nature portfolio

Peer Review File

Charcoal in ocean sediments track fire regimes in Africa

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

30th Sep 22

Dear Dr Haliuc,

Your manuscript titled "Charcoal in ocean sediments track fire regimes in Africa" has now been seen by 3 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.

Specifically, for publication in Communications Earth & Environment to be appropriate, we will need you to:

1) Provide compelling evidence that charcoal profiles in marine sediments off the west African coast are a robust record of fire activity in subtropical and equatorial/tropical Africa;

2) Quantify and communicate uncertainties in sedimentation times for the upper 1.5 cm of marine sediment and demonstrate that estimated charcoal ages are accurate and robustly correlate with the presented modern fire data;

3) Provide an in-depth discussion of the related literature and clearly articulate the advance your findings offer to our interpretation of marine paleofire records and the potential implications for our understanding of fire regime in Africa;

4) Provide clear and comprehensive detail and justification for all methods and analyses in the main manuscript (please note: we allow unlimited space for methods).

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

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 reviewers' comments with a list of your changes to the manuscript text (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,

Alienor Lavergne, PhD
Associate Editor
Communications Earth & Environment

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 (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

General Comments:
Haliuc et al. analyzed charcoal and fire data in southern Africa to relate palaeofire and observational fire data. They find that open savanna-grassland ecosystems produce great numbers of small, elongated charcoal whereas forested ecosystems produce low numbers of

squared charcoal. They also speculate as to how these findings might impact the interpretation of several down-core marine records. This manuscript builds on previous work focusing on charcoal in marine environments, but is looking at a spatially broader area than has been attempted before.
The analyses are generally interesting and should make for an impactful paper in the palaeofire research community. However, the manuscript falls short in a few ways. Namely, it presents two PCA analyses that do not clearly support the inferences made (or alternatively, the inferences made are not clearly explained in the manuscript). The authors could more clearly exhibit how their analyses support their conclusions. Additionally, there are some generalizations made in the main text, which although more adequately explained in the SI, would benefit from greater nuance to ensure accuracy and precision. To this end, the SI roughly the same length as the main text and also includes material that is not as closely linked to the main text as might be expected. I would suggest this material be moved to another paper or more clearly integrated into the main text.
Overall, I think this manuscript has great potential for the palaeofire community but needs revision in order to ensure clarity and its intended impact.

Specific comments:
L39: since -> for
L41-44: references to support these factual statements?
L50: I am not sure I understand the meaning of 'current field'
L61-64: As phrased, this is not a wholly accurate assessment of the state of palaeofire research. There is a large body of research which has empirically linked palaeofire records to these quantitative metrics. For example, the whole entreprise of peak analysis of charcoal provides fire frequency metrics. Likewise, many of the references cited later on (e.g., Hennebelle et al.) directly did what is claimed to be a limitation.
L65-67: Akin to the last comment, the presentation of these studies as 'few' is an oversimplification that misrepresents the literature. The studies referenced here are representative of a small subset of published charcoal calibration studies. Although I would agree that more work of this kind is needed, the way it is presented here does not do justice to the numerous insights we do have from charcoal calibration studies.
L112-113: I don't think this is a statistically accurate phrasing. These variables are associated with the PC1 axis, but, unless I am misunderstanding the analysis, I don't believe the explanation can be derived from the eigenvectors.
L113-116: In my view of figure 3, there is so much overlap between the regions that any assertion of coherent patterns between the regions seems misplaced.
L116-120: All of these statements refer to Figure 3 as support, but I am not able to clearly see how this figure or the PCA analysis actually supports these statements. An alternative interpretation of the PCA results is that the variability of charcoal particle shape and size does not clearly group between regions.
L120-122: Why is that? The reasoning here should be made explicit.
L127: disclose -> identify/assess?
L129: Figure 3B is too small for the reader to see the results described here. Consider zooming in on the origin here.
L135: The positive and negative directions of the principal components are not statistically meaningful here. This should be rephrased.
L138-142: Akin to comment referring to L116-120, these interpretations are not easily seen

in Figure 3. Likewise, the phrasing here seems to overstep the interpretive power of a PCA to infer causative relationships between variables.
L159-161: This isn't really demonstrated though. Two separate PCAs can only be compared so much. I feel it is a bit of a leap to far to make this conclusion.
L169-171: Many others would probably say that fire itself controls the amount of charcoal produced and preserved in sediments.
L176-178: Feurdean (2021, Biogeosciences) showed the exact opposite with experimental data.

Supplementary Information
Overall, I am surprised by how long the SI is relative to the main text (they are roughly the same length). I would urge the authors to move some of this material to the main text as it would help to support many of the interpretations made in the main text. In particular, much of the background information provided about charcoal transport and pathways would be helpful to readers in the main text and would help to address several of my comments above.
L85: The dispersion modelling text seems out of place here and in the manuscript generally. This text could make for an interesting, self-standing paper of its own that would be of interest to the palaeofire community. But, if I am understanding correctly, all of this text and analysis was included solely to support the statement about estimated source area of charcoal in the main text?

Reviewer #2 (Remarks to the Author):

General Comments:

This manuscript explores charcoal records from "mud-water" or surface sediment samples collected from the top 1-2 cm of ocean sediment samples off the African coast. The authors have previous experience working with marine sediment records and leverage their expertise to explore linkages among sedimentary charcoal morphometrics, terrestrial fire regimes and observational fire data. The methodological approach and analysis are appropriate and the discussion and conclusion report several key findings. Through measuring the ratios of small and elongated charcoal, the authors link tree- and grassland-fires to changes in charcoal ratio. The key finding that fire intensity is an important driver of charcoal morphotypes has important implications for fire scientists using charcoal morphology to reconstruct past grassland-savanna fires in Africa. The authors conclude that evidence of grassland-savanna fires (based on charcoal morphology) can be used to link past changes (millennial-scale climate reconstructions) to understand the role of fires in contributing to regional and global carbon budgets.

I believe the manuscript is well-written, provides new and important calibration tools for the paleofire community, and warrants publication in Communications Earth & Environment. However, I do have some specific concerns about the linkages to fire intensity and some of the authors' use of specific (or sometimes unspecific/vague) fire regime characteristics throughout the manuscript. I would like to see the authors clarify the use of their terminology as related to fire regime (e.g. fire intensity) and a few minor comments outlined below.

Specific Comments:

Page 3: reference to "one of the major limitations of paleofire reconstructions is that charcoal records provide relative changes in fire activity...". What do the authors mean by this statement? Are relative changes specifically linked to marine charcoal records or all charcoal records? Also, are CharAnalysis metrics, for example (fire frequency, charcoal influx, peak detection etc), relative or absolute?

RESULTS:
Page 5: "The source areas were built by assuming the dominant transport is fluvial....." It is helpful the authors discuss a dispersion and deposition model in the SI, and also suggest "...most micro char is deposited within a 300 km range, within the hydrographic basin limits". There appears to be limited experimental or calibration research to test this assumption. Additionally, the assumption that wind is the primary driver, suggests river transport of suspended microchar in major river systems may be an unimportant component. Some studies have explored river delta charcoal and suggested otherwise (e.g. see Haberle and Maslin 1999). More justification is warranted on why river transport is minimized. Hydrographic basin analysis is a helpful spatial domain to explain much of the variance in charcoal ratios, however, can the authors address how hydrographic basin, or alternative spatial units of analysis, may impact the ability to understand source type (e.g. wind versus water transport).

Reviewer #3 (Remarks to the Author):

Major comments
The manuscript compares the structure of charred particles found and measured in the tropical ocean off and around Africa including the southern part of the continent that is not tropical with wildfire data from the continent. This type of study is crucial for reconstructions of past fire to attempt to unravel the past regimes, their changes, and their interactions with other environmental element, for instance vegetation. Here the authors are probably more concerned by climate issues according to the end of the manuscript dedicated to orbital forcing and climatic periods.
The paper is rather convincing, thanks to sophisticate analyses and comparative statistics. But, I detect a serious problem in the study, and precisely within the Supplementary information. The problem concerns the sedimentation time assessed in the top-most 1.5-cm sediments that served to extract charcoal for proxy-fire data (SI 2, excel sheet). The ages at 1.5 cm are calculated based on the sedimentation times of each series that result from the isotopic chronologies. Only 16 samples on 43 present an assessed time, meaning that the time content in the sediment of 27 samples is unknown. However, this comment would be not important if the “age at 1.5” (column k) was not weird. Indeed, the range of ages varies between 40 and 2680 years. That data suggest a huge temporal variability between sediments, and that the modern wildfire data used for the comparison with the fire-proxy could be totally unconnected. In other words, I had the feeling that authors compared sometimes and somehow fire data with a fire proxy that is not synchronous. I would have no real problem if the age at 1.5 cm was within a 50-year range, but only two sample are

assessed within this 50-yr range, 10 between 120 and 960 yrs, and 3 between 1790 and 2680 yrs. The mean age of sediment is 720 years! That is extremely problematic.
Whatever, the quality of the analyses, this point of the age at 1.5 cm is a fundamental weakness that which obliges me to reserve my opinion, whereas I first appreciated and supported the whole strategy of the manuscript until I check in detail the SI.
Further, the end of the manuscript on pages 10 and 11, between lines 213 and 237 seems at this stage, without any quantitative reconstruction a pure speculation. This should be removed from this short and technical paper.
These inconsistency between fire statistics and age of charcoal must be solved before any decision of acceptance for publication. Other minor comments are listed below.

