Reply to review 1:

Emily H Hollingsworth (Referee)

The authors thank the reviewer for the positive feedback and constructive suggestions. We have addressed most of the comments and a point-by-point reply is provided below.

In this paper, Zhang et al. applies a proxy- and model-based approach to reconstruct changes in the local hydrology of central California, from pre-PETM to the PETM. This work builds on data published from a previous study (δ^{13}Corg; John et al., 2008) as well as contributing new proxy records (e.g. grain size analyses; clay assemblage analyses; δ^{13}Cn-alkane; and δ^{2}Hn-alkane). Climate model simulations further support the proxy-based findings and were additionally utilised to constrain the effects of seasonal precipitation on δ^{2}Hn-alkane values. They conclude that both the models and proxies indicate an overall drier central California, although the summer saw a slight increase in precipitation. Results from extreme events analyses suggests that intense rainfall events were more frequent during both the winter and the summer.

This paper is well written, containing very little spelling and/or grammatical mistakes. The introduction nicely outlines the significance and the key question that was being investigated. Multiple proxies are utilised in conjunction with a novel method employing models to improve proxy-based reconstructions. The findings address the relevant gaps in our knowledge regarding how the hydrological cycle in central California may respond to future warming. It is exciting to see another study that applies n-alkanes as a hydrology proxy, especially as there are only currently seven records for the PETM (Carmichael et al., 2017).

I believe the author can improve on the manuscript by refining the structure of the text. Specifically in regards to the discussion, in addition to creating more continuity between the main text and supplementary information (i.e., removing repetition) (See Section 3). Furthermore, there are a few major questions pertaining to how some of the δ2Hn-alkane record has been interpreted (See Section 1).

1) Interpreting the δ^{2}Hn-alkane record

1.1 Orbitally driven shift in pre-PETM δ2Hn-alkane?
The author states that the 25‰ negative excursion in δ2Hn-alkane record, just prior to the onset, is likely representative of orbitally forced variability (Line 303-304). Although this is just a brief sentence and not the focal point of the discussion, I am curious as to what the author based this on. Was there any spectral analyses done to see if the fluctuations in the δ^{2}Hn-alkane could correspond to any astronomical forcings? Could the author cite any papers that have looked into potential cyclicity in the hydrological cycle during the Paleogene (e.g., Campbell et al., 2023)? What does this interpretation mean for other sites? The author noted that several subtropical/mid-latitude sites have shown a similar magnitude (~20‰) negative shift at the onset of the PETM (e.g., Handley et al., 2008; Jaramillo et al., 2010). How can we go about

deconvolving whether such trends are driven by the abrupt perturbations in temperature at the onset of the PETM vs. changes in orbital parameters?

> This is a fair criticism. The signal is small and at this location there's not a sufficiently long upper Paleocene record to establish the background variability (related to orbital or other forcing) prior to the PETM. We mention orbital forcing simply because there are several sites (e.g. Forada, Tanzania, New Zealand, Venezuela etc.) showing a positive shift prior to PETM (line 296-297) opposite of Lodo. If somehow co-eval in time, opposite patterns would be more consistent with orbital forcing on local precipitation, in part supported by theory (see Kiel et al 2018; Lunt et al., 2007; Rush et al., 2021). Given the poor age control on all these sites, however, it is just as likely that these changes may not be coincident. Sure, the local signal could be related to warming but really difficult to prove either way (w/o constraints on T). As suggested, we added a citation to Campbell et al., 2023**.** The initial enrichment (~5‰) at the onset of the PETM is consistent with simulated response for this region.

1.2 Stable δ2H*n*-alkane through the PETM?

The results section states that the δ2H*n*-alkane are relatively invariable throughout the PETM (Line 178-179). Although the PETM is not defined in the figures, if assuming that the PETM includes the CIE up to 20 (unsure depth unit as not defined in figures), Figure 3 presents relative stability at the beginning of the CIE, yet the upper CIE shows larger variability. There is one very negative value at the onset, however, this is one data point and seems to be only with the C29 *n*-alkane. On the other hand, the variability in later in the section shows correlation between all the chain lengths and more than one data point. The discussion section largely focuses on explaining the reasons why the record is stable. I was wondering if the author could also touch on why the upper record is more variable. Several other sites show such variability, for example, TDP Site 14 exhibits oscillations throughout the PETM although the frequency is higher and the magnitude of change lower (Handley et al., 2008).

> Yes, overall through the onset and CIE, δ^2H is relatively stable with just a slight enrichment (as noted above). The CIE recovery interval, now highlighted, is truncated around 22m roughly coincident with an increase in the variability of $\delta^2H_{n\text{-alkane}}$. As we have limited age model control, mainly relying on biostratigraphy, it's difficult to interpret the cause of the increased variability without a lot of speculation. Just considering the depositional facies and environment, there's potential for artefacts (e.g., truncation) related to stratigraphic breaks, etc.

1.3 Evidence for a stable hydrological cycle during the beginning of the PETM?

Although the author describes all the potential factors that may have muted any changes in δ2Hn-alkane (i.e., changes in temperature on fractionation vs. the source of water), I was curious as to how they ruled out the simplest explanation that the hydrological cycle may have been stable during the main body of the PETM? Is it because the models and published proxy *records suggest the opposite, i.e., higher frequency of extreme rainfall events (Carmichael et al., 2016, 2017). If so, could the author add a sentence to rule out that the lack of change in the δ2Hn-alkane record is reflecting the climate, then go on to discuss the other potential explanations.*

> We do favor the simplest explanation from the observational perspective, no clear pattern of a "major" change in regional hydrology (as compared to other sections), whereas the models suggest a significant reduction in winter precipitation. We modified the text (start line 419) to emphasize the relatively muted response of the leaf wax record.

2) Utilising n-alkane distributions to help interpret the δ2Hn-alkane record

The discussion section mentions that the lack of knowledge on vegetation changes through time hinders the ability to calculate the δ2H of precipitation (Line 308-310). I think the lack of change in the average chain length (ACL) is very much worth mentioning here and fits well with the Korasidis et al. (2022) paper, which also shows little change in the Koppen-Geiger climate type within the central California region. There are limitations to using ACL as an indicator for vegetation type (Bush and McInerney, 2013), but it provides some evidence that suggests that the effects of varying fractionation (caused by changing plant types) may have been minimal. With the ACL indicating a mostly terrestrial higher-plant source for the n-alkanes, the comment on plant types recording hydrological conditions at a specific season (Line 324-326) can also be of a lesser concern. Even with a strong seasonal signal, if this remained constant throughout the record then the relative changes would be unaffected.
Line 310-312 highlights that the δ2Hn-alkane values may be influenced by re-worked n-alkanes. I suggest that the author look into the carbon preference index (CPI; Bray and Evans, 1961). This would not require too much work as the author already has n-alkane abundance data. The CPI may help indicate any input of thermally mature older sediments/n-alkane. CPI values >3–30 would suggest that most of the organic matter is unaltered (Diefendorf and Freimuth, 2017). Furthermore, several studies have suggested input of thermally mature material based on an antiphase between the δ13C of bulk organic vs. bulk carbonate (e.g., Lyons et al., 2019). If neither of these indicates re-worked n-alkanes, this may be highlighted as less of a concern.

> Thanks for the suggestions. We added explanation of the lack of change in ACL and cited Korasidis et al. (2022) to better constrain the effects of varying fractionation caused by vegetation changes. We also added the CPI in the figure to support limited recorking of *n*-alkanes.

3) Improving the structure
3.1 Structure of the methods section and supplementary information

There is repetition between the methods section in the main manuscript and supplementary information. In addition, there are information that is found in the main manuscript but not the *supplementary information and vice versa. For example, it would be useful to have information on how many samples were analysed in Section 2.2.2, instead of noting the instrument used for analyses in both. Similarly, Section 2.2.4 contains a lot of detail that is in the supplementary information, but urea adduction is only in the main manuscript and the column chromatography method is only in the supplementary information. This means that unless the reader looks through both the manuscript and the supplementary information, they are not getting the full picture.*

> We added the 35 samples analyzed in the main text method section. We removed the sample preparation in the supplementary information to avoid repetition and merged the column chromatography into the main text method section.

Furthermore, there are no references to the supplementary information in the methods or the results/discussion for the additional figures. This is a minor comment but if the subheadings were labelled in the supplementary information and ordered in a similar way to the main manuscript (i.e.,
leaf wax n-alkane extraction and separation – grain size analyses – extreme value analyses - leaf wax proxy model), then it may be easier to refer to for additional information.

