

Dear reviewer,

First, we would like to extend our gratitude to the reviewers, for their time and constructive comments, which allowed us to substantially will enhance the quality of our manuscript. Please find below our responses to each of the individual comments made by the reviewer 2. The text in black represents our response whereas reviewer 2's queries are in red. This response have citation from the revised manuscript with in green changed or added parts.

Best regards,

Emeline Bellet, on behalf of the coauthors

Major comments

Thank you for your comments.

First a methodological concern. Concentrations of paleo-proxies in a sediment core are inherently confounded by changes in sediment accumulation that can bias the signal (for example see Marlon et al. 2013, QSR). The gold standard is to instead report the influx of a chemical or physical constituent of the sediment using the mass accumulation rate determined by the age model of the sediment core, which mitigates the impact of accumulation rate change effects on concentration. At a minimum, the authors should determine the accumulation rate of the PAHs instead of the concentration as this will provide a more appropriate comparison to regional fire activity.

We agree with reviewer that it would have been better to report influx rather than concentration. Unfortunately, we did not have the possibility to calculate the flux from our data. We used the remaining samples after other analysis were performed (Teixeira et al. 2021) and the volume weight of the sediment had not been computed. However, the sedimentation rate of the Holocene part of this sediment core is relatively constant (cf. figure 2), which prevents changes in concentration due to be potentially related to changes of sedimentation rate.

Second, the authors appear to flip-flop on when PAH concentration in their core is driven by wet deposition vs increased fire activity. They claim that high PAHs early in the Holocene are the result of higher wet deposition, but when PAHs also increase in the last few thousand years, despite no accompanying rise in rainfall, they attribute that to higher fire activity. They can't have it both ways.

Thank you for your remark, we acknowledge we were not sufficiently clear in the paper's first version. During the Early and Mid- Holocene, we assume that there is no fire event in the coring site vicinity because the charcoal signal is null (Teixeira et al, 2021). As a consequence, we must interpret high concentrations of PAHs as the predominant influence of Eastern Africa wildfires. On the contrary, over the last thousand years, a local source cannot be neglected as charcoals were identified within our studied core. We did not exclude that precipitation favoured the scavenging of PAHs from the atmosphere to the lake, during this period. We highlight that a new source of PAHs appeared and we cannot deconvolute the two signals. Moreover, on the same period the PAH composition changed, which reinforces the argument of another source which at this time could have been local. Based on your remark, we attempted at making our statement clearer with the following sentence (line 494-497):

“Then, over the last millennium, the PAH signal may also have been significantly influenced by local fire activity, which not mean that precipitation did not improve the efficiency of PAHs scavenging. If precipitation had rise on this period, the PAHs signal could be a convolution of the two signals (local fire and precipitation). With our knowledge it is impossible to deconvolute the two signals, Therefore our data did not bring new knowledge about variation of precipitation on the last 1000 cal a BP in Madagascar.”

Moreover, they gloss over a growing body of evidence that changes in fire activity in grassy ecosystems is dominantly controlled by rainfall via controls on fuel abundance {refs}. Their claim that wet deposition is driving the signal needs to supersede the existing literature showing that rainfall increases lead to higher fires in grassy regions (e.g., Eastern Africa which the authors (I believe) rightfully identify as the source area of the PAHs). I feel like the authors set up a strong argument for PAHs in this Malagasy lake being sensitive to fire activity in Eastern Africa. A very interesting and useful result but instead pivot toward the end of the paper into a poorly supported alternative hypothesis, the reason for which is unclear to me.

Thank you for this comment which pushes us to distinguish two consequences of our results: i) PAH flux in Madagascar mostly originates from Africa – at least until the 1000 years and ii) our argument favouring an enhanced atmospheric aerosol scavenging rather than a higher wildfire activity in Africa during the African Humid Period. We are well aware about the literature showing in some places in Eastern Africa, a rise in fire activity during humid periods due to its control by the fuel abundance (Karp et al., 2021). However, the same authors also mention that in some places where the fuel is abundant, fire activity is on the contrary limited by moisture. We have based our first interpretation on a curve of fire activities based on the different charcoal records from the Eastern Africa. This curve shows that in average there is no peak in fire activity in Eastern Africa during the Early Holocene. However, thanks to your comment we have distinguish some uncertainties from this curve. So, we have realised an extraction from Bremond et al 2024 to compute a plausible curve of biomass burning from Eastern Africa and added it to the figure 4 of our manuscript (see this figure in supplementary). This curve indeed shows an increase in biomass burning from the onset of the Holocene up to 8000 cal a BP. Therefore, it is not impossible that our high concentrations in PAHs before 8000 cal a BP was due to an increase in biomass burning in East Africa. However, after this date the biomass burning clearly dropped in Eastern Africa, whereas our concentration in PAH increased and reached its highest value. Therefore, with the actual knowledge, we conclude that the high value of PAHs between 8000 and 5500 cal BP are not caused by an increase in fire activity in the Eastern Africa. Based on your comment we modified our discussion to make it clearer and clearly separate the questions of i) PAH sources and ii) processes explaining a higher PAH concentration between 8000 to 5500 cal a BP .

