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

18th Jun 20

Dear Dr Nikonovas,

Your manuscript titled "Near-complete loss of fire-resistant primary forest cover in tropical peatlands of Sumatra and Kalimantan" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to
• address these criticisms(that is, either to incorporate the suggestions or provide a compelling argument why the point made by the reviewer is not valid, or relevant to the editorial threshold as outlined below)
AND
• meet our editorial thresholds as outlined below,

then we would be happy to look at a substantially revised manuscript. However, please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

In the following, we list our main editorial concerns:

****Provide a thorough and compelling analysis of the role of deforestation in the distribution of fires and fire risk in Sumatra and Kalimantan with full consideration of the complexities of regional climatic and other drivers of wildfire activity****

****Provide full details and justification for the criteria used for land use classification in the data sets you use and ensure the limitations of these sources and your methodology are fully acknowledged****

In addition, please supply point-by-point responses to all the reviewers' comments.

If the revision process takes significantly longer than three months, we will 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.

We are committed to providing a fair and constructive peer-review process. Please do not 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 any 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 **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Joe Aslin

Associate Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements

For your information, you can find some guidance regarding format requirements summarized on the following checklist:
Communications Earth & Environment formatting checklist

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Tropical forests of SE Asia, including peat swamp forests, are recognised globally as carbon-rich ecosystems that contribute significantly to terrestrial carbon (C) storage in both aboveground biomass and belowground peat deposits. Nevertheless, climatic changes towards drier conditions and longer fire seasons combined with ongoing human disturbances, including widespread use of fire in land use change, are inevitably resulting in modified fire regimes and faster rates of forest loss and C emissions to the atmosphere. It is important, therefore, to examine the drivers and consequences of forest and peatland fires in various regions of the world, and in particular to understand how landscape-scale fire resistance may be shifting. In this paper, the authors report on the impact of forest fragmentation and disturbance on fire incidence in the primary forests of western Indonesia, while also stressing the role that forest restoration practices could play in mitigating future fire occurrence and severity. They show evidence for the role that forest disturbance, including peat drainage, plays in an enhanced risk of fire, including the 'edge effect'. Although the link between forest disturbances and changing fire regimes in tropical forests is not a new concept – for example, it has been established for some time for the Amazon (e.g. work by Cochrane, Barlow and others published over at least the last 20 years) and is also an accepted factor in forest fire occurrence in SE Asia (e.g. Siegert et al., 2001). Nevertheless, for SE Asian forests, there have been limited studies providing quantification of these changes, and this paper therefore could play a role in complementing and reinforcing previous studies. As the paper currently stands it does not provide a particularly novel analysis but with some reworking, it does have the potential to make a useful contribution to our knowledge of the synergies between climate, land use change, economic drivers and fire, with implications for efforts to reduce Indonesia's greenhouse gas emissions from these sources.

The paper is mostly well written but I do have several concerns about the analysis; these focus mainly on the analytical approach (see below). As it stands, the paper does not greatly advance our knowledge of the drivers of fire and fire risk, although the authors clearly have the data to do this. With additional analysis there could be an opportunity to draw out sub-regional trends with an attempt to separate climatic from other, economic drivers of fire activity. This would provide a more perceptive and novel account of the drivers of forest susceptibility to fire and the likely impact that a changing climate will have on future fire risk.
Proposed Minor/Major Amends
Line 55 – 'loss of forest vegetation which covered the majority of the region since the last ice age' greatly over simplifies the long-term vegetation dynamics of this region, with some authors contending that during the LGM (which would be a better term to use rather than 'ice age') there was extensive forest loss and expansion of savannah (e.g. see Bird et al. 2005) although others have disputed this interpretation.
Line 60 – suggest adding 'at least' before 'the last 15,000 years' given that the Page et al. (2004) reference indicates peat genesis earlier than 20,000 yrs BP whilst other peatlands in the region may have had an even earlier genesis (e.g. Anshari et al. 2001).
Lines 62 and 63 – Can the authors add another sentence by way of explanation of how the forests have been 'effective at preventing this important carbon pool from being released into the atmosphere by fires'? In part this is due to the ever-moist, humid microclimate in closed canopy

forest (i.e. potential fire fuels are too moist to support combustion). For peat swamp forests this is also an important consideration, but fire resilience is also a function of an intact peatland hydrology. Furthermore, it appears from the palaeo-record that most fires in the humid forests of SE Asia have been associated with human activities – i.e. most fires have anthropic rather than natural (lightning strike) ignition sources. Given, until recently, relatively low human access into primary peat swamp forests, this has also limited forest fire ignitions – nevertheless, there is some evidence of use of fire/evidence of burning at some peatland sites (e.g. Anshari et al. (2001) for Danau Sentarum, Hope et al. (2005) for Kutai peatland, Cole et al. (2015) for Sarawak peatland).

Line 81 – Neither of the two cited studies (Hooijer et al. 2012, Evans et al. 2019) demonstrate a link between lower peatland water table at the plantation/forest edge and increased peat flammability, although, in the presence of fire ignition sources this could be a reasonable assumption. I suggest adding 'potentially' before 'increasing peat flammability' and note the additional requirement for an ignition source (i.e. fuel load alone is insufficient to increase the risk).

