nature portfolio

Peer Review File

Earliest evidence of fire-making

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
This manuscript presents a very convincing case for preserved remnants of 400 kyr old Paleolithic hearths in an open-air context in Europe. It will always be difficult for features such as this to preserve outside of caves, but the Barnham site still contains multiple lines of evidence for controlled use of fire: heated substrate (and presumably multiple features that are similar), a substrate that was likely fired multiple times, multiple heated lithic artifacts, and a non-local material that could be used as a fire striker. The work is original and impressive in its range of analytical approaches that include micromorphology, magnetic analyses, lithic refits, FTIR and PAH analysis. It is also refreshing to see photographs of people excavating – in other words, the fundamental observations that are made during the archaeological fieldwork are still important! I think that these findings absolutely warrant publication; however, I found the presentation of the evidence confusing or lacking in a few places. In particular, I suggest revisions to the text and figures that deal with the pyrite pieces and the characteristics of the reddened areas of the paleosol.

1) The pyrite.

The recovery of two fragments of pyrite in association with the burned materials is significant. The implication of this discovery is that the pieces are either fragments of pyrite that were used as fire strikers (but no use-wear traces remain), or that they were debitage produced from larger pieces (nodules) that fell off when struck. The discussion hinges heavily on these pieces because their presence implies that occupants of the site were not only able to control fire but to produce it on demand. Given the importance of these finds, I am startled at the lack of information about the pieces themselves.

First, the composition is rather ambiguous. The pieces are identified as pyrite based on the interior “typical crystalline structure.” I interpret this statement to mean that either the crystal habit is visible or that there is a characteristic cleavage pattern for pyrite that is visible, but this habit/cleavage pattern is not readily viewed in Figure 4, nor is it elaborated upon what a “typical” habit/pattern for this type of nodule might be. I suggest clarifying what is observed and revising Figure 4 to show the interior surface better (perhaps with an inset).

Second, there is no report of the actual identification of the mineral (primary or secondary) using either elemental analysis or x-ray diffraction. The authors state that both fragments are both completely replaced by iron oxide but it is not clear how this was determined. If neither fragment is today composed of pyrite, then it becomes even more imperative that the “typical” habit or fracture pattern be documented.

The third aspect of the pyrite analysis is its rarity on the surrounding landscape. I needed to reread this section of the paper several times in order to fully understand it. The argument is that the most probable local sources of pyrite would be fluvial or glacial gravels. These deposits were extensively analyzed regionally and no pyrite pieces (or iron oxide that was formerly pyrite??) were documented. I am convinced from this information that if the pieces recovered are indeed pyrite, that they are likely non-local. On the other hand, I found some information lacking. First, it is not clear if the Lower Chalk in the vicinity actually contains pyrite nodules or if it is only expected to contain it. Second, I’m not sure that there would be a high likelihood to find pyrite in fluvial gravels in the first place given the high density/low transport potential of the material. Nevertheless I think it is significant that all of the likely sources for rocks around the site do not contain pyrite.

Finally, in the discussion section Line 186 is almost word-for-word a line from the Sorenson et al. 2018 paper so it should be rephrased.

2) The reddened clay paleosol.

Many of the arguments hinge on the identification of reddened areas of the paleosol as heated. The implication is that these areas are the former hearth substrates and the ashes and charcoal have not survived. Overall, I find much of these analyses convincing and I am particularly heartened to see the use of experiments on the sediment from the site used to calibrate both the magnetic susceptibility and the FTIR results. The magnetic experiments could be an entire paper on their own. What is lacking for me are good photographs of the unaltered paleosol. It appears to me that all of the photographs in Figure 2 and in the supplemental show primarily the reddened features in isolation. In Figure 2a, the surrounding sediment at the same level is not yet excavated, while in Figure 2b, the feature has now been pedestalled and the surrounding sediment is removed. In Figure 2c, it is not clear if the features being exposed are reddened and these are not the features that were analyzed. I suggest adding another image to help the reader understand how different these features appear from the sediment that is more typical of unit 6. In addition, it is unfortunate that a micromorphology sample was not collected from an equivalent section of unaltered paleosol that preserves the contact between the yellow brown clayey silt and the black clayey silt to provide a better comparison.

The micromorphology is mostly presented in the supplemental section. The main point of this is that although some portions of the paleosol contain iron oxides that are in places typical of pedogenic formation (e.g. along voids), within the reddened area the iron oxide is evenly distributed throughout the matrix. This distribution is not usually found in soils and is therefore due to heating. I found this portion of the study convincing, but the figure caption in the supplemental section should be edited for small spelling and grammatical errors. In addition, there seems to be one image in the photoplate that is not labeled – it is the XPL image in the upper left corner.

The FTIR analyses are also presented well; however, experimental work that investigated the mixing of heated and non-heated clays found that it takes very little non-heated clay mixed into the heated clay to produce a low temperature or non-heated signature. I am referring to the work of Ogloblin et al 2023. What concerns me especially is that the micromorphology documented evidence of clay illuviation in the sampled sediment. This process could introduce non-heated clay into a heated clay matrix. In particular, this process could make it especially difficult to interpret some of the more subtle changes in the FTIR spectra that the authors rely on for intermediate temperatures (for example, a slight decrease in the intensity of the 915 cm-1 peak at 350 degrees or slight upward/downward shifts in wavenumber for peak positions). I would request that the authors better integrate the micromorphology with the FTIR and address the complications that might arise from sediment mixing due to pedogenic processes if it is determined that some of the clay illuviation could post-date the heating.

Ogloblin Ramirez, I., Dunseth, Z.C., Shalem, D. and Shahack-Gross, R., 2023. Infrared spectra of mixtures of heated and unheated clay: Solving an interpretational conundrum. Geoarchaeology, 38(6), pp.822-829.

Referee #2

(Remarks to the Author)
The manuscript "Earliest evidence of making fire" is an important contribution to the understanding of the development of pyrotechnics and the origins of the production of fire. As such, the control of fire marks a significant technological milestone in human evolution. This paper offers a fresh perspective on this ongoing debate, presenting new insights into the earliest evidence of fire production at Barnham site (UK), making its contribution particularly relevant.
The origins of fire remain controversial in the palaeolithic record, as the evidence is highly susceptible to alteration and often complicated by taphonomic bias and even more complicated in open-air sites. The authors have done a remarkable work in carrying out almost all possible analyses, mainly on the reddened sediments, to present these results.
The manuscript is well-written, with clearly formulated hypotheses, well-defined methods, clear results, and appropriate interpretations. I believe the article's greatest contribution is the finding of two fragments of pyrite, which, associated with the rubified sediment and stone tools, support the idea of fire production and control of fire by human groups. Early evidence for the use of fire in Europe is scarcely documented, as such, the contribution of this work evidenced that around ~400 ka, humans were not only controlling fire but also capable of producing it. For this reason, I strongly recommend that this paper be published in Nature.

I indicate below where I have questions or comments:
In my opinion, some aspects of the structure could be refined to better align with the purpose and relevance of the article. As such, the current 'main' section focuses on the archaeological description and context of Barnham (lines 28-72). While this information is essential, I believe that the focus of the main text (i.e. introduction) should be changed to fire and its production in the palaeolithic. This includes both the evidence currently known and the processes involved in the production of fire. Although this topic is addressed in the discussion (e.g. lines 179-199), I believe it would be positive to place more emphasis on these aspects within the main body, strengthening the relevance of the research because managing of fire is currently being debated in relation to some archaeological sites and chronologies. Such clear evidence reinforces the use and production of fire ~400 ka claimed in Europe, as documented at a few other sites such as Menez Dregan, Beeches Pit, Aroeira,… Accordingly, I suggest restructuring the paper to move the detailed description of the archaeological site into a separate section (e.g. 'Archaeological Context'). This approach would improve the readability of the main text and highlight the importance of fire and its production.
Line 48. “Heated materials (including natural and artefactual flint, sediment, but rarely charcoal)…” What about large

