Reply to RC1:

We thank the reviewer for the positive and constructive feedback given on our manuscript. The answers follow below:

General Comments

The authours present an interesting test of the intermediate productivity aridity hypothesis. I quite like the combined use of clay / phytoclasts / charcoal as multiple lines of evidence to address their objectives. Overall, I enjoyed the reading manuscript and was interested in the authours' findings. Thank you to the authours for sharing!

Their link with grasses is interesting though potentially not hugely important for the conclusions. It is evident from your results that sufficient fuels existed to support fire, and I assume that some analogous fuel existed that served a similar role then that grass does today. I was a bit disappointed not to see a stronger link with grasses as a specific pyrophylic biome component and fuel, as it is one of the pieces in the abstract that made me interested to read more. I may be missing an important point, and if I am I encourage you to add more information in your introduction to set readers like me up to understand your point about grasses. Perhaps it is that biomes that contain grass serve as the basis for the intermediate productivity hypothesis and without them the hypothesis should fall apart. Currently it reads that grasses are important for current global fire patterns, and then there is not much direct follow-up. Please clarify this argument.

Thank you for pointing this out. In the introduction we explain that the current global fire pattern is dominated by grasslands, such as tropical savannah systems. This is an ecosystem in which intermediate levels of productivity and aridity occur and fire activity is high. Because >80% of the burnt area globally in the present day occurs in grasslands, grasslands reinforce the generalization of the intermediate productivity gradient. However, the intermediate-productivity gradient explains optimum fire conditions along an aridity – productivity gradient in which biomass, fuel moisture and temperature are important factors of the fire regime. Hence, independent of the type of vegetation the intermediate-productivity gradient should apply. In this study we test that for the Early Jurassic world, in which grasses were not yet evolved. Vegetation/fuel during the Early Jurassic in NW Europe was a mixture of gymnosperms, cycads, horsetails, ferns and mosses.

We have clarified the above point and added information and discussion on vegetation in the manuscript.

Added to the conclusion: "This study illustrates that the intermediate-productivity gradient holds up during two contrasting climatic states in the Jurassic at a time well before grass Savannah existed."

L93: added "Although the intermediate-productivity gradient hypothesis of the present day is strongly linked to the expanse of grassland habitats, it should not require the presence of grasses to explain the impact of climate and seasonality on fire frequency in other vegetation types. The crucial concept is that an optimum fire window exists when there is a sufficiently moist season that allows fuel growth which is followed by a drier season in which fuel moisture levels are lowered, allowing ignition and fire spread."

L568 to L621 added: “The studied Early Jurassic time-interval likely had five distinct biomes; a seasonal dry (summerwet or subtropical) biome in the low latitudes, a desert biome in the subtropics, narrow latitudinal bands of a winterwet biome at low-mid latitude, and warm temperate and cool temperate biomes at mid- and high-latitudes, respectively (Rees et al., 2000; Willes and McElwain, 2014). The Cardigan Bay Basin was likely positioned within the winterwet biome at approximately 35 °N (Torsvik et al., 2017). It therefore would have sat within the bounds of the fire window of the intermediate fire-productivity hypothesis (Fig. 5). The winterwet biome in both the Sinemurian and Pliensbachian stages were dominated by conifers as the canopy tree, with a mid-canopy vegetation of cycads and tree-ferns, and an understory mixture of seed ferns, horsetails and ferns that likely flourished during wetter periods (Rees et al., 2000; Slater et al., 2019; Bos et al., 2023). This is evidenced from sporomorph data from the Mochras borehole that hosts abundant fossil pollen in the Sinemurian and Pliensbachian (>94%) (Van de Schootbrugge et al., 2005). Additionally, nearby locations also show evidence of orbitally paced shifts in vegetation assemblages from sites at St. Audries Bay, UK and in NW Germany (Bonis et al., 2010; Bos et al., 2023). During the 100 kyr eccentricity maxima in the UK pollen from the dry-adapted cheirolepidacean conifers is found to be highly abundant (Bonis et al., 2010). Whilst, in Germany a mire-conifer community is apparent with sporomorphs indicating variations in abundance of ferns and fern allies occurring over a 405-kyr eccentricity cycle, with ferns most abundant during eccentricity maxima (Bos et al., 2023).

Dry-adapted vegetation, such as the cheirolepidacean conifers likely thrived during more extreme seasonal droughts, maintaining their biomass. In contrast, ferns and fern allies, and mire-conifers as humid-loving plants would grow rapidly during sustained, year-round, periods of rainfall (eccentricity minima), likely inhabiting both open environments and colonising the understory of conifer forests. Furthermore, they would also be able to build dense connected fuel loads during the wet-season of eccentricity maxima, that were then easily dried during the annual dry-season. Ferns, when cured, carry high intensity fires (Adie et al., 2011; Belcher and Hudspith, 2016) and during the Mesozoic ‘fern prairies’ have been linked to intensive surface fires (Harris, 1981; Van Konijnenburg-Van Cittert, 2002; Collinson et al., 2007, 2009). Hence, they are suggested to have functioned in a similar fashion to support fires as grasslands and fern stands do today; Mesozoic fern prairies and savannahs therefore likely filled a similar ecological niche to grasses in the modern day (Belcher, 2013 and references therein). Ferns are indeed a common feature of Mesozoic charcoal assemblages, showing their association with fire throughout time (e.g. Collinson et al., 2000; Brown et al., 2012).

In the present-day, temperature is an important regulator of fire occurrence. Whilst dead fuel moisture (e.g. that of litter and cured herbaceous components) is primarily influenced by the variability in relative humidity, live fuels are controlled by the combination of temperature and moisture availability, where long periods of drought or heat wave extremes can strongly influence the flammability of live fuels. Sea surface temperatures (TEX86) during the Sinemurian and Pliensbachian were at times apparently higher than 28 °C (Robinson et al., 2017). But high-resolution temperature reconstructions are lacking for the Early Jurassic. Orbital forcing of regional–global seawater temperatures occurred throughout the Cenozoic (Westerhold et al., 2020), and likely also the Mesozoic; however, the climate response to changes in orbital insolation is non-linear, and the mean annual insolation is not impacted by precession (Rubicam, 1994). Therefore, the biomes of the SPB not only existed in an overall warm world that was characterized by background orbitally driven climate shifts across the moister side of the fire-productivity gradient, but superimposed on this live fuels were also responsive to extreme weather linked to periods of drought and heat."