Lines 83 to 88 – This paragraph greatly over simplifies the fire regimes in this part of SE Asia. This is a major criticism of the paper, and given the subsequent focus in the analysis on climatic (rainfall) anomalies that follows, I am also critical of the simplified methodology used to identify links between climate and fire occurrence. There is considerably more regional detail that could be brought out of the data sets that might help separate climatic from other drivers of fire occurrence. As the authors briefly indicate, drought occurrences in Indonesia follow various patterns. The variability of rainfall is often linked in literature to the ENSO phenomenon and it is now generally recognised that the warm phase of ENSO is indeed associated with reduced rainfall and convective activity over some parts of the Indonesian archipelago. But, ENSO does not have the same strength of influence across all regions of Indonesia: this is a nuance often missed in literature, as it is here. For example, while Langner and Siegert (2009) found that wildfires affected an area three times larger during ENSO years (1997-98, 2002 and 2006), they also found a pronounced difference in fire occurrence across the region, and, importantly, that ENSO was not the only driver of recurrent fire years. Aldrian and Susanto (2003) divide Indonesia into three regions according to mean annual climate patterns. Precipitation and fire activity in central Sumatra (including Riau) are not as strongly coupled with the ENSO phenomenon, but more with the Indian Ocean Dipole (IOD, e.g. Field and Shen 2008). The ENSO and IOD can interact in their effects: all of the major SE Asian fire events that occurred from 1960 to 2006 happened during years of drought induced by ENSO and/or IOD conditions, with the 1997-98 fires occurring over a period that saw the strongest recorded ENSO and IOD events in the 20th century. Pan et al. (2018) also found that IOD plays an important role alongside ENSO in modulating the intensity of Indonesian fires: when the IOD is positive Indonesian fires are at least twice as intensive as when the IOD is negative. Importantly, they also show that the ENSO will have heterogenous impacts across Indonesia: fires are more intense in southern Kalimantan than in southern Sumatra in all ENSO years by at least 36%, even when less severe drought conditions are found in southern Kalimantan. Furthermore, during the 2019 fire season the haze in Riau reached dangerous levels (in air quality), and this was during a positive IOD but neutral ENSO – so drought in central/northern Sumatra is driven more by IOD than ENSO events.

To further complicate the link between fires and ENSO/IOD, Gaveau et al. (2014) showed that the 2013 fires in Sumatra were widespread but occurred during a short dry period – i.e. a normal year, not an ENSO or IOD year and were likely associated with oil palm agriculture, i.e. they were more strongly driven by economic rather than climatic factors. And to add further complication, Oozer et al. (2020) report that the 2013 fires and haze in Sumatra were influenced by tropical cyclone Bebinca and the dry phases of a strong Madden-Julian Oscillation event. In summary, there are additional climatic patterns and events, apart from ENSO and the IOD that need consideration when analysing peatland fire events in different parts of Indonesia and whilst ENSO and the IOD are certainly important drivers of fire, it is incomplete to attribute their occurrence to only one or even both of these events.

Lines 132-146 – Section on forest fragmentation. Can the authors confirm that they did not misinterpret the degradation status of peat swamp forest. Peat swamp forests in this region show a natural zonation from tall, closed canopy mixed swamp forest on the margins of the peat dome to low pole, often more open canopy forest on the central parts of the dome. In some forest classifications the low pole forest is mis-classified as degraded/logged, owing to its naturally lower stature and more open canopy.

Line 172 – replace 'what' at the end of the line with 'that'

Line 198 – what definition is used by the authors to define a 'dry' year? Given my comments earlier (lines 83 to 88), this analysis is overly simplistic. Spatial fire occurrence will vary according to prevailing climatic conditions, with a positive IOD having a stronger influence on a prolonged dry season in central Sumatra (e.g. Riau, north Jambi) than an ENSO event, which is more strongly linked to drought in southern Sumatra and southern Borneo. Also, as again commented earlier, some years see widespread burning even during a 'normal' dry season (e.g. Sumatra, 2013 – see Gaveau et al., 2014) that is driven by economic rather than climatic factors. It must be possible for the authors to draw out some of these nuances from their data in order to provide a more robust, informative and novel study – e.g. by separating climatic trends for different geographical regions (Indonesian provinces) and for different strengths/drivers of rainfall anomaly (ENSO vs IOD etc).

Lines 220-221 – The proposed length of the fire return interval is speculation (hundreds to thousands of years), and as indicated earlier, a number of studies have actually shown evidence of forest fire in the palaeo-record. Suggest this statement is rephrased.

Lines 278 – 299 – Was any effort made to compare the fire activity data with an actual burned area product or with field checking – i.e. have you confirmed that your active fire detections actually reflect fire impacts (burned area) on the ground - either using optical remote sensing or ground data? As the authors acknowledge, there are significant issues (errors of commission and omission) with using so-called 'hot spot' data : hotspots are indications of potential fires, but they are not equal to actual fires and confirmation of an actual fire from hotspot data requires ground truthing. Conversely, low temperature, smoldering peat fires may evade detection using this approach. Regular users of MODIS data for fire monitoring in Sumatra, for example, report very high levels (even as high as 90%) of both commission and omission errors. Whilst the authors have taken steps to avoid over reporting of fire detections, the levels of uncertainty associated with using these data need to be better acknowledged. The authors also need to acknowledge the likely scale of the omission errors.