mammals? I suggest briefly addressing why large faunal remains are absent at Barnham (sedimentation,…), as well as why ash and charcoal are rare or absent (erosive agents, such as wind and rain,…).
Lines 129-166. It would be helpful to specify how far (in ~km, for example) where the nearest pyrite is believed to be located for those of us unfamiliar with the geography and geological context of the area.
Figure 1.D. The pyrite fragments are not included in the plot, they would have to be added.
The heat-shattered handaxe (Figure 4.B) is reconstructed by 25 refitted elements. Are they plotted separately at Figure 1.D? If refits are done, it would be also positive to indicate in the schematic plan the refitting connections to illustrate the event and relation with fire, if possible. It can be relevant to determine whether burned materials remain in their original combustion location or have been moved after burning to stablish their depositional history.
Table 1.1. Thermal alterations on artifacts are summarized in the table. However, there are several thermal alterations, such as cracking, reddening, and spalling. Why this degree of heating? How do the authors explain the burning on artifacts—postdepositional, direct exposure to fire, or other factors?
It is confusing the Table 1.1. It would be more readable to give everything in numbers of remnants rather than number for artefacts and percentages for heated elements. In Table 2.2. the percentage (%) of each raw material is given, while the total is the number. This should be indicated in the heading.
Section 1. Artefacts: Lithic artefacts have been examined to identify traces that could be interpreted as evidence of fire making, as documented in Neanderthal contexts?
I think the images could be improved, for example by including scale, location or some spatial references, such as:
Figure 2. B. Where is located B-B' in figure 2.A? It could be also marked.
Figure 1.1. A scale of measurement is missing, and the quarry cut (or other spatial reference) could be marked.
Figure 2.1. It is understood that this corresponds to section A-A', but, if possible, it would be good to mark.
Figure 3.2. B. The charred plant residue (black arrow): Could any anatomical structure be observed? Could it be related to organic material occurring naturally in soils?

Referee #3

(Remarks to the Author)
Evaluation report – Davis, Ashton et al.
by Dr. Ségolène Vandevelde

General comment

Identification of ancient traces of fire is a real challenge. Indeed, the taphonomic processes affecting combustion structures make them particularly difficult to identify the further back in time we look. It is even more complicated to distinguish with certainty between fires of anthropogenic origin and those of natural origin. Clues are often slight, indirect or equivocal. It is generally difficult to be conclusive, especially in open-air contexts, where natural fires can thermally alter artifacts more easily than in enclosed environments.
Pyroarchaeological research has made great strides in recent decades. Archaeomagnetic analyses have shown that signatures can differ between natural and anthropogenic fires. FTIR analyses have shown when heating occurred, enabling the identification of heating when no trace of it is visible to the naked eye, but also the distinction to be made between heat-induced rubefactions and reddenings due to other processes. More recently, breakthroughs in PAH analysis have shown that the ratio of light to heavy PAHs differs between anthropogenic and natural fires. The demonstration of the recurrence of multiple fire events in the same place is also a good indication of the habitual use of fire by hominins.

In this article, the authors present a case of what, in the field, by the naked eye, has been identified as a potential open-air fireplace. The case is difficult: the traces are very old (~400ky BP), on an open-air site, with a few artifacts that appear to be burnt (which, on their own, are not conclusive to unequivocally demonstrate the use of fire by humans in the past). The presence of chars is not sufficient either, as they can be redeposited (even in closed environments); their presence at Caune de l'Arago (Deldicque et al. 2021), for example, is a good indication of fire use, but was not sufficient to conclude that it resulted from in situ anthropogenic fire.
In order to demonstrate that the remains observed at Barnham are indeed the remains of an in-situ hearth, a multidisciplinary analysis has been carried out here, using highly complementary techniques. With this approach, the authors have succeeded in building a convincing case, with a strong body of corroborating evidence for the repeated use of fire by humans on this site. What's more, the discovery of pyrite in the vicinity of this anthropogenic fireplace argues not only for the control of fire (which can be collected and maintained over the long term, or produced at will), but also strongly suggests hominin fire-ignition capacity.

Unfortunately, there are no finds of "lighters" (such as in Sorensen's 2019 study of much more recent contexts) unequivocally demonstrating this ignition ability. But there is strong evidence here for the habitual use of fire by hominins as early as 400ka BP (pushing back the origin of the incorporation of fire into the hominin technological repertoire, since it is generally considered that the habitual use of fire emerges somewhere between 400 and 300ky BP). And there is enough evidence to strongly suggest the mastery of fire including the ability to ignite, and therefore the possibility of producing fire at will by hominins 400 ky ago. [By the way, the chrono-cultural context needs to be spelled out a little more clearly in the article (at least in a summary figure), since antiquity is an important point; referring to bibliographical references is fine, but not enough (see specific comments below).]
These results encourage a closer search for traces of fire on ancient sites, where they may be difficult to perceive due to taphonomic phenomena, but where they are not necessarily absent! This article also encourages the formation of multidisciplinary teams to work on ancient fire evidence, in order to reveal, characterize and interpret it properly.

All in all, this is not the oldest evidence of fire use, but rather one of the earliest cases of fire mastery / habitual use in Europe, and above all the earliest case for which it can be suggested that hominins were capable of igniting fire. Demonstrating the anthropogenic nature of fire use is complicated, especially in an open-air context, where it is not necessarily the most parsimonious hypothesis. A single analysis would lead to the hypothesis being formulated, but that would not be sufficient to be convincing. Here, there are several concordant clues, as there is a multi-method analysis of a possible combustion zone and its context, allowing a convincing interpretation of this feature as the remains of an anthropic fireplace. The presence of pyrite (whose geological occurrence is shown to be very rare in this precise context) is the cherry on the cake, and even allows the authors to put forward the idea of a capacity for fire ignition.
Beyond this “oldest case of” aspect, it's also a fine article, easy to read and follow, presenting a rigorous and effective combination of complementary multi-method studies, drawing on robust experimental archaeology reference frames. This type of approach is an example to follow for pyroarchaeological analyses, but more generally for pushing back the limits currently in the way of our understanding of the past. For all these reasons, I recommend publication of this article in Nature, after some minor modifications and clarifications listed in the specific comments below.

Specific comments
Page 1
l.28-30: indicate you are presenting things bottom to top from the beginning (the numbering is in chronological order and not in order of discovery during excavation).

l.44: units 4, 5c and 5e: is it an ensemble that is culturally coherent over 3 layers? The 3 layers are all interglacial sediments. Are they formed at +/- the same time (and result in 3 layers because of pedological processes) or are they 3 different layers deposited in different conditions over 3 different periods during the interglacial period?

Page 2
l.46-47. According to this wording (‘respectively’) we understand that 4+5c+5e correspond to Early temperate (pollen zone HoII), and that level 6 corresponds to the peak interglacial (pollen zone HoII/III) of the Hoxnian. (if it’s not that, change the text to clarify, if it is right, then it’s ok :-)).
To avoid any doubt please add a figure of the stratigraphy (it can be a simple log or schematic drawing) synthetizing all the information (lithostratigraphy, biostratigraphy and geochronology) from ref 1 to 4, with all the levels (at least 1 to 6, and especially relation between 4, 5c, 5e, and 6). Indeed, the chronological context is very important since the paper is about the earliest evidence of fire making. In this regard fig. 2.2 in Supplementary online Information (SI) is not enough.

Page 3
l.90: “pyrogenic origin.” -> add at the end of this sentence “(as opposed to control samples)” for more clarity.

l.93: “extensive” -> here (and therefore in the rest of the text) it is not clear what “extensive” means. Is it extensive in time or in space? Or both?

l.104-105: “…which suggests localized fires were related to the regional wildfire regime in this deposit.” This sentence brings confusion, since it is said before that magnetism does not show localized fire area in this level… Please clarify.

Page 4
l.140-141: it is said “and neither shows evidence of use”.
But
l. 152: it is said “Despite the Breckland having Chalk Bedrock, pyrite is very rare.”
It is very interesting that the authors have been able to mobilize these results from independent studies!
Although there are many faint indications of direct evidence (magnetism, PAH, FTIR) and indirect evidence (pyrite with no obvious trace of use, but rare naturally) of anthropogenic fire-use in this study, the authors brilliantly manage to show a coherent picture by presenting them all together. The elements are convincingly presented in the text.

l.159: please insert the bibliographical references of the lithological studies used here and not only in SI.

