events is then Sept. 2013 – Oct. 2019, while keeping the longer time range (July 2012 – Dec. 2020) for the studied variables. This modification enables the inclusion of many cases occurring in Australia in the study, while avoiding an unbalanced spatial distribution of fires. The location and corresponding time series are shown in Fig. R2 and R3, for each biome.

NB : in Fig. R3, which corresponds to the updated Fig. 5 of the manuscript, we added the number of points in order to clarify that this number is not constant, as fires are artificially shifted to collocate in time all fire events.

Nevertheless, we added the interesting explanations that you provided to the manuscript :

Line 397: *"Grasslands, croplands, shrublands and savannas do not show signs of pre-fire drought (Fig. 5a, 5b, 6). Indeed, in these dry ecosystems, the standard summer conditions are often prone to wildfire ignitions (Chaparro et al., 2016)."*

Line 398: *"A substantial increase in vegetation variables, C- and X-VOD in particular, occurs 1 to 2 years before fire, which implies an increase in vegetation density, e.g. available fuel. This is consistent with the fact that C- and X-bands are more sensitive to dry low shrubland vegetation (Jackson et al., 1982; de Jeu et al., 2008)."*

L 82: "*Fire-prone areas are then excluded from this study.*"

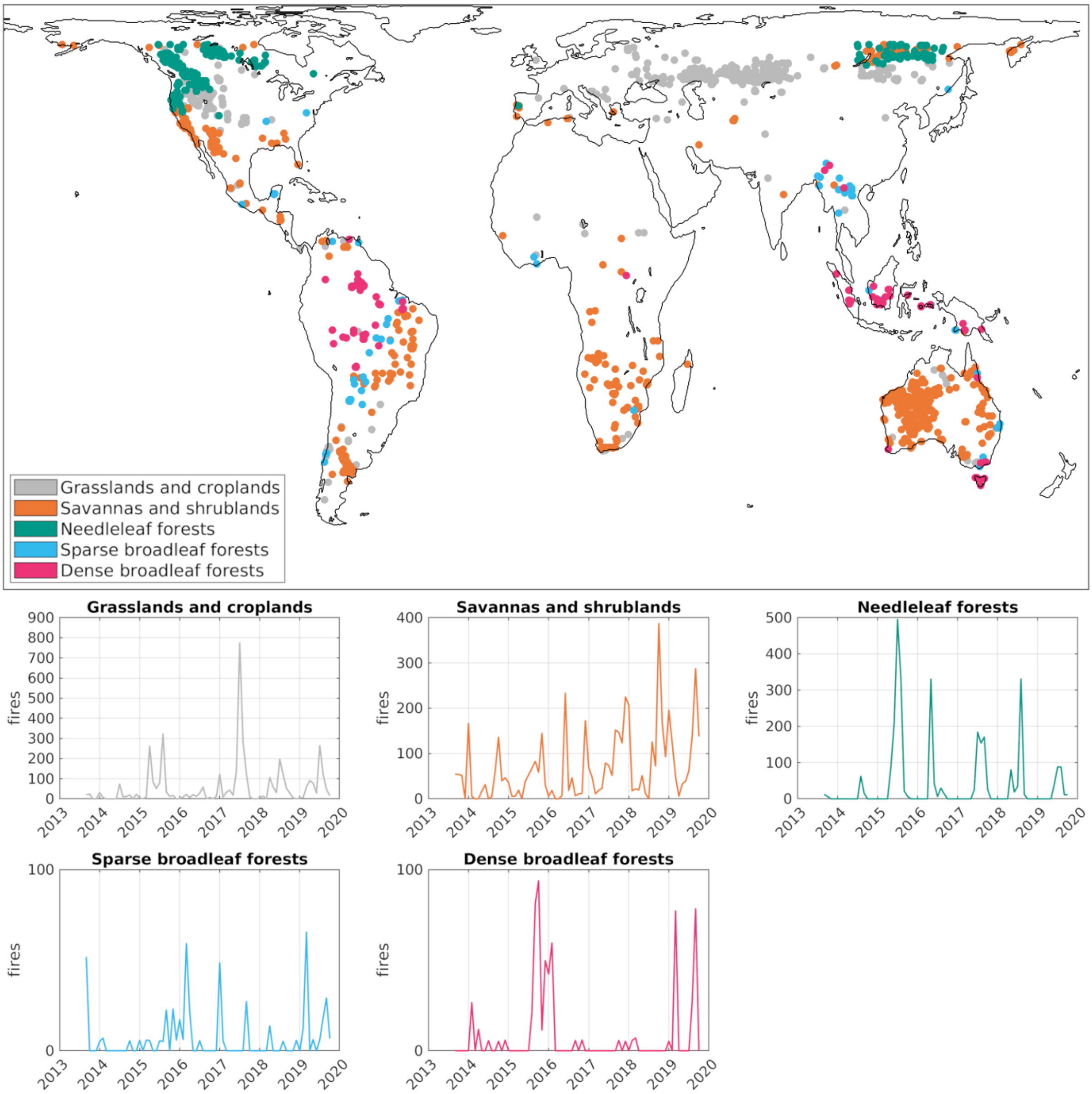
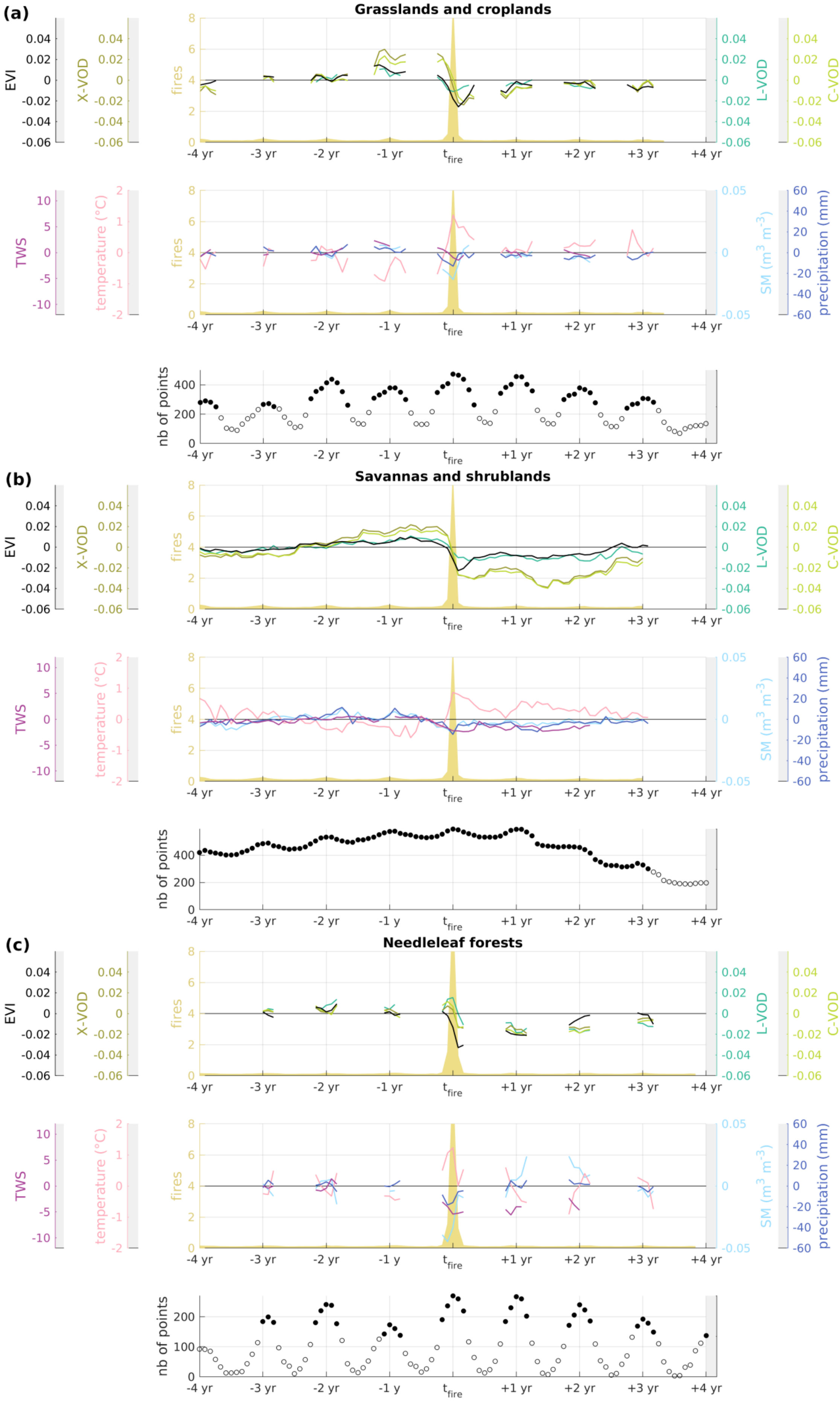


Fig. R2 - Location of the selected fires and histograms of the fire dates for each biome, with 1) fires from Sept. 2013 to Oct. 2019; 2) the inclusion of Australia; and 3) a first lower threshold of 5 fires and a second higher threshold of 2.5.