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Reviewer #2 (Remarks to the Author):

Dear authors

I read the manuscript with great interest. It clearly demonstrates how deforestation and forest fragmentation can be linked to fire occurrence. The study covers two main Indonesian islands that are highly relevant to the deforestation and forest and peat fire problems. This study provides another proof to convince the need to maintain the remaining primary forests in Indonesia. I have several points to clarify.

Title: This study pay a balanced attention to primary forests both on peatland and dryland. Why is only the one on peatland reflected in the title?

Line 101-103: I think the claim is too much. Please clarify what do you mean with “substantially underestimate.”

Line 113-114: Figure 1 and Figure 2 do not include data on highland montane forest. How did you state that the large extent of deforestation take place in that type of forest?

Line 128: The moratoriums apply not only on primary forest in peatland. The moratoriums apply on primary forests (on peatland and dryland) and on peatland (forested and not forested). Please provide specific reference for the Indonesian moratorium policy.

Line 151: Did you do statistical analysis to test the significance? Does this mean that the fire-affected area is not significantly different between areas with partially, mostly, and completely deforested?

Figure 3 and Figure 4: I see that Figure 3 is actually part of Figure 4. Can the figures be combined?

Line 153-157: This study found that the percentage of fire affected in mostly deforested area is higher that that in completely deforested area, both in peatland and non peatland. Do you have any explanations about this findings? The readers might expect that the worse the level of deforestation, the higher the risk of fire. Hence, additional explanation would be useful to clarify the ‘unexpected’ findings.

Line 167: The statement that fire is extensively used as a tool for deforestation needs reference.

Line 251-252: This study used different sources of land cover maps (for land cover map of 2000 and 2001-2018). How did you ensure that the maps have the same criteria in classifying the undisturbed primary forest? What is the implication to the findings of this study if the maps used different criteria in classifying primary forests?

Line 265-268: Do you have any reasons to classify the rate of deforestation in that way? The classification with unequal interval usually needs to provide reasonable explanations.

The quality of land cover maps: This study used land cover maps as the main inputs for analysis. The accuracy of the findings of this study then also depends on the quality of the land cover maps. Although the sources of the maps have been stated in the text, the authors should provide sufficient description about the quality of the maps, for example about the criteria used to classify undisturbed primary forests and particularly about the accuracy of the classification.

Dear reviewers,

On behalf of all authors, I would like to thank you for the constructive feedback and suggestions for revision. Please find below a point by point response to the comments received.

Reviewer comments received are *italicised* *while our responses are in normal font. Line numbers in our responses refer to those in the* ***revised*** *manuscript.*

Reviewer #1:

They show evidence for the role that forest disturbance, including peat drainage, plays in an enhanced risk of fire, including the 'edge effect'. Although the link between forest disturbances and changing fire regimes in tropical forests is not a new concept – for example, it has been established for some time for the Amazon (e.g. work by Cochrane, Barlow and others published over at least the last 20 years) and is also an accepted factor in forest fire occurrence in SE Asia (e.g. Siegert et al., 2001). Nevertheless, for SE Asian forests, there have been limited studies providing quantification of these changes, and this paper therefore could play a role in complementing and reinforcing previous studies. As the paper currently stands it does not provide a particularly novel analysis but with some reworking, it does have the potential to make a useful contribution to our knowledge of the synergies between climate, land use change, economic drivers and fire, with implications for efforts to reduce Indonesia's greenhouse gas emissions from these sources.

We thank Reviewer 1 (R1) for this insightful comment. Indeed, as we have highlighted in the manuscript, the link between forest disturbance and fire regime in tropical ecosystems has been reported before. We concur with R1 in stating that we are also not aware of any previous studies that quantify primary forest loss and disturbance in relation to changes in fire regimes across Sumatra and Kalimantan. This provides the fundamental motivation for the manuscript. Our work has been enabled by overlapping longest available series of high resolution annual forest loss and fire activity data. The results evidently show that while undisturbed primary forests are resilient to fire, such forests are on the brink of disappearance in Sumatra and Kalimantan, and that the on-going loss and degradation of natural forests in the region has resulted in increase in land area that is prone to burning, exacerbating Indonesia's fire-related problems.

The paper is mostly well written but I do have several concerns about the analysis; these focus mainly on the analytical approach (see below). As it stands, the paper does not greatly advance our knowledge of the drivers of fire and fire risk, although the authors clearly have the data to do this. With additional analysis there could be an opportunity to draw out sub-regional trends with an attempt to separate climatic from other, economic drivers of fire activity. This would provide a more perceptive and novel account of the drivers of forest susceptibility to fire and the likely impact that a changing climate will have on future fire risk.

The main aim of our study was to examine the fire-resilience of undisturbed tropical primary forests, while also highlighting the magnitude of loss of such land cover across the region. Our results show a strong contrast in fire occurrence between undisturbed primary forests and land affected by deforestation. A key finding is that fires did not penetrate more than 2 km into primary forests anywhere in the region. This was observed in all parts of the region irrespective of drought severity, duration and timings. The uniformity and consistency of this finding was the reason why we did not split the analysis into sub-regions in the original manuscript. We did not perform an analysis of climatic drivers for fire activity on land affected by deforestation, as we believe that the inclusion of a detailed analysis of this nature would significantly lengthen the manuscript and diminish rather than strengthen the main message which states simply: protecting the integrity of forests buffers against future drought and fire events. We have, however, expanded the discussion of different climatic drivers of drought occurrence in the manuscript and have explored the consequences of splitting the analyses into sub-regions. More details on these aspects are given in response to the specific comment concerning lines 83-88 of the original manuscript.

Proposed Minor/Major Amends

Line 55 – 'loss of forest vegetation which covered the majority of the region since the last ice age' greatly over simplifies the long-term vegetation dynamics of this region, with some authors contending that during the LGM (which would be a better term to use rather than 'ice age') there was extensive forest loss and expansion of savannah (e.g. see Bird et al. 2005) although others have disputed this interpretation.

Thank for identifying this ambiguity. We indeed meant that forests covered the majority of the region during the Holocene. The phrase "since the last ice age" was replaced with "at least since the end of the Last Glacial Maximum" (line 54).

Line 60 – suggest adding 'at least' before 'the last 15,000 years' given that the Page et al. (2004) reference indicates peat genesis earlier than 20,000 yrs BP whilst other peatlands in the region may have had an even earlier genesis (e.g. Anshari et al. 2001).

Added "at least" and changed "the last 15,000" with "the last 20,000 years" (line 59), as indeed this a more appropriate estimate for the given reference (Page et al., 2004).

Lines 62 and 63 – Can the authors add another sentence by way of explanation of how the forests have been 'effective at preventing this important carbon pool from being released into the atmosphere by fires'? In part this is due to the ever-moist, humid microclimate in closed canopy forest (i.e. potential fire fuels are too moist to support combustion). For peat swamp forests this is also an important consideration, but fire resilience is also a function of an intact peatland hydrology. Furthermore, it appears from the palaeo-record that most fires in the humid forests of SE Asia have been associated with human activities – i.e. most fires have anthropic rather than natural (lightning strike) ignition sources. Given, until recently, relatively low human access into primary peat swamp forests, this has also limited forest fire ignitions – nevertheless, there is some evidence of use of fire/evidence of burning at some peatland sites (e.g. Anshari et al. (2001) for Danau Sentarum, Hope et al. (2005) for Kutai peatland, Cole et al. (2015) for Sarawak peatland).

The statement was specifically referring to the climate regulation function of tropical forests, which is discussed in the previous and the following paragraph. We have amended the sentence (lines 61 – 63) to ensure that it is clear what the statement implies:

"Throughout this time, forests were very effective at preventing this important carbon pool from being released into the atmosphere by fires through its regulating effect on climate at local and regional scales."

Line 81 – Neither of the two cited studies (Hooijer et al. 2012, Evans et al. 2019) demonstrate a link between lower peatland water table at the plantation/forest edge and increased peat flammability, although, in the presence of fire ignition sources this could be a reasonable assumption. I suggest adding 'potentially' before 'increasing peat flammability' and note the additional requirement for an ignition source (i.e. fuel load alone is insufficient to increase the risk).

We have used the term "flammability" in a general sense where the presence of an ignition source is an inherent prerequisite. To remove any ambiguity, we have replaced "flammability" with the more precise term "ignitability" throughout.

Lines 83 to 88 – This paragraph greatly over simplifies the fire regimes in this part of SE Asia. This is a major criticism of the paper, and given the subsequent focus in the analysis on climatic (rainfall) anomalies that follows, I am also critical of the simplified methodology used to identify links between climate and fire occurrence. There is considerably more regional detail that could be brought out of the data sets that might help separate climatic from other drivers of fire occurrence.
As the authors briefly indicate, drought occurrences in Indonesia follow various patterns. The variability of rainfall is often linked in literature to the ENSO phenomenon and it is now generally recognised that the warm phase of ENSO is indeed associated with reduced rainfall and convective activity over some parts of the Indonesian archipelago. But, ENSO does not have the same strength of influence across all regions of Indonesia: this is a nuance often missed in literature, as it is here. For example, while Langner and Siegert (2009) found that wildfires affected an area three times larger during ENSO years (1997-98, 2002 and 2006), they also found a pronounced difference in fire occurrence across the region, and, importantly, that ENSO was not the only driver of recurrent fire years. Aldrian and Susanto (2003) divide Indonesia into three regions according to mean annual climate patterns. Precipitation and fire activity in central Sumatra (including Riau) are not as strongly coupled with the ENSO phenomenon, but more with the Indian Ocean Dipole (IOD, e.g. Field and Shen 2008). The ENSO and IOD can interact in their effects: all of the major SE Asian fire events that occurred from 1960 to 2006 happened during years of drought induced by ENSO and/or IOD conditions, with the 1997-98 fires occurring over a period that saw the strongest recorded ENSO and IOD events in the 20th century. Pan et al. (2018) also found that IOD plays an important role alongside ENSO in modulating the intensity of Indonesian fires: when the IOD is positive Indonesian fires are at least twice as intensive as when the IOD is negative. Importantly, they also show that the ENSO will have heterogenous impacts across Indonesia: fires are more intense in southern Kalimantan than in southern Sumatra in all ENSO years by at least 36%, even when less severe drought conditions are found in southern Kalimantan. Furthermore, during the 2019 fire season the haze in Riau reached dangerous levels (in air quality), and this was during a positive IOD but neutral ENSO – so drought in central/northern Sumatra is driven more by IOD than ENSO events.