Page 5
l.182-183: the sentence is strangely worded, given that the oldest traces of fire are in Africa. I do not think this is what the authors mean, but because of diffusionist models (often mobilized in archaeology...) one might think that the authors are implying that the use of fire, and perhaps also its mastery, spread from England and Europe to Africa... As the article concentrates on Europe, perhaps it should be limited to that and not mention the Levant and Africa? or else indicate the oldest traces on continents other than Europe before this sentence.

l.183: “habitual use” might need to be defined. It can imply different things according to the authors.

l.188: reference 22
and l.197: reference 32
Maybe also site Perles 1977 that was stating these ideas much earlier than the references here listed.

l.201-203: long term conservation of fire is also possible (even if logistically demanding).

l.204-206: yes, it is a strong possibility (even if some authors stated fire is not necessary -> see the publications criticizing this cooking hypothesis – possibility of digestibility of meat without fire through fermentation, etc.). But there is cooking evidence in the Lavant 780ky ago… so it is also a good argument to state that use of fire is important once it is mastered (an indirect evidence of fire mastery the authors might want to add/discuss?).

l.207-210: yes, it is strong indirect evidence of habitual use and control of fire, since it is necessary part of the technological repertoire as a tool to produce heat-synthesized adhesives.

l.211: change “indicates” by “strongly suggests”. If the presence of pyrite reinforces the interpretation of the traces of fire as anthropogenic (the most plausible solid hypothesis given all the elements presented in the article), then to interpret it as solid proof (without obvious traces of use or without a lighter with traces of use as in ref.19) is perhaps a little abusive. It is just a question of framing.

l. 214: locate the references accordingly in text. -> “… including bones43 and wood44 working, and the creation of adhesives for hafting45.” (rather than putting them all in the same package at the end of the sentence.

Figures
Figure 1: great figure. But cite in caption (or in SI) the sources of these data. Maybe a synthetic table in the SI?

Add a complementary figure with stratigraphy (schematic drawing or log is enough, but the reader needs a synthetic figure with chronostratigraphic elements and Archaeological levels presented in the text). See previous comments.

Figure 2: great figure also. Make sure that people are ok and happy to appear on the picture C -> I like it better than blurred faces, and not anonymizing the excavators is always better! But they have to agree to appear there I did not check in the forms provided with the paper if something was already written about this…

Figure3: it is really a great synthetic figure, very clear. For even more clarity, add on y.b.c.s. that it has to be seen as the parent mineral matrix in the table.
It is interesting to see that there is always a regional fire record. But it would be great to have a “control layer” for PAH analysis without this signal (maybe in colder layers of the site where it is less likely that natural fires occur?) just to be sure that there is no false signal / problem of tuning or contamination during the analyses…
Also, why is there no FTIR data for b.s.? And for FTIR also it is weird to have always a heating signal (since it is not what is stated in the SI).

Methods
They are clearly presented in main text (enough to understand) with more precisions in SI.
Page 12
l.367: Environmental magnetism (the E needs to be in Bold font like the rest).
l.390: delete coma (,) after the brackets before at 4.7 kHz.

Supplementary information
Page 6 – Micromorphology
“Such reddened, cemented sediment lenses have been reported and suggested to present early evidence of fire control.” The sentence could be understood as the presence of red lenses indicate early evidence (and not recent ones that would appear differently?) but it is not indicative of the age. It’s not what the authors meant but the framing should be change so the reader does not understand it wrong. For example “Such reddened, cemented sediment lenses have been reported; they are susceptible to be better preserved over time and therefore provide early examples of fire control”?

Page 8
Figure 3.2: the shapes of the squares in A do not correspond to the pictures presented in B to F, and there is two D but no E. Also, check spelling in the caption (Micromorphological – R is missing).

Page 9
Please de-group references 21 to 23 (like remark for line 214 in main text).
Page 10
L1: be consistent with use of – or - and spacing.
Table 4.1 last square: delete second space in front of magnetotatic (-> magnetostatic?)

4.3. experimental design -> here we find the use of “extensive” again with no precision. It is not clear what it means.

Last line: “successively 1 to 4 times dependent on the results”. It is not clear what is the expected result justifying to do it just once?

Page 11
“Table 3.2” -> change for Table 4.2.
Please check all the figures and tables labelling.

Page 12

Figure 4.1. Caption: change “experiment one” for “experiment 1” like in the rest of the text.

Table 4.2. Add a column on the left indicating that 1056 and 1055 are yellowish brown clayey
And 1054, 1053 and 1100 are reddened clayey silt.

Last line: “of the unaltered substate” -> substrate. + needs to be specified with more clarity that it refers to the yellowish-brown clayey silt (if I understood well).

Page 15
Experiment 4: It is very interesting to show these changes in mineralogy and how to achieve to produce them experimentally.
The summary tables are informative but there is a need to have a figure to show these changes in experiment4, like in figure 4.1 (which is great), this time not according to Temperature but to repetitions.

The summary is super interesting and clear. It is a lot of work!

Page 16
Maybe add a “Mineralogy” column for better synthesis of what is presented in the text.

Page 17
Table 4.4: 1st square: missing information. I guess it should be “4-Hours experiments” (and not “Hour experiment”). Also 12-Hours (and not 12-Hour).
A complementary graph including mineralogy info (like fig.4.1) would be great.

Page 18
9 samples collected by who? Since it is specified for the 2 control samples… Say who does what all along or only in “authors contribution” section.

Also, please indicate the size and weight of samples taken and what is needed to conduct this analysis.

l.45: “mixed with recovery standards” -> in order to….?

Page 19
Suppress indents in table 5.1. for Unit 6 line.
l.72: “coal” -> charcoal? Or mineral coal?

Page 20
Table 5.3: indicate the interpretation of the results in last column of the table (add “interpretation” column on the right). Also add a column on the left with context (Barnham units and type of sediment -> to help better follow and jump between table and text).

l.91: r
l.97: capital R

Page 21
Figure 5.2. Please indicate all the numbers of the other samples (without circling them).

Page 22
l.138-139: climate melioration with rise of Temperatures? Compatible with rise of lighting frequency? (as discussed in Sorensen 2017 / Dibble et al 2018)

Great demonstration of on-site anthropogenic fire by combining magnetism and mineralogy, and PAH analyses. Presence of pyrite (rare in this geological context) also strongly suggest ignition capacities 400ky ago.

6. FTIR
l.149: “sample 1084 contains the same mineralogy as samples from the purported fire feature” -> That’s weird since archaeomagnetism experiments and study just showed a change in mineralogy between unheated and heated sediment. I guess the use of “mineralogy” term differs a bit in this section. Please precise what you mean and at which scale… otherwise it looks contradictory.

Page 23
Figure 6.1. Samples 1059, 1061, 1068: how were they selected among the 9?
Last sentence of the caption -> they all appear unheated. It contradicts main text and figure 3 where they are labelled as heated (<400°C) and not unheated (<300°C). Please clarify.

l.169-170. Please precise why you do stuff and not only what you do (especially when you do not refer to a published protocol explaining the whys of the hows).

Page 24

l.191. Why are the 9 samples taken not all presented in figure 6.1.?

Page 26
Put the text paragraph after the remaining figures, just before the references.
l.234: suppress the URL and replace by citing reference 66.
At the end of the paragraph please add a sentence for comparison with archaeomagnetic data -> consistent results of the 2 methods regarding T°C?

Figure 6.3: tell in caption why discontinuity and dashed line in B).

Page 27
Figure 6.3 should be 6.4.

Also, a figure 6.5. comparing the archaeological samples AND the corresponding reference spectra is needed to illustrate what is stated in last text paragraph.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
I congratulate the authors on the resubmission of a significantly improved manuscript. The combined list of comments and suggestions from the reviewers was very long and encompassed many different aspects of the work, and I appreciated that the authors tackled the workload and addressed nearly everything. I am especially heartened to see the new analyses of the oxidized pyrite pieces. I think that the additional documentation very much strengthens their arguments and interpretations. I also appreciate the addition of new micromorphological observations which I also think strengthen the work.

I recommend publication with only very minor edits to the main text (three small rephrases suggested below). I also suggest some edits to the micromorphology section of the supplemental information.