Minor comments
Line 28 and in the whole manuscript: change uppercase of Central, Southern, Northwest, etc. These adjectives of geographical location are not propre names of areas or regions.
L 30: “forest” instead of “forested”.
L 34: “revealed” instead of “imply”.
L35: “of fire-inferred carbon” instead of “of fire carbon”.
L 41: remove because useless: “the second largest (…) on Earths”.
L 44 and in the whole text: change and harmonized “burnt” by “burned”.
L 49: and feedbacks?
L 53: the ref 10 is rather obsolete (almost 20 years, and other papers more recent exist), and the time depth based on “natural archives” in this paper is very limited.
L 62 and after: in paleoecology of tropical grasslands, some terms of past fire like “frequency” or “number” classically used in temperate of boreal countries are not applicable, because the temporal resolution of charcoal measurement per sample is larger than the fire intervals in tropical ecosystems. Further, some terms like “intensity” still remain an inaccessible quest in paleoecology, because the charcoal production is not related to intensity but to the severity of fires. Moreover, the charcoal production does not result from a linear relationship with wildfire intensities, but probably a inverse U-shape distribution where low and high intensities do not result enough charcoal. At too high intensities, the whole organic matter is consumed, and no charcoal are produced.
L 22: ref 22 is an obsolete paper. Better, more sophisticated and more recent papers have been published since 1998.
L 67-69: I am stunned that only two papers have been published on this methodological (taphonomical) issue, which is crucial in reconstruction based on marine sediment. Would that mean that most reconstruction of past fire based on marine sediments relies on theoretical principles not supported by data? Further the paper 24 is very obsolete.
L 73: the distance of oceanic coring from continent and wildfires is also a crucial issue that question the possibility to reconstruct a continental process (wildfire) with marine sediment. Further, the transportation time of particle is probably long in the ocean that are characterised by strong oceanic streams, meaning that particles could be trapped in sediments at hundreds km from their source areas, and charcoal are probably dispersed and mixed in the ocean on millions of km^2. I am skeptical on the use of charcoal from marine sediments to reconstruct continental processes based on very small and light particles that float very well.
L 74: add (>10 µm) after microcharcoal.
L 74-82: very descriptive aims; please be more scientific and transform the text with a

working hypothesis to test, and indications about the functional response expected, and then develop the objectives.
L 98: add “conceptual” before “model”. Here, there are no formal model, I mean mathematical based.
L 102, 103: nb/g, this is obsolete. Please adopt nb.g^-1
L 103-107: interesting but please test the differences between box plot in order to support the description of differences or similarities.
L 111: instead of “breadth” I would have used “width”.
L 112: with only 5 parameters, is rather expected a high percentage. Further, I quester whether the ratio (“elongation”) and area are independent on the other parameters, i.e. from length and breadth.
L 116: I don’t understand on what the KS-tests were applied. What did you test precisely?
Figure 3 is too small; it is very difficult to unravel. What represent the oval-shape perimeters. It contains too many acronyms and symbols not explained. The symbol-coloured chart of the right contains 8 categories, but these eight categories are impossible to find in the sub-fig B. In the figure caption, the Graminoids and Trees vegetation should be substituted by grassland-savanna vegetation and forest or woodlands. Indeed, there are no tree vegetation without herbs and forbs, and graminoids vegetation are also associated to forbs and often dwarf-shrubs (e.g., the karoo vegetation in South Africa that is not a savanna nor a grassland). Better written on line 160.
L 120-124: I agree, but this section must be moved into the Discussion section. It is not a result, but a discussion sentence on results.
L 124: ref 26 is an inappropriate reference because archaeological. The processes are not the same in nature and in archaeological contexts.
L 130: “mean fire radiative power”, is it the “intensity” of the rest of the text?
L 145-147: move this sentence into the Discussion section.
Discussion section: too long. The paragraph between lines 213 and 237 is very speculative; it is only an extrapolation of present outputs to former studies without any actual results such as quantitative reconstructions. These paragraphs are useless.
L 152: “a similar approach on marine sediments (…)”
L 153: Mediterranean with upper case.
L 166: “devastate” is not a scientific word to mean “burned larges areas”. “Devastate” is very subjective.
L 159-168: here, only papers matching with your outputs have been mentioned. But, are there contrasting papers, or papers that contradicts your results? Be honest by evoking all possibilities.
L 179: “submicron” are u sure?
L 184-185: ratio of 3.6 vs 3. Is the difference significant? The difference is 0.6 corresponding to 17%. Please, be more convincing.
L 192: “grassland and woodland” instead of “grasses and trees”.
L 195-198: other explanation for small grain-size of charcoal: lab methods vary between sites (cf. SI-1); but I think that the small fragment float long-time (weeks to months) before they attain the bottom of the see.
L 246-249: speculation. Delete.
L 291-297: woah! This is the weakness of the paper… no age for most samples. Most samples with ages, cover a very long period of several centuries, while the fire data are modern. That means that charcoal data are probably disconnected with fire statistics. See major comment,

above.
L 305-310: are there solid paper that demonstrate that the chemical treatment respects the integrity of charcoal? Several aggressive acid-attacks (HNO_3, HCl x2, HF) could result a digestion of charcoal fragments that are little charred due to low burning temperature (<300°C). Please provide evidence of the rational of this chemical treatment before measuring and tallying charcoal.
L 312: what is PPMA?
L 316: do u mean the concentration of microcharcoal number?
L 322 and after: too many unexplained acronyms (ASTER GDEM LC FRY…). Boring and painful!
Ref 11 and 13 are the same!
Ref 36 is a not a convincing ref. Check papers by Lynch et al 2004 in Canadian Journal of Forest Research and Ohlson and Tryterud 2000 in The Holocene.
Figure 1: too small fonts in the figures and the charts.

2nd of December 2022

Dear Editor,

Re: Manuscript reference No. *COMMSENV-22-0710-A*

Please find attached a revised version of our manuscript "*Charcoal in ocean sediments track fire regimes in Africa*", submitted for publication as a research article in Communications Earth & Environment, that fully addresses issues raised by the three reviewers.

The prompt summary of the reviewers' comments and specific recommendations were highly insightful and have benefited the manuscript considerably. We also thank the reviewers for their careful attention to details, which we have closely followed to clarify the content of our manuscript by addressing all the weaknesses raised.

Enclosed is a detailed account of how we have implemented the requested revisions. We organized the letter below into two sections: 1) a summary of revisions following editor' recommendations and 2) specific actions that we have taken in response to reviewer' comments. To assist you with assessing our revisions, we presented our responses in blue. We provide also a track changes version of the revised manuscript.

1) EDITOR COMMENTS:

a) Provide compelling evidence that charcoal profiles in marine sediments off the west African coast are a robust record of fire activity in subtropical and equatorial/tropical Africa;

We have clarified the main text by introducing information from the SI about fluvial and aeolian transport of microcharcoal from land to the ocean to discuss the source area of microcharcoal (See response to reviewer#1). In response to reviewer#1 and 3 we have also given more details about the few published calibration studies showing that microcharcoal preserved in marine sediments along coastlines records fire activity of the close continent.

b) Quantify and communicate uncertainties in sedimentation times for the upper 1.5 cm of marine sediment and demonstrate that estimated charcoal ages are accurate and robustly correlate with the presented modern fire data;

We have revised the chronology information table (Supplementary Data) providing missing information about the chronologies and sedimentation times for all the samples (n=128). In addition, we have modified the main text to inform the reader about our approach using these top-core samples, balancing between a minimum number of samples (robustness) versus the quality of the chronological control. We explained in more detail why we based our calibration on a spatial approach. Firstly, we included information about commonly used marine and terrestrial tools based on similar types of calibration using surface samples with the same issues. Secondly, we anticipate that the spatial differences in climate, vegetation and fire activity observed today in central western Africa versus southern Africa exceed their temporal variability over several past centuries. In particular, Genet et al. (2021) showed that the temporal variability of microcharcoal concentrations in interface cores off Iberia is within the range of

the modern spatial variability, which suggests that mean environmental conditions on land and in the ocean over the past century did not radically depart from conditions recorded by the instrumental period. This supports our comparison of the different datasets.

c) Provide an in-depth discussion of the related literature and clearly articulate the advance your findings offer to our interpretation of marine paleofire records and the potential implications for our understanding of fire regime in Africa;

We modified the introduction section presenting 1) the limit of charcoal records interpretation, 2) the limit of marine paleofire records interpretation and 3) the scientific questions we want to address: "The first inferences of fire regime characteristics from marine paleofire records were derived using indirect theoretical principles developed on charcoal from lake sediments and experimental studies. A peak in microcharcoal concentration or influx was interpreted as an increase in fire frequency and intensity, while the particle shape was used to infer intensity and vegetation type. Recently, it has been demonstrated that charcoal abundance and morphotype in marine surface sediments off Iberia can trace specific fire regimes on land, in particular fires of high intensity in a mixed open Mediterranean woodland vegetation[29]. However, there is no empirical data yet to support paleofire regime inferences made from marine microcharcoal records off Africa. Here, we aim to explore the relationships (...)".

d) Provide clear and comprehensive detail and justification for all methods and analyses in the main manuscript (please note: we allow unlimited space for methods).

We modified the method section by 1) adding SI information in the main text; 2) adding detailed information about the chemical treatment steps and their potential biases in charcoal interpretation; 3) adding information of what is Hysplit's model to avoid confusion by readers between the different modules of Hysplit.

We hope that the revisions in the manuscript and our accompanying responses will be sufficient to make our manuscript suitable for publication in Communications Earth & Environment.

We shall look forward to hearing from you at your earliest convenience.

Yours sincerely,
Aritina Haliuc, PhD, (corresponding author)

Address for correspondence:

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2) REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

General Comments:

Haliuc et al. analyzed charcoal and fire data in southern Africa to relate palaeofire and observational fire data. They find that open savanna-grassland ecosystems produce great numbers of small, elongated charcoal whereas forested ecosystems produce low numbers of squared charcoal. They also speculate as to how these findings might impact the interpretation of several down-core marine records. This manuscript builds on previous work focusing on charcoal in marine environments, but is looking at a spatially broader area than has been attempted before.

The analyses are generally interesting and should make for an impactful paper in the palaeofire research community. However, the manuscript falls short in a few ways. Namely, it presents two PCA analyses that do not clearly support the inferences made (or alternatively, the inferences made are not clearly explained in the manuscript). The authors could more clearly exhibit how their analyses support their conclusions. Additionally, there are some generalizations made in the main text, which although more adequately explained in the SI, would benefit from greater nuance to ensure accuracy and precision. To this end, the SI roughly the same length as the main text and also includes material that is not as closely linked to the main text as might be expected. I would suggest this material be moved to another paper or more clearly integrated into the main text.

Overall, I think this manuscript has great potential for the palaeofire community but needs revision in order to ensure clarity and its intended impact.

Specific comments:

-L39: since -> for

Done.

- L41-44: references to support these factual statements?

We have added a new reference to support the statement *(Werf, G. R. van der et al. Global fire emissions and the contribution of deforestation, savanna, forest, agricultural, and peat fires (1997–2009). Atmos Chem Phys 10, 11707–11735 (2010).*

-L50: I am not sure I understand the meaning of 'current field'

We thank the reviewer for raising this point. We have modified the phrase to make it clearer: "Because the projected climate change far exceeds the time-scale of instrumental or air/space borne observations".

-L61-64: As phrased, this is not a wholly accurate assessment of the state of palaeofire research. There is a large body of research which has empirically linked palaeofire records to these quantitative metrics. For example, the whole entreprise of peak analysis of charcoal provides fire frequency metrics. Likewise, many of the references cited later on (e.g., Hennebelle et al.) directly did what is claimed to be a limitation.

We thank the reviewer for this comment and agree that there is a large body of research linking paleofire records to quantitative metrics but these studies are mainly based on continental/terrestrial archives. As far as we know, this is not the case of marine sedimentary archives. We have now moved the phrase upper in the text, added more references and modified this part that now reads: "One of the major limitations of paleofire reconstructions obtained from sedimentary archives is that although charcoal records provide an estimate of the changes in fire activity or biomass burning, they do not provide yet quantitative measurements directly comparable to simulated values of the fire regime parameters such as fire number, size, frequency and intensity".

- L65-67: Akin to the last comment, the presentation of these studies as 'few' is an oversimplification that misrepresents the literature. The studies referenced here are representative of a small subset of published charcoal calibration studies. Although I would agree that more work of this kind is needed,

the way it is presented here does not do justice to the numerous insights we do have from charcoal calibration studies.