To further complicate the link between fires and ENSO/IOD, Gaveau et al. (2014) showed that the 2013 fires in Sumatra were widespread but occurred during a short dry period – i.e. a normal year, not an ENSO or IOD year and were likely associated with oil palm agriculture, i.e. they were more strongly driven by economic rather than climatic factors. And to add further complication, Oozer et al. (2020) report that the 2013 fires and haze in Sumatra were influenced by tropical cyclone Bebinca and the dry phases of a strong Madden-Julian Oscillation event. In summary, there are additional climatic patterns and events, apart from ENSO and the IOD that need consideration when analysing peatland fire events in different parts of Indonesia and whilst ENSO and the IOD are certainly important drivers of fire, it is incomplete to attribute their occurrence to only one or even both of these events.

We thank the reviewer for this thorough and insightful comment. We agree that additional discussion on local variability of climatic drivers in the region highlighting the complexity would be useful to provide further context. We have therefore expanded the discussion of different climatic drivers of drought occurrence in the relevant paragraph in the Introduction (lines 82 - 91). However, we disagree with the assessment of the manuscript "*...given the subsequent focus in the analysis on climatic (rainfall) anomalies that follows, I am also critical of the simplified methodology used to identify links between climate and fire occurrence.*" The rainfall anomalies presented in the manuscript play only peripheral role in deriving our main message, and were simply provided to give further context. As highlighted above, we did not aim to analyse and identify specific relationships between climate and fire occurrence in the region. We added visualisations of rainfall anomalies in the Figures 3 and 4 simply to illustrate that while total precipitation over the region and fire activity in areas affected by deforestation varied from year to year, fires only very rarely penetrated into undisturbed primary forests, and almost never deeper than 2 km. This was true for all of Sumatra and Kalimantan, both in regions where droughts are primarily driven by El Nino (south Sumatra, south Kalimantan) and in areas where climate is more affected by positive IOD (central/northern Sumatra). Given the consistency of this finding across the region, which underpins the main outcome of this study, we have presented the analysis in the manuscript in a single figure.

In response to the reviewer request, however, we have split the data presented in Figure 4 into three different sub-regions; central Sumatra, west Kalimantan and south Kalimantan (figs. S1 – S4, manuscript Supplement, pp. 21 - 24). South Sumatra, which shares climatic patterns with south Kalimantan (Aldrian and Susanto 2002), was not included in this analysis as there were very few undisturbed primary forest grid cells left in 2002. Precipitation anomalies shown for the three sub-regions in the figures above indeed indicate different patterns of dry season occurrence. However, burning events did not penetrate more than 2 km into primary forests neither in areas where droughts are more influenced by IOD (central Sumatra and west Kalimantan) nor in areas where ENSO is more important (south Kalimantan). This key result demonstrates that despite the enormous variability in climate across these sub-regions during the past two decades, climate and fire was not the most important driver of primary forest loss across the region. We believe that this critical message is not evident in the majority of previous studies focusing only on assessing climate variability versus fire activity in the region. We collectively believe that a detailed analysis of the complex relationships among large scale climate variability modes, precipitation and fire activity in areas affected by deforestation, while potentially useful in its own right as another paper would, if included in this study, significantly dilute the main focus and message of this manuscript. As R1 notes in their comment, there’s already a large and growing number of studies which are focused on this complex issue (references 43 – 46, 50, 51, 58 and 60 in the manuscript). As such, we do not think that it is feasible to both I) include a substantial sub-regional analyses of climate and fire relationships and II) still keep the focus and the main message of the study clear and convey the information in a short format paper.

In order to address the reviewer’s and editor’s concerns, we propose to include the new figures (S1 – S4) as a manuscript Supplement, and refer to these new figures from the Figure 4 caption.

Lines 132-146 – Section on forest fragmentation. Can the authors confirm that they did not misinterpret the degradation status of peat swamp forest. Peat swamp forests in this region show a natural zonation from tall, closed canopy mixed swamp forest on the margins of the peat dome to low pole, often more open canopy forest on the central parts of the dome. In some forest classifications the low pole forest is mis-classified as degraded/logged, owing to its naturally lower stature and more open canopy.

The definition of “undisturbed primary forests” at 1 km scale used throughout the study is based on primary forest extent in the year 2000 dataset (Morgano et al., 2012). In this product, forest was defined as tree cover with a minimum height of 5 m and canopy cover of at least 30% at the Landsat pixel scale (~25 m), and primary forest as mature natural forest cover that has not been completely cleared in recent history and consisted of a contiguous block of 5 ha or more, including both intact and degraded types. Note that while the Morgano et al., (2012) dataset does differentiate between intact and degraded primary forests, we have included all primary forest pixels in our analysis. As a result, even if some low pole forest areas were misclassified as “degraded” at the Landsat pixel scale, they were considered as primary forests in our analysis.

The Methods section describing the primary forest loss methodology was amended (lines 258 - 262) to clarify that we considered all primary forest pixels as primary forest: “…, including both intact and degraded types[61]”

Line 172 – replace ‘what’ at the end of the line with ‘that’

Thank you for noting this mistake, swapped ‘what’ with ‘that’.