We have modified the initial manuscript between lines 431 and 437:

“The charcoal records from the Eastern Africa presented high value during the Early Holocene until 8000 cal a BP as our PAH record. However, between 8000 and 5500 cal a BP our PAH record presented high value of concentration. PAH concentration increase in Lake Maudit sediment from 8000 to 6000 cal a BP. Whereas on the same period the biomass burning in the Eastern Africa decrease and present its lowest values. As a consequence, we note that whereas PAHs likely originate from African biomass burning, their variations in concentration seems decoupled from the gross amount of African biomass burning on the period from 8000 to 5500 cal a BP.”

Third, the authors need to improve the description of the modern conditions (e.g., add fire and compare the wind fields and potential transport from source to sink of PAHs to show that the timing of fire and transport align in the potential sources they discuss) and bolster the data they compare their results to, e.g., including all of the fire records they discuss in the text and improve the labelling of figures. A description of the current fire regime of Madagascar and its relation to moisture and fuel as well as connecting the fire regimes of the potential source areas to Lake Maudit via wind direction comparison would greatly improve the background the reader needs to understand the paper.

We have improved the description on modern conditions in adding 2 paragraphs between lines 114 and 127. However, we want to highlight that nothing proves that the current fire seasonality was similar during the African Humid Periods. We think about this potential difference due to the paper of Tierney et al. (2011) showing that at 6000 cal BP, the wet season (June to November) was drier, whereas the dry season (December to February) was wetter than today.

“Moreover, from the point of view of air mass origin, those wind conditions isolate the Northern Madagascar from the rest of the Island. Wind condition prevent air masses to passing from the central of Madagascar to the Northern part. The southward shift of the ITCZ during the southern summer period (DJF), allowing the air masses originated from the Eastern Africa to reach the Northern of Madagascar (Fig. 1B).

The current fire distribution presents a seasonal pattern (Fig. 1E). Both at Madagascar and Eastern Africa, the fire season currently extends from August to November. However, it is important to note that nothing proves that this fire seasonality was the same during the whole Holocene. The fire activity depends on the precipitation cycle (Giglio et al., 2006; Karp et al., 2021; Vachula et al., 2022). Yet, Tierney et al. (2001) showed that the seasonality of precipitation was notably different during the Mid-Holocene. In Eastern Africa, the annual increase in precipitation was due to wetter dry periods whereas wet periods were drier than today (Tierney et al., 2011). Therefore, it is possible that the seasonality of fire activities has changed throughout the Holocene.”

Fourth, there are multiple mislabelings of hydrophobic vs hydrophilic PAHs that make some of the arguments difficult to parse. In revision, the authors should much more carefully make sure they are using the correct terms in each instance otherwise, their argument requires undue inspection by the reader.

Thank you to have note it. The whole manuscript has been edited accordingly.

In-line comments

Thank you for the comments concerning the grammar, spelling and for the words preferable to used.

line 18 : as recommand 'paleo studies' have been used instead of 'past studies'

line 32 : thank you to your remarks. We will rework on this introduction sentence to not realise an oversimplification.

“However, the interactions between wildfire dynamics and human land uses, climate, and environmental perturbations, particularly in the tropics, are **complex** (Bremond et al., 2024; Daniau and Brücher, 2016; Denis et al., 2012). **Observation on the past decanals showed that agricultural expansion tends to a decrease in wild fire in tropical environments (Andela et al. 2017).** Moreover, **past** studies provide a better understanding of wildfire behaviour and improve predictions (Daniau and Brücher, 2016; Karp et al., 2020a). **Karp et al. notably showed that the response of rainfall on savana wildfire activities was not linear.”**

Line 48-50 : More precision have been added to said that “**PAHs are used to study fire event or anthropogenic pollution**”.