We thank the reviewer for pointing this out. We modified the sentence, moved it upper in the text and extended the references cited to read: "Numerous charcoal calibration studies on terrestrial sediment records suggested a positive relationship between charcoal accumulation and one of the physical parameters of the fire regime, i.e., burnt area[18–20,22–25], intensity[18,22], fire number[18,26] and severity[27]".

-L112-113: I don't think this is a statistically accurate phrasing. These variables are associated with the PC1 axis, but, unless I am misunderstanding the analysis, I don't believe the explanation can be derived from the eigenvectors.

We used square cosine, squared coordinates - COS2 to show the quality of representation of variables on the principal components, i.e., a higher COS2 (>0.5) indicates a good representation of the variable on the principal component. We also used the contribution of variables which account for the variability in each principal component and added a new figure showing this. We modified the sentence to read: "PC 1 is correlated with the area, length and width of charcoal particles, and PC 2 is correlated with charcoal elongation and concentration (Fig. 3A); (correlation >0.5; Fig. S.4, Fig.S.5)."

-L113-116: In my view of figure 3, there is so much overlap between the regions that any assertion of coherent patterns between the regions seems misplaced.

We appreciate the reviewer's insightful suggestion and agree that in the PCA on charcoal parameters there are overlaps between regions. However, as we explained in the main text our interpretation and inferences are based on the Kruskall-Wallis tests performed on the PCA results (PC1 and PC2) on each hydrographic basin and also on the main two regions, western central and southern Africa. To increase the clarity of our interpretation we added the results of the Kruskal-Wallis tests.

We modified the paragraph that now reads: "The PCA results show high heterogeneity in the distribution of samples. However, a coherent pattern of charcoal abundance and morphotype was evidenced among the different hydrographic basins and between Western Central and Southern Africa (Figure 3A, Kruskall-Wallis tests, $p<0.05$). The Kruskal-Wallis test applied on PC2 shows that there is a significant difference between hydrographic basins and also between the two main regions driven by concentration and elongation.

-L116-120: All of these statements refer to Figure 3 as support, but I am not able to clearly see how this figure or the PCA analysis actually supports these statements. An alternative interpretation of the PCA results is that the variability of charcoal particle shape and size does not clearly group between regions.

We thank the reviewer for this point, please see also our response to the previous comment where we explain in details the results and the interpretation. As stated in the main text, the PCA results in Fig. 3A show high heterogeneity in the distribution of samples but, as shown by Kruskal-Wallis test, there is a significant difference between hydrographic basins and between the main regions based on charcoal abundance and morphotypes. The PCA grouping results show that the shape and size of microcharcoal particles group together samples from Namibia, Orange and SAW (southern Africa - southern region) whereas only the shape of particles groups the basins from western central Africa, Congo, Angola and Gulf of Guinea (see our response to previous comment). We added a sentence between L115 and L116: "The shape and size of microcharcoal particles group together samples from Namibia, Orange and SAW (southern Africa - southern region), whereas the shape of particles groups samples from Congo, Angola and Gulf of Guinea basins (western central Africa)." To increase the readability of the figures and make them clearer we enlarged the size of Fig. 3A and modified the colors.

-L120-122: Why is that? The reasoning here should be made explicit.
"A possible explanation for the observed distribution pattern of charcoal morphotypes (concentration, shape and size) could result from different transport mechanisms between regions influenced by grain-size sorting during eolian transport and others dominated by river discharge, or by surface ocean circulations. However, all regions combine the influence of large river basins (Congo and Orange rivers) and intense ocean currents (BCC and AC, Fig. 1A), and prevailing southeasterly trade winds. Another possibility lies in a higher sediment export concentrating charcoal in preferential areas. However, both the Benguela upwelling system and the Congo fan are high sedimentation accumulation systems. Consequently, it seems unlikely that sediment transport processes control the observed latitudinal distribution pattern of charcoal in seafloor sediment.."

-L127: disclose -> identify/assess?
We agree and have modified to read: "To identify the potential link between charcoal accumulated in the ocean and fire on land".

- L129: Figure 3B is too small for the reader to see the results described here. Consider zooming in on the origin here.
To increase the readability of Figure 3B we present a zoomed version (excluding one sample which is an outlier) and placed the original un-zoomed version in the SI.

-L135: The positive and negative directions of the principal components are not statistically meaningful here. This should be rephrased.
We have made the change to make the phrase clearer. The new sentence reads as follows: "Samples from Congo, Angola and the Gulf of Guinea tend to cluster on the positive values of PC 1 and are associated with burnt trees. Samples from Orange, SAW and Namibia are found along the negative part of PC 1 and are associated with FRP, burnt mixed, RoS, area and burnt graminoids".

-L138-142: Akin to comment referring to L116-120, these interpretations are not easily seen in Figure 3. Likewise, the phrasing here seems to overstep the interpretive power of a PCA to infer causative relationships between variables.
We have now implemented the changes to make the interpretation of the PCA on fire parameters clearer. It is important highlighting that our interpretation is based on the PCA results but also on the Kruskal-Wallis test performed on the PC1 and PC2 on both hydrographic basins and among regions, i.e., western central and southern Africa. We also present a zoomed version of Fig. 3B.
We modified to read: "Our assessment of African fire regimes (PCA) indicates that small (local) fires of low intensity occurring in tree-dominated areas are more concentrated in Western Central Africa, whereas large (more regional) fires of high intensity and high rate of spread involving mixed, graminoid-shrub vegetation are mostly associated with Southern Africa (Figure 3.B)."

-L159-161: This isn't really demonstrated though. Two separate PCAs can only be compared so much. I feel it is a bit of a leap to far to make this conclusion.
We rephrased to read: "Taken together, our results indicate that low amounts of charcoal particles with squared shape are observed in marine sediments proximal to regions dominated by small fires of low intensity in tree-dominated ecosystems, whereas larger amounts of small charcoal particles with elongated shape are observed in proximity to landscapes experiencing large and high-intensity fires in graminoid-mixed ecosystems."

-L169-171: Many others would probably say that fire itself controls the amount of charcoal produced and preserved in sediments.

We appreciate the reviewer's insightful comment regarding the fire controls. However, here we tried to open the discussion and present what is stated in the literature regarding charcoal production in forested ecosystems highlighting that biomass availability is a major controller of charcoal production/elemental carbon accumulation in different depositional environments. We don't rule out the other fire controls and we discuss them later in the same section and actually, our conclusion is that fire itself, especially fire intensity, can control the charcoal production.

L176-178: Feurdean (2021, Biogeosciences) showed the exact opposite with experimental data.

We thank the reviewer for pointing this out. We fully agree that the work of Feurdean, 2021 shows exactly the opposite trend of what we observe and what other work from our region or similar region says. However, here, we wanted to focus on experimental studies conducted in (biogeographical) regions similar with Africa. It is important to note that the experimental studies are reinforced by the information provided by the satellite observations. To extend the discussion, we have added a phrase to explain the opposite pattern observed in other studies.

We have modified the text to read: "However, our findings contrast with the experimental work suggesting that low-temperature surface fires produce more elongated particles than high-temperature fires[51], which typically result in less elongated particles. This opposite trend is not surprising considering the different biogeographical regions (Africa vs Siberia) corresponding to vegetation types/plant associations (grassland-savanna vs boreal forests) in which fire behaviour and charcoal production and type are expected to differ significantly."

Supplementary Information

-Overall, I am surprised by how long the SI is relative to the main text (they are roughly the same length). I would urge the authors to move some of this material to the main text as it would help to support many of the interpretations made in the main text. In particular, much of the background information provided about charcoal transport and pathways would be helpful to readers in the main text and would help to address several of my comments above.

L85: The dispersion modelling text seems out of place here and in the manuscript generally. This text could make for an interesting, self-standing paper of its own that would be of interest to the palaeofire community. But, if I am understanding correctly, all of this text and analysis was included solely to support the statement about estimated source area of charcoal in the main text?

Yes this section was included to support the statement about the estimated charcoal source area in the main text. There is no specific study concerning the transport of airborne microscopic charcoal up to the ocean. Studies concerning pollen show pollen grains reach the ocean mainly by fluvial and atmospheric transport processes, with oceanic currents playing a negligible role (see synthesis in Sánchez Goñi et al., 2018). It is likely that microscopic charcoal can be transported by both processes up to the ocean. The fluvial "dispersion" is relatively easy to constrain, likely contained in the hydrographic basin.

The question was whether microscopic charcoal produced at a certain location can be transported in another hydrographic basin. Numerical modelling studies supported by empirical data were developed to aid in interpreting the source area of macroscopic charcoal in the lake-sediment records (for example Vachula et al., 2018). We wanted, therefore to explore the question of a direct dispersion of microscopic charcoal from the land to the ocean by wind.

We understand that this dispersion modelling section could be a self-standing paper. However, we think this information is necessary to address the airborne source area of the charcoal. The geographical limit of our source areas and the assignment of samples per basin is very dependent on the results of this

dispersion and deposition model. We have placed it in the SI as this was not the main scope of our study but we believe this information is capital for complete understanding of our approach. We tried to keep the section as short as possible providing detailed information (for reproducibility) of how we ran the model.

In the main text, we modified the sentence L104 to introduce the text from the SI about charcoal transport and dispersal. In addition to this, we also moved into the main text (method section) the information about the cruises and dispersion modelling.

Sánchez Goñi MF, Desprat S, Fletcher WJ, Morales-Molino C, Naughton F, Oliveira D, Urrego DH and Zorzi C (2018) Pollen from the Deep-Sea: A Breakthrough in the Mystery of the Ice Ages. Front. Plant Sci. 9:38. doi: 10.3389/fpls.2018.00038

Vachula, R.S., Russell, J.M., Huang, Y., Richter, N., 2018. Assessing the spatial fidelity of sedimentary charcoal size fractions as fire history proxies with a high-resolution sediment record and historical data. Palaeogeogr. Palaeoclimatol. Palaeoecol. 508, 166–175. https://doi.org/10.1016/j.palaeo.2018.07.032.

Reviewer #2 (Remarks to the Author):

General Comments:

This manuscript explores charcoal records from "mud-water" or surface sediment samples collected from the top 1-2 cm of ocean sediment samples off the African coast. The authors have previous experience working with marine sediment records and leverage their expertise to explore linkages among sedimentary charcoal morphometrics, terrestrial fire regimes and observational fire data. The methodological approach and analysis are appropriate and the discussion and conclusion report several key findings. Through measuring the ratios of small and elongated charcoal, the authors link tree- and grassland-fires to changes in charcoal ratio. The key finding that fire intensity is an important driver of charcoal morphotypes has important implications for fire scientists using charcoal morphology to reconstruct past grassland-savanna fires in Africa. The authors conclude that evidence of grassland-savanna fires (based on charcoal morphology) can be used to link past changes (millennial-scale climate reconstructions) to understand the role of fires in contributing to regional and global carbon budgets.

I believe the manuscript is well-written, provides new and important calibration tools for the paleofire community, and warrants publication in Communications Earth & Environment. However, I do have some specific concerns about the linkages to fire intensity and some of the authors' use of specific (or sometimes unspecific/vague) fire regime characteristics throughout the manuscript. I would like to see the authors clarify the use of their terminology as related to fire regime (e.g. fire intensity) and a few minor comments outlined below.