This work provides an excellent template for analyses of fire features in sites where the main lines of evidence (charcoal, ash, burned bone) are not preserved.

Minor edits:

Line 54 – rephrase to “Barnham – the site presented here – suggests that…”

Line 128 – rephrase to “could have arisen from either one or multiple heating events”

Line 187 – “indicating longer exposure above ground.” I think that this statement is important to clarify further. Here, the authors are reporting evidence that the pyrite oxidized in place (at Barnham), which would of course be a necessary step in the formation processes if the pyrite had been collected in its original state. The authors have now clearly established that that pyrite is oxidized to goethite, AND that the oxidization occurred post-depositionally within the site, which means that it was composed of pyrite when it was brought there. This is significant, and also necessary for the interpretation of the use of the pieces as fire strikers.

Supplemental materials

Fig 2.4 caption – should be pseudomorphic?

Micromorphology section – add scale bars to the images in the figures, or specify the height of each block in the captions

Page 15 – in two places, please change to “open porphyric related distribution.”

Avoid referring to void infillings as “illuviation.” Describe them as clay coatings or infillings depending on their degree of development (e.g. limpid, crescent clay infillings) and then interpret them as resulting from illuviation.

“Iron staining related to voids” – be more clear about this. Are they iron coatings, hypocoatings…etc?

“A few channels are apparent and dark brown amorphous staining.” Something is missing in this sentence. Where is the dark brown amorphous staining with respect to the channels?

“Iron nodules are also present and some exhibit a reddish hue indicating haematite formation (Fig. 3.6D).” I think this should be Fig. 4.6D. Also consider adding a few additional terms to describe the nodules, such as orthic (are they formed in place?) and concentric.

Figure 4.6F – This looks very strange for a charred root, but I am not opposed to including it in the publication. Perhaps

rephrase as “charred tissue within a void, interpreted as a possible charred root.”

“The reddening occurs only within void spaces and is related to microfacies not the matrix.” Unclear what is meant here by microfacies.

“Microphotograph of a root within a channel from the top of the palaeosol providing evidence of plant growth, roots are not well-preserved within this thin section, but this may be a more recent feature in PPL.” Break into two sentences and add comma before “in PPL.”

I am agreeing to the new transparent review process and signing my review - Susan M. Mentzer

Referee #2

(Remarks to the Author)
The authors have satisfactorily addressed the comments provided in the first review. As a result, the manuscript has been significantly improved and is now clearer and more coherent.
The restructured introduction, particularly the discussion of fire and its production in the Palaeolithic, strengthens the relevance of the research presented. In addition, the figures have been improved (with the inclusion of scales, among other details), and the new section (2. Pyrite) in the Supplementary Information provides valuable data on one of the most important materials identified.
Accordingly, the paper is suitable for publication.

Referee #3

(Remarks to the Author)
This is a second round of reviews so I will be much quicker and not remind all the good I think of the paper and its place in pyroarchaeology research and implications for prehistory.

I would like to thank the authors for all their modifications and detailed answers to reviewers’ comments. Many clarifications have been made regarding the context, stratigraphy and sedimentology. It was really important for a paper focusing on sediment analysis, and I am very happy with the modifications and complementary figures (in main text and SI). It is much clearer for the reader.

Some comments are not addressed regarding refitting studies and heated flint distribution (refer to reviewer’s 2 requests). It would indeed have been great to have these data, but it is ok to not be able to do everything in a single paper, and here I think this absence does not undermine at all the quality of the paper. Authors say some further analyses are planned for further studies and I am looking forward to it in a future paper :)

I think the authors have done a great job and the article is much stronger now. I only have a few minor specific comments:

Abstract:
Line 24: Delete « in later prehistory ». Pyrite is used also in historical times to strike sparks.

Also: I am not sure why the accent is put only on the cooking hypothesis at the end of the abstract. As the authors say later in the text, it is probably just a part of hominization drivers, with social brain hypothesis. Fire is a powerful driver of hominization, not only through cooking but also through the social restructuring that it enables (and requires). Maybe rephrase to broaden the impact of this study.

Line 38: I am not sure what the authors mean. Write plural -> « However, these traces/elements are often difficult to interpret. » Or if it refers only to the last case, please rephrase to avoid ambiguity and elaborate?

Lines 47-55: NB: There is also possibly the site of Schöningen (13 II-4) with fire traces (heated flint fragments) dating from MIS11-9. It indicates fire but not if it is natural or anthropogenic though.

Line 54. Repeated use of fire in Orgnac 3 site (cf. Vandevelde et al. 2025) demonstrated from the repetition of soot traces in a speleothem is also a good example for this 350-200ka period. It exhibits how difficult it is to discuss this question of repeated use / mastery of fire for remote periods, and that several alternative research strategies must be implemented regarding the contexts. And therefore, it shows how exceptional this study in Barnham is for such a distant period!

Line 73: missing dot at the end of the sentence.

Line 162: “In summary,” could be in bold and italics so the reader can easily see that this summary concerns all the previous sub-sections and not only the FTIR analyses.

Line 192: Pyrite is also used and recovered in much more recent periods such as the Middle Ages for example, or latter in XVI-XVII th century for early riffles.
It is not limited to prehistory and I think it shows how fundamental the use of pyrite is; it has been part of the fire tool kit throughout the ages!

Line 252: why this c. 500-300 ka period? Before in text it is always the 400-300ka period that is mentioned regarding fire… Why here put the early limit 100,000 years before Barnham? The reference 39 refers only to the increase of brain size, not the “significant transition in human behaviour from c.500-300ka”.

Line 254: for ca.500-300ka palaeoanthropologists do not refer only to “early neanderthals”. Pre-Neanderthals and Homo heidelbergensis also must be considered in western Eurasia.
It might be better to use a broader / more cautious wording.
But if you have paleoanthropological elements in Barnham to refer to “Early Neanderthals” that is great, but it should be presented at least in SI.

Figure 1: Area V appearing in figure 2 is missing. Could you please add it here? If Area V should not appear in fig.1, maybe add a note in caption to explain where it is regarding the areas represented or refer to SI fig.4.1 where it appears.

Figure2: it is much better and clearer with this information, thanks!

SI is well presented and clearer with the new elements.

We thank all the referees for providing highly useful and detailed comments on the manuscript, which we believe has led to a much stronger submission. Specifically, we have undertaken new imaging and XRD analysis on the pyrite fragments, and undertaken new micromorphological analyses on both the reddened and unaltered clay, both recommended by Reviewer #1. We have also restructured the introduction, as suggested by Reviewer #2 and a new figure to explain the chronology, as suggested by Reviewer #3. In addition, we have provided better clarity in the text and images where required, and dealt with all the small edits and grammatical errors.

Referees' comments:

Referee #1 (Remarks to the Author):

This manuscript presents a very convincing case for preserved remnants of 400 kyr old Paleolithic hearths in an open-air context in Europe. It will always be difficult for features such as this to preserve outside of caves, but the Barnham site still contains multiple lines of evidence for controlled use of fire: heated substrate (and presumably multiple features that are similar), a substrate that was likely fired multiple times, multiple heated lithic artifacts, and a non-local material that could be used as a fire striker. The work is original and impressive in its range of analytical approaches that include micromorphology, magnetic analyses, lithic refits, FTIR and PAH analysis. It is also refreshing to see photographs of people excavating – in other words, the fundamental observations that are made during the archaeological fieldwork are still important! I think that these findings absolutely warrant publication; however, I found the presentation of the evidence confusing or lacking in a few places. In particular, I suggest revisions to the text and figures that deal with the pyrite pieces and the characteristics of the reddened areas of the paleosol.

1) The pyrite.

The recovery of two fragments of pyrite in association with the burned materials is significant. The implication of this discovery is that the pieces are either fragments of pyrite that were used as fire strikers (but no use-wear traces remain), or that they were debitage produced from larger pieces (nodules) that fell off when struck. The discussion hinges heavily on these pieces because their presence implies that occupants of the site were not only able to control fire but to produce it on demand. Given the importance of these finds, I am startled at the lack of information about the pieces themselves.