74-76: Do these studies actually do a frequency analysis (e.g., Higuera et al., 2007, QSR)? A rise in grass combustion does not imply more frequent fires, just more burning (which could be higher burned area). You need to be very careful when discussing fire activity vs amount vs frequency as all have related but distinctly different meanings.

319-320: Presence of charcoal does not imply fire frequency, only occurrence. You cannot separate frequency from burned area with charcoal amount. To get frequency you have to look at a peak vs background analysis (see Higuera et al., 2007, QSR).

The words “frequency” have been delated.

84-95: This paragraph and the previous to some extent read as discussion-like. The introduction should lead up to your central question, but you appear to do that mid-way through leading to (as previously discussed) a question that I do not think should be the central point of the paper. This warrants reframing and reorganizing.

Thank you for your remarks. Our central question is which influence the PAH sediment record in the Northern of Madagascar. Is it the importance of fire event, the transport efficiency or the deposition efficiency? We have noted that this part looks like a discussion we have worked on it to be more an introduction part.

Two paragraphers have been deleted (lines 89-95 from the initial manuscript). The question has been simplified to

“In this paper, we take advantage of known climatic and local wildfire frameworks of northern Madagascar to investigate PAH deposition pathways over the whole Holocene.”

101-115: The potential fire sources are a large component of this paper, yet in the region description you only discuss the winds, temperature and rainfall. Why not the modern distribution/seasonality of fire? They claim later that the wind regimes exclude the southern portion of Madagascar as a PAH source for lake Maudit, so it would be good to set this up. Moreover, how does the Eastern African fire seasonality correspond with the timing of northwesterly winds? Setting up these potential sources at the beginning here would make your job in the discussion a lot easier. Same goes for the Equatorial Asia and Australia argument. I think that you may be able to just skip that because it is so unlikely to be the dominant source due to its distance but showing that the winds and fire timing align is the baseline to set up if that source even makes sense to consider.

Two paragraphers have been added on the origin of air masses and on current fire seasonality.

« **Moreover, from the point of view of air mass origin, those wind contions isolate the Northern Madagascar from the**

rest of the Island. Wind condition prevent air masses to passing from the central of Madagascar to the Northern part. The southward shift of the ITCZ during the southern summer period (DJF), allowing the air masses originated from the Eastern Africa to reach the Northern of Madagascar (Fig. 1B).

The current fire distribution present a seasonal pattern (Fig. 1D). At Madagascar and in the Eastern Africa, the fire season currently extend from August to November. However, it important to note that nothing prove that this fire seasonality was the same during the all Holocene. The fire activities depend on the precipitation cycle (Giglio et al., 2006; Karp et al., 2021; Vachula et al., 2022). Yet, Tierney et al. (2001) have proved that the seasonality of precipitation was different notably during the Mid-Holocene. In the Eastern Africa, the annual increase in precipitation was due to wetter dry periods whereas wet periods were drier (Tierney et al., 2011). Therefore, it is possible that the seasonality of fire activities has change during the Holocene. »

« Simultaneously, charcoal counts exhibit that fires started to occur, probably not in Montagne d'Ambre, but more likely occurring in the low elevation areas surrounding the mountains. » What is the evidence of the fire location?

Following your question, we have changed the text to be clearer.

“Finally, from ca. 1000 yr ago, an increase in Cyperaceae and Poaceae supports the development of a peat bog, which currently covers a large part of the lake and indicates increasingly shallow conditions (Teixeira et al., 2021). At the same time, charcoal particles began to occur in the sediment core, indicating an increase in fire activity. However, today, the rainforest surrounding the lake in the Montagne d'Ambre is too humid to burn regularly, whereas fires occur frequently in the drier piedmont areas. Charcoal recorded in the uppermost core sediments is therefore likely to have been transported by wind from these surrounding drier areas rather than produced directly within the rainforest or its immediate catchment. The same process may have contributed to the occurrence of charcoal in the sediment record during the past 1000 years. This interpretation is further supported by evidence from a second lake in the Montagne d'Ambre, also surrounded by rainforest (Lake Mahasirika; Montade et al., 2024). At this site, shallower conditions were also recorded at around the same time, suggesting that the increase in fire activity and human influence documented in the region may have occurred in association with drier environmental conditions (Montade et al., 2024). “

For lines 205-206, we have not note that the special character has disappeared, sorry for this mistake.

“However, if the value of a PAH concentration in a sample is comprised between the value of the LOD and the the LOQ, then its concentration is estimated equal to $LOQ/\sqrt{2}$ ”

208-209: More information here? What did the blanks look like? What was removed? This is really vague.