You discuss charcoal largely in terms of overall abundance. Given that you consider fine and coarse charcoal, would it be possible to do any sort of discussion on fire intensity? E.g., greater coarse charcoal has been linked to larger more intense fire activity that generated sufficient convective energy to distribute larger particles. My experience is more with lakes and this may not translate to your system. But if it is possible it might be an interesting addition to discussion or future work. Perhaps you will see more or less intensity along the productivity-aridity gradient?

Thank you for the suggestion about the link of charcoal size and fire intensity. It would be interesting if we could say anything about fire intensity based on this dataset, but unfortunately microcharcoal (10-125 µm) and macrocharcoal (>125 µm) in the marine Jurassic borehole cannot be used for this. For this geological location charcoal is derived from the surrounding emergent landmasses, either by wind or river, and likely further influenced by both shallow and deep marine currents.

Larger *in situ* charcoal particles are generally found in terrestrial biomes and their depositional environments, in soils, lakes and mires. In contrast, smaller charcoal particles that are wind-blown could potentially end up in a marine environment, as well as in more distal terrestrial settings. Experimental research showed that riverine transport has the potential to carry the larger charcoal particles further away from shore, with the smaller charcoal particles becoming water saturated at a shorter distance and settling down closer to the shoreline (Nichols et al., 2000). In addition to this, other studies have indicated that larger charcoal particles (up to 7 cm) can be windblown and travel up to 50 km from the original source, depending mainly on their morphology (Woodward & Haines, 2020). Combined, charcoal size, shape, properties, wind direction, plume height, but also riverine and marine transportation, all have a different impact on the travel distance of different charcoal size classes. Hence, it too difficult to construct any hypothesis on fire intensity from our two charcoal size classes in the Mochras borehole.

We have added in a paragraph on the provenance setting of the Mochras borehole for a better understanding.

Added in L180 to L202: "The Welsh Massif was likely the main detrital source to the Cardigan Bay Basin (Deconinck et al., 2019), although other emergent areas in proximity likely also contributed (Deconinck et al., 2019). The nearby Irish Massif, situated west of the Welsh Massif, also cannot be dismissed as a source of nutrients, terrestrial organic particles, clay and coarser mineral grains to the Cardigan Bay Basin (Deconinck et al., 2019). Another possible source area is the emergent land of the Scottish Massif to the north of the Mochras Borehole and the London-Brabant Massif to the east of the Mochras Borehole (van de Schootbrugge et al., 2005). The multiple nearby landmasses contributing runoff to the here studied relatively deeper marine depositional environment, allow for the charcoal record presented in this study to reflect a regional expression of fire activity, likely of multiple fires from the surrounding emergent landmasses. It is important to note that one stratigraphic rock sample in this study represents a ~2 kyr average signal, which likely is more than the fire return interval at this time. And hence represents an averaging of the overall fire signal. Therefore, the term 'fire activity' here describes the overall occurrences as increases and decreases in wildfires across the region."

Specific Comments

The methods are generally intuitive as written. I had one major point of confusion: the number of samples taken and used for each analysis in each period was unclear. I suggest that you make a table showing these numbers explicitly. It would support the methods and support the reader in interpreting your results from SBP and LPE, which had different resolutions.

Thank you for the feedback. We have incorporated a table (SI Table 1) with an overview of the samples per interval and proxy for clarity.

The results are fair as written. I have three suggestions: 1) I find it difficult to follow and be confident in your conclusions about terrestrial phytoclasts and charcoal particles given visual analysis alone. I see the importance for your conclusions that charcoal not be related to terrestrial inputs. I suggest that you demonstrate this relationship (or non relationship as you suggest) by some formal statistical test, perhaps a Mann-Kendall test.

We have used a Pearson's correlation in our study to illustrate that there is no statistical evidence that terrestrial organic matter (phytoclasts) and charcoal abundance correlate. A very weak correlation for microcharcoal and phytoclasts for the Late Pliensbachian Event (LPE) exists, and no correlation for microcharcoal and phytoclasts for the Sinemurian–Pliensbachian Boundary (SPB) interval. No correlation between macrocharcoal and phytoclasts is found in either interval. We have added the Pearson correlation in this revised version of the MS into the methods section. In addition we have incorporated the scatter plots with the r and p values of the Pearson's correlation in the SI (SI Fig. 3).

As extra evidence that there is no influence from terrestrial runoff to the charcoal record, we normalized the microcharcoal for the LPE interval with XRF terrestrial elemental data, following the method of Daniau et al. (2013) in Hollaar et al. (2023). Here we show that XRF total terrestrial elemental corrections do not change the charcoal pattern found in the LPE interval. Also, the overall abundance of wood is higher during the LPE in the Mochras borehole (Ullmann et al., 2021), whereas the charcoal abundance is overall higher during the SPB (L299-307).

We have looked into a Mann-Kendall test, however, this will test the hypothesis that a time series has a trend. It is unclear for us how this test can help to statistically underpin that the trend within two proxies/time series is the same or not. In addition, even though a trend in two data series can be similar, this does not mean that the individual sample points correlate. The latter is the important factor regarding the influence of the abundance of terrestrial phytoclasts and charcoal, as you want to know if individual spikes in charcoal correlate to highs in phytoclasts. For this purpose, a Pearson's correlation is sufficient.

2) I suspect that Fig 4 is unnecessary, and I suggest that you remove it given that you do not refer to it in text (I checked with a search) and one could reasonably be expected to understand these distributions from Fig 2/3. If you want to keep Fig 4 I suggest you expand your discussion of micro- vs macro-charcoal partitioning and how that may be associated with fire intensity (which I think would be very interesting but may not be within your intended scope).