First, the composition is rather ambiguous. The pieces are identified as pyrite based on the interior “typical crystalline structure.” I interpret this statement to mean that either the crystal habit is visible or that there is a characteristic cleavage pattern for pyrite that is visible, but this habit/cleavage pattern is not readily viewed in Figure 4, nor is it elaborated upon what a “typical” habit/pattern for this type of nodule might be. I suggest clarifying what is observed and revising Figure 4 to show the interior surface better (perhaps with an inset).

Second, there is no report of the actual identification of the mineral (primary or secondary) using either elemental analysis or x-ray diffraction. The authors state that both fragments are both completely replaced by iron oxide but it is not clear how this was determined. If neither fragment is today composed of pyrite, then it becomes even more imperative that the “typical” habit or fracture pattern be documented.

New imaging and XRD analysis has been undertaken and is now included in the Supplementary Information. This confirms that the fragments, indicated by the structure, are pseudomorphs of pyrite, converted to iron oxide.

The third aspect of the pyrite analysis is its rarity on the surrounding landscape. I needed to reread this section of the paper several times in order to fully understand it. The argument is that the most probable local sources of pyrite would be fluvial or glacial gravels. These deposits were extensively analyzed regionally and no pyrite pieces (or iron oxide that was formerly pyrite??) were documented. I am convinced from this information that if the pieces recovered are indeed pyrite, that they are likely non-local. On the other hand, I found some information lacking. First, it is not clear if the Lower Chalk in the vicinity actually contains pyrite nodules or if it is only expected to contain it. Second, I'm not sure that there would be a high likelihood to find pyrite in fluvial gravels in the first place given the high density/low transport potential of the material. Nevertheless I think it is significant that all of the likely sources for rocks around the site do not contain pyrite.

This paragraph has been rewritten to indicate the rarity of pyrite in the Chalk in the Breckland and to explain more clearly the disposition of the Chalk and the processes that could have been responsible for making it accessible to humans.

Finally, in the discussion section Line 186 is almost word-for-word a line from the Sorenson et al. 2018 paper so it should be rephrased.

The sentence has been modified.

2) The reddened clay paleosol.

Many of the arguments hinge on the identification of reddened areas of the paleosol as heated. The implication is that these areas are the former hearth substrates and the ashes and charcoal have not survived. Overall, I find much of these analyses convincing and I am particularly heartened to see the use of experiments on the sediment from the site used to calibrate both the magnetic susceptibility and the FTIR results. The magnetic experiments could be an entire paper on their own. What is lacking for me are good photographs of the unaltered paleosol. It appears to me that all of the photographs in Figure 2 and in the supplemental show primarily the reddened features in isolation. In Figure 2a, the surrounding sediment at the same level is not yet excavated, while in Figure 2b, the feature has now been pedestalled and the surrounding sediment is removed. In Figure 2c, it is not clear if the features being exposed are reddened and these are not the features that were analyzed. I suggest adding another image to help the reader understand how different these features appear from the sediment that is more typical of unit 6.

In addition, it is unfortunate that a micromorphology sample was not collected from an equivalent section of unaltered paleosol that preserves the contact between the yellow brown clayey silt and the black clayey silt to provide a better comparison.

Three new micromorphological samples of unit 6 have been analysed, two of which are on unaltered sediments and accompanied by additional figures in the Supplementary Information. In combination with the original analysis, these support the interpretation that the reddened clay is an isolated feature.

The micromorphology is mostly presented in the supplemental section. The main point of this is that although some portions of the paleosol contain iron oxides that are in places typical of pedogenic formation (e.g. along voids), within the reddened area the iron oxide is evenly distributed throughout the matrix. This distribution is not usually found in soils and is therefore due to heating. I found this portion of the study convincing, but the figure caption in the supplemental section should be edited for small spelling and grammatical errors. In addition, there seems to be one image in the photoplate that is not labeled – it is the XPL image in the upper left corner.

The figure caption has been corrected for grammatical errors.

The FTIR analyses are also presented well; however, experimental work that investigated the mixing of heated and non-heated clays found that it takes very little non-heated clay mixed into the heated clay to produce a low temperature or non-heated signature. I am referring to the work of Ogloblin et al 2023. What concerns me especially is that the micromorphology documented evidence of clay illuviation in the sampled sediment. This process could introduce non-heated clay into a heated clay matrix. In particular, this process could make it especially difficult to interpret some of the more subtle changes in the FTIR spectra that the authors rely on for intermediate temperatures (for example, a slight decrease in the intensity of the 915 cm-1 peak at 350 degrees or slight upward/downward shifts in wavenumber for peak positions). I would request that the authors better integrate the micromorphology with the FTIR and address the complications that might arise from sediment mixing due to pedogenic processes if it is determined that some of the clay illuviation could post-date the heating.

Ogloblin Ramirez, I., Dunseth, Z.C., Shalem, D. and Shahack-Gross, R., 2023. Infrared spectra of mixtures of heated and unheated clay: Solving an interpretational conundrum. Geoarchaeology, 38(6), pp.822-829.

We have now added the Ogloblin et al. reference and discuss how clay illuviation could lead to a mixing of heated and unheated in the samples used in the FTIR analysis.

Referee #2 (Remarks to the Author):

The manuscript "Earliest evidence of making fire" is an important contribution to the understanding of the development of pyrotechnics and the origins of the production of fire. As such, the control of fire marks a significant technological milestone in human evolution. This paper offers a fresh perspective on this ongoing debate, presenting new insights into the earliest evidence of fire production at Barnham site (UK), making its contribution particularly relevant. The origins of fire remain controversial in the palaeolithic record, as the evidence is highly susceptible to alteration and often complicated by taphonomic bias and even more complicated in open-air sites. The authors have done a remarkable work in carrying out almost all possible analyses, mainly on the reddened sediments, to present these results.
The manuscript is well-written, with clearly formulated hypotheses, well-defined methods, clear results, and appropriate interpretations. I believe the article's greatest contribution is the finding of two fragments of pyrite, which, associated with the rubified sediment and stone tools, support the idea of fire production and control of fire by human groups. Early evidence for the use of fire in Europe is scarcely documented, as such, the contribution of this work evidenced that around ~400 ka, humans were not only controlling fire but also capable of producing it. For this reason, I strongly recommend that this paper be published in Nature.

I indicate below where I have questions or comments:

In my opinion, some aspects of the structure could be refined to better align with the purpose and relevance of the article. As such, the current 'main' section focuses on the archaeological description and context of Barnham (lines 28-72). While this information is essential, I believe that the focus of the main text (i.e. introduction) should be changed to fire and its production in the palaeolithic. This includes both the evidence currently known and the processes involved in the production of fire. Although this topic is addressed in the discussion (e.g. lines 179-199), I believe it would be positive to place more emphasis on these aspects within the main body, strengthening the relevance of the research because managing of fire is currently being debated in relation to some archaeological sites and chronologies. Such clear evidence reinforces the use and production of fire ~400 ka claimed in Europe, as documented at a few other sites such as Menez Dregan, Beeches Pit, Aroeira,... Accordingly, I suggest restructuring the paper to move the detailed description of the archaeological site into a separate section (e.g. 'Archaeological Context'). This approach would improve the readability of the main text and highlight the importance of fire and its production.

Thank you for this suggestion. The paper has been restructured accordingly.

Line 48. “Heated materials (including natural and artefactual flint, sediment, but rarely charcoal)…” What about large mammals? I suggest briefly addressing why large faunal remains are absent at Barnham (sedimentation,…), as well as why ash and charcoal are rare or absent (erosive agents, such as wind and rain,…).

This sentence has been modified to explain the rarity or absence of charcoal, ash and burnt bone.

Lines 129-166. It would be helpful to specify how far (in ~km, for example) where the nearest pyrite is believed to be located for those of us unfamiliar with the geography and geological context of the area.

Details have been added.

Figure 1.D. The pyrite fragments are not included in the plot, they would have to be added. The heat-shattered handaxe (Figure 4.B) is reconstructed by 25 refitted elements. Are they plotted separately at Figure 1.D? If refits are done, it would be also positive to indicate in the schematic plan the refitting connections to illustrate the event and relation with fire, if possible. It can be relevant to determine whether burned materials remain in their original combustion location or have been moved after burning to stablish their depositional history.

The location of the pyrite has been indicated on Figure 1C. The refitting studies are not yet complete as the site is still under excavation. It would be difficult to plot the refits on the current figures, so this will be left until the refitting analyses has been fully undertaken.

Table 1.1. Thermal alterations on artifacts are summarized in the table. However, there are several thermal alterations, such as cracking, reddening, and spalling. Why this degree of heating? How do the authors explain the burning on artifacts—postdepositional, direct exposure to fire, or other factors?

The process(es) by which the flint was heated and distributed is certainly of great interest, but this will have to wait until full analysis.

It is confusing the Table 1.1. It would be more readable to give everything in numbers of remnants rather than number for artefacts and percentages for heated elements.

The number of remnants has now been added.

In Table 2.2. the percentage (%) of each raw material is given, while the total is the number. This should be indicated in the heading.

The captions of Tables 2.1 and 2.2 have been changed to explain this.

Section 1. Artefacts: Lithic artefacts have been examined to identify traces that could be interpreted as evidence of fire making, as documented in Neanderthal contexts?

This has now been explained in Section 1

I think the images could be improved, for example by including scale, location or some spatial references, such as:

Figure 2. B. Where is located B-B’ in figure 2.A? It could be also marked.

Scales and location of section B-B’ have been added.

Figure 1.1. A scale of measurement is missing, and the quarry cut (or other spatial reference) could be marked.

All scales are now included and the quarry cut is indicated

Figure 2.1. It is understood that this corresponds to section A-A', but, if possible, it would be good to mark.

Section B-B' has been added, as section A-A' has been removed by excavation at this point.

Figure 3.2. B. The charred plant residue (black arrow): Could any anatomical structure be observed? Could it be related to organic material occurring naturally in soils?

Unfortunately, this could not be resolved.

Referee #3 (Remarks to the Author):

Evaluation report – Davis, Ashton et al. by Dr. Ségolène Vandevelde

General comment

Identification of ancient traces of fire is a real challenge. Indeed, the taphonomic processes affecting combustion structures make them particularly difficult to identify the further back in time we look. It is even more complicated to distinguish with certainty between fires of anthropogenic origin and those of natural origin. Clues are often slight, indirect or equivocal. It is generally difficult to be conclusive, especially in open-air contexts, where natural fires can thermally alter artifacts more easily than in enclosed environments.

Pyroarchaeological research has made great strides in recent decades. Archaeomagnetic analyses have shown that signatures can differ between natural and anthropogenic fires. FTIR analyses have shown when heating occurred, enabling the identification of heating when no trace of it is visible to the naked eye, but also the distinction to be made between heat-induced rubefactions and reddenings due to other processes. More recently, breakthroughs in PAH analysis have shown that the ratio of light to heavy PAHs differs between anthropogenic and natural fires. The demonstration of the recurrence of multiple fire events in the same place is also a good indication of the habitual use of fire by hominins.

In this article, the authors present a case of what, in the field, by the naked eye, has been identified as a potential open-air fireplace. The case is difficult: the traces are very old (~400ky BP), on an open-air site, with a few artifacts that appear to be burnt (which, on their own, are not conclusive to unequivocally demonstrate the use of fire by humans in the past). The presence of chars is not sufficient either, as they can be redeposited (even in closed environments); their presence at Caune de l'Arago (Deldicque et al. 2021), for example, is a good indication of fire use, but was not sufficient to conclude that it resulted from in situ anthropogenic fire.

In order to demonstrate that the remains observed at Barnham are indeed the remains of an in-situ hearth, a multidisciplinary analysis has been carried out here, using highly complementary techniques. With this approach, the authors have succeeded in building a convincing case, with a strong body of corroborating evidence for the repeated use of fire by humans on this site. What's more, the discovery of pyrite in the vicinity of this anthropogenic fireplace argues not only for the control of fire (which can be collected and maintained over the long term, or produced at will), but also strongly suggests hominin fire-ignition capacity.

Unfortunately, there are no finds of "lighters" (such as in Sorensen's 2019 study of much more recent contexts) unequivocally demonstrating this ignition ability. But there is strong evidence here for the habitual use of fire by hominins as early as 400ka BP (pushing back the origin of the incorporation of fire into the hominin technological repertoire, since it is generally considered that the habitual use of fire emerges somewhere between 400 and 300ky BP). And there is enough evidence to strongly suggest the mastery of fire including the ability to ignite, and therefore the possibility of producing fire at will by hominins 400 ky ago. [By the way, the chrono-cultural context needs to be spelled out a little more clearly in the article (at least in a summary

figure), since antiquity is an important point; referring to bibliographical references is fine, but not enough (see specific comments below).]

These results encourage a closer search for traces of fire on ancient sites, where they may be difficult to perceive due to taphonomic phenomena, but where they are not necessarily absent! This article also encourages the formation of multidisciplinary teams to work on ancient fire evidence, in order to reveal, characterize and interpret it properly.

All in all, this is not the oldest evidence of fire use, but rather one of the earliest cases of fire mastery / habitual use in Europe, and above all the earliest case for which it can be suggested that hominins were capable of igniting fire. Demonstrating the anthropogenic nature of fire use is complicated, especially in an open-air context, where it is not necessarily the most parsimonious hypothesis. A single analysis would lead to the hypothesis being formulated, but that would not be sufficient to be convincing. Here, there are several concordant clues, as there is a multi-method analysis of a possible combustion zone and its context, allowing a convincing interpretation of this feature as the remains of an anthropic fireplace. The presence of pyrite (whose geological occurrence is shown to be very rare in this precise context) is the cherry on the cake, and even allows the authors to put forward the idea of a capacity for fire ignition. Beyond this “oldest case of” aspect, it's also a fine article, easy to read and follow, presenting a rigorous and effective combination of complementary multi-method studies, drawing on robust experimental archaeology reference frames. This type of approach is an example to follow for pyroarchaeological analyses, but more generally for pushing back the limits currently in the way of our understanding of the past. For all these reasons, I recommend publication of this article in Nature, after some minor modifications and clarifications listed in the specific comments below.

Specific comments
Page 1
l.28-30: indicate you are presenting things bottom to top from the beginning (the numbering is in chronological order and not in order of discovery during excavation).

Now explained.

l.44: units 4, 5c and 5e: is it an ensemble that is culturally coherent over 3 layers? The 3 layers are all interglacial sediments. Are they formed at +/- the same time (and result in 3 layers because of pedological processes) or are they 3 different layers deposited in different conditions over 3 different periods during the interglacial period?

These are formed in part concurrently with the processes now explained more clearly in the text.

Page 2

l.46-47. According to this wording (‘respectively’) we understand that 4+5c+5e correspond to Early temperate (pollen zone Holl), and that level 6 corresponds to the peak interglacial (pollen zone Holl/III) of the Hoxnian. (if it’s not that, change the text to clarify, if it is right, then it’s ok :-)).

That is correct

To avoid any doubt please add a figure of the stratigraphy (it can be a simple log or schematic drawing) synthetizing all the information (lithostratigraphy, biostratigraphy and geochronology) from ref 1 to 4, with all the levels (at least 1 to 6, and especially relation between 4, 5c, 5e, and 6). Indeed, the chronological context is very important since the paper is about the earliest evidence of fire making. In this regard fig. 2.2 in Supplementary online Information (SI) is not enough.

There is more detail in the main text on dating with a new schematic cross-section through the site to help explain this. There is also a new section on stratigraphy and chronology in the SI.

Page 3

l.90: “pyrogenic origin.” -> add at the end of this sentence “(as opposed to control samples)” for more clarity.

This has been added.

l.93: “extensive” -> here (and therefore in the rest of the text) it is not clear what “extensive” means. Is it extensive in time or in space? Or both?

We changed ‘extensive’ to ‘multiple heating events of varying durations’

l.104-105: “…which suggests localized fires were related to the regional wildfire regime in this deposit.” This sentence brings confusion, since it is said before that magnetism does not show localized fire area in this level… Please clarify.

Hydrocarbons provide a different way of determining heating compared to magnetics, and therefore the lack of a clear signal for heating in the yellowish brown clayey silt by the magnetics does not preclude it being picked up by other methods.

Page 4

l.140-141: it is said “and neither shows evidence of use”.