We thank the reviewer for this suggestion. We used the mean FRP (Watt/m2) as a measure of the fire intensity, more explicitly, FRP measures the energy emitted through radiative processes released during the combustion and can be associated with fire intensity throughout the whole fire burning process. The mean FRP obtained from Laurent et al. 2018 database is explained as "the energy emitted by fire through radiative processes (i.e. the total fire intensity minus the energy dissipated through convection and conduction) over its total area". We clarified this in the method section by adding: "We used the mean FRP (Watt/m2) as a measure of fire intensity; FRP measures the energy emitted through radiative processes released during the combustion and can be associated with fire intensity throughout the whole fire-burning process[55].It is used for assessing carbon emissions by converting FRP into fuel biomass consumption rate (Kaiser et al. 2012), and is linked to burn severity (Heward et al. 2013). FRP is also a key component of global fire regime characterization (Garcia et al. 2022)".

Laurent et al. (2019) Varying relationships between fire radiative power and fire size at a global scale. Biogeosciences, 16, 275–288, 2019
Heward, Heather; Smith, Alistair M. S.; Roy, David P.; Tinkham, Wade T.; Hoffman, Chad M.; Morgan, Penelope; Lannom, Karen O. 2013. Is burn severity related to fire intensity? Observations from landscape scale remote sensing. International Journal of Wildland Fire 22(7):910-918.
Garcia M., Pettinari M. L., Chuvieco E., Salas J., Mouillot F. Chen W., Aguado I. (2022). Characterizing global fire regimes from satellite-derived products. Forests, 13 (5), p. 699
Kaiser, J. W., Heil, A., Andreae, M. O., Benedetti, A., Chubarova, N., Jones, L., Morcrette, J.-J., Razinger, M., Schultz, M. G., Suttie, M., and van der Werf, G. R.: Biomass burning emissions estimated with a global fire assimilation system based on observed fire radiative power, Biogeosciences, 9, 527–554, https://doi.org/10.5194/bg-9-527-2012, 2012.

Specific Comments:
Page 3: reference to "one of the major limitations of paleofire reconstructions is that charcoal records provide relative changes in fire activity...". What do the authors mean by this statement? Are relative changes specifically linked to marine charcoal records or all charcoal records? Also, are CharAnalysis metrics, for example (fire frequency, charcoal influx, peak detection etc), relative or absolute?
We thank the reviewer for this comment and we acknowledge these sentences were unclear (please see also comment from reviewer 1) which we have now modified the sentences to read: “One of the major limitations of paleofire reconstructions obtained from the sedimentary archive is that although charcoal record provide changes in fire activity or biomass burning, they do not provide yet quantitative measurements directly comparable to simulated values of the fire regime parameters such as fire number, size, frequency and intensity”. We hope it is clearer.

RESULTS:
Page 5: "The source areas were built by assuming the dominant transport is fluvial....." It is helpful the authors discuss a dispersion and deposition model in the SI, and also suggest "...most micro char is deposited within a 300 km range, within the hydrographic basin limits". There appears to be limited experimental or calibration research to test this assumption. Additionally, the assumption that wind is the primary driver, suggests river transport of suspended microchar in major river systems may be an unimportant component. Some studies have explored river delta charcoal and suggested otherwise (e.g. see Haberle and Maslin 1999). More justification is warranted on why river transport is minimized.
We discussed a wind dispersion and deposition model in the SI. Our aim was not to minimize the river transport of suspended microcharcoal but to take into account the potential influence of winds through the direct transport of charcoal to the ocean or the mixing of charcoal between hydrographic basins. We acknowledge this was confusing for readers.
We modified this section to make it clearer as follows: “L92: Following vegetation burning, microcharcoal particles are released and further transported with the fine-grained sediment fraction to the ocean, via aeolian dust or riverine inputs (Patterson) (SI Section 1). Following vegetation burning, microcharcoal particles are released and transported with the fine-grained sediment fraction to the ocean, via aeolian dust or riverine inputs[31] (SI Section 1). Given the size and weight of microcharcoal particles, ranging between >10µm and <100µm in length[32], it is expected that they behave similarly to pollen and fine terrigenous particles during transport and sedimentation[33]. In marine sediments proximal to river plumes, pollen and most likely microcharcoal are primarily of fluvial origin[34,35].
In this study, we defined eight regional source areas for sedimentary microcharcoal, roughly based on the proximity to the river system entering the ocean, i.e., Gulf of Guinea, Congo, Angola, which represent the central western Africa region, Namibia, Orange, western South Africa (WSA), eastern South Africa (ESA) and Limpopo which represent Southern Africa (Figure 1.B). Although microcharcoal and pollen share similar fluvial behaviour during fluvial transport, they behave slightly differently during aerial dispersal due to differing physical-mechanical characteristics36. In this study,

we therefore, evaluated the potential contribution of windblown microcharcoal particles using a dispersion and deposition model, showing that the microcharcoals transported by winds are mainly deposited <300 km, within the hydrographic basin limits (see Material and Methods and SI Section 3). With this in mind, each marine sediment sample was assigned to a source area represented by the closest hydrographic basin."

Hydrographic basin analysis is a helpful spatial domain to explain much of the variance in charcoal ratios, however, can the authors address how hydrographic basin, or alternative spatial units of analysis, may impact the ability to understand source type (e.g. wind versus water transport).
We appreciate the reviewer's insightful suggestion but as explained previously, we didn't want to give more weight to wind versus water transport of microcharcoal but we intended to introduce both transportation vectors and test the wind as a potential direct carrier. We hope the revised version is satisfying.

Reviewer #3 (Remarks to the Author):
Major comments
The manuscript compares the structure of charred particles found and measured in the tropical ocean off and around Africa including the southern part of the continent that is not tropical with wildfire data from the continent. This type of study is crucial for reconstructions of past fire to attempt to unravel the past regimes, their changes, and their interactions with other environmental element, for instance vegetation. Here the authors are probably more concerned by climate issues according to the end of the manuscript dedicated to orbital forcing and climatic periods.
The paper is rather convincing, thanks to sophisticate analyses and comparative statistics. But, I detect a serious problem in the study, and precisely within the Supplementary information. The problem concerns the sedimentation time assessed in the top-most 1.5-cm sediments that served to extract charcoal for proxy-fire data (SI 2, excel sheet). The ages at 1.5 cm are calculated based on the sedimentation times of each series that result from the isotopic chronologies. Only 16 samples on 43 present an assessed time, meaning that the time content in the sediment of 27 samples is unknown. However, this comment would be not important if the "age at 1.5" (column k) was not weird. Indeed, the range of ages varies between 40 and 2680 years. That data suggest a huge temporal variability between sediments, and that the modern wildfire data used for the comparison with the fire-proxy could be totally unconnected. In other words, I had the feeling that authors compared sometimes and somehow fire data with a fire proxy that is not synchronous. I would have no real problem if the age at 1.5 cm was within a 50-year range, but only two sample are assessed within this 50-yr range, 10 between 120 and 960 yrs, and 3 between 1790 and 2680 yrs. The mean age of sediment is 720 years! That is extremely problematic.
Whatever, the quality of the analyses, this point of the age at 1.5 cm is a fundamental weakness that which obliges me to reserve my opinion, whereas I first appreciated and supported the whole strategy of the manuscript until I check in detail the SI.
These inconsistency between fire statistics and age of charcoal must be solved before any decision of acceptance for publication.
We appreciate the reviewer's insightful suggestion and agree that our study would have been advantaged by using samples of <50-year range. However, it should be highlighted that marine surface sediment samples of similar age ranges with satellite records (<50 years) are extremely rare, firstly, given the high sedimentation rate in the ocean deposition system and secondly, they are so precious (for studies focused on microplastics, ecosystem state etc) that it was impossible to have access to some of them from official repositories or sometimes, the sediments were already used.
In this situation, our approach was to balance between a minimum number of samples considered (robustness) versus quality of ages. We believe however that the calibration based on a spatial approach is still worthwhile as 1) other calibration of several marine or terrestrial proxies in marine sediments

using surface samples with same issues were fruitful, 2) we expect that the spatial differences in climate, vegetation and fire activity observed today in central western Africa versus southern Africa exceed their temporal variability over the several past centuries; in other words, we do not expect both regions to be characterized by a tropical climate during the very late Holocene.

We modified the text and revised the SI dating information table to clarify this point as follows:
-L26: To address this conceptual gap, we report here the abundance and morphometric data for a large set of microscopic charcoal samples (n=128)
-L86: Microscopic charcoal particles, between 10 and 92 µm in length, were extracted from surface (core-top) marine sediment samples (n=128; Material and Methods)
- L288: The samples are spread across different depositional settings, from estuary (1 sample), the continental shelf (1 sample), the continental slope (51 samples) to the abyssal plain (75 samples).
- We modified L292: "Chronological information was extracted from published studies for 58 core-top samples showing the samples cover less than two decades to a few or several centuries of environmental changes (Supplementary Data). Twelve samples are dated less than 50 years, four less than 100 years, ten less than 150 years, nineteen less than 500 years and thirteen of several hundred years up to late Holocene. Dating was not available for the other 71 samples. Studies[63] in the region show that core-top of marine sediments represents the late Holocene and covers few hundred to few thousand years which might be problematic in terms of comparison with more recent observations. However, prior studies have successfully used spatial calibration of marine surface sediment-based proxies to reconstruct different physical and biological ocean and sea-ice properties (for example[61,62]) or fire regime characteristics[64] suggesting that core-top samples are suitable for our calibration approach. In particular, [64] showed that the temporal variability of microcharcoal concentrations in interface cores off Iberia is within the range of modern spatial variability. This supports our comparison of the different datasets as mean environmental conditions on land and in the ocean over the past 100 years, corresponding to these past century biomasses burning records, did not radically depart from conditions recorded by the instrumental period."

-Further, the end of the manuscript on pages 10 and 11, between lines 213 and 237 seems at this stage, without any quantitative reconstruction a pure speculation. This should be removed from this short and technical paper.
We present (L213-222) results and interpretation of paleofire records from three marine archives published by Daniau et al., 2013; Dupont and Schefuss (2018) and Dupont et al. 2021. We then used (L222-237) our modern land-ocean qualitative relationship to provide alternative interpretations to the original ones. We believe this part is also important to show how our modern land-ocean qualitative relationship can be used.
Daniau, A.-L. et al. Orbital-scale climate forcing of grassland burning in southern Africa. Proc National Acad Sci 110, 5069–5073 (2013).
Dupont, L. M. & Schefuß, E. The roles of fire in Holocene ecosystem changes of West Africa. Earth Planet Sc Lett 481, 255–263 (2018).
Dupont, L. M., Zhao, X., Charles, C., Faith, J. T. & Braun, D. Continuous vegetation record of the Greater Cape Floristic Region (South Africa) covering the past 300 000 years (IODP U1479). Clim Past 422 18, 1–21 (2021).

Minor comments
-Line 28 and in the whole manuscript: change uppercase of Central, Southern, Northwest, etc. These adjectives of geographical location are not propre names of areas or regions.
We have modified the names throughout the manuscript.