Figure 4 is referenced in L231: “Micro- and macro-charcoal are more abundant in the SPB compared to the LPE (Fig. 4).” The reason that this boxplot is included is to illustrate the higher charcoal abundance (in both size fractions) for the SPB compared to the LPE interval. This is an important argument for the LPE to be more fire-limited and closer to the aridity edge.

3) Fig 1 is difficult to read given its current size. I suggest that you either stack panel d below the other panels to allow all to be larger, or rotate the table to allow it to be larger.

Thank you for the feedback, we have moved panel D to the bottom in the revised MS. We have also added in panel E.

4) Figure 1 and 5 do not work well with black/white printing or for folks who struggle to differentiate colours. Consider differentiating with shape or texture rather than colour.

Thank you we have revisited the layout of figure 1 and figure 5 and comply with the Copernicus article template illustrations to make the graphs colourblind friendly. Figure 1 has been enlarged (by moving panel d and e to the bottom) for better visibility. In addition, we have labelled the pale red and blue intervals with SPB and LPE to support identification.
In figure 5 we have enlarged the three images for better visibility and we have added an outline around the LPE stars and not the SPB stars, so next to the colour difference there is a texture difference as well.

Technical Corrections

1. Please include the methods you used to generate SI Fig 3 in methods.

Thank you, we have added “In SI Fig. 2 we overlay the 3.2 – 10 m filter (based on Ruhl et al., 2016) derived from the macrocharcoal record with the normalized dataset of the macrocharcoal record.“

2. The caption on SI Fig 3 is confusing, please edit for clarity.

We have changed the caption of SI Fig 3 (now SI Fig. 2): “SI Fig. 2: Macrocharcoal and the 10.2 – 3.2 m filter. (a) The macrocharcoal record (blue) of the LPE interval is linear detrended and the 10.2 – 3.2 m period is filtered out of the macrocharcoal record in Acycle. This filter represents the 100 kyr periodicity in the depth domain (Ruhl et al., 2016). The number of peaks corresponds to the number of short eccentricity cycles in the studied interval found by Ruhl et al. (2016) and do capture the ~5 m bundles observed in the macrocharcoal record. (b) The macrocharcoal record of the SPB is linear detrended (blue). The 10.2 – 3.2 m signal (orange) is filtered from the macrocharcoal record. The individual peaks capture the ~5 m peaks in macrocharcoal observed in this record. Also, nine peaks are observed, which is in agreement with Ruhl et al. (2016) who found nine 100 kyr eccentricity cycles for the same interval.”

Please see attached highlights/comments

1. L64: concept has been changed to hypothesis.
2. L67: ingredients has been changed to determinants.
3. L104-105: "… allows for time constraints." Has been replaced by "provides an age model for the Mochras borehole.".
   The point here is to inform the reader that the borehole our study is using, has already been astronomically constrained by 3 independent studies. Hence, we have a good age/depth model.
4. Panel e has been included in Fig. 1.
5. L130: Sinemurian – Pliensbachian and Late Pliensbachian interval (preference).
6. L130 double barrels has been adapted.
7. L177 double barrels has been adapted.
8. L324-325 ref to SI Fig. 8.
9. L335-336: reference to Fig. 2/3 has been included.
10. Derive/deduce (preference).
11. L420 changed "long" to "405".
12. L422: ref. to Fig. 5 has been included.
13. L454: "period" has been added.
14. L462: ignition "and sustained fire spread" has been added.

References:

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Hollaar, T. P., Hesselbo, S. P., Deconinck, J. F., Damaschke, M., Ullmann, C. V., Jiang, M., & Belcher, C. M. (2022). Environmental changes during the onset of the Late Pliensbachian Event (Early Jurassic) in the Mochras Borehole, Cardigan Bay Basin, NW Wales. *Climate of the Past Discussions*, *2022*, 1-28.

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Collinson, M. E., Steart, D. C., Scott, A. C., Glasspool, I. J., & Hooker, J. J. (2007). Episodic fire, runoff and deposition at the Palaeocene–Eocene boundary. *Journal of the Geological Society*, *164*(1), 87-97.

Collinson, M. E., Steart, D. C., Harrington, G. J., Hooker, J. J., Scott, A. C., Allen, L. O., ... & Gibbons, S. J. (2009). Palynological evidence of vegetation dynamics in response to palaeoenvironmental change across the onset of the Paleocene-Eocene Thermal Maximum at Cobham, Southern England. *Grana*, *48*(1), 38-66.

Nichols, G. J., Cripps, J. A., Collinson, M. E., & Scott, A. C. (2000). Experiments in waterlogging and sedimentology of charcoal: results and implications. *Palaeogeography, Palaeoclimatology, Palaeoecology*, *164*(1 – 4), 43 – 56.

Ullmann, C. V., Szȕcs, D., Jiang, M., Hudson, A. J., & Hesselbo, S. P. (2022). Geochemistry of macrofossil, bulk rock and secondary calcite in the Early Jurassic strata of the Llanbedr (Mochras Farm) drill core, Cardigan Bay Basin, Wales, UK. *Journal of the Geological Society*, *179*(1), jgs2021-018.

Woodward, C., & Haines, H. A. (2020). Unprecedented long-distance transport of macroscopic charcoal from a large, intense forest fire in eastern Australia: Implications for fire history reconstruction. *The Holocene*, *30*(7), 947 – 952.

Reply to RC2:

We thank the reviewer for the positive and constructive feedback given on our manuscript. The point-to-point replies follow below:

First, what attributes of fire are being recorded here? It's long been a goal of what we might call "Quaternary paleofire" studies to separate the effects of fire frequency and fire magnitude, including severity and area burned, but there seems to be little consensus there, any many studies simply fall back to using "fire activity" as a not totally ambiguous descriptor. In high (cm-scale) resolution lake records, peaks in charcoal are generally thought of as individual fires within the catchment of a lake, distinct from background levels related to extra-local fires and the general level of biomass burning in a region, with the magnitude of the peaks providing some kind of index of fire severity. The record here probably represents more of a regional index, which in Quaternary studies are often shown as smooth composite curves constructed using multiple records in a region, with the composite curve usually interpreted as a measure of area burned. It would be good to discuss a little what particular attributes of fire the charcoal represents (i.e. not individual fires, more likely regional biomass-burning levels), and to explicitly state what is meant by the term "fire activity". (More discussion can be found in Marlon, 2020, Quaternary Research doi:10.1017/qua.2020.48.)