Line 198 – what definition is used by the authors to define a ‘dry’ year? Given my comments earlier (lines 83 to 88), this analysis is overly simplistic. Spatial fire occurrence will vary according to prevailing climatic conditions, with a positive IOD having a stronger influence on a prolonged dry season in central Sumatra (e.g. Riau, north Jambi) than an ENSO event, which is more strongly linked to drought in southern Sumatra and southern Borneo. Also, as again commented earlier, some years see widespread burning even during a ‘normal’ dry season (e.g. Sumatra, 2013 – see Gaveau et al., 2014) that is driven by economic rather than climatic factors. It must be possible for the authors to draw out some of these nuances from their data in order to provide a more robust, informative and novel study – e.g. by separating climatic trends for different geographical regions (Indonesian provinces) and for different strengths/drivers of rainfall anomaly (ENSO vs IOD etc).

The term “dry” year was used to indicate years with prolonged negative precipitation anomalies as shown in Figure 3a and Figure 4a. To avoid the possible ambiguity, we have now replaced “dry years” with “years with negative monthly precipitation anomalies” (lines 205 - 206). We intentionally did not define strong El Nino or positive IOD phase years as “dry”, as this would lead to assumption that no drought can occur during a non-El nino or neutral/negative IOD years. Indeed, dry season length, severity and timing differs greatly within the region, but despite of this local variability, precipitation over the whole region is clearly related to fire activity. Years with larger number of negative monthly precipitation anomalies across the region (i.e. 2002, 2006, 2009, 2014, 2015 and 2019) fire activity was higher as well. 2007, 2008, 2010 and 2017 were relatively “wet” years (Figures 3a and 4a) during which fire activity was relatively low. Note that in contrast to our definition of “dry” year, Gaveau et al., (2014) define positive ENSO/IOD years as “dry”, and therefore conclude that severe burning episodes are now happening during “normal” or even relatively “wet” years.

In order to show rainfall anomalies and contrasting fire regimes for sub-regions where climatic trends are driven by different drivers, we have split the analysis presented in figure 4 into three separate figures and have included these figures as a supplement to the manuscript. Please refer to our response to the comment regarding lines 83 – 88 of the initial manuscript version on page 4 of this document.

Lines 220-221 – The proposed length of the fire return interval is speculation (hundreds to thousands of years), and as indicated earlier, a number of studies have actually shown evidence of forest fire in the palaeo-record. Suggest this statement is rephrased.

We agree that the suggested fire return interval was indeed a speculation. The sentence was rephrased removing return interval estimate as follows:

“Extremely low numbers of fire-affected grid cells in such forests recorded during the study period suggests a very long fire return interval.”

Lines 278 – 299 – Was any effort made to compare the fire activity data with an actual burned area product or with field checking – i.e. have you confirmed that your active fire detections actually reflect fire impacts (burned area) on the ground - either using optical remote sensing or ground data? As the authors acknowledge, there are significant issues (errors of commission and omission) with using so-called ‘hot spot’ data : hotspots are indications of potential fires, but they are not equal to actual fires and confirmation of an actual fire from hotspot data requires ground truthing. Conversely, low temperature, smoldering peat fires may evade detection using this approach. Regular users of MODIS data for fire monitoring in Sumatra, for example, report very high levels (even as high as 90%) of both commission and omission errors. Whilst the authors have taken steps to avoid over reporting of fire detections, the levels of uncertainty associated with using these data need to be better acknowledged. The authors also need to acknowledge the likely scale of the omission errors.

The Collection 6.1 MODIS active fire dataset (Giglio et al., 2016), which was the basis for our fire-affected area estimates, was used as a proxy for fire activity (or presence) in any given year within 1 km^2 grid cells in the region. The critical detail here is the fact that we did not estimate total fire-emitted energy or burned area, as such attempts would inevitably lead to large uncertainties in the estimates and would perhaps require validation. In terms of detecting fire presence during a satellite overpass, the active fire dataset performs much better than what the reviewer suggests here, especially for larger fires. Giglio et al., (2016) estimated ~8% false alarm rate (error of commission) for Equatorial Asia based on validation against high resolution optical imagery. Low levels of false detections were also confirmed during the 2013 burning episode in north Riau, where 96% of MODIS active fire pixels where found to fall within the burned area extent estimated using high resolution imagery (Gaveau et al., 2014). Omission errors for small fires of the MODIS active fire product are inevitably high due spatial resolution of the sensor (pixel size 500 m). However, for fires over 0.125 km^2 in size (consisting of more than 140 high resolution (30 m) optical imagery pixels used for validation), Giglio et al., (2016) estimate omission error of 10% globally, while for fires of 0.250 km^2 or larger, omission error is below 5%.

It is true that low temperature smouldering fires are more difficult to detect using thermal anomalies algorithms (Giglio et al., 2016), however, such fires typically have long residence times and as a result detection probability increases with each satellite overpass. A recent study comparing fire emission inventories based on burned area and active fire products for Indonesia showed that active fires based inventories reproduce regional aerosol optical thickness better when compared to area burned based methods, and result in smaller underestimation of fire activity in extreme years (Liu et al., 2020).

A discussion of the errors has been added to the relevant section of the Methods (lines 310 - 313).