During the extraction we realised also a blank which was the procedure of extraction which is repeated without sediment. Like this if there is some extern PAHs contamination, we can subtract the PAHs quantities of the blank from the PAHs quantities of the sample. The behind concept is that if there is a contamination during the extraction it appear in the blank and we can remove it from the PAHs results of the samples. More information has been added to be clearer in the manuscript.

“During chemical procedures, blank controls were performed, meaning that the same extraction procedure was realised without sediment. The potential contamination quantities in PAHs found in the blanks analysis were removed from the sample PAHs results.”

211-212: I don't know what this means, do you mean the extraction efficiency? How does that compensate for evaporative loss, which happens post-extraction?

Yes, we used certified soil to determine the efficiency of the extraction. In the certified soil, the concentration in PAHs was known. We realised on 5 certified soil the extraction of PAHs and we quantified their PAHs. Usually, the extraction

has not an efficiency at 100%. For PAHs it dependent mostly from PAHs evaporation. Then, we quantified the loss in PAHs thanks to the certified soils, and the loss percentage was applied to the PAHs quantified in the samples. Like this we prevent the losses of PAHs during and post extraction.

223-224: Where are the exclusion criteria discussed? Why do you choose the dates that you kept? Are their physical indicators of erosion, and if so is it safe to assume the ages can be linearly interpolated across this disturbed interval?

Exclusion criteria were discussed by Teixeira et al. (2021), and we followed the same approach. However, as eight new radiocarbon ages have been added and as the reviewer requested, we considered it useful to more explicitly recall the processes that may explain the observed age inversions and to clarify the exclusion criteria applied here.

The text was modified as follows (see also lines 255-270):

“The calibrated ages are provided in Table 1, and the resulting age-depth model is shown in Figure 2. In the lowermost part of the core (>15 ka BP; >970 cm), three of the five radiocarbon dates are notably older and become progressively older with increasing depth. These age inversions may result from contamination by old carbon, potentially related to the dissolution and erosion of older carbon-rich soils during this period (see Teixeira et al., 2021, for further details). As artificially younger ages are generally more difficult to explain, the two younger ages were retained, while the three older ages were considered outliers and excluded from the age-depth model. Between 15 and 10 ka BP (940–630 cm), five additional radiocarbon dates also show age inversions, although the difference is less pronounced. This interval coincides with the onset of the African Humid Period, marked by increasing humidity and lake-level rise, as evidenced by vegetation changes (Teixeira et al., 2021). This wetter period was also associated with increased erosion, which may have promoted the reworking of older sediments or soils from the lake shore, potentially explaining the observed age inversions. As this study mainly focuses on the Holocene period, eight new radiocarbon dates were added to the initial age model. No age inversions were observed among either the new or previously retained dates, further strengthening the revised age-depth model and providing a very robust chronology for the Holocene reconstruction presented in this study.”

157: RStudio is an R editor, you need to credit the R language not RStudio here. And it appears you used CLAM not BACON, so it's a little concerning you are citing the wrong software here.

A first age-model depth model was initially built on the 11-m sediment core using 19 radiocarbon ages by Teixeira et al. (2021) on CLAM. A total of 8 new radiocarbon dates were added to the first 7 m to strengthen the Holocene section. The new age model was performed as a function of the composite depth with RStudio software using the R-package ‘Bacon’ (v. 4.0.5) (Blaauw and Christen 2011) and using SHCal20 calibration curve and bomb ¹⁴C curve (Hogg et al. 2020, Hua et al. 2016). The ages are expressed in calibrated years before present (years BP).

283: What regions?

The words “from region’ have been deleted because the sentence was not understandable with it.

307-308: What size class of charcoal is this? And what do you mean by “and Poaceae?” in the fig caption. Is this a true charcoal count or char:pollen ratio? If this is a macro charcoal count, it is not surprising that it will not correspond with PAHs because larger charcoal has a more local source. If this is a char:pollen ratio however, this does not provide direct fire information but instead a mixture of burning relative to grassy fuel available.

Thank you for your comments and sorry for the mistake. Initially, we plot the curve of Poaceae in the figure 4 but finally we have removed it but we forgot to change the title. It is well the curve of charcoal counting uniquely. Before counting, the sample were sieved through a 160 µm mesh (Teixeira et al., 2021). Following your comment, we have completed the information in the text and in the legend.