Yes, thank you for your feedback, we have added some information in our manuscript on what the charcoal represents in this study's context and state what we therefore define as fire activity. The charcoal records in this study represent a regional expression, likely of multiple fires from nearby emergent landmasses. One processed rock sample in this study represents ~2 kyr (and is thus very likely more than the fire return interval) and therefore represents an averaging of the overall signal. In addition, the geological setting is a marine setting that is below storm wave base, but in close proximity to emergent landmasses. This indicates that wind, riverine runoff and ocean currents all impact transport to the depositional site. Multiple landmasses surrounding the Cardigan Bay Basin likely are the source of terrestrial input to the basin and influenced the charcoal content. Hence, the charcoal abundance records presented here represent a regional expression in the Cardigan Bay Basin of burning on the nearby emergent landmasses. For a Mesozoic study, this sample resolution is relatively high and the time constraint is also as exact as it can be (except for the unique case of a 200 Myr old varved lake deposit in Falcon-Lang, 2000). This is different from Quaternary studies that are often carried out on lake sediments and can therefore infer more about the fire frequency. The use of fire activity in this MS is defined as the 'occurrences of wildfires in the region'.

In the revised manuscript we have added in two paragraphs that discuss the fire signal presented in this manuscript and a definition of fire activity in this context.

L152-162: ”The Welsh Massif was likely the main detrital source to the Cardigan Bay Basin (Deconinck et al., 2019), although other emergent areas in proximity likely also contributed (Deconinck et al., 2019). The nearby Irish Massif, situated west of the Welsh Massif, also cannot be dismissed as a source of nutrients, terrestrial organic particles, clay and coarser mineral grains to the Cardigan Bay Basin (Deconinck et al., 2019). Another possible source area is the emergent land of the Scottish Massif to the north of the Mochras Borehole and the London-Brabant Massif to the east of the Mochras Borehole (van de Schootbrugge et al., 2005).
The multiple nearby landmasses contributing runoff to the here studied relatively deeper marine depositional environment, allow for the charcoal record presented in this study to reflect a regional expression of fire activity, likely of multiple fires from the surrounding emergent landmasses. It is important to note that one stratigraphic rock sample in this study represents a ~2 kyr average signal, which likely is more than the fire return interval at this time. And hence represents an averaging of the overall fire signal. Therefore, the term 'fire activity' here describes the overall occurrences as increases and decreases in wildfires across the region.”

Second, the “intermediate-productivity gradient hypothesis” of Pausas and Bradstock (2007) was originally proposed and tested in an environment where vegetation productivity was clearly and solely linked to the moisture gradient. Pausas and Ribeiro's (2013) extension of the idea to the globe, while still focused on productivity as represented by NPP, relates NPP to temperature, and Daniau et al. (2012) show that fire activity, in both charcoal records from the LGM to present, and in satellite remote-sensing data, depends not only on effective moisture, but also temperature. Temperature is often invoked in the discussion to explain features in the sedimentary record and paleoclimate in general, so it would be good to do two things: 1) discuss the idea that the productivity gradient isn't strictly related to effective moisture,

Thank you for the feedback. Fuel moisture is controlled by relative humidity, which is a balance of temperature and precipitation/water availability. In Pausas & Ribeiro (2013) it is concluded that temperature is an important factor in high productivity ecosystems, because temperature increases lead to increases in drought and flammability “(i.e. drought-driven fire regimes)”. In the revised manuscript we have explained the role of temperature in the intermediate fire-productivity hypothesis.

L74-76: “ Rising temperatures can lead to increased drought and flammability in high productivity ecosystems and further accelerate this drought-driven increase in fire activity (Pausas & Ribeiro, 2013).”

L484-488: “In the present-day, temperature is an important regulator of fire occurrence. Whilst dead fuel moisture (e.g. that of litter and cured herbaceous components) is primarily influenced by the variability in relative humidity, live fuels are controlled by the combination of temperature and moisture availability, where long periods of drought or heat wave extremes can strongly influence the flammability of live fuels.”

and also 2) discuss the paleoclimatic setting of the record (and why temperature is also a useful variable for explaining the record).

Unfortunately, no temperature reconstructions are available on this resolution for this period (see also response to previous comment). Lower resolution temperature reconstructions exist (mostly of sea floor temperature from benthic and nekto-benthic molluscs) of other locations (Korte & Hesselbo, 2011; Korte et al., 2015; Price et al., 2016; Robinson et al., 2017) and indicate an overall predominantly warm temperature in the Sinemurian and Pliensbachian (>28 °C sea surface (Robinson et al., 2017)).

For other time periods, such as the Cenozoic temperature reconstructions are available on orbital time scales. The carbon-cycle and temperature (via potential CO_2 feedbacks) do vary over 20 kyr time scales in the Cenozoic (Westerhold et al., 2020). At mid-latitudes precession dominates temperature responses in orbitally driven insolation models (Laepple & Lohmann, 2009).

We focussed on the humidity changes of seasonal contrast because the temperature influences the fire regime via droughts (see previous comment) and this is what we do have data for in our study. Clay mineralogy indicates changes in hydrolysis with high precipitation/evaporation being a necessary factor to drive clay mineral transformation in soils prior to incorporation in the marine sedimentary record.

In the revised version of the manuscript, we have included some text on the Sinemurian/Pliensbachian climate and the evidence that temperature fluctuates on an orbital time scale.

L488-493: “Sea surface temperatures (TEX86) during the Sinemurian and Pliensbachian were at times apparently higher than 28 °C (Robinson et al., 2017). But high-resolution temperature reconstructions are lacking for the Early Jurassic. Orbital forcing of regional–global seawater temperatures occurred throughout the Cenozoic (Westerhold et al., 2020), and likely also the Mesozoic; however, the climate response to changes in orbital insolation is non-linear, and the mean annual insolation is not impacted by precession (Rubicam, 1994).”