Reviewer #2

(Remarks to the Author):
Dear authors

I read the manuscript with great interest. It clearly demonstrates how deforestation and forest fragmentation can be linked to fire occurrence. The study covers two main Indonesian islands that are highly relevant to the deforestation and forest and peat fire problems. This study provides another proof to convince the need to maintain the remaining primary forests in Indonesia. I have several points to clarify.

Title: This study pay a balanced attention to primary forests both on peatland and dryland. Why is only the one on peatland reflected in the title?

Thank you for this suggestion. We agree that the focus of the manuscript is primary forests, both on peatlands and non-peatlands. The new title is now:

“Near-complete loss of fire-resistant primary tropical forest cover in Sumatra and Kalimantan”

Line 101-103: I think the claim is too much. Please clarify what do you mean with “substantially underestimate.”

We agree that the phrase “substantially underestimated” is ambiguous. We were referring to large discrepancy between estimates for total primary forest extent (~30% of region in 2019) and fire-resilient undisturbed primary extent of this study (~3% and 4.5% for peatlands and non-peatlands respectively). The sentence has been rephrased (lines 103 - 105) to: “As a result, the magnitude of forest degradation and associated increase in high fire risk area is far greater than what the total extent of the remaining primary forest in the region would suggest".

Line 113-114: Figure 1 and Figure 2 do not include data on highland montane forest. How did you state that the large extent of deforestation take place in that type of forest?

In this study we did not differentiate between lowland and highland primary forests, and both forest types were represented by the "non-peatlands forest" category. The ambiguous sentence has been changed to now state: the loss has occurred in non-peatlands and peatlands forests, without referring to lowland and montane forests (lines 113 - 115).

Line 128: The moratoriums apply not only on primary forest in peatland. The moratoriums apply on primary forests (on peatland and dryland) and on peatland (forested and not forested). Please provide specific reference for the Indonesian moratorium policy.

Thank you for clarification. We were specifically referring to moratorium on forest clearance. We have changed the text to now explain the moratoriums apply to both peatlands and non-peatlands, and added a supporting reference detailing the moratorium (Purnamo et al., 2012). The sentence now reads (lines 126 - 129):

"This may be a sign that the policy changes and incentives in Indonesia such as the peatland restoration plan and moratoriums on primary forest clearing for oil palm plantations and logging operations[65] have begun to improve the situation[66,54]."

Line 151: Did you do statistical analysis to test the significance? Does this mean that the fire-affected area is not significantly different between areas with partially, mostly, and completely deforested?

Statistical analysis was not performed (fire-affected area estimates are for the whole region, not for samples), and "significantly" was used simply to emphasize important difference in fire occurrence in undisturbed forests when compared to areas affected by deforestation (discussion on lines 153 - 164) . Replaced with "notably" to avoid ambiguity.

Figure 3 and Figure 4: I see that Figure 3 is actually part of Figure 4. Can the figures be combined?

While Figure 4 indeed shows a subset of the whole dataset presented in the Figure 3, there is a small but important difference between the figures which is that Figure 4 shows (tracks in time) only the grid cells which were > 99% primary forest in year 2001. Since Fig. 4 clearly demonstrates the change in fire regime in areas affected by deforestation during the study period, it is critical in illustrating one of main messages of our study. By contrast, Figure 3 illustrates differences in fire occurrence and changes in area for the different primary forest cover categories across the region, but in itself, it does not show if deforestation during the study has resulted in increased fire activity on deforested land. An additional benefit of having Figure 4 is that it allows to show areas of primary forests located at different distances from the forest edge more clearly. Consequently, we believe that it is important to keep Figure 4 as separate figure, as it is instrumental in demonstrating the increase in high fire-risk area during the study period.

We have rephrased the Figure 4 caption to make the distinction between the Figure 3 and Figure 4 more clear (Fig. 4 caption):

"In contrast to Fig. 3, this Figure only shows (b) peatland and (c) non-peatland areas which were undisturbed primary forest (grid cells having 99% or more primary forest cover) at the beginning of year 2002."

Line 153-157: This study found that the percentage of fire affected in mostly deforested area is higher that that in completely deforested area, both in peatland and non peatland. Do you have any explanations about this findings? The readers might expect that the worse the level of deforestation, the higher the risk of fire. Hence, additional explanation would be useful to clarify the 'unexpected' findings.

The interpretation of these results was presented in the following paragraph. However, perhaps it was not clear enough. The paragraph has now been rephrased and split into two paragraphs (lines 164 – 182) explaining why we think fire occurrence rates are highest in mostly deforested land (coincident with frequent land clearance fires) and why they are somewhat lower (but nonetheless relatively high) on fully deforested land:

"These results illustrate that once the microclimate regulation of the closed-canopy primary forests is lost, the many anthropogenic ignitions in the region[42] often develop into persistent and large burning events during dry periods. The highest fire-affected area on mostly deforested land estimated in this study can be attributed to extensive use of fire as a tool for clearing the land for agriculture in the region[5,58,66]. The tool is very effective because highly fragmented and degraded patches of primary forests contain dead woody fuel and exhibit little ignition-resistance[33] and as a result are easily ignited and consumed by both intentional burning or escaped fires.

