Reply to Reviewer 1

On "Climate variability off Africa's southern Cape over the past 260, 000 years"

Reviewer 1:
This paper presents climatic data derived from a marine core spanning the last 260,000 years off the southern Cape coast of South Africa. The southern Cape is a major focus of Quaternary climate research, both for its own sake as well as for understanding archaeological, ecological, and biogeographic change. The paper is appropriate for CP and I recommend publication pending revision.

Perhaps my biggest concern with the paper is the setup. Both in the abstract and introduction, the manuscript emphasizes the southern Cape's Middle Stone Age (MSA) archaeological record and the "pressing need for precise climate data" to better understand this record. The archaeology appears to be the motivation for the paper. This would be an appropriate setup for the paper if there was going to be some serious engagement with the MSA record, and how the new climate record helps to understand it. However, outside of the abstract and introduction, there is very little meaningful engagement with the MSA record from the southern Cape. In the discussion section of the paper, the main reference to the archaeology is "Finