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Peer Review File

Targeted land management strategies could halve peatland fire occurrences in Central Kalimantan, Indonesia

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

28th Mar 22

Dear Dr Horton,

Please accept my apologies for the delay in obtaining reports on your manuscript titled "Peatland fire occurrences in Central Kalimantan could be reduced to less than half with targeted land management strategies". Your manuscript has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. In particular, we ask that you consider the points raised regarding input feature statistical importance, and the potential influence of plantation type and varied water management regime on model performance. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Clare

Dr Clare Davis
Senior Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

In my opinion, this manuscript is good and fulfills scientific principles, and is original. I am sure that the addition of some new scientific data, especially from the field in Central Kalimantan, will increase the value and quality of this publication.

I believe in the persistence of the authors with field work and in-depth discussions among the team to produce scientific contributions for policy makers and the sustainability of tropical peat forests in Indonesia, especially in Central Kalimantan.

Reviewer #2 (Remarks to the Author):

In this paper, the authors utilize a fully convolutional network to assess different simulated scenarios in managing a wildfire prone area in the island of Borneo. The network was trained using pre-fire season conditions to annually predict fire hotspot data derived from MODIS and VIIRS products during the actual fire season. The results look promising and the way the authors use an empirical model like CNN for evaluating simulated wildfire management scenarios is a strong point of the paper. However, I have a few clarifications, questions, and suggestions I would like the authors to address first, to further improve the quality and clarity of the manuscript.

In page 6, lines 96--97, did you only use hotspots location that were recorded within the fire season (Aug-Oct) and ignored all detected hotspots outside of this time period for training and testing? Could you provide some statistics/discussion describing these hotspots outside the fire season period?

In page 7, line 114, a more precise term for "layers" would be "feature maps". Because the term layer is also used to describe a group of operation kernel/convolutional or dense layer within the

network.

In page 8, Figure 2, why is the 0th line not shown?

In page 10, lines 168--169, when is a model good enough? > 75% median precision and > 45% median recall? Could you add a discussion as well comparing the importance of precision over recall or vice versa, whichever applies for the case of assessing the management scenarios evaluated in this work?

In page 11, Figure 4, it is not clear for me what the scale/unit of the x-axis is. Could you please clarify/describe?

In page 13, lines 235--236, any discussion on relevance or connection of prescribed burning with the scenarios explored? Are these practices applicable to study areas in the tropics?

In page 14, lines 270--273, shouldn't the input variable distance from drainage canals be correspondingly updated for this scenario then?

In page 19, line 382, is this dimension and corresponding spatial resolution mentioned somewhere?

In page 21, lines 422--425, the description of a dense layer sounds inaccurate/ambiguous to me, specifically the statement "acting only on a single grid cell at a time". Generally speaking, in a dense or fully-connected laye, one unit/cell in the succeeding layer has a connection to all units in the previous layer. This is conventionally preceded by a "flattening" operation to transform the 2D/3D feature map into a 1D array. Or can equivalently be (inefficiently) substituted by a convolution/kernel layer having the same spatial dimensions of the preceding feature map.

In page 23, lines 454--456, can be avoided by having overlapping tiles, the overlap being at least half the size + 1 of the effective receptive field size of the network. The receptive field size depends on the number of convolutional and downsampling layers.

Did the authors look into methods [1, 2] quantifying input feature statistical importance? Would such methods be relevant for this work?

Also please check the manuscript with some typographical errors, specifically the word "hotspots" is mispelled as "hotpots" a couple of times.

[1] Vasilakos, C., Kalabokidis, K., Hatzopoulos, J., Matsinos, I., 2009. Identifying wildland fire ignition factors through sensitivity analysis of a neural network. Nat. Hazards 50, 125–143.
[2] Bergado J.R., Persello C., Reinke K., Stein A., 2021. Predicting wildfire burns from big geodata using deep learning[J]. Safety Science.

Reviewer #3 (Remarks to the Author):

General comments:

The use of CNN model can be seen as a novelty founded by this paper. By using pre-fire season

inputs to test multiple land management scenarios the CNN capable to estimate the potential of fire reduction in a number of peatland restoration project scenarios.