Third, throughout the manuscript the term “seasonal climate” is used in a casual way. Not until line 328 is it clear that it’s a seasonal contrast in effective moisture that is being emphasized, but orbitally related changes in the annual cycle of temperature are also important particularly in mid-latitude, mid-continental regions. So it would be good to be more explicit, and avoid terms like “seasonal climate”.

The studied sediments are deposited in a marine setting, surrounded by islands in the Laurasian Seaway (not really a mid-continental setting). But we understand the role of temperature (see reply above) and we have refined our definition of seasonal contrast. Extremes and lows in seasonal contrast in this study are inferred from clay mineralogy and existing literature. The alternating phases of high kaolinite (accelerated hydrolysis, with annual high humidity + high temperature) and high smectite (relatively slower hydrolysis, annual dry season + warm temperature).

L31: ".. greater seasonality in rainfall and temperatures …"
L96-98: "The crucial concept is that an optimum fire window exists when there is a sufficiently moist season that allows fuel growth which is followed by a drier season in which fuel moisture levels are lowered, allowing ignition and fire spread."

L484-488: "In the present-day, temperature is an important regulator of fire occurrence. Whilst dead fuel moisture (e.g. that of litter and cured herbaceous components) is primarily influenced by the variability in relative humidity, live fuels are controlled by the combination of temperature and moisture availability, where long periods of drought or heat wave extremes can strongly influence the flammability of live fuels."

Specific comments/replies:

Throughout: Hyphenate compound words, e.g. "Present-day" (line 19), "fuel-moisture status (line 27).

We have hyphenate compounds words throughout in a revised version of the MS in cases where there is any risk of ambiguity in meaning in line with journal house style.

Line 21: Replace "whereas" with "where". We changed this.

Line 31: Replace "heightened" with "greater". We changed this.

Line 31: Seasonality of what? Moisture? Temperature?
We have changed this. L31: ".. greater seasonality in rainfall and temperatures …"

Line 34-35: "… more pronounced seasonality during eccentricity maxima, explained by the overall cooler climate …" This implies to me that indeed there is some dependence upon temperature. Yes, but here we indicate the global background climate and temperature instead of annual changes in temperature. As there is evidence of global cooler temperatures during the Late Pliensbachian Event (Korte et al., 2015).

Line 47: "Shifts" implies to me a change in distribution or pattern. Change to "… explain changes in biomass abundance, moisture availability, and fire frequency or magnitude." Yes, thank you, we have changed this.

Line 53: “productivity limited (minima) and the optimum fire-window (maxima)” Reverse the order. The modes are minima or maxima, the explanations are productivity limitation or not. We are here talking about the modes in the fire regime, which are caused by climatic forcing.

Line 67: Replace “ingredients” with “determinants”. We have changed this.

Line 67-68: This might be a good point to add an in-line definition of “fire regime”. We have added this in. L69: “Where fire regime reflects the frequency, behaviour, type of fire, and the impact on the ecosystem (Bradstock, 2010).”

Line 68-69: “high moisture and biomass production, for example in tropical rainforests.” It’s likely that temperature as well as moisture is responsible for high productivity in tropical climates. But how does high biomass productivity limit fire? Fire is often limited in high productivity systems (Pausas & Ribeiro, 2013), due to the high humidity in these systems. We have rephrased this sentence in the revised manuscript L70: “Fire is either limited by high moisture in ecosystems with high biomass production, for example in tropical rainforests, …”

Line 76: Replace “lowers” with “decreases”. Have changed this.

Line 84: I’m not sure “biases” is the right word because it implies that the optimum would occur somewhere else along a moisture gradient owing to the influence of grassland fires. That it doesn’t is basically the take-home message of the paper. So maybe grassland fires “reinforce” the generalizations? Yes, good point, thank you. We have changed it in the revised version of the manuscript. L89-90: “Although the intermediate-productivity gradient hypothesis of the present day is strongly linked to the expanse of grassland habitats, it should not …”

Line 109 (Fig. 1): Explcitly label the SPB and LPE intervals in the figure, so it can stand alone without its legend. We have labelled the SPB and LPE intervals in Fig. 1 in the revised manuscript.

Line 110: Define “mbs” here (as well as on Line 130). Changed this.

Line 116: “Orbital filters of the 100 kyr and 405 kyr cycle based on the Ca and Ti elemental records in

the depth domain from Ruhl et al. (2016).” I see bandpass filtered time series for the Ca record in Rohl et al. (2016), but not for Ti. Also, you’re confusing the bandpass filter with the filtered time series. “Orbital filters” is jargon in this context.

Yes, we have changed orbital filters to bandpass filters. And spectral analysis in Ruhl et al. 2016 has been carried out on the Ca, Ti and Fe records. However, only the Ca and Fe records are used to create bandpass filters. The bandpass filter we show in this manuscript is based on Ca. Therefore, we have adapted this in the text in the revised manuscript.

Lines 155-156: I'm not sure I understand the sample counts here. Should one of the "macrocharcoal"s be replaced by "microcharcoal"? No both are macrocharcoal counts. The 50 macrocharcoal samples are an additional set of samples to elongate the record into the cooling event of the Late Pliensbachian and are unpublished (interval 934-951 mbs), whereas the 204 macrocharcoal samples (interval 951-934 mbs) have been previously published in Hollaar et al. 2021. An overview of the number of samples per proxy has been included in the SI (SI Table 1).

Line 193: "a syringe following Stokes [sic] law…" Replace with "a syringe (following Stokes' law). Have changed this.

Lines 207-211: This paragraph confused me at first. I think it should be reorganized to describe the stratigraphy of the whole core first, then that of the two intervals analyzed in detail here. We have adapted this paragraph in the revised manuscript.