Our results indicate that there is a long-lasting increase in fire risk in the deforested landscapes, extending beyond the immediate deforestation period. While areas with fragments of primary forests (between 50% to 1% cover) had the highest percentage of fire-affected grid cells, fire occurrences were nearly as high in land which has been completely deforested (less than 1% primary forest cover (Fig.3)). Although deforestation fires are reduced once the land is fully cleared of primary forests, fire is still used in agricultural practices and as a means to prevent secondary regrowth[58,52]. Degraded and unmanaged peatlands in this region have therefore short fire return intervals and as a result such areas become dominated by flammable grasses and ferns and effectively switch to a stable treeless state[31,14]. While in some managed land cover types, such as large scale oil palm and pulp plantations fire may be undesirable and is actively suppressed, such areas nonetheless have higher fire occurrence rates when compared to primary forests[15]."

Line 167: The statement that fire is extensively used as a tool for deforestation needs reference.

Added references (Miettinen et al., 2012, Gaveau et al., 2014 and Purnamo et al., 2017) to support the statement.

Line 251-252: This study used different sources of land cover maps (for land cover map of 2000 and 2001-2018). How did you ensure that the maps have the same criteria in classifying the undisturbed primary forest? What is the implication to the findings of this study if the maps used different criteria in classifying primary forests?

Only Morgano et al., (2012) primary forest dataset was used in determining the extent of primary forests in the region in year 2000. The annual tree cover loss dataset (Hansen et al., 2013), was used solely for purpose of estimating tree cover loss which has occurred within primary forests during the study period (2001 - 2018). The tree cover loss product does not differentiate between primary forests or other tree cover, and as a result, classification of primary forests is solely dependent on and consistent with the Morgano et al., (2012) dataset. Validation of the primary forest dataset against the official Ministry of Forestry of Indonesia primary forest map for year 2000 shows high degree of agreement between the products (~90% overall agreement, discussion on this added to the Methods section, lines 262 - 263), which indicates that the key results of this study would have been similar if an alternative primary forest map for year 2000 would have been employed.

Line 265-268: Do you have any reasons to classify the rate of deforestation in that way? The classification with unequal interval usually needs to provide reasonable explanations.

Thank you for pointing out the omission of explanation of rationale behind the selected thresholds used to define different primary forest cover categories. The explanation has been added to the "Primary forest loss and extent" section in the Methods (lines 282 - 294).

"Grid cells were classified into one of four categories: 'undisturbed forest' (over 99% primary forest cover); 'partially deforested' (50% < primary forest cover < 99%), 'mostly deforested (1% < primary forest cover < 50%); and 'fully deforested'(<1% primary forest cover). The 99% primary forest threshold in classifying undisturbed primary forests was used in order to allow for a small number of deforestation to occur within a 1 km grid cell (up to 16 out of 1600 25 m Landsat pixels in a 1 km grid cell) before it was reclassified as partially deforested. This was done to accommodate a small amount of natural canopy succession and/or erroneous forest loss pixels. Correspondingly, a 1% primary forest threshold was used in defining fully deforested grid cells. While the selected 99% threshold caused the estimated extent of undisturbed primary forests to be approximately 20% larger when compared to a scenario when a strict 100% threshold was used, this only had a negligible effect on fire occurrence rates for the category. However, lowering the threshold further resulted in a large increase in fire detections in undisturbed primary forests, hence a 99% threshold was used."

The quality of land cover maps: This study used land cover maps as the main inputs for analysis. The accuracy of the findings of this study then also depends on the quality of the land cover maps. Although the sources of the maps have been stated in the text, the authors should provide sufficient description about the quality of the maps, for example about the criteria used to classify undisturbed primary forests and particularly about the accuracy of the classification.

A discussion of the accuracy of the primary forest dataset and the tree cover loss dataset is now included in the Methods. The paragraph stating the criteria used to classify primary forests and tree cover loss in the corresponding dataset has also been rephrased (lines 257 - 269):

"In this study, primary forest extent in region for the year 2000 was determined from the primary forest cover dataset based on multi-temporal analysis of Landsat imagery[6,61]. In this product, all Landsat pixels (~25 m x 25 m) with tree height of at least 5 m and canopy cover of > 30% were considered as forest, and primary forest was defined as old-growth natural forest forming a contiguous block of at least 5 ha and which has not been deforested in recent history, including both intact and degraded types[61]. The product was shown to have 90.2% overall agreement (80% Kappa statistic) when compared to the primary forest map for the year 2000 of Ministry of Forestry of Indonesia[6]. In order to determine loss in primary forest cover in Sumatra and Kalimantan during the study period, we performed a co-located analysis of the primary forest cover of the year 2000 data[6] and a subset of the version 1.6 global annual forest cover loss dataset covering 2001 through 2018[62]. In this product, tree cover loss is defined as a stand replacement disturbance. Validation of tree cover loss for tropical regions suggest that forest loss was correctly identified in more than 80% of the cases (producer's accuracy 83.1%)[62]. "

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22nd Oct 20

Dear Dr Nikonovas,

Your manuscript titled "Near-complete loss of fire-resistant primary tropical forest cover in Sumatra and Kalimantan" 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).

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have dealt with reviewer comments and revised the manuscript to my satisfaction. I have no further suggestions for amendments.

Reviewer #2 (Remarks to the Author):

I carefully read the responses from the authors and the revised version of the manuscript. I saw that all my comments, questions, and suggestions have been well responded. The authors provided clear explanations on them. The manuscript has also been well revised in response to my comments and suggestions. I recommend publication for the manuscript.