Despite its novelty, however, the paper is raised issues as the following:
1. Hotspot and fire spot are not always equal; hence, careful justification is needed when using both data interchangeably. Ground checking is absolutely necessary to yield more reliable, accurate and precision fire spot data. Apart from ground checking, the use of higher than 70% level of confidence is recommended to improve the likelihood fire spot confident.
2. The use of the 2016 land cover map produced Ministry of Environment and Forestry (MoEF) seems unreliable and improper as the changing of forest and land covers within the EMRP (Ex-Mega Rice Project) is highly dynamic and changing over time. These dynamic and change due to various factors such as population growth, recurrent fires, local economic activities, agricultural activities, timber and non-timber harvesting, crop and timber plantations, restoration activities and so forth. The MoEF produces and updated regularly the forest and land cover dynamic throughout Indonesia archipelagic which publicly accessible through the following link: https://nfms.menlhk.go.id. The used of 2016 LC map may imply the accuracy, effectiveness and usefulness scenarios used notably with regards to the recent status of 'Swamp shrubland', 'Scrubland', 'Swamp Forest', and 'Plantation'.
3. The authors are unable to define and make distinction of the type of 'Plantation'. Many types of plantations are currently happening in the EMRP, among others are: oil palm plantation, agricultural crops plantation, rubber plantation, industrial timber plantation, etc. Each type of plantation has different physical and hydrological management regimes so as to be grown productively and sustainably on peatland.
4. The authors neglected the hydrological restoration interventions that have been implemented by various parties on the ground. Thousands of rewetting infrastructures as as means of hydrological restoration to reduce fire incidences in the EMRP since 2003 up to recent years (Dohong et al., 2017; Dohong et al., 2018 & Dohong & Tanika, 2021).

Specific comments:
Line 28 and line 31: Lounela, 2019 not found in the reference list
Lines 26-28 not support by cited reference, rather speculative statement.
Lines 32-35 please provide reference to support the statement.
line 177: please clearly define what kind of plantation scenario (monoculture or multicultural plantation?, crops, timber or oil palm plantations?)

RESPONSES TO REVIEWERS' COMMENTS:

We would like to thank the Reviewers and Editor very much for their careful reading and constructive comments. We made revisions following the suggestions by Reviewers, including:

- expanding the discussion around the model performance and utility
- stating more of the model limitations and omissions
- clarifying many aspects of the methodology

These and other revisions are detailed below in our point-by-point replies to the comments. All the changes in the manuscript are highlighted in the tracked manuscript version. The updated line numbers are reported in the responses where applicable.

Reviewer #1 (Remarks to the Author):

Comment 1.1: In my opinion, this manuscript is good and fulfils scientific principles, and is original. I am sure that the addition of some new scientific data, especially from the field in Central Kalimantan, will increase the value and quality of this publication.

I believe in the persistence of the authors with field work and in-depth discussions among the team to produce scientific contributions for policy makers and the sustainability of tropical peat forests in Indonesia, especially in Central Kalimantan.

Reply 1.1: We thank the reviewer for the positive feedback and also hope that our work might impact policy decisions made in the area.

Reviewer #2 (Remarks to the Author):

Comment 2.1: In page 6, lines 96--97, did you only use hotspots location that were recorded within the fire season (Aug-Oct) and ignored all detected hotspots outside of this time period for training and testing? Could you provide some statistics/discussion describing these hotspots outside the fire season period?

Reply 2.1: Yes, we did indeed use hotspots for the fire season period (Aug-Oct). In our previous paper (Horton et al 2021: https://doi.org/10.1029/2021EA001873), we include a figure showing the distribution of fire hotspots as monthly averages. This demonstrated that only a small fraction of fires was detected outside these months. We now state this in the main text and refer to this figure for illustration (lines 118-119).

"Very few fires occur outside the fire season months, accounting for >90% of the total fire hotspots detected in our study area between 2000-2019 (Horton et al., 2021)."

C2.2: In page 7, line 114, a more precise term for "layers" would be "feature maps". Because the term layer is also used to describe a group of operation kernel/convolutional or dense layer within the network.

R2.2: Thank you for pointing this out. We now refer to feature maps as opposed to layers as before.

C2.3: In page 8, Figure 2, why is the 0th line not shown?

R2.3: This was to save space in the figure, but we have now added the 0% line for clarity.

C2.4: In page 10, lines 168--169, when is a model good enough? > 75% median precision and > 45% median recall? Could you add a discussion as well comparing the importance of precision over recall or vice versa, whichever applies for the case of assessing the management scenarios evaluated in this work?