L412-L421: "The Pliensbachian of the Mochras core has a well-established astrochronological framework (Ruhl et al., 2016; Hinnov et al., 2018; Storm et al., 2020; Hollaar et al., 2021; Pienkowski et al., 2021). Based on the existing cyclostratigraphy, the 100 kyr eccentricity cycle lies within the range of 3.2–10.2 m (Ruhl et al., 2016; Hinnov et al., 2018), 6.3–4.8 m (Storm et al., 2020), and ~5.3 m (Pieńkowski et al., 2021) for the here studied SPB and LPE intervals. These intervals each compromise ~7–8 short eccentricity cycles. No spectral analysis has been performed on the records presented here because of the limited time span represented. Instead, we compare the charcoal and clay records visually with the 100 kyr and 405 kyr filters based on Ca and Ti (Ruhl et al., 2016; Hinnov et al., 2018). In SI Fig. 2 we overlay the 3.2 – 10 m filter (based on Ruhl et al., 2016) derived from the macrocharcoal record with the normalized dataset of the macrocharcoal record."

Lines 212-213: "we compare the charcoal and clay records visually with the 100 kyr and 405 kyr filters based on Cabe and Ti…" Do you mean you compared the charcoal and clay records with the filtered Ca and Ti records? Yes.

Line 230: "… with bundling of peaks ever ~4-5 m." I'm not sure I see that, but ok. We have indicated the phases of high-low charcoal abundance in the figure in the revised manuscript with the 100 kyr filter from SI Fig. 2 as overlay to the raw macrocharcoal data.

Line 233: "… in the context of the orbital filters" See earlier comment—"orbital filters" is jargon. Also, which time series is being filtered? Yes, we changed it to Ca derived bandpass filters.

Lines 235-236: "The macrocharcoal abundance shows ~5 peaks throughout the studied interval." It would be helpful to label these. I see one peak at about 1239 m. We indicated the phases of relative increases and decreases in charcoal abundance by overlaying the 100 kyr filter from SI Fig. 2 over the raw macrocharcoal data.

Line 242: "The peaks in the macrocharcoal record occur on a 100 kyr time scale." How is this demonstrated? Based on the Ruhl et al. 2016 Ca 100-kyr bandpass filter and the correspondence of the alternating phases of high and low charcoal abundance. In addition, we have filtered the 100 kyr cycle signal in depth domain from the macrocharcoal record (SI Fig. 3).

Lines 244-254: Same comments and questions as for Fig. 2. We have indicated the relatively high phases of charcoal abundance in Fig. 2 as well.

Lines 268-272: The boxplots suggest that the charcoal data have long-tailed distributions, and that the variances of the groups differ from one another. Does this have any impact on the comparison. Yes, the boxplots indicate that the variance of the LPE charcoal samples is greater compared to the SPB. This is the reason why we argue that the LPE indicates a wider range from humid (low to no charcoal) to arid (high charcoal) on Fig. 5.

Lines 314-315: "Smectite preferentially forms under a hot and seasonally arid climate, similar to a monsoonal climate system or the winter-wet climate of the Mediterranean zone." Because these climates differ substantially in the seasonality of moisture (hot monsoon/summer wet, Mediterranean/summer dry), it might be good emphasize just what aspect of those climates smectite reflects. (Presumably a pronounced dry season.) Also, which of the two climates are you imagining applies here?

Thanks, we have clearly state that smectite indicates the presence of a dry season in the manuscript.

L425-426: "Smectite preferentially forms under a hot and seasonally arid climate, similar to a monsoonal climate system or the winter-wet climate of the Mediterranean zone (Chamley, 1989; Deconinck et al., 2019)."

Line 315: What is an "accelerated hydrological cycle"?

Intensification of hydrolysis.

Line 324: Again, what exactly is varying seasonally? Temperature? Moisture?

As discussed above, we have refined extreme seasonality in regards of absolute and relative humidity with respect to seasonality, temperature, and sustained fires.

Lines 328-330: Ok, it sounds like it's seasonality of effective moisture.

Lines 344-345: Replace "orbital filter representing the ~100 kyr cycle" with "the ~100 kyr bandpass filtered time series of [macrocharcoal?]"

Changed this to L456: "The bandpass filter representing the ~100 kyr cycle in the Pliensbachian of the Mochras core (derived from the Ca and macrocharcoal records), captures the observed ~5 m oscillations in the fire record (SI Fig. 2, Fig. 2 and 3) (Ruhl et al., 2016; Hinnov et al., 2018; Storm et al., 2020; Pieńkowski et al., 2021)."

Lines 374+: "… where fire activity is plotted along an aridity and productivity gradient" Although Pausas and Ribeiro (2013), for example, discuss the variations of fire activity along a productivity (NPP) gradient, Daniau et al. (2012) show that fire activity, in both charcoal records from the LGM to present and in satellite remote-sensing data, depends on both temperature and effective moisture (see also Bistinas et al., 2014, Biogeosci. doi:10.5194/bg-11-5087-2014). Because NPP or productivity is not easily reconstructable, it may be advantageous to discuss the separate and joint influence on fire of temperature and effective moisture, which can be inferred from the evidence in the paper. In fact, temperature is invoked frequently in the discussion; it's not just moisture that explains the data.

Thank you for your feedback. Temperature further enhances the impact of humidity on fuel and fire. In essence, if humidity and fuel moisture status are very low, the fuels will still burn even if the temperatures are cooler. Temperature only modifies fuel moisture via relative humidity % and when it is warm, it can favour combustion to some extent.

On an orbital time scale the temperature would be more extreme, with higher temperatures during one season and colder temperatures during the other season. Effect on fire regime then depends on whether the warm or cold season occurs at the same time as droughts or rainfall (i.e. summer or winter rain).

However, as explained above, we do not have temperature data on this time scale for the Early Jurassic. The discussion in this manuscript in respect of fire and seasonal contrast (humidity) is based on the charcoal and clay records. The clay records are also affected by temperature (but only relative changes in regard of hydrolysis). From the fire perspective however, the most crucial factor is the presence of a dry season during maximum orbital configurations. This dry season allows the fuel moisture to drop and fire to ignite, sustain and spread more easily. This is why the focus is on seasonal contrast from a humidity standpoint in the current manuscript. In the revised manuscript, we have include some information on the role of temperature on fire (see answers above).

Line 392: "hyperbola". Changed.

Fig. 5: The tiny pictures are nice, but way too tiny. We have increased the landscape pictures in size and adapted the aridity and fire window pictures to more suitable vegetation types for the Jurassic (see discussion on vegetation included in the revised manuscript).