-L 30: "forest" instead of "forested".
Done

-L 34: “revealed” instead of “imply”.
We have now modified to “suggested”.

-L35: “of fire-inferred carbon” instead of “of fire carbon”.

We thank the reviewer for this suggestion but using this expression will change the meaning of the sentence.

-L 41: remove because useless: “the second largest (…) on Earths”.
Done

-L 44 and in the whole text: change and harmonized “burnt” by “burned”.
We used the UK spelling and also “burnt” as an adjective and “burned” as a verb. We will follow the recommendations of the journal.

-L 49: and feedbacks?
We have changed to read “therefore neglect potential changes in interactions and feedbacks between climate, vegetation and fire beyond the range of modern observations”.

-L 53: the ref 10 is rather obsolete (almost 20 years, and other papers more recent exist), and the time depth based on “natural archives” in this paper is very limited.

We don’t know exactly what the word “obsolete” means in regard to scientific articles. We agree that other papers were published in this field more recently but the reasons for citing these papers is that they were opening directions in the field.

-L 62 and after: in paleoecology of tropical grasslands, some terms of past fire like “frequency” or “number” classically used in temperate of boreal countries are not applicable, because the temporal resolution of charcoal measurement per sample is larger than the fire intervals in tropical ecosystems. Further, some terms like “intensity” still remain an inaccessible quest in paleoecology, because the charcoal production is not related to intensity but to the severity of fires. Moreover, the charcoal production does not result from a linear relationship with wildfire intensities, but probably a inverse U-shape distribution where low and high intensities do not result enough charcoal. At too high intensities, the whole organic matter is consumed, and no charcoal are produced.
We thank the reviewer for this informative comment. As we stated in our original manuscript L300: “charcoal is produced by pyrolysis for temperatures ranging from 200 to 600°C (Conedera et al. 2009).”, hence the too low and too high intensities do not produce charcoal; and we addressed the limitations of charcoal calibration in the original manuscript L69.

We modified the sentence L62 (please see also response to reviewer 2). Our aim here was to give an overview of the outputs of previous calibration studies developed on charcoal from terrestrial paleorecords in diverse ecosystems (temperate, boreal, savanna and grassland ecosystems). We modified the sentences to read: “One of the major limitations of paleofire reconstructions obtained from marine sedimentary archives is that although charcoal records provide changes in fire activity or biomass burning, they do not provide yet quantitative measurements directly comparable to simulated values of the fire regime parameters such as fire number, size, frequency and intensity”.

To our knowledge, we don’t know about studies showing that:

(1) for a broad range of ecosystems, charcoal production is not related to fire intensity but to the severity of fires. We further explained in the material and method that “FRP is used for assessing carbon emissions by converting FRP into fuel biomass consumption rate[70]. FRP is also a key component of global fire regime characterization[72]”.
(2) [after demonstrating in (1) that charcoal production is not linked with intensity] the charcoal production follows an inverse U-shape distribution of fire intensity.

We would be pleased to improve this paragraph if the reviewer could share with us the references he/she is referring to.
Adolf, C. et al. The sedimentary and remote-sensing reflection of biomass burning in Europe. Global Ecol Biogeogr 27, 199–212 (2018).
Duffin, K. I., Gillson, L. & Willis, K. J. Testing the sensitivity of charcoal as an indicator of fire events in savanna environments: quantitative predictions of fire proximity, area and intensity. Holocene 18, 279–291 (2008).
Hennebelle, A. et al. The reconstruction of burned area and fire severity using charcoal from boreal lake sediments. Holocene 30, 1400–1409 (2020).
Higuera, P. E., Peters, M. E., Brubaker, L. B. & Gavin, D. G. Understanding the origin and analysis of sediment-charcoal records with a simulation model. Quaternary Sci Rev 26, 1790–1809 (2007).
Leys, B., Brewer, S. C., McConaghy, S., Mueller, J. & McLauchlan, K. K. Fire history reconstruction in grassland ecosystems: amount of charcoal reflects local area burned. Environ Res Lett 10, 114009 438 (2015).

-L 22: ref 22 is an obsolete paper. Better, more sophisticated and more recent papers have been published since 1998.

We thank the reviewer for this suggestion. We have now added more recent papers.

- L 67-69: I am stunned that only two papers have been published on this methodological (taphonomical) issue, which is crucial in reconstruction based on marine sediment. Would that mean that most reconstruction of past fire based on marine sediments relies on theoretical principles not supported by data? Further the paper 24 is very obsolete.

To our knowledge, very few marine charcoal calibration studies exist. This gap in knowledge is exactly what we want to address with this study. We don't understand why the reference of *Mensing, S. A., Michaelsen, J. & Byrne, R. A 560-Year Record of Santa Ana Fires Reconstructed from Charcoal Deposited in the Santa Barbara Basin, California. Quaternary Res 51, 295–305 (1999)* is qualified to be obsolete by the reviewer as this is one of the first publications attempting to link charcoal in marine sediments to fire characteristics on land.

We have modified L67 to report more information on this methodological aspect: "In the marine environment, a few published calibration studies have linked charcoal accumulation and morphotypes with burnt area and fire intensity in California [28] and the Iberian Peninsula fire intensity[29]. In addition, previous work has shown that the distribution pattern of charcoal concentrations correlated with charcoal influx in North Pacific marine sediments [30] , suggesting that charcoal distribution patterns in sediments are not controlled by any dilution effect but instead faithfully reflect the sedimentary flux of charcoal particles."

-L 73: the distance of oceanic coring from continent and wildfires is also a crucial issue that question the possibility to reconstruct a continental process (wildfire) with marine sediment. Further, the transportation time of particle is probably long in the ocean that are characterised by strong oceanic streams, meaning that particles could be trapped in sediments at hundreds km from their source areas, and charcoal are probably dispersed and mixed in the ocean on millions of km^2. I am skeptical on the use of charcoal from marine sediments to reconstruct continental processes based on very small and light particles that float very well.
We have addressed this concern in the original SI. We modified the main text (please see response to reviewer 2 page 5) introducing some SI information in the main text to provide evidences on the charcoal transportation and dispersal. Pollen is also small and light but they are preserved in marine sedimentary archives;they provide an integrated image of the regional vegetation (review synthesis from Sanchez Goni et al. 2018).

Sánchez Goñi MF, Desprat S, Fletcher WJ, Morales-Molino C, Naughton F, Oliveira D, Urrego DH and Zorzi C (2018) Pollen from the Deep-Sea: A Breakthrough in the Mystery of the Ice Ages. Front. Plant Sci. 9:38. doi: 10.3389/fpls.2018.00038

-L 74: add (>10 µm) after microcharcoal.
Done

-L 74-82: very descriptive aims; please be more scientific and transform the text with a working hypothesis to test, and indications about the functional response expected, and then develop the objectives.

We thank the reviewer for this suggestion. We have modified the paragraph to read: "The first inferences of fire regime characteristics from marine paleofire records were derived using indirect theoretical principles developed on charcoal from lake sediments and experimental studies. A peak in microcharcoal concentration or influx was interpreted as an increase in fire frequency and intensity while the particle shape was used to infer intensity and vegetation type. Recently, it has been demonstrated that charcoal abundance and morphotype in marine surface sediments off Iberia can trace specific fire regimes on land, in particular fires of high intensity in a mixed open Mediterranean woodland vegetation[29]. However, there is no empirical data yet to support paleofire regime inferences made from marine microcharcoal records off Africa. Here, we aim to explore the relationships (...)".

-L 98: add "conceptual" before "model". Here, there are no formal model, I mean mathematical based.
We don't understand this comment as the HYSPLIT's (Hybrid Single-Particle Lagrangian Integrated Trajectory) dispersion and deposition model is a mathematical one based on the physics of particles. This simulation uses a fixed number of particles which are transported by a wind field, spread by a turbulent component and, in our case, removed by dry and wet mechanisms.

-L 102, 103: nb/g, this is obsolete. Please adopt nb.g^-1
We used this nb/g to avoid editorial problems with the power symbol. We replaced nb/g by nb.g^{-1} in the manuscript.

-L 103-107: interesting but please test the differences between box plot in order to support the description of differences or similarities.
We modified the sentence to read: "Samples located off Namibia, Orange, WSA and Limpopo show higher mean elongation ratio (>1.8) whereas samples located off Gulf of Guinea, Angola, Congo and ESA coast display values <1.8 (t-test, $p < 0.05$) (Figure 2)."

-L 111: instead of "breadth" I would have used "width".
Done

-L 112: with only 5 parameters, is rather expected a high percentage. Further, I quester whether the ratio ("elongation") and area are independent on the other parameters, i.e. from length and breadth.
We agree that five parameters could be associated with a high explanative percentage of variance on the first axes of the PCA. Here, we just pointed out that the high proportion of variance explained allows the distribution of the charcoal data set on the first two axes only, and an interpretation of the dataset by these eigenvectors.
Concerning the potential dependence of parameters, charcoal area refers to the size of a particle, whereas the elongation ratio is an indicator of the shape. They are unrelated.
The elongation ratio is a mathematical calculation (length to width ratio) of each charcoal particle and is thus related to both length and width. However, a small elongation ratio can be the result of a high width or a small length, and these two components taken separately help to disentangle this.

Finally, the bigger the area the longer the length or the wider the width. These three are the most related one to each other, as represented on the PCA axis 1, although charcoal particle types could have presented bigger particles mostly longer or wider. The 5 parameters are thus somehow related (except elongation and area) but are necessary to fully describe charcoal particle types.

-L 116: I don't understand on what the KS-tests were applied. What did you test precisely?
The tests were applied for exploring whether there were some differences/similarities between hydrographic basins and between larger regions (western central Africa and southern Africa) for 1) the charcoal parameters and 2) the fire parameters. We also modified the text to make it clearer (please see our response to reviewer 1).

-Figure 3 is too small; it is very difficult to unravel. What represent the oval-shape perimeters. It contains too many acronyms and symbols not explained. The symbol-coloured chart of the right contains 8 categories, but these eight categories are impossible to find in the sub-fig B.
To increase the readability of the figures and make them clearer we enlarged the size of Fig. 3A and modified the colors. For Figure 3B we present a zoomed version (excluding one sample which is an outliers) and placed the original un-zoomed version in the SI. We also modified the figure caption to include all the information.

-In the figure caption, the Graminoids and Trees vegetation should be substituted by grassland-savanna vegetation and forest or woodlands. Indeed, there are no tree vegetation without herbs and forbs, and graminoids vegetation are also associated to forbs and often dwarf-shrubs (e.g., the karoo vegetation in South Africa that is not a savanna nor a grassland). Better written on line 160.
The classification we used here is based on the 'growth habitat' classification from Vachula et al., 2021 paper. We agree that this classification is quite broad and does not describe fully the composition of the vegetation. To make the classification more clear, we have added in the figure caption that Graminoids growth habitat corresponds to grassland-savanna vegetation and Trees growth habitat to woodland vegetation.
Vachula, R. S., Sae-Lim, J. & Li, R. A critical appraisal of charcoal morphometry as a paleofire fuel type proxy. Quaternary Sci Rev 262, 106979 (2021).