R2.4: We agree that this is an important point and deserves to be discussed more fully. When a model is good enough really depends on the model application, the purpose of the model as well as the modelled phenomena. We have now added a paragraph in the discussion highlighting the difference between precision and recall, and stating that, for the intended purposes of the model (i.e. testing potential land management strategies), precision is the more important indicator.

We added the following to the text in the place that you suggested (lines 196-198)

"For using the model as a tool for testing potential land management strategies, the precision metric is most important, as it indicates the model's ability to state with great confidence that a certain combination of input variables will produce a fire."

And we added the following to the discussion to expand further on this point (lines 336-342)

"These inaccuracies in the hotspot and input variable data may in part explain the poor recall metric scored by our model. However, recall is less important than precision for our purposes, as we require the model to say with a high degree of confidence that a set of input variables will produce a fire. We then change the set of input variables and ask the model for a comparison. That the model misses many fire occurrences is not an issue for making comparisons between scenarios so long as it consistently misses many fire occurrences."

Finally, we did not find from literature any comparable CNN model used for forest fires for which we could have compared our precision and other model metrics. When compared to our previous study where we used a XGBoost machine learning model for the same area (Horton et al 2021; https://doi.org/10.1029/2021EA001873), the precision of the FireCNN model outperformed it clearly (78% vs 60%; both without using the fire season data). In recall, the difference was also clear: 46% vs 25%.

C2.5: In page 11, Figure 4, it is not clear for me what the scale/unit of the x-axis is. Could you please clarify/describe?

R2.5: The violin plot shows the distribution of percentage values within each classification about the mean. Therefore, the x-axis is a proportion of the total number of observations (0-1). We have now added values to the x-axis, and an explanation to the figure caption.

C2.6: In page 13, lines 235--236, any discussion on relevance or connection of prescribed burning with the scenarios explored? Are these practices applicable to study areas in the tropics?

R2.6: This is also another good point, which we have now mentioned in the revised text. The act of burning is prohibited in the area, but in practice it does still happen. It is thought that this is how many of the fires start each year. We now mention this in Introduction (lines 44-47)

"Although lighting fires is prohibited in the region, most ignitions are anthropogenic in origin, started by either small-scale farmers for land-clearance or to renew the topsoil fertility, or by private companies and government agencies to expand agricultural land into forest stands (Cattau et al., 2016; Sumarga 2017; Vetrita and Cochrane 2019)."

C2.7: In page 14, lines 270--273, shouldn't the input variable distance from drainage canals be correspondingly updated for this scenario then?

R2.7: Without knowing the precise management practices of the plantations, we were unable to assign new values for the distance to canals input for new plantation areas. However, we agree that this should be done in a more detailed analysis of the potential impact of conversion to plantation in the future. We have now added a comment stating this in the discussion (lines 303-306)

"In future analysis, new canals should be simulated alongside the areas converted to plantations to better represent the probable impacts, though this would require a thorough description of the management practices associated with establishing new plantations."

C2.8: In page 19, line 382, is this dimension and corresponding spatial resolution mentioned somewhere?

R2.8: The input feature maps were resampled to rasters with a resolution of 0.002 degrees in the WGS84 coordinate system, which corresponds to roughly 225 m. We have now stated this in the methods (lines 431-434)

"*Once pre-processed, all predictor variable rasters were resampled to the same dimensions (with a resolution of 0.002 degrees in the WGS84 coordinate system)*"

C2.9: In page 21, lines 422--425, the description of a dense layer sounds inaccurate/ambiguous to me, specifically the statement "acting only on a single grid cell at a time". Generally speaking, in a dense or fully-connected layer, one unit/cell in the succeeding layer has a connection to all units in the previous layer. This is conventionally preceded by a "flattening" operation to transform the 2D/3D feature map into a 1D array. Or can equivalently be (inefficiently) substituted by a convolution/kernel layer having the same spatial dimensions of the preceding feature map.

R2.9: Thank you for pointing out the ambiguity. I think this confusion has come about through a mixing of terminology. When we say a single grid cell, we mean a grid cell of the input feature stack, not a single input feature map. Therefore, the single grid cell contains all the input channels (either 1x1x31 or 1x1x128), and as you say will have connections between all of these dense layers. This is unlike the kernel layers, which operate on a subsection of 3x3 grid cells of the feature stack (i.e. 3x3x128). We hope the renaming of layers to feature maps will reduce this confusion, but we have also extended this section for added clarity (lines 471-475).