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Reply to RC3:

The manuscript aims to test the intermediate fire productivity hypothesis based on analyses of charcoal particles, total organic matter, d13C, CaCO3 and clay mineralogy on sediments from two periods of the early Jurassic. Charcoal particles are used as a proxy of fire.

Based on my expertise (paleofire, paleoecology), I will discuss only the fire issue.

Two categories of charcoal measurements have been carried out, “macrocharcoal” based on sieving approach, and microcharcoal based on palynological slides. Technically, this reminds the methodological study by Carcaillet et al. (2001), but on different type of sediments and geological period.

I strongly recommend completing such study with SEM images of these “charcoal” extracted from these Jurassic sediments. This would be of great interest for people specialist of plant anatomy to prove that the measured charcoal was, indeed, burned plant material and not coal or any other type of artefacts. Nobody is protected from lab error. Indeed, I personally had a bad experience with one of my assistants that measured coal particles in lake sediments within a catchment area with coal. My assistant produced nice sedimentary “charcoal” series; fortunately, a second assistant worked in parallel on these sediments on other proxies and indicated me that he never observed charcoal in these sediments, but he found abundant coal fragments. After cross-verification, the second assistant was right. In such old material, I absolutely need images (and why not cross-verification with another lab, abroad, with no conflict of interest), to verify and validate the charcoal report.

A great deal of attention was given to the correct identification of charcoal in this study. We have studied the charcoal particles in the Mochras borehole for nearly 10 years now, with multiple experts looking at it. Our first article studying the charcoal preserved in Jurassic sediments of this borehole (Baker et al., 2017, *Nature Communications*) already clearly shows that the charcoal particles quantified in this study are not coal or coalified plant material.

Furthermore, part of the charcoal data in the present study were already previously published (Hollaar et al., 2021, *Communications, Earth & Environment;* Hollaar et al., 2023, *Climate of the Past*), as clearly outlined in the present manuscript. Importantly, the 2021 publication already included SEM images of the charcoal particles in the studied borehole.

In all the above papers, charcoal particles are identified as opaque and black, angular, reflective of light, with lustrous shine, no brown edges, elongated, and splinter during breakage, often showing anatomical structure of the plant preserved (criteria from Scott, 2000 and Scott & Damblon, 2010). Coalified material lacks these characters and can be easily (with a trained eye) distinguished from charcoal.

The wildFIRE lab, led by Prof. C.M. Belcher and where this work was carried out, is an internationally recognized lab specialized in deep time palaeo-fire records. The first author was trained in this lab and has now over 6-years experience in pre-Quaternary (Mesozoic) fire studies including charcoal recognition.

To accommodate the reviewers' suggestions, the revised manuscript includes charcoal recognition characteristics in SI Table 2. Also, it includes additional SEM images from charcoal particles that were studied here for complete transparency and verification in SI Fig. 1.

If charcoal identification is validated with an independent lab, this study of sedimentary charcoal during the Jurassic, would be a great finding showing that fire is a global process since millions of years, maybe since the settlements of plant fuel on terrestrial habitats as already evidenced by Glasspool and co-workers (2004).

The present study does not aim to, or cannot show that fire has been a global process for millions of years; primarily because the present study considers only one site.

Furthermore, there is a huge body of literature and data that shows the presence of charcoal through Earth's history, ever since the first arrival of land plants (a few recent papers include Jasper et al., 2021; Baker, 2022; Glasspool & Gastaldo, 2022). The aim of this study is to test the applicability of the intermediate productivity gradient hypothesis to understand climate-vegetation-fire relations in the Mesozoic world.

The first problem is less the quantification method than the use of data. Indeed, to reconstruct fire history, whatever the fire intervals/frequency or the fire severity, a solid chronology is absolutely needed to transform charcoal concentration in terms of accumulation rate (or influx). Same charcoal concentration can result different charcoal influx according to differences in sedimentation time inferred from measured chronology, and vice versa, different charcoal concentration can correspond to the same charcoal influx.

The present study is applied to a much longer time series than suggested in the above comment (approximately 1 Myr for the Late Pliensbachian Event). Obtaining annual-resolution data in deep time marine records is not possible, and it is also not necessary given the objectives of our study (charcoal influx cm^{-2} yr^{-1}). The presented/utilized age model is based on precession cycles (~20 kyr) at its most resolved level. Sedimentation rate is accounted for when assessing charcoal abundance in Quaternary sediments (often lake deposition), by expressing the charcoal accumulation rate (charcoal influx) as the number of fragments per unit area per unit time (Marlon et al., 2016). A similar charcoal influx measure has been proposed for older periods, where the charcoal influx is based on the mass per unit area per unit time (Herring, 1985). However, reporting the charcoal flux can impose uncertainty based on an incorrect age-depth model and potential core stretching (Daniau et al., 2019); these possible biasing factors are more likely to occur in deep time geological records.

More common practice is to take potential changes in sedimentation rate and terrestrial run-off into the marine environment into account in deep time charcoal studies. This is either done by counting the total terrestrial organic fraction and comparing this to the charcoal record (Belcher et al., 2005) or to terrestrial sediment influx determined through elemental proxies (Daniau et al., 2013; Hollaar et al., 2021). In this study we compare changes in palynofacies (total terrestrial organic particles) with the charcoal record to indicate any concomitant major changes in terrestrial runoff or preservation, of which there is no evidence. For the LPE interval in the studied borehole we normalized the charcoal record to total terrestrial elemental influx (already published in Hollaar et al., 2021, 2023). Importantly, normalizing the charcoal record to the terrestrial elemental influx does not influence the pattern observed in the charcoal record (SI Fig. 4 and SI Fig. 4 respectively of Hollaar et al., 2021, 2023).

Second, in international high-profile paleo-fire paper, no one uses today charcoal series without decomposing the time series to detect charcoal peaks to determine the fire intervals and thus the fire frequency, and to eventually assess changes in fire severity thanks to magnitude of charcoal peaks (see for instance Higuera 2006 or Blarquez et al. 2013 or also Higuera 2009).