(a)
Grasslands and croplands
(b)
Savannas and shrublands
(c)
Needleleaf forests
EVI
X-VOD
fires
L-VOD
C-VOD
TWS
temperature (°C)
SM (m^3 m^-3)
precipitation (mm)
nb of points
-4 yr
-3 yr
-2 yr
-1 yr
t_fire
+1 yr
+2 yr
+3 yr
+4 yr

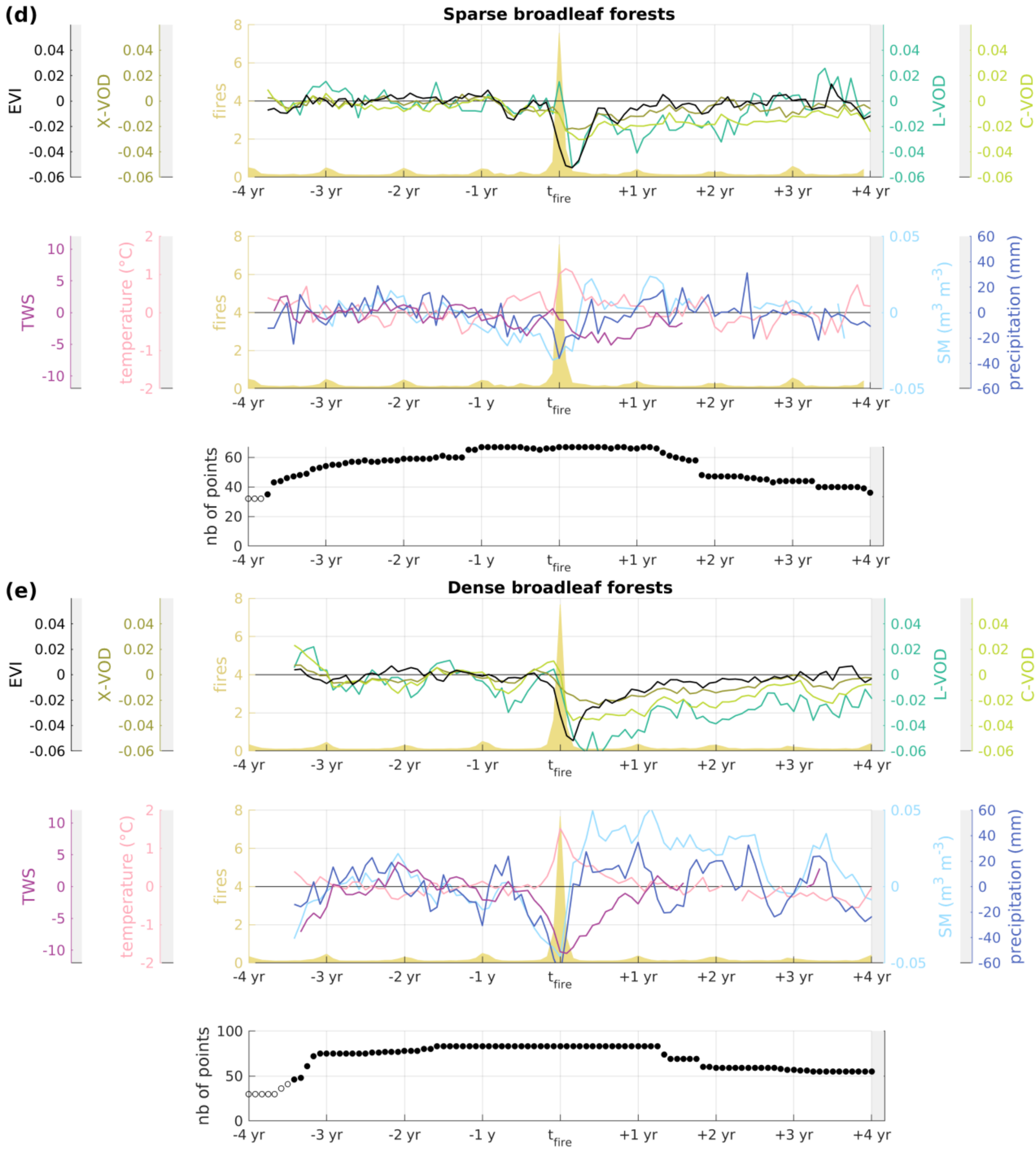


Fig R3 - Time series of the number of fires, and anomaly time series of EVI, X-, C-, L-VOD, P, SM, TWS, and T, shifted on the fire date, for the (a) grasslands and croplands biome; (b) savannas and shrublands biome; (c) needleleaf forest biome; (d) sparse broadleaf forest biome; and (e) dense broadleaf forest biome. Missing values appear when the number of available points is lower than half the maximum number of points of the biome (empty circles in the lower panel). This is mostly due to snow filtering. Data are kept otherwise (black filled dots).

<u>Minor comments</u>

• 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.