We’re not quite clear whether this comment needs addressing.

l. 152: it is said “Despite the Breckland having Chalk Bedrock, pyrite is very rare.”
It is very interesting that the authors have been able to mobilize these results from independent studies!

Although there are many faint indications of direct evidence (magnetism, PAH, FTIR) and indirect evidence (pyrite with no obvious trace of use, but rare naturally) of anthropogenic fire-use in this study, the authors brilliantly manage to show a coherent picture by presenting them all together. The elements are convincingly presented in the text.

l.159: please insert the bibliographical references of the lithological studies used here and not only in SI.

Unfortunately, this would add seven references to the paper, exceeding the 50 reference limit.

Page 5

l.182-183: the sentence is strangely worded, given that the oldest traces of fire are in Africa. I do not think this is what the authors mean, but because of diffusionist models (often mobilized in archaeology…) one might think that the authors are implying that the use of fire, and perhaps also its mastery, spread from England and Europe to Africa… As the article concentrates on Europe, perhaps it should be limited to that and not mention the Levant and Africa? or else indicate the oldest traces on continents other than Europe before this sentence.

This section has now been moved to the introduction at the suggestion of Reviewer #2 and slightly rephrased.

l.183: “habitual use” might need to be defined. It can imply different things according to the authors.

We changed this to ‘regular use’.

l.188: reference 22 and l.197: reference 32. Maybe also site Perles 1977 that was stating these ideas much earlier than the references here listed.

The reference has been added.

l.201-203: long term conservation of fire is also possible (even if logistically demanding).

This is discussed as 'costs of maintenance' in previous paragraph.

l.204-206: yes, it is a strong possibility (even if some authors stated fire is not necessary -> see the publications criticizing this cooking hypothesis – possibility of digestibility of meat without fire through fermentation, etc.). But there is cooking evidence in the Lavant 780ky ago… so it is also a good argument to state that use of fire is important once it is mastered (an indirect evidence of fire mastery the authors might want to add/discuss?).

We think suggesting cooking as a strong possibility is probably sufficient for this paper, as not all researchers accept the evidence from the Levant at 780ka.

l.207-210: yes, it is strong indirect evidence of habitual use and control of fire, since it is necessary part of the technological repertoire as a tool to produce heat-synthesized adhesives.

l.211: change "indicates" by "strongly suggests". If the presence of pyrite reinforces the interpretation of the traces of fire as anthropogenic (the most plausible solid hypothesis given all the elements presented in the article), then to interpret it as solid proof (without obvious traces of use or without a lighter with traces of use as in ref.19) is perhaps a little abusive. It is just a question of framing.

This has been changed.

l. 214: locate the references accordingly in text. -> "… including bones43 and wood44 working, and the creation of adhesives for hafting45." (rather than putting them all in the same package at the end of the sentence.

This is now changed.

Figures

Figure 1: great figure. But cite in caption (or in SI) the sources of these data. Maybe a synthetic table in the SI?

Source data for the geology has been added.

Add a complementary figure with stratigraphy (schematic drawing or log is enough, but the reader needs a synthetic figure with chronostratigraphic elements and Archaeological levels presented in the text). See previous comments.

A synthetic figure with a cross-section through the site has been added in the SI, which includes chronological data.

Figure 2: great figure also. Make sure that people are ok and happy to appear on the picture C -> I like it better than blurred faces, and not anonymizing the excavators is always better! But they have to agree to appear there ϑ I did not check in the forms provided with the paper if something was already written about this…

Yes, we have checked with all individuals

Figure 3: it is really a great synthetic figure, very clear. For even more clarity, add on y.b.c.s. that it has to be seen as the parent mineral matrix in the table.

It is interesting to see that there is always a regional fire record. But it would be great to have a "control layer" for PAH analysis without this signal (maybe in colder layers of the site where it is less likely that natural fires occur?) just to be sure that there is no false signal / problem of tuning or contamination during the analyses…

Unfortunately, there are no 'cold' layers that would be suitable for PAH.

Also, why is there no FTIR data for b.s.?

No duplicate sample of b.s. was taken for FTIR. Sample 1063 was on the y.b.c.s. and b.s. boundary

And for FTIR also it is weird to have always a heating signal (since it is not what is stated in the SI).

This is not actually the case, but it has now been clarified in the relevant figure (now Figure 3).

Methods

They are clearly presented in main text (enough to understand) with more precisions in SI.

Page 12

l.367: Environmental magnetism (the E needs to be in Bold font like the rest).

Done

l.390: delete coma (,) after the brackets before at 4.7 kHz.

Done

Supplementary information

Page 6 – Micromorphology

“Such reddened, cemented sediment lenses have been reported and suggested to present early evidence of fire control.”

The sentence could be understood as the presence of red lenses indicate early evidence (and not recent ones that would appear differently?) but it is not indicative of the age. It’s not what the authors meant but the framing should be change so the reader does not understand it wrong. For example “Such reddened, cemented sediment lenses have been reported; they are susceptible to be better preserved over time and therefore provide early examples of fire control”?

The sentence has been amended.

Page 8

Figure 3.2: the shapes of the squares in A do not correspond to the pictures presented in B to F, and there is two D but no E. Also, check spelling in the caption (Micromorphological – R is missing).

This has been corrected.

Page 9

Please de-group references 21 to 23 (like remark for line 214 in main text).

They are now de-grouped.

Page 10

L1: be consistent with use of – or - and spacing.

Done

Table 4.1 last square: delete second space in front of magnetotatic

Done

4.3. experimental design -> here we find the use of “extensive” again with no precision. It is not clear what it means.

Changed ‘more extensive’ to ‘longer’. The time-lengths considered are given in the experiments.

Last line: “successively 1 to 4 times dependent on the results”. It is not clear what is the expected result justifying to do it just once?

Changed to “Each experiment was then repeated successively (4 repeats for the 4 and 12 hour heating times and 2 repeats for the 24 and 48 hour heating times. The lower number for the longer heating times was due to inversion to haematite on the second experiment which is not representative of the mineralogy of the archaeological samples)”

Page 11

“Table 3.2” -> change for Table 4.2. Please check all the figures and tables labelling.

Done

Page 12

Figure 4.1. Caption: change “experiment one” for “experiment 1” like in the rest of the text.

Done

Table 4.2. Add a column on the left indicating that 1056 and 1055 are yellowish brown clayey And 1054, 1053 and 1100 are reddened clayey silt.

The sediment name has been added to column 1.

Last line: “of the unaltered substate” -> substrate. + needs to be specified with more clarity that it refers to the yellowish-brown clayey silt (if I understood well).

The sentence has been amended.

Page 15

Experiment 4: It is very interesting to show these changes in mineralogy and how to achieve to produce them experimentally.

The summary tables are informative but there is a need to have a figure to show these changes in experiment 4, like in figure 4.1 (which is great), this time not according to Temperature but to repetitions.

Done

The summary is super interesting and clear. It is a lot of work!

Page 16

Maybe add a “Mineralogy” column for better synthesis of what is presented in the text.

This refers to what is now Table 5.3, where the experiment is all undertaken on the yellowish brown sandy silt, so it is not quite clear how a mineralogy column could be added.

Page 17

Table 4.4: 1st square: missing information. I guess it should be “4-Hours experiments” (and not “Hour experiment”). Also 12-Hours (and not 12-Hour).

Done

A complementary graph including mineralogy info (like fig.4.1) would be great.

Figure 5.2 has been added.

Page 18

9 samples collected by who? Since it is specified for the 2 control samples… Say who does what all along or only in “authors contribution” section.

Added RD.

Also, please indicate the size and weight of samples taken and what is needed to conduct this analysis.

The weight is given at the start of the methods section.

l.45: “mixed with recovery standards” -> in order to….?

Changed “to measure and quantify the efficiency of analytical methods”

Page 19

Suppress indents in table 5.1. for Unit 6 line.

Done

l.72: “coal” -> charcoal? Or mineral coal?

It should be coal.

Page 20

Table 5.3: indicate the interpretation of the results in last column of the table (add “interpretation” column on the right). Also add a column on the left with context (Barnham units and type of sediment -> to help better follow and jump between table and text).

These columns have been added.

l.91: r, l.97: capital R

Changed to r.