-L 120-124: I agree, but this section must be moved into the Discussion section. It is not a result, but a discussion sentence on results.
We moved the paragraph "At this sub-continental scale, it is difficult to argue that transport and/or sedimentation processes mostly control the observed distribution pattern of sedimentary charcoal in seafloor sediments. Instead, previous studies have shown that the amount of charcoal is generally controlled by production source characteristics such as fuel type, biomass, fire size and intensity[38,39]." after "Here, we demonstrate at a larger sub-continental scale and for environmental settings raging from tropical to subtropical climate, tropical to desert biomes, that concentration and elongation of charcoal in marine sediments off Africa can be used to identify different fire regimes."

-L 124: ref 26 is an inappropriate reference because archaeological. The processes are not the same in nature and in archaeological contexts.
We have now moved the text and deleted the reference.

-L 130: "mean fire radiative power", is it the "intensity" of the rest of the text?
Yes. The mean fire radiative power is expressed in Watt.m^2. This information is in the caption of Fig. 3B. We have added "we used the mean FRP in Watt.m^2 as a measure of fire intensity" after "PCA was applied on land fire parameters including number of fires (Nb fires), mean fire radiative power (FRP),

area, rate of spread (RoS) and the burnt vegetation types (graminoid - G, mixed – M and trees - T) to characterize the fire regime specific for each hydrographic basin (Figure 3B). We clarified this L130 including "FRP measures the energy emitted through radiative processes released during the combustion and can be associated with fire intensity throughout the fire burning process[71]. FRP is used for assessing carbon emissions by converting FRP into fuel biomass consumption rate[70]. FRP is also a key component of global fire regime characterization[71]".

-L 145-147: move this sentence into the Discussion section.
Done

-Discussion section: too long. The paragraph between lines 213 and 237 is very speculative; it is only an extrapolation of present outputs to former studies without any actual results such as quantitative reconstructions. These paragraphs are useless.

We responded already to this comment (see above).

-L 152: "a similar approach on marine sediments (…)"
Done

-L 153: Mediterranean with upper case.
Done

-L 166: "devastate" is not a scientific word to mean "burned larges areas". "Devastate" is very subjective.
We modified to read "and burned large areas".

-L 159-168: here, only papers matching with your outputs have been mentioned. But, are there contrasting papers, or papers that contradicts your results? Be honest by evoking all possibilities. This part is devoted to characterize the type of vegetation that is burning and the type of fire regimes in central-western and southern Africa. The discussion is continued in the following paragraph were we evoke all the possibilities including both positive and negative ones about fire regimes particularly, in our study area using previously published studies which are based on both sediment records and instrumental observations.

-L 179: "submicron" are u sure?

We agree, micron to submicron is for soot and graphitic black carbon in Conedera et al. 2009. Thanks to have spotted this mistake. We modified the sentence to read "In addition, based on the combustion continuum, the size of charcoal is reduced from cm to micron with increasing temperature38".

-L 184-185: ratio of 3.6 vs 3. Is the difference significant? The difference is 0.6 corresponding to 17%. Please, be more convincing.
The ratio values used here are from Vachula et al. 2021 experimental work. There are some overlaps between the graminoid/shrub and graminoid ratio (median elongation about 3 and 3.6) but as we stated in the manuscript we compared ratios between the group 'graminoid and graminoid-shrub' versus 'tree and (sub)shrub to interpret our results. The limit at 3 between those two groups corresponds to the upper quartile of tree(sub)shrub and almost to the lower quartile of graminoid/shrub. We modified the text to make it clearer: "Experimental studies showed that charcoal particles produced by graminoid and graminoid-shrub burning (median elongation ~3) are more elongated than particles derived from the burning of tree and (sub)shrub (median elongation ~2)[44]."

-L 192: “grassland and woodland” instead of “grasses and trees”.

We modified the text accordingly.

-L 195-198: other explanation for small grain-size of charcoal: lab methods vary between sites (cf. SI-1); but I think that the small fragment float long-time (weeks to months) before they attain the bottom of the see.

We use the same chemicals to treat all samples. In few cases, depending on the composition of sediments, carbonate vs silicate, we slightly modified the quantity of chemicals to make sure we treat them the samples in the same manner, by removing the unwanted parts. In this step, we added a small amount of chemicals until the reaction stopped, when the degassing bubbles stop or when we observed a change in color. This section discusses smaller elongation ratios compared to the ratios found by experimental studies, not the particle size. The physical breakdown of charcoal into smaller grain-size could explain smaller elongation ratios (see for example Umbanhowar and McGrath, 1998).

-L 246-249: speculation. Delete.
We nuanced the text to read: “At the orbital timescale, our results suggest that intense and large open grassland-savanna fires occur during wet periods in subtropical regions of Africa. These intense fires have been a source of carbon to the atmosphere during periods of precession and summer insolation maxima in the geological record. However, more geological data as well as model simulations are necessary to quantify the contribution of fires to the atmospheric carbon cycle over orbital timescale.”

-L 291-297: woah! This is the weakness of the paper… no age for most samples. Most samples with ages, cover a very long period of several centuries, while the fire data are modern. That means that charcoal data are probably disconnected with fire statistics. See major comment, above.
See our previous response above.

-L 305-310: are there solid paper that demonstrate that the chemical treatment respects the integrity of charcoal? Several aggressive acid-attacks (HNO3, HCl x2, HF) could result a digestion of charcoal fragments that are little charred due to low burning temperature (<300°C). Please provide evidence of the rational of this chemical treatment before measuring and tallying charcoal.

The method is presented in Beaufort et al. (2003). The preparation techniques for charcoal were adapted from the work of Winkler (1985) and classical palynology methods (see a synthesis in Daniau et al. 2019, pollen and spores section). A first step of HCl treatment is used to remove calcium carbonates (CaCO3) until the degassing bubbles stop (end of chemical reaction). The HF treatment is used to remove siliceous material. A second step of HCl treatment after HF is used to remove colloidal SiO2and silicofluorides formed during the HF digestion. Tsakiridou et al. (2020) submerged modern charcoal and peat sediment containing charcoal with HNO3 or with H202 and they reported minimal fragmentation of particles. Here, we show that fragmentation is minimal because both concentrations (in the number of fragments and in total surface area per gram of sediment) are significantly correlated. Schlachter and Horn (2010) observed that the use of H202 may bleach or remove macroscopic charcoal particles that were not fully charred and recommended: *“the need to use identical preparation methods to treat all macroscopic charcoal samples in a study. When using H2O2 to prepare macroscopic charcoal samples, it is very important to ensure that solutions are of consistent strength.”* Following this recommendation, we used solutions of the same concentration.
We don’t think that these potential biases affect our results and conclusion however, as our results show charred particles in front of low-intensity fires areas (western central Africa) we strongly believe that charcoal produced at lower temperatures is not affected by the chemical treatment.

We modified the text to make it clearer:

"The microcharcoal preparation techniques were adapted from the work of 65 and classical palynology methods66 and petrographic mounted slides[67]. Microcharcoal samples were processed following the sediment preparation protocol previously published[25] slightly modified depending on the composition (organic-, silica-, carbonate-rich) of marine surface sediment samples. The microcharcoal extraction technique was applied to approximately 0.2 g of dried bulk sediment and consisted of the following steps: a) a chemical treatment of hydrochloric acid (37% HCl) to remove calcium carbonates (CaCO3) until the reaction, i.e., degassing bubbles, stopped; b) followed by nitric acid (68% HNO3) and hydrogen peroxide (33% H2O2) attack on warm water-bath to remove or bleach the organic matter; c) centrifugation to remove the supernatant before; d) a chemical attack of hydrofluoric acid (HF) to remove siliceous material followed by centrifugation to remove HF; e) followed by an HCl treatment to remove colloidal SiO2 and silicofluorides formed during the HF digestion followed by a centrifugation to remove HCl. For samples with more than 1% organic matter, we added additional steps with alternating nitric acid and hydrogen peroxide chemical attack. This chemical treatment is used to remove carbonates, pyrites, humic material, labile or less refractory organic matter (OM), to bleach non-oxidized OM and to remove silicates. A dilution of 0.1 is applied to the residue. The suspension was then filtered onto a membrane of 0.45 µm porosity. A portion of this membrane was mounted onto a Polymethyl Methacrylate (PMMA) slide with ethyl acetate before gentle polishing with alumin powder."

Previous work[68] reported some fragmentation of particles when using HNO_3 or H_2O_2 on modern charcoal and on peat sediment containing charcoal. The microcharcoal concentration (CCnb representing the number of microcharcoal particles per gram of dry weight sediment) and microcharcoal surface area (CCsurf, representing the sum of all surfaces of microcharcoal particles in one sample per gram) are significantly and positively correlated (Fig. S.7, $r=0.86$, $p<0.01$), suggesting that potential fragmentation due the chemical procedure[68] is minimal. Both parameters record the same pattern of microcharcoal variability highlighting that CCnb can be used here to inform about these changes. A bias in our charcoal record might come from the use of H202 that may bleach or remove partially charred particles resulting from low temperature fires[66]. To minimize the error, we followed their recommendations by using solutions of consistent strength to treat all samples.

L 312: what is PPMA?

We corrected the acronym and modified to read "Polymethyl Methacrylate (PMMA)".

L 316: do u mean the concentration of microcharcoal number?
We modified to read: "The microcharcoal concentrations (CCnb representing the number of microcharcoal particles per gram of dry weight sediment, and CCsurf representing the sum of all surfaces of microcharcoal particles in one sample per gram)"

-L 322 and after: too many unexplained acronyms (ASTER GDEM LC FRY…). Boring and painful!
We modified the text and included the full name for all the acronyms used.

-Ref 11 and 13 are the same!
We modified it accordingly.

-Ref 36 is a not a convincing ref. Check papers by Lynch et al 2004 in Canadian Journal of Forest Research and Ohlson and Tryterud 2000 in The Holocene.
We thank the reviewer for this recommendation. We have now added the suggested references.

-Figure 1: too small fonts in the figures and the charts.
We increased the font in the figures and the charts.

30th Jan 23

Dear Dr Haliuc,

We had a problem on Friday with our system for sending emails and so I am re-forwarding the decision email I sent you on your manuscript in case you never received it.

All the best,
Alienor Lavergne

Your manuscript titled "Charcoal in ocean sediments track fire regimes in Africa" has now been seen by 3 reviewers, and I include their comments at the end of this message. Reviewers #1 and #2 are satisfied with your revisions and find that your manuscript has been largely improved, however Reviewer #3 still raises major concerns regarding the sedimentation times of the top samples of marine sediments which are only partly reported and hence the lack of solid chronologies to compare with current African fire patterns. We are still 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. Please highlight all changes in the manuscript text file. We also ask you to demonstrate in your revised manuscript how our top editorial thresholds have been met:

1) Provide compelling evidence that charcoal profiles in marine sediments off the west African coast are a robust record of fire activity in subtropical and equatorial/tropical Africa;

2) Quantify and communicate uncertainties in sedimentation times for the upper 1.5 cm of marine sediment and demonstrate that estimated charcoal ages are accurate and robustly correlate with the presented modern fire data

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,

Alienor Lavergne, PhD
Associate Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements (Download the link to your computer as a PDF.)