Thank you for your suggestion. We modified the sentence at line 290 : "*Strong positive temperature anomalies (+3°C), negative precipitation anomalies (-160 mm) and TWS* *anomalies (-60) are visible~~ before and~~ during the fire, and reach their maximum at the end of the fire period.*"

The explanation is then provided in the Discussion, at line 385 : "*These extreme drought conditions worsened during and at the end of the fire, and may explain its strength. Several factors can explain this observation. First, MODIS may not detect all fires in Jan. 2016 in this area, because of i) the cloud coverage (Roy et al., 2008) and ii) the dense vegetation cover hiding understory fires (Withey et al., 2018). This would be in line with the 2016 Hansen et al. tree cover loss detection (Fig. 2). Secondly, drought may sometimes keep increasing after fire extinguishment, because the removal of the vegetation cover and the deterioration of the soil contributes to maintaining a hot and dry climate (Auld and Bradstock, 1996; Veraverbeke et al., 2010). This phenomenon is also visible in the savanna and in the sparse broadleaf biome (Fig. 5b and 5d).*"

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

Referee #2: Matthias Forkel

Dear authors,

Thank you for carefully addressing the comments by the two reviewers and the associate editor. I feel that most of you revisions and responses are appropriate.

However, I still think that the objective of the study should be stated more clearly in the abstract and the introduction. Thereby you also should make clear from the beginning that you focus on regions with a low fire frequency/without seasonal fires (but specify how you define "low" fire frequency). For example, you could write "Our objective is to quantify and compare the post-fire vegetation recovery based on visible-infrared EVI and microwave X-, C- and L-VOD for selected fire events and for different biomes."

The reviewer is right, our study is not global. We clarified this in the manuscript :

L. 13: *"we investigated pre and post fire vegetation anomalies ~~at the global scale~~ over different biomes"*

L. 80: *"To evaluate the long-term impact and recovery, the study focused on areas with unique fire events, thereby excluding areas with regular seasonal fires where the vegetation cannot fully recover before the following fire event. Fire-prone areas are then excluded from this study. We first observed three particular cases of large fires and then extended the analysis for different biomes."*

L. 186, 300 and 484: *"~~global~~ ecosystem scale"*

L. 187: "*The rationale was to capture significant and unique events occurring over an area large enough to be observed with the SMOS satellite without any ambiguity."*

L. 254, 299, and 396: *"Extension to the ~~global~~ ecosystem scale"*

L. 255: *"Fires were then studied at the ~~global~~ ecosystem scale to assess the general factors and impacts of fire according to the specific features of each biome."*

Personally, I'm not really happy with the title of your study. The title is too focussed on a single element of your results and is with using four different abbreviations (i.e. SMOS, L-VOD, X-VOD, C-VOD) very technical and not very attractive for a non-specialist audience. Your results actually nicely demonstrate the different recovery times of different vegetation variables in different ecosystems and hence are much richer than what the title is suggesting. In addition, for the specialist VOD-related community the title is rather obvious as L-VOD is known to be more sensitive to woody components than shorter wavelengths and it is obvious that woody components recover slower than grass or shrubs. Hence as a VOD-specialist, I find the title not very novel and as a general fire/vegetation scientist, I feel discouraged from the technical jargon. I suggest to better reflecting the richness and diversity of your results in the title, which could make the paper more attractive for a wider audience.

We agree with the Reviewer and decided to change the title as follows : "*Monitoring post-fire recovery of various vegetation biomes using multi-wavelength satellite remote sensing*".

L262-264: "For that, a minimum threshold of 5 was applied on the maximum number of fires; and a maximum threshold of 2 was applied outside the main fire event period (i.e. outside the period -6/+6 months around the fire event)." - This sentence is very difficult to understand. Does the "maximum number of fires" refer to the number of fires in a month? But why "maximum"? Does this mean that you selected fire events if more than five fires occurred in a month but only if less than two fires occurred in the 6 months before or after? Please try to revise this sentence.

Yes, absolutely. We rephrased this sentence in order to clarify its meaning:
"Two conditions were empirically defined as mandatory to select a fire event over a given pixel: i) a minimum number of fires of 5 at the height of the fire; ii) a maximum number of fires of 2.5 outside the period [-6/+6] months around the main fire event, to ensure that the vegetation recovery is linked with the main fire event and is not affected by another significant one."

Figure 5: The figure is still very small (almost not readable at 100%) and I'm wondering if it can be really improved during the final edits. Maybe consider to rearrange the figure or split it over two pages in order to improve it.

Indeed, we wish to split Fig. 5 over two pages in order to be more readable. Colours were also changed with a colorblind-friendly palette designed by Paul Tol (2021).

Best regards,
Matthias Forkel

References

Tol, Paul. 2021. "Colour Schemes." Technical note SRON/EPS/TN/09-002 3.2. SRON. https://personal.sron.nl/~pault/data/colourschemes.pdf.