This statement is incorrect as it only relates to studies on Quaternary records. For studies of older materials, such as recorded in Mesozoic marine sedimentary archives, this is simply not possible. One charcoal/sediment sample represents 2,000 years, on average, which in fact represents a remarkably high data resolution for pre-Quaternary charcoal records.

As also per the answer above, none of the recent studies of geological deep-time charcoal in international journals even attempt to determine the fire return interval/fire frequency because such data-resolution is impossible to obtain in deep time geological archives, except under very, very rare and exceptional circumstances. Some recent and relevant papers outlining and exemplifying charcoal and palaeo botanical data resolution in deep time geological archives are: Götz & Uhl, 2022 *Annales Societatis Geologorum Poloniae*, Vajda et al., 2020 *Earth and Planetary Science Letters*, Baker et al., 2019 *GSA Bulletin,* Pole et al., 2018 *Palaeobiodiversity and Palaeoenvironments,* Baker et al., 2017 *Nature Communications*, Tanner & Lucas, 2016 *PPP*, Petersen & Lindstrom, 2012 *Plos One*, Uhl & Montenari, 2011 *Geological Journal*, Glasspool & Scott, 2010 *Nature Geoscience*, Belcher et al., 2010 *Nature Geoscience*, Marynowski & Simoneit, 2009 *Palaios*, Collinson et al., 2007 *Journal of the Geological Society* (and many more).

We note that the reviewer uses the term 'fire severity' differently from what is now commonly accepted; we here refer to Keeley (2009), where fire severity describes the loss of carbon from an ecosystem.

Additionally, this study does not contain any statistics. It is not acceptable to read such a manuscript whose interpretation is completely intuitive.

Statistics do form an important component of this study, we strongly object to the suggestion that interpretations are merely intuitive. Extensive visual comparison of trends, Pearson correlation, box plots, and bandpass filtering form the building blocks of our interpretations and conclusions. We understand that that it is always possible to explore statistical space more extensively and have included the reviewers suggestions of a PCA and a Wilcoxon test in the revised manuscript.

For example, a Wilcoxon test is a prerequisite for analysing the boxplots in Figure 4. Such a boxplot could be complemented by a kernel density which could be useful for detecting data distribution patterns. It is astonishing that this text is so intuitive (cf. L 270 or LL 301-304).

As per the above, we disagree on the suggestion that interpretations are merely intuitive.

L270: "*… to the LPE interval, however, the absolute minimum and maximum are similar*." Part of figure description Fig. 4. The exact values (mean, min, max are given in the text).

L301-304: "*However, the mean abundance of macrocharcoal and microcharcoal is higher during the SPB (mean of 787 and 2x10*^{5} *respectively) compared to the LPE (mean of 376 and 1.1x10*^{5} *respectively) in the Mochras borehole, suggesting that the shore proximity did not impact overall charcoal abundance.*" This text gives the statistical mean.

For clarification, in the revised manuscript we have added a statistical data table of the micro- and macro-charcoal (SI Table 3).

As per the reviewers suggestion, we have included the results of the Wilcoxon test and added this in the revised manuscript (macrocharcoal LPE and SPB H0 rejected at significance level p=0.006^{-10} and microcharcoal LPE and SPB H0 rejected at significance level p=0.005^{-9}; and thus supports the conclusions drawn from the boxplots.

Also, LL 304-307 mentions comparisons of means, even though no statistics have been carried out and the data are not illustrated.
The statistical values are now in addition shown in the SI Table 3.

Generally, the authors speculate on the interactions between bio- and geo-proxies, sometimes indicating correlations (r-values) associated with p-values, when a simple principal component analysis (PCA) would have been very efficient if carried out as a preliminary analysis. to make a solid descriptive statistic of the environmental data (all proxies) to clearly distinguish those that exhibit the same behaviour (correlated or anti-correlated) from those that have no links. With such a PCA, the authors would have interpreted their data based on a good methodology allowing a rational sedimentological interpretation (e.g., Clark-Wolf et al. 2023). Such a basic strategy should avoid “visual comparison” of data (L. 212) and speculation (the entire manuscript). I am not sure r (linear correlation coefficient) is appropriate. I would have used the coefficient of determination (r2) (LL261-264).

For the aim of this study a Pearson correlation is effective: to indicate no correlation between terrestrial organic matter content and charcoal. We performed a PCA analysis previously for the LPE interval (published in Hollaar et al., 2023, *Climate of the Past*, Fig. 5). However, following the reviewers suggestion we explored further PCA analysis for the SPB and LPE intervals in the revised manuscript here. Importantly, the previous analysis confirms the trends we discuss in the current manuscript based on the Pearson correlation. As the PCA in Hollaar et al. (2023) does not yet include macrocharcoal and does include elemental proxies that are not a part of the current manuscript, it will indeed be valuable to re-do the PCA also for the LPE interval and include the proxies presented in this study.

The results of the PCA analysis are shown in SI Fig. 6 and 7 and confirm the trends discussed in the manuscript. Enhanced weathering and hydrolysis (kaolinite) occur over orbital time scales and limit charcoal abundance.

LL 81-83: I partly disagree with the assertion that “high fire activity in ecosystems (…) is strongly driven by grass biomes”. Archibald et al. 2018 is cited. This a very good paper, but many papers demonstrated that it is not grass component of the ecosystem that drive high biomass burning or fire risk but an intermediate tree-cover (Archibald et al. 2009; Frejaville et al. 2016; Aleman et al. 2017), which increases the ETP, then dryness, and finally the development of grass cover in the understorey. Grass cover is a secondary process, but the main process is the intermediate tree-cover, which can be sustained by wildfires resulting a feedback-loop.

In L81-83 we explain that the observation of the intermediate-productivity gradient is based on a world in which grass ecosystem burning (>80% of all burned area) dominates.

L81-83: “However, the observation of high fire activity in ecosystems that are of intermediate aridity and productivity is strongly driven by grass biomes (Archibald et al., 2018).”

The sentence has been rewritten in the revised manuscript L90-93: The observation of high fire activity in ecosystems that are of intermediate aridity and productivity is strongly driven by grass biomes today (Archibald et al., 2018), where >80 % of area burnt is in grasslands (van der Werf et al., 2006).

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