> We added the related references to the supplementary information where appropriate. We reorganized each section in the supplementary information to align with the main text.

3.2 Structure of the discussion section

The first paragraph of the discussion states how sedimentation rates may provide information on the hydrological cycle. Since this study does not present new constraints on the age model or sedimentation rates, I wonder if this could be incorporated into a couple of sentences within the 4.1 section. The crucial point is that higher sedimentation rates suggest more runoff and therefore more rainfall. It would also be interesting to compare the timing of the shift to higher sedimentation rates with the changes in the clay assemblages. The caveats surrounding the lack of tie-points can be raised, but is already discussed in John et al. (2008) and not so much linked to the main proxies within this study.

> The previous observation of a shift in sedimentation rate is consistent with increased runoff so seemed appropriate to start the discussion with. Given the uncertainties in age control, it sets the stage for discussing the other observation proxies.

The discussion paragraphs begin with an introduction to the other studies that have used the same proxy, then highlight the caveats and main assumptions that have to be made. By starting with the issues, the subsequent discussion on the authors results is somewhat downplayed. I personally think that starting with the key findings of this study, then seeing how that compares to other published findings, and then discussing the caveats may flow better. This applies for the *paragraph on sedimentation rates but also the paragraph beginning on Line 288 vs. the paragraph beginning on Line 332. Much of the suggestions for why the δ2Hn-alkane values might be muted feel speculative in the first paragraph, however from Line 332 there are really nice evidential based explanations that could be discussed first then the other potential ideas after. In addition, since there is one sentence in the first paragraph (Line 231-232) pertaining to the modelling results, would it make sense to first discuss the modelling results then how the proxies compare to them? However, most of the suggestions on structural changes are based on a subjective preference, so please consider these comments as so.*

This is a reasonable suggestion as we struggled a bit with organization of the discussion. We have revised the structure to start with a discussion of the model simulations, followed by the comparison with observations. We believe it now flows more smoothly.

Minor comments:

Line 28: the sentence beginning with "indeed" sounds like it should be related to the previous point, however I would argue that they are two separate and important points. In addition, I think there should be a "the" for "just over the last few decades"

Done.

Line 35: this may be my misunderstanding of what defines a "drought", but is it repetition to say "extreme droughts" and "longer precipitation deficits"?

Done.

Line 47-52: for ease of the reader finding the relevant literature, could the citations on Line 48-49 be put next to the relevant locality?

Agreed.

Line 55-57: Cramwinckel et al. (2023) also looks into this. Might be a citation to add here.

An oversight. Citation added.

Line 74: missing a comma after "Here"

Done.

*Line 76 (plus many other locations): Most often "**n**-alkane" is found italicised*

Done.

Line 99: how many samples of the originally collected were analysed?

27 Samples added.

Line 100: how many new samples were collected and also how many of these were analysed

Done.

Line 100: good place to note the acronym of bulk sediment organic carbon isotope (δ13Corg), and change "analyses" to with an "e" to make it plural.

Done

Line 107: "analyses"

Done

Line 108: how many samples were analysed for grain size analyses?

39 samples added in the text.

Line 112: "analyses"

Done

Line 113: should "Kemp et al., 2016" be in brackets?

Changed into Kemp et al., (2016)

Line 118: what temperature were the samples dried in?

40 °C added into the text.

Line 120: "analyses"

Done

Line 122: could the clay species be specified here?

Yes, added.

Line 126: how many samples were analysed for biomarker work? And how many for the CSIA?

Added in the text.

Line 127: v/v should be italicised

Done

Line 129-130: "Normal-alkanes" can be "n-alkanes"

Done

Line 136: numbers by C should be a subscript, e.g., C29

Done

Line 143: "analyses"

Done

Line 147 and 141: missing "i" in front of iCESM 1.2?

No i for this model since it is the original Climate Earth System Model. When using isotope-enabled CESM, the acronym can be iCESM.

Line 163: CIE can be abbreviated here rather than on Line 165. How big is the CIE (value ‰). Overall, the results could do with more values replacing descriptive words such as "slight" increase etc.