Legend is now : “B. Counting of charcoal (particules >160 µm) in lake Maudit (Teixeira et al., 2021). ”

In the text line 326 : “We confronted our PAH data with the charcoal record of particles >160 µm (Teixeira et al. 2021; Fig. 4A, B and C). ”

337-338: “We suppose... variation”. This similarity runs counter to your deposition control argument. If wet deposition rather than production via fires is driving the signal, you would expect a stronger rainfall/PAH connection between the hydrophilic vs hydrophobic PAHs.

Thank you for your comments. The reviewer 1 suggest that our terminology hydrophobic vs hydrophilic was not well explained and confusing. All PAH are hydrophobics there is just some which are less hydrophobic that we name the hydrophilic groups. This explain why there is not a much more difference between the two groups between lines 323 and 327:

“For the rest of the article we will use the terms “hydrophobic group” and “hydrophilic group” to characterise those two groups. However, one must note that all PAHs are hydrophobic. This separation hence separates the most hydrophobic from the least – yet – still - hydrophobic PAH congeners.”

337-339: You are mixing up the hydrophobic/hydrophilic, hydrophobic = heavy, particle bound. This is mislabeled in figure 4.

Thank you for your comments. We have changed it.

343-345: “Secondly... Holocene.” What is this sentence saying? You should just state what each record shows and how it relates to yours. Also, what is the “cf” in the first line?

The sentence has been deleted to have directly the description of the curves.

346-347: “In the southeastern... Holocene.” Why isn’t this plotted in figure 4 as well? This is important to your argument.

The figure 4 will be reworked following your comments. Please see it in supplementary of this response.

350-351: See my major comment, this needs to be set up earlier in the manuscript.

Thank you for your comment. We have added a paragraph on the origin of air masses in the “material and methods” 1.119-123.

3543-355: “distinctive wind pattern” You can really drive this home with a supp figure showing the wind field during peak Eastern African fire season.

In Figure 1 we have add a map of the seasonality of biomass burning following your advices in figure 1.

365: “Lake Tanganika and Lake Victoria” You are comparing your record to these records of fire in the source region you identify for the PAHs, why not include these in the comparison plot (Fig 4), being able to visually compare these records is critical to your readers to make the comparison and evaluate your claim that they are “markedly distinct” from your record.

375-376: What is the evidence for this?? They broadly agree with the Eastern Africa fire history.

For the two previous comments, the figure 4 have been reworked. For the comparison the curve of Lake Tanganika and Lake Victoria will be added. Moreover, an extraction of charcoals records from the Eastern Africa have been added. The comparison with the different lake one by one it may be an issue, because it is more probable that the air masses arriving from the Eastern Africa to the North of Madagascar is a mixture from air masses from the global region and not just from one lake area.

369: “until 11,000 cal a BP” This mischaracterizes the data, they show elevated fire activity in Turkana from ~15-6ka.

We agree with you. We have added the record of the lake Turkana in figure 4. We think that air masses did not come from a unique lake but from the region which is subjected to different fire regime.

374-375: Is is different, but overall consistent that fires where high during 11-6 ka and had a secondary increase in the last few thousand years. But agreed that this supports your assertion that the lake PAHs are sourced generally from Eastern Africa.

Thank you for your comment. We have adapted our interpretation following your comment and the curve of the Eastern Africa as mentioned before and for your comment line 388.

379-380: Panel B: What is plotted here? Caption says charcoal and pollen, but y-axis says charcoal?

381: Two panel D's, which is which?

Thank you for your two last comments. Initially, we plot the curve of Poaceae in the figure 4 but finally we have removed it but we forget to change the title. It is well the curve of charcoal counting uniquely. We also remake the panel notation.

370: Karp et al. show that fire declined with local declines in rainfall showing that declining fuel availability drove reductions in fire.

388: "This period..." Yes, and Karp 2023 argues that the precipitation is the main control on the fuel abundance and thus fire activity. If you disagree with this conclusion, you need to provide a strong counter argument.

We agree with the conclusion of Karp et al. (2023) that increased precipitation does not necessarily result in increased fire activity. The relationship between precipitation and fire activity is nonlinear and varies among environments. As shown by Karp et al. (2023), the response to similar precipitation changes differs among sites: fire activity increased at Lake Turkana, increased initially and then decreased at Lake Tanganyika, whereas it decreased at Lake Victoria. When the data were available, they have been added in Figure 4. Moreover, based on the available fire reconstructions from Eastern Africa, we added a syntheses of these records into a regional-scale fire activity curve (Bremond et al. 2024).