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I appreciate the revisions undertaken by Haliuc et al. They have broadly addressed my comments. I do have one remaining (minor) note to consider (see below). Though I retain a few disagreements regarding interpretation, Haliuc et al. have clearly and convincingly justified their conclusions. As I said in my first review, this manuscript will surely be impactful for the paleofire research community. In light of these revisions, I believe that the manuscript should be accepted.

Specific comment (referring to line numbers in the tracked changes document):
L55-61: This text remains slightly inaccurate. These two sentences essentially contradict one another. Although I understand and agree that charcoal in marine sediment archives have not been linked to these metrics, that is not what this text says. Please consider adding “marine” before “sedimentary archives” in Line 55.

Reviewer #2 (Remarks to the Author):

The responses and edits provided have significantly improved this manuscript. Perhaps the most critical modification to the manuscript is addressing the chronology challenges of the top core sediment comparison with satellite/modern data. The authors suggest that previous work in the region has shown that core-top samples can generally represent the late Holocene and range in age from a few hundred to few thousand years. The authors justify this through citing previous work that sucessfully used spatial data of marine surface sediment for core-top calibration. Authors also suggest other research priorities (e.g. microplastic analysis) have been prioritized for near surface sediment samples. Despite this challenge and limitation for the marine sediment research, I believe the methods, results and discussion offer significantly new evidence on the validity of using marine charcoal sediment data to understand changes in terrestrial fire regimes.
One final comment is to suggest the authors recommend a retrieval process (e.g. frozen sediment cores from the mud-water interface), that could be used in future studies to capture sediment that could be more precisely sampled at ~1mm intervals, for example, for top-core versus "modern" calibration.
I commend the authors for their detailed responses and I recommend the manuscript for publication

Reviewer #3 (Remarks to the Author):

This is a revised version of an original manuscript. The authors have carried out many modifications in response to editor's and reviewers' comments, both in the main text and the Supplementary Information.
The table of the SI contains a bottleneck information for the present study. Authors have partially completed this table by adding a time frame or a sedimentation rate for some marine samples. Actually, only 34 out of 128 (26%) have an assessment of sedimentation rate (column H) that varies from 22,000 to 0.0087 cm/ka (i.e. 0.0087 cm/ka is equivalent to 0.0000087 cm/yr, or 115000 yr.cm), which is extremely heterogeneous and variable and in some case incredibly inconsistent with the capacity to measure correctly the sedimentation rate by any lab method. However, only 26 samples out of 128 (20%) indicate time measurements based on isotopic methods (14C, 137Cs). Other samples have estimates based on sediment pattern ("carbonate fluctuation") that is questionable because based on strong sedimentological assumption(s), which increases the uncertainty on time frames. And, surprisingly, 7 samples have a sedimentation rate based on no indication ('N/A'), meaning that the analysis is not reproducible. Finally, 74% of samples have no indication of what sedimentation rates could be, despite vague indications of time periods covered by sediments in the column N. This is not scientific.
These facts, clearly indicate that the time issue captured by top samples of marine sediments remains unsolved or largely questionable. Unfortunately, this is a methodological bottleneck for the rest of the paper. The lack of any solid chronologies in many marine samples hinders any serious comparisons with fire pattern on the continent.
Furthermore, the text ambition to solve the question of the comparison of fire parameters simulation with reconstructions of paleofires (line 55-61), based on criticisms that terrestrial reconstruction at site or landscape scale did not provide "(…) quantitative measurements directly comparable to simulated values of fire regime parameters (…)". I think it is much more unrealistic to try to convince people that marine sediment, sampled sometimes at hundreds of km, if not thousand, from burnt areas would be more accurate that analyses based on terrestrial sediments near the so-called burnt area. This is pulling the wool over someone's eyes.
Finally, I regret that keystone aspects of the study are move into the SI, while the main text is associated with a graph for kids' magazine (figure 4). The figure 3 is still almost impossible to read.

Furthermore, there are only 6 envelopes of hydro-basins in the fig 3a and 7 in the fig 3b while there are 8 hydro-basins in the legend.
The Results section contain many times the terms correlation (e.g., lines 145, 146, 151) or make echo to statistical test, but with no test outputs or vague outputs (e.g., '>0.5' on line 147): correlation values, p-values, test name.
The discussion starting on line 288 on the possible coincidence between monsoon and fire activity is very premature at best, or totally off topic considering that the semi-experiment based on comparison of fire parameters on the African continent and the marine recent sediments does not include weathering/climatic parameters. So the last part of the discussion is speculative.

2) REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I appreciate the revisions undertaken by Haliuc et al. They have broadly addressed my comments. I do have one remaining (minor) note to consider (see below). Though I retain a few disagreements regarding interpretation, Haliuc et al. have clearly and convincingly justified their conclusions. As I said in my first review, this manuscript will surely be impactful for the paleofire research community. In light of these revisions, I believe that the manuscript should be accepted.

Specific comment (referring to line numbers in the tracked changes document):

L55-61: This text remains slightly inaccurate. These two sentences essentially contradict one another. Although I understand and agree that charcoal in marine sediment archives have not been linked to these metrics, that is not what this text says. Please consider adding “marine” before “sedimentary archives” in Line 55.

We thank the reviewer for his/her insightful comment. We agree that this sentence is still inaccurate. Based also on one of the comments of reviewer#3, and to avoid misunderstanding, of our message we modified sentence line 55 that now reads: “Sedimentary charcoal records provide an estimate of the changes in fire activity or biomass burning, and have the potential to offer quantitative measurements directly comparable to simulated values of the fire regime parameters such as fire number, size, frequency, and intensity [11,14–16]. However, despite the efforts of the paleofire international community, we are still missing comprehensive fire-proxy calibrations[8] (Hantson et al. 2016).”

We hope this revision is satisfactory.

Reviewer #2 (Remarks to the Author):

The responses and edits provided have significantly improved this manuscript. Perhaps the most critical modification to the manuscript is addressing the chronology challenges of the top core sediment comparison with satellite/modern data. The authors suggest that previous work in the region has shown that core-top samples can generally represent the late Holocene and range in age from a few hundred to few thousand years. The authors justify this through citing previous work that sucessfully used spatial data of marine surface sediment for core-top calibration. Authors also suggest other research priorities (e.g. microplastic analysis) have been prioritized for near surface sediment samples. Despite this challenge and limitation for the marine sediment research, I believe the methods, results and discussion offer significantly new evidence on the validity of using marine charcoal sediment data to understand changes in terrestrial fire regimes.

One final comment is to suggest the authors recommend a retrieval process (e.g. frozen sediment cores from the mud-water interface), that could be used in future studies to capture sediment that could be more precisely sampled at ~1mm intervals, for example, for top-core versus "modern" calibration.

I commend the authors for their detailed responses and I recommend the manuscript for publication

Reviewer#2 reports that: “Authors also suggest other research priorities (e.g. microplastic analysis) have been prioritized for near surface sediment samples.” To avoid misunderstanding, we did not refer to microplastic analysis in our study or in future research priorities. The examples of proxies calibrated in marine sediments we cited papers focused on dinocyst and foraminifera.

We thank the reviewer for this recommendation. Following reviewer's suggestion we added some context before "studies57": "We were unable to date some core-top samples using the ^{210}Pb method since some of the marine sediment cores were collected during oceanographic cruises from the 1990s."

We added at the end of the paragraph line 370: " For the development of future calibration studies, it is recommended to focus on sediment samples collected with a multitube corer device that preserves the mud-water interface"

Reviewer #3 (Remarks to the Author):

This is a revised version of an original manuscript. The authors have carried out many modifications in response to editor's and reviewers' comments, both in the main text and the Supplementary Information.

The table of the SI contains a bottleneck information for the present study. Authors have partially completed this table by adding a time frame or a sedimentation rate for some marine samples. Actually, only 34 out of 128 (26%) have an assessment of sedimentation rate (column H) that varies from 22,000 to 0.0087 cm/ka (i.e. 0.0087 cm/ka is equivalent to 0.0000087 cm/yr, or 115000 yr.cm), which is extremely heterogeneous and variable and in some case incredibly inconsistent with the capacity to measure correctly the sedimentation rate by any lab method. However, only 26 samples out of 128 (20%) indicate time measurements based on isotopic methods (14C, 137Cs). Other samples have estimates based on sediment pattern ("carbonate fluctuation") that is questionable because based on strong sedimentological assumption(s), which increases the uncertainty on time frames. And, surprisingly, 7 samples have a sedimentation rate based on no indication ('N/A'), meaning that the analysis is not reproducible. Finally, 74% of samples have no indication of what sedimentation rates could be, despite vague indications of time periods covered by sediments in the column N. This is not scientific.

These facts, clearly indicate that the time issue captured by top samples of marine sediments remains unsolved or largely questionable. Unfortunately, this is a methodological bottleneck for the rest of the paper. The lack of any solid chronologies in many marine samples hinders any serious comparisons with fire patterns on the continent.

We thank the reviewer for these insightful comments and his/her attention to details.

There was actually a mistake in the Table: the sedimentation rate written as 0.0087 cm/ka was actually 8690 cm/ka (0.115 yr/cm). We corrected it in the Table and we cross-checked again the numbers reported in the table.

The 7 samples with a sedimentation rate with no indication (NA) on dating was obtained from Baudin et al. (2020) which compiled only the sedimentation rate.

For this new revised version, we added additional information about the dating methods used for our top-core samples in the supplementary table. The table shows that the age estimates of 46 top samples are based on direct chronologies of the sediment cores, the ages for 13 samples were inferred from closeby cores which had direct chronologies, the age for 1 sample collected from the estuary was the year of the collection, the ages for 4 samples from multi-cores that preserved the sediment-water interface (fluffly oxic layer present) was the year of the collection. For 4 samples which had no closeby cores with direct chronologies, we considered that "Holocene" was the most appropriate age to give, based on foraminifera assemblages or benthic oxygen isotopic measurements. For the remaining 60 samples which are located off western and central Africa coast,

we inferred the ages from the sedimentation rates of published papers with multiple cores located very close to our samples (Jansen et al., 1984, Baudin et al., 2017) (please see the additional Maps in the Supplementary table, the yellow squares indicate the location of the cores from literature, used to estimate the ages). The study of Jansen et al. (1984) present means terrigenous fluxes (g/cm3/ka) during the Late Quaternary. We calculated the sedimentation rates by dividing the terrigenous flux by the dry density (g/cm3), using a dry density of 0.776 g/cm3 (estimated using a humid density of 1.5 g/cm3, a 2.65 grain density and a 0.707 porosity). For each sample, we estimated a (maximum) average sedimentation rate from which we inferred the largest estimated ages (calendar years) at 1 cm depth. We made the assumption that the range of estimated ages includes age uncertainties. We hope that now the information dipslayed in the table is clearer.