"*A dense layer will do the same operation, but acting only on a single grid cell of the input stack, acting at the same location upon all input feature maps within the stack at a time – using all values at that location (i.e. the 1x1 subsection) and transforming them according to assigned weights to pass forward a new set of channels to a single grid cell on the output stack.*"

C2.10: In page 23, lines 454--456, can be avoided by having overlapping tiles, the overlap being at least half the size + 1 of the effective receptive field size of the network. The receptive field size depends on the number of convolutional and downsampling layers.

R2.10: Thank you for your suggestion, we will be sure to incorporate this into our future work.

C2.11: Did the authors look into methods [1, 2] quantifying input feature statistical importance? Would such methods be relevant for this work?

R2.11: Thank you for the suggestion. We did indeed look into these methods, however we decided that rating the statistical importance of each driving variable was not within the remit of this paper. The reason for this decision is that our previous work (Horton et al 2021: https://doi.org/10.1029/2021EA001873) does precisely this using an XGBoost machine learning model to assess the relative importance of each driving variable of fire prevalence for our study area.

C2.12: Also please check the manuscript with some typographical errors, specifically the word "hotspots" is mispelled as "hotpots" a couple of times.

R2.12: Thank you for pointing this out, we have now changed each of these instances and checked carefully the whole manuscript to avoid such typos.

[references from the reviewer]

[1] Vasilakos, C., Kalabokidis, K., Hatzopoulos, J., Matsinos, I., 2009. Identifying wildland fire ignition factors through sensitivity analysis of a neural network. Nat. Hazards 50, 125–143.
[2] Bergado J.R., Persello C., Reinke K., Stein A., 2021. Predicting wildfire burns from big geodata using deep learning[J]. Safety Science.

Reviewer #3 (Remarks to the Author):

General comments:

Comment 3.0: The use of CNN model can be seen as a novelty founded by this paper. By using pre-fire season inputs to test multiple land management scenarios the CNN capable to estimate the potential of fire reduction in a number of peatland restoration project scenarios.

Reply 3.0: Thank you for the positive comment.

C3.1: Hotspot and fire spot are not always equal; hence, careful justification is needed when using both data interchangeably. Ground checking is absolutely necessary to yield more reliable, accurate and precision fire spot data. Apart from ground checking, the use of higher than 70% level of confidence is recommended to improve the likelihood fire spot confident.

R3.1: Thank you for the observation. Whilst we agree that the accuracy of both the MODIS and VIIRS hotspot data is limited to their respective grid cell resolutions, which makes identifying the precise fire location (fire spot) difficult, our input variables are represented at a similar resolution, and so we do not feel it unduly impacts on the results. And providing ground checking is certainly not possible for these historic data.

The issue of confidence in the hotspot data is also a source of uncertainty. However, requiring >70% confidence level reduces the number of hotspots significantly, which is particularly problematic in years with few observations (2010 for example). Training the neural network requires large amounts of observations to construct meaningful relationships, therefore we decided to include the entire dataset. We now acknowledge this source of uncertainty in the discussion (lines 318-321).

“Rather than only use observations with a high confidence associated, we have included all recorded hotspots to maximise the volume of training data available for the neural network model, which requires large numbers of observations to produce meaningful relationships between the input variables.”

And (lines 336-337)

“These inaccuracies in the hotspot and input variable data may in part explain the poor recall metric scored by our model.”

C3.2. The use of the 2016 land cover map produced Ministry of Environment and Forestry (MoEF) seems unreliable and improper as the changing of forest and land covers within the EMRP (Ex-Mega Rice Project) is highly dynamic and changing over time. These dynamic and change due to various factors such as population growth, recurrent fires, local economic activities, agricultural activities, timber and non-timber harvesting, crop and timber plantations, restoration activities and so forth. The MoEF produces and updated regularly the forest and land cover dynamic throughout Indonesia archipelagic which publicly accessible through the following link: https://nfms.menlhk.go.id. The used of 2016 LC map may imply the accuracy, effectiveness and usefulness scenarios used notably with regards to the recent status of 'Swamp shrubland', 'Scrubland', 'Swamp Forest', and 'Plantation'.