Page 21

Figure 5.2. Please indicate all the numbers of the other samples (without circling them).

Done

Page 22

l.138-139: climate melioration with rise of Temperatures? Compatible with rise of lighting frequency? (as discussed in Sorensen 2017 / Dibble et al 2018).

Good idea. Added this as a possibility, or potentially increased anthropogenic increase in fire use regionally.

Great demonstration of on-site anthropogenic fire by combining magnetism and mineralogy, and PAH analyses. Presence of pyrite (rare in this geological context) also strongly suggest ignition capacities 400ky ago.

6. FTIR

l.149: “sample 1084 contains the same mineralogy as samples from the purported fire feature” -> That’s weird since archaeomagnetism experiments and study just showed a change in mineralogy between unheated and heated sediment. I guess the use of “mineralogy” term differs a bit in this section. Please precise what you mean and at which scale… otherwise it looks contradictory.

Text is now clarified

Page 23

Figure 6.1. Samples 1059, 1061, 1068: how were they selected among the 9?

Their inclusion in the figure was to show the presence of the same unaltered peaks as in S1084. The figure caption has been clarified.

Last sentence of the caption -> they all appear unheated. It contradicts main text and figure 3 where they are labelled as heated (<400°C) and not unheated (<300°C). Please clarify.

Figure 3 in the main paper has now been clarified as per comment in revised paper

l.169-170. Please precise why you do stuff and not only what you do (especially when you do not refer to a published protocol explaining the whys of the hows).

These are widely used, standard procedures, so added this and a reference.

Page 24

l.191. Why are the 9 samples taken not all presented in figure 6.1.?

See above comment

Page 26. Put the text paragraph after the remaining figures, just before the references.

Done

l.234: suppress the URL and replace by citing reference 66.

Done

At the end of the paragraph please add a sentence for comparison with archaeomagnetic data -> consistent results of the 2 methods regarding T°C?

A sentence has been added.

Figure 6.3: tell in caption why discontinuity and dashed line in B).

Figure re-done and caption clarified

Page 27

Figure 6.3 should be 6.4.

Done

Also, a figure 6.5. comparing the archaeological samples AND the corresponding reference spectra is needed to illustrate what is stated in last text paragraph.

This has now been added.

Response to referees

Referee #1 (Remarks to the Author):

Minor edits:

Line 54 – rephrase to "Barnham – the site presented here – suggests that…"

Amended as suggested

Line 128 – rephrase to "could have arisen from either one or multiple heating events"

Amended as suggested

Line 187 – "indicating longer exposure above ground." I think that this statement is important to clarify further. Here, the authors are reporting evidence that the pyrite oxidized in place (at Barnham), which would of course be a necessary step in the formation processes if the pyrite had been collected in its original state. The authors have now clearly established that that pyrite is oxidized to goethite, AND that the oxidization occurred post-depositionally within the site, which means that it was composed of pyrite when it was brought there. This is significant, and also necessary for the interpretation of the use of the pieces as fire strikers.

Unfortunately, it is unclear when the pyrite oxidised, so this statement, although likely, cannot be stated categorically.

Supplemental materials

Fig 2.4 caption – should be pseudomorphic?

This has been changed.

Micromorphology section – add scale bars to the images in the figures, or specify the height of each block in the captions.

The dimensions have been stated in the captions.

Page 15 – in two places, please change to "open porphyric related distribution."

These have been changed.

Avoid referring to void infillings as "illuviation." Describe them as clay coatings or infillings depending on their degree of development (e.g. limpid, crescent clay infillings) and then interpret them as resulting from illuviation.

This has been changed.

"Iron staining related to voids" – be more clear about this. Are they iron coatings, hypocoatings…etc?

This has been clarified.

"A few channels are apparent and dark brown amorphous staining." Something is missing in this sentence. Where is the dark brown amorphous staining with respect to the channels?

This sentence has been removed as it is unclear.

"Iron nodules are also present and some exhibit a reddish hue indicating haematite formation (Fig. 3.6D)." I think this should be Fig. 4.6D. Also consider adding a few additional terms to describe the nodules, such as orthic (are they formed in place?) and concentric.

This has been amended.

Figure 4.6F – This looks very strange for a charred root, but I am not opposed to including it in the publication. Perhaps rephrase as "charred tissue within a void, interpreted as a possible charred root."

This has been amended as suggested.

"The reddening occurs only within void spaces and is related to microfacies not the matrix." Unclear what is meant here by microfacies.

This has been changed to "not related to the matrix".

"Microphotograph of a root within a channel from the top of the palaeosol providing evidence of plant growth, roots are not well-preserved within this thin section, but this may be a more recent feature in PPL." Break into two sentences and add comma before "in PPL."

This has been amended.

Referee #3 (Remarks to the Author):

Minor specific comments:

Abstract:

Line 24: Delete « in later prehistory ». Pyrite is used also in historical times to strike sparks.

This has been changed to 'from later prehistory' which includes later periods.

Also: I am not sure why the accent is put only on the cooking hypothesis at the end of the abstract. As the authors say later in the text, it is probably just a part of hominization drivers, with social brain hypothesis. Fire is a powerful driver of hominization, not only through cooking but also through the social restructuring that it enables (and requires). Maybe rephrase to broaden the impact of this study.

We have added '…provided significant social and adaptive benefits.' The social aspects are also mentioned near the beginning of the abstract.

Line 38: I am not sure what the authors mean. Write plural -> « However, these traces/elements are often difficult to interpret. » Or if it refers only to the last case, please rephrase to avoid ambiguity and elaborate?

We have now made it clear that it refers to all these cases of early fire.

Lines 47-55: NB: There is also possibly the site of Schöningen (13 II-4) with fire traces (heated flint fragments) dating from MIS11-9. It indicates fire but not if it is natural or anthropogenic though.

Recent dating of Schöningen places it in MIS 9, or possibly MIS 7 (Hutson et al. 2025) and is therefore outside the main timeframe considered by this paper.

Line 54. Repeated use of fire in Orgnac 3 site (cf. Vandevelde et al. 2025) demonstrated from the repetition of soot traces in a speleothem is also a good example for this 350-200ka period. It exhibits how difficult it is to discuss this question of repeated use / mastery of fire for remote periods, and that several alternative research strategies must be implemented regarding the contexts. And therefore, it shows how exceptional this study in Barnham is for such a distant period!

We agree, but we have chosen not to include Orgnac 3 as it dates from c. 270 ka, so is also outside the timeframe considered by this paper.

Line 73: missing dot at the end of the sentence.

Thank you. Now added.

Line 162: "In summary," could be in bold and italics so the reader can easily see that this summary concerns all the previous sub-sections and not only the FTIR analyses.

Good idea. We have added a subheading 'Summary' in bold.

Line 192: Pyrite is also used and recovered in much more recent periods such as the Middle Ages for example, or latter in XVI-XVII th century for early riffles. It is not limited to prehistory and I think it shows how fundamental the use of pyrite is; it has been part of the fire tool kit throughout the ages!

This has been changed to '…from the late Middle Palaeolithic to the historic periods.'

Line 252: why this c. 500-300 ka period? Before in text it is always the 400-300ka period that is mentioned regarding fire… Why here put the early limit 100,000 years before Barnham? The reference 39 refers only to the increase of brain size, not the "significant transition in human behaviour from c.500-300ka".

Changes in human behaviour are usually referenced to the period of 500-300 ka, whereas use of fire is much better documented to between 400-300 ka. There may well be earlier evidence of fire-use and making, but we have yet to find it.

Line 254: for ca.500-300ka palaeoanthropologists do not refer only to "early neanderthals". Pre-Neanderthals and Homo heidelbergensis also must be considered in western Eurasia. It might be better to use a broader / more cautious wording. But if you have paleoanthropological elements in Barnham to refer to "Early Neanderthals" that is great, but it should be presented at least in SI.

This has been changed to '…certainly involved early Neanderthals and other lineages of contemporaneous humans in western Eurasia, it is likely that…'.

Figure 1: Area V appearing in figure 2 is missing. Could you please add it here? If Area V should not appear in fig.1, maybe add a note in caption to explain where it is regarding the areas represented or refer to SI fig.4.1 where it appears.

Area V has now been added to the figure.