Done

Line 165: The sentence beginning here and beginning on Line 166 can probably be merged into one sentence, but please cite the other records that are being referred to on Line 166

Done

Line 169: is there a reason why the δ13Corg was plotted in a separate figure to δ13Cn-alkanes? A figure with the bulk and biomarker based isotope records and a figure with the clay assemblage results may work better with the flow of the text

Both fig 2 and fig3 have $\delta^{13}C_{org}$ record.

Line 188: how much does the illite/smectite (extra l in illite) and chlorite/smectite ratio increase by?

The illite/smectite ratio increases from 0.45 (pre-PETM) to 2 (PETM), and chlorite/smectite ratio increases from 0.29 to 1. Numbers have been added in the text.

Line 198-199: how much does the monthly precipitation decrease by and how much is the increase in the summer?

Added in the text.

Line 228: different labelling with (2H/1H and 18O/16O) may confuse some readers

Done. Changed into δ^2H or $\delta^{18}O$

Line 232-235: the two sentences here could be merged

Done

Line 253-255: when was the increase in kaolinite/smectite (can be specified in terms of relative to PETM or depth)

Clarified in the result section 3.3.

Line 257-259: again when was this?

Clarified in the result section 3.3.

Line 265: similar to comment 1.1 (above) what is the suggestion of orbitally forced variations in the clay assemblage pre-PETM based on?

See reply in 1.1

Line 272: is there a citation for this?

Based on the references cited in the model simulations right before the sentence.

Line 287: 2H/1H can be used, however, it is nice to remain consistent with naming, i.e., 2H/1H or δ2H

Done. All in δ^2H.

Line 312-315: quite a long sentence which makes it hard to follow

Revised.

Line 318-320: is there a citation for this?
Added

Line 321: I think "affect" should be "effect"?

Prefer to use affect as verb to address the action of influencing.

Line 328: Add "is" to "If most soil water is from…"

Done

Line 333: could the word "significant" be replaced?

No

Line 335: This is the first time δ2Hprecip is being used. Could this be defined earlier and used throughout both the main text and supplementary information? (assuming the plant is always sourced by precipitation). Also, the “i" is missing in “precip”

Done

Line 351-352: starting the sentence with “higher plants leaf wax” and then saying “long-chain n-alkane” is repetitive. This sentence could remove one and it would still make sense.

Done

Figures:

Figure 1: Is it possible to make the site location more eye-catching by making the red spot larger?

Done

Figure 2: Unit is missing on depth scale (meters?) and some of the clay ratios. Furthermore, could the PETM be highlighted, in addition to using a different symbol to show the already published δ13Corg data (this should also be cited in the caption).

Done

Figure 3: Unit is missing on depth scale. Could the PETM be highlighted? In addition, the caption says “Marine δ13C” (Line 182) which suggests δ13Ccarbonate but this is not plotted here. The “n-“ can be removed from Line 184.

Done.

Figure 5: different labelling to figure 4 (PETM vs. 6x and LP vs. 3x). Is this because they are differently defined? If not, could the same labelling be used for continuity?

Yes. It’s defined in different climate models.

Supplementary materials:

Subheadings with analysis should be analyses with an “e” to make it plural.

Done

*Most often "**n**-alkane" is found italicised (3 places in supplementary materials).*

Done

Second paragraph on "Grain size analysis" section – missing a comma after "and thus hydroclimate during the PETM".

Done

First sentence of "Leaf wax proxy model" – "affect" should be "effect". This sentence currently reads to me like seasonal precipitation effects the fractionation process in plants. Unless this is what is intended, could the sentence be reworded to make clearer what is being discussed, e.g., "To investigate how seasonal variations in the δ2H of precipitation effects δ2Hn-alkanes values…" or something of that nature.

Done

Fourth paragraph of "Leaf wax proxy model" – could the author cite the paper for which the average chain length equation was taken from or note the equation used in the supplementary information?

Done

First paragraph of "Leaf wax n-alkane extraction and separation" – methanol can be shortened to MeOH as done for DCM. Further in the paragraph, when describing the amount of solvent used during column chromatography, these acronyms can be utilised again. Also "Normal-alkane" x2 can just be labelled as "n-alkane"

Done

Third paragraph of "Leaf wax proxy model" – could specify that it is for compound specific analyses.

This is a general leaf water proxy model.

Fig. S1. Could the boxes be labelled a,b,c etc. to make the caption easier to follow? Also highlight the PETM in the lower most plot.

Done