R3.2: We agree that only using the 2016 land cover map would be a gross misrepresentation of this dynamic environment. However, the landcover map which we reference as MoF (2016) is actually a collection of historic landcover maps from 1996-2016 at 2-3 years intervals as listed in Table 1 in the methods. This is now also explicitly explained in the revised manuscript (lines 394-395)

"*We used a collection of historic land cover maps generated by the Ministry of Forestry Indonesia from 1996 - 2016 at 2-3 years intervals (MoF 2016).*"

C3.3. The authors are unable to define and make distinction of the type of 'Plantation'. Many types of plantations are currently happening in the EMRP, among others are: oil palm plantation, agricultural crops plantation, rubber plantation, industrial timber plantation, etc. Each type of plantation has different physical and hydrological management regimes so as to be grown productively and sustainably on peatland.

R3.3: Thank you for bringing this to our attention, it is a very good point that we should have addressed. Unfortunately, we have no way of being able to discern the type of plantation from these historic land cover maps as the type of a plantation is not specified in those (or in any other available reference). We have now acknowledged this limitation in the discussion… (lines 326-329)

"…the historic land cover maps that we have used fail to distinguish between plantation types, and each will have a different land management associated. In recent years oil palm plantations are most common, but there are also agricultural crop plantations, rubber plantations, and industrial timber plantations in the region."

C3.4. The authors neglected the hydrological restoration interventions that have been implemented by various parties on the ground. Thousands of rewetting infrastructures as as means of hydrological restoration to reduce fire incidences in the EMRP since 2003 up to recent years (Dohong et al., 2017; Dohong et al., 2018 & Dohong & Tanika, 2021).

R3.4: We agree that this is also an omission. Unfortunately, we were not able to incorporate the hydrological impact of these small-scale canal blockages within our model structure as identifying the precise locations, the extent of the damming, and the timing of the interventions isn't practical. However, we have now acknowledged the potential impact of these schemes on our model results in the discussion, citing the references that you kindly provided (lines 329-332).

"Furthermore, we were unable to represent the hydrological impact of restoration efforts blocking many small-scale canals throughout the MRP (Dohong et al., 2018; Dohong & Tanika, 2021), as identifying the precise locations, the extent of the damming, and the timing of the interventions proved impractical."

Specific comments:

C3.5: Line 28 and line 31: Lounela, 2019 not found in the reference list

R3.5: Thanks for noting this. We have added this citation (https://doi.org/10.21435/sfa.4) to the references.

C3.6: Lines 26-28 not support by cited reference, rather speculative statement.

R3.6: We have now added the cited reference to the list. Lounela, 2019 does indeed support the statement and adds additional information about the REDD+ mitigation projects in the footnote. Though we have now added an additional reference, which expands upon the topic further…

Dohong, A. (2011). Central Kalimantan province as REDD+ pilot project: current status and challenges. In Proceedings of 3rd International Workshop on Wild Fire and Carbon Management in Peat-Forest in Indonesia, Bogor: Wild Fire and Carbon Management.

C3.7: Lines 32-35 please provide reference to support the statement.

R3.7: We weren't able to reference this statement, so we have softened the language in order to no longer make a definite statement (lines 57-61)

"*Whilst there are many obstacles to reducing fires throughout Kalimantan and the wider region, one major obstacle may be the strict adherence to land management regulations at all levels, from government officials to small scale land holders. A better understanding and clearer demonstration of the potential benefits of these projects might generate more support from the local communities.*"

C3.8: line 177: please clearly define what kind of plantation scenario (monoculture or multicultural plantation?, crops, timber or oil palm plantations?)

R3.8: We are uncertain about the precise nature of these plantations marked on historic land use maps, and have now added a comment to indicate this uncertainty. (lines 326-329)

"*…the historic land cover maps that we have used fail to distinguish between plantation types, and each will have a different land management associated. In recent years oil palm plantations are most common, but there are also agricultural crop plantations, rubber plantations, and industrial timber plantations in the region*."

20th Jun 22

Dear Dr Horton,

Your manuscript titled "Peatland fire occurrences in Central Kalimantan could be reduced to less than half with targeted land management strategies" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Clare

Dr Clare Davis
Senior Editor
Communications Earth & Environment

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REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

I am satisfied with how the authors addressed the issues I raised. No further comments from my side.

Reviewer #3 (Remarks to the Author):

I am satisfied as my previous points have been already incorporated in the manuscript revision. Hence, I support this manuscript to be published soon in the Communications Earth and Environment Journal.