We modified in the method section line 349 to read: "Chronological information was extracted from published studies for our core-top samples showing the samples cover less than two decades to a few or several centuries of environmental changes (Supplementary Data). Thirty samples are dated less than 50 years, 20 less than 100 years, twenty one less than 150 years, twenty four less than 200 years, fifteen less than 500 years and twelve of several hundred years up to late Holocene, and six samples of Holocene age. Ages of 46 core-top samples are based on direct chronologies of the sediment cores. Ages of 13 samples were inferred from closeby cores with direct chronologies. The year of collection was assigned to one sample from the estuary and to 4 samples from multi-cores that preserved the sediment-water interface (fluffy oxic layer present). For other 4 samples that had no closeby cores with direct chronologies, we assigned an Holocene age based on foraminifera assemblages or benthic oxygen isotopic measurements. For the remaining 60 samples, located off western and central Africa coast, we inferred their ages from published sedimentation rates in multiple cores surrounding our samples (Jansen et al., 1984, Baudin et al., 2017) (please see the Maps in the Supplementary table, the yellow squares indicate the location of the cores from literature, used to estimate the ages). The study of Jansen et al. (1984) presents mean terrigenous fluxes (g/cm3/ka) during the Late Quaternary. We calculated the sedimentation rates by dividing the terrigenous flux by the dry density (g/cm3), using a dry density of 0.776 g/cm3 (estimated using a humid density of 1.5 g/cm3, and a 2.65 grain density and a 0.707 porosity). For each sample, we estimated a maximum average sedimentation rate from which we inferred the largest estimated ages (calendar years). We made the assumption that the range of estimated ages include the age uncertainties."

Furthermore, the text ambition to solve the question of the comparison of fire parameters simulation with reconstructions of paleofires (line 55-61), based on criticisms that terrestrial reconstruction at site or landscape scale did not provide "(…) quantitative measurements directly comparable to simulated values of fire regime parameters (…)". I think it is much more unrealistic to try to convince people that marine sediment, sampled sometimes at hundreds of km, if not thousands, from burnt areas would be more accurate that analyses based on terrestrial sediments near the so-called burnt area. This is pulling the wool over someone's eyes.

The aim of our study is to show the potential of charcoal analysis based on marine sediments and not to undermine or compare it with the potential of terrestrial sediment archives. Following the comment made by reviewer#1, we modified sentence line 55 to read:"Sedimentary charcoal records provide an estimate of the changes in fire activity or biomass burning and have the potential to offer quantitative measurements directly comparable to simulated values of the fire regime parameters such as fire

number, size, frequency, and intensity [11,14–16]. However, despite the efforts of the paleofire international community, we are still missing comprehensive fire-proxy calibrations[8]".

Finally, I regret that keystone aspects of the study are move into the SI, while the main text is associated with a graph for kids' magazine (figure 4).

We moved a substantial part of the SI into the main text following the reviewers' recommendations. The SI is now composed of additional figures and tables to support the main text. It also details the Hysplit experiment we conducted to understand the regional windblown transport of charcoal particles. We believe Figure 4 summarizes our interpretation about what can happen on the continent and it might be of interest to the large audience of Communications Earth & Environment journal. Other more scientific figures, according to the reviewer, might be very cryptic to some non-expert readers and figure 4 will provide them with a understandable take-home message. For this reason, we decided to keep it in the main text.

The figure 3 is still almost impossible to read.

We don't understand this comment as we already increased the size of figure 3, changed colours and used bold font to draw ellipses in the previous revised version. Figure 3 reads well on our files and computers. We therefore supposed that the "bad quality" relates to the transformation to pdf via the online system. High-resolution figure will be provided, if the manuscript is accepted, to improve readibility. .

Furthermore, there are only 6 envelopes of hydro-basins in the fig 3a and 7 in the fig 3b while there are 8 hydro-basins in the legend.

Yes, this is true, the reason for this is explained in the captions of Fig. 3, Fig. S4: „Ellipses with different colours and symbols represent the hydro-basins (please note that Namibia and Limpopo do not form an ellipse on the ocean data due to the restricted number of samples)."

The Results section contain many times the terms correlation (e.g., lines 145, 146, 151) or make echo to the statistical test, but with no test outputs or vague outputs (e.g., '>0.5' on line 147): correlation values, p-values, test name.

We have now modified the text where the term correlation was used repetitively. Also, we have now added detailed information about the statistical analysis including the values of the cos2 and the Kruskall Wallis test with df and p-values. In the method section we included information about the R statistical packages we used for these tests.

Line 135-137: "The Kruskal-Wallis test applied on PC2 (correlated with concentration and elongation) shows evidence for significant differences between basins (Kruskal-Wallis chi-squared = 21.199, df = 7, p-value = 0.003486) as well as a significant difference between the two main regions (Kruskal-Wallis chi-squared = 8.1685, df = 1, p-value = 0.004262)."

Line 442: "We used squared cosine (cos2) to determine the quality of representation and strength of the relationship for each variable on the dimension."

Lines 145, 146: „...we used the term correlation here because it is the best term for describing the results of the PCs. We were criticised in the original version of the manuscript about inappropriate terms used to describe the PCs.

Line 151: We modified this sentence that now reads "connected with concentration and elongation".

The discussion starting on line 288 on the possible coincidence between monsoon and fire activity is very premature at best, or totally off topic considering that the semi-experiment based on comparison of fire parameters on the African continent and the marine recent sediments does not include weathering/climatic parameters. So the last part of the discussion is speculative.

According to reviewer#3 previous comments, we already nuanced this last section in the revised version. In this paragraph (L288-314), we do not use charcoal parameters from marine sediments to infer and discuss climate (monsoon) but we infer fire regime characteristics based on charcoal concentration or morphology from different deep-sea cores, in two subtropical regions at different time periods in the past. From line 327, we compared fire regimes with climate obtained from published studies including climatic proxies (Daniau et al. 2013; Dupont et al. 2018; Dupond and Schefuss (2018).

Daniau, A.-L. et al. Orbital-scale climate forcing of grassland burning in southern Africa. Proc National Acad Sci 110, 5069–5073 (2013).

Dupont, L. M. & Schefuß, E. The roles of fire in Holocene ecosystem changes of West Africa. Earth Planet Sc Lett 481, 255–263 (2018).

Dupont, L. M., Zhao, X., Charles, C., Faith, J. T. & Braun, D. Continuous vegetation record of the Greater Cape Floristic Region (South Africa) covering the past 300 000 years (IODP U1479). Clim Past 18, 1–21 (2021).

20th Mar 23

Dear Dr Haliuc,

Your manuscript titled "Charcoal in ocean sediments track fire regimes in Africa" 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 address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alienor Lavergne, PhD
Associate Editor
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #4 (Remarks to the Author):

I have received the revised version and the rebuttal letter from the authors. As I was asked to look at specific aspects on the study, I will address them.
issue 1) the sedimentation times of the top marine sediment samples are robust enough for the chronologies to be compared to current African fire patterns.
I believe that the authors have done a good job addressing all the concerns of all reviewers, in particular reviewer #3 regarding the age control of recent sediments. I have worked in that part of the world, looking at dinoflagellate cyst assemblages in recent sediments off West Africa; the findings show a strong coherence in the distribution of dinocysts which implies that the ages of surface sediments are quite uniform, about a few hundred years or very late Holocene (so very few climate variability). I would suggest to include the following papers that would support the age control of this study:
Hessler, I., et al., Millennial-scale changes in vegetation records from tropical Africa and South America during the last glacial, Quaternary Science Reviews (2009), doi:10.1016/j.quascirev.2009.11.029
Hardy et al., 2018. Quantification of last glacial-Holocene net primary productivity and upwelling activity in the equatorial eastern Atlantic with a revised modern dinocyst database https://doi.org/10.1016/j.palaeo.2018.06.025 - May be use this one instead of Radi and de Vernal 2008 as the paper from Hardy et al. deals with recent distribution of dinocyst assemblages in the studied zone of the authors.
Dupont et al 2007 Variability in glacial and Holocene marine pollen records offshore from west southern Africa DOI 10.1007/s00334-006-0080-8
These are some published, peer-reviewed papers which have demonstrated that recent sediments off West Africa do contain recent terrestrial proxies and can be used as modern analogues.

Issue 2) the manuscript provides compelling evidence that charcoal patterns in marine sediments off the West African coast are a record of fire activity in subtropical and equatorial/tropical Africa.
I do believe that the authors have made a strong compelling case that the patterns of charcoal remains in marine sediments record fire activity from equatorial to subtropical west Africa. Past records have indeed shown fire activity related to climate and vegetation type. This new study goes further as they have investigated the type of charcoal (size and shape) and related it to specific type of vegetation. The modelling aspect is very strong also.

Overall, I think that the authors have here a new and solid approach to reconstruct fire activity for a region that is notoriously difficult to study from terrestrial archives (lack of suitable environments to collect material). There always will be a debate about the age model of surface sediments; however, this is a very well-studied region (since the late 1980s) and all studies dealing with surface sediments have shown that recent samples do cover the last few hundred years to a few millennia.

26th March 2023

Dear Editor,

Re: Manuscript reference No. *COMMSENV-22-0710-B*

Please find attached a revised version of our manuscript "*Charcoal in ocean sediments track fire regimes in Africa*", submitted for publication as a research article in Communications Earth & Environment.

The prompt summary of the reviewers' comments and specific recommendations was highly insightful and has benefited the manuscript considerably. We also thank the reviewers for their careful attention to detail, which we have closely followed to clarify the content of our manuscript by addressing all the weaknesses raised. We presented our responses in blue to assist you with assessing our revisions.

We remain at your disposal if there is a need for complementary information.

We shall look forward to hearing from you at your earliest convenience.

Yours sincerely,

Aritina Haliuc, PhD, (corresponding author)

Address for correspondence:

Aritina Haliuc

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aritinahaliuc@gmail.com

1) REVIEWER COMMENTS:

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Dupont et al 2007 Variability in glacial and Holocene marine pollen records offshore
from west southern Africa DOI 10.1007/s00334-006-0080-8

These are some published, peer-reviewed papers which have demonstrated that recent sediments off West Africa do contain recent terrestrial proxies and can be used as modern analogues.

We would like to thank the reviewer for these suggestions. We have now included the recommended studies into the paper. Please see Page 20, Line 365.

Issue 2) the manuscript provides compelling evidence that charcoal patterns in marine sediments off the West African coast are a record of fire activity in subtropical and equatorial/tropical Africa.

I do believe that the authors have made a strong compelling case that the patterns of charcoal remains in marine sediments record fire activity from equatorial to subtropical west Africa. Past records have indeed shown fire activity related to climate and vegetation type. This new study goes further as they have investigated the type of charcoal (size and shape) and related it to specific type of vegetation. The modelling aspect is very strong also.

We would like to thank the reviewer for the appreciation.

Overall, I think that the authors have here a new and solid approach to reconstruct fire activity for a region that is notoriously difficult to study from terrestrial archives (lack of suitable environments to collect material). There always will be a debate about the age model of surface sediments; however, this is a very well-studied region (since the late 1980s) and all studies dealing with surface sediments have shown that recent samples do cover the last few hundred years to a few millennia.