As explained before your comments allowed to improve the characterisation of the Eastern Africa fire activity, notably in choosing a better curve. The interpretation and the text have been adapted in adequation.

"The charcoal records from the Eastern Africa presented high value during the Early Holocene until 8000 cal a BP as our PAH record. However, between 8000 and 5500 cal a BP our PAH record presented high value of concentration. PAH concentration increase in Lake Maudit sediment from 8000 to 6000 cal a BP. Whereas on the same period the biomass burning in the Eastern Africa decrease and present its lowest values. As a consequence, we note that whereas PAHs likely originate from African biomass burning, their variations in concentration seems decoupled from the gross amount of African biomass burning on the period from 8000 to 5500 cal a BP."

392-393: This is context dependent. It matters if you are in a forest or grassland because precipitation controls on fire can have opposite effects (fuel vs moisture limited fire regimes).

We agree with your comment.

401-403: Then why do your hydrophilic and hydrophobic PAHs follow the same pattern? If PAH accumulation is all deposition driven, there should be deviation between the philic/phobic PAH time series, which there does not appear to be.

Thank you for your comments. The fact is the PAHs are not hydrophilic but hydrophobic. The group of hydrophilic are just less hydrophobics than the other. This explain why hydrophobics are also scavenging in a lesser quantity than hydrophilics. This explain why there is not a much more difference between the two groups between lines 323 and 327:

"For the rest of the article we will use the terms "hydrophobic group" and "hydrophilic group" to characterise those two groups. However, one must note that all PAHs are hydrophobic. This separation hence separates the most hydrophobic from the least – yet – still - hydrophobic PAH congeners."

414-415: This can just as easily (and has been previously) interpreted as a fuel driven change in fire occurrence. In a few sentences you report that PAHs increased with modern human activity driving more fires without an associated increase in rainfall. So why is it that when rainfall is high, it is deposition that drives PAHs and not fire amount, but when rainfall is low it doesn't matter and PAHs are driven by fire amount. How do you have this both ways?

On the last 1000 cal a BP, the regional context in the Northern of Madagascar change. At this time there is a local source. We see it also because there is change in PAH composition with heavier PAHs. We did not say that precipitation did not help the scavenging of PAH. We would like to said that there is potentially several factors the local sources which emitted PAHs and potentially the precipitation. We will try to review this paragraph to be more understandable between lines 460 and 465:

“Then, over the last millennium, the PAH signal may also have been significantly influenced by local fire activity, which not mean that precipitation did not improve the efficiency of PAHs scavenging. If precipitation had rise on this period, the PAHs signal could be a convolution of the two signals (local fire and precipitation). With our knowledge it is impossible to deconvolute the two signals, therefore our data did not bring new knowledge about variation of precipitation on the last 1000 cal a BP in Madagascar. “

428-429: I do not agree with this, if anything this speleothem record shows disagreements in the timing of PAH accumulation and local rainfall change: e.g., between 10-6 ka PAHs have a broad decline between 9-7 ka but while precipitation declines at 10.5-9 ka, it is just as high from 8.5-6 ka. If deposition was the main control one would expect a much tighter relationship with the local precipitation that displayed.

We think that firstly the divergence is accrued by the low resolution of our PAH records.

445: Huh? It's elevated. When did you discuss it being reduced during this time?

Sorry, you right its is not 'reduced' but 'elevated'.

448-449: “However,... precipitation.” I don't think this argument is supported by the data.

Thank you for your two last comments. We have reworked the conclusion in consequence of the change realised in the all manuscript.

“Concentration profiles of PAH stored in sedimentary archive, are usually used to infer palaeofires. In the North of Madagascar, whereas there was no fire activity until 1000 cal a BP, the concentrations in PAH present high values and significant variations over the Holocene, which means they represent a remote signal of fire activities from East Africa, thanks to monsoon winds that are set in motion when the ITCZ is its southern seasonal position. However, the signal of PAH differs from the fire activity signal from Eastern Africa between 8000 and 5500 cal a BP but is similar to the one of precipitation in Madagascar. To explain this pattern, two hypotheses can be highlighted: 1). PAHs originated from specific region of the Eastern Africa where biomass still burned, 2). We argue that variations wet deposition must not be neglected when interpreting pluri-centennial PAHs profiles. Under some conditions which remain to be constrained, PAH could hence be complementary tool in the attempt of reconstructing past hydroclimate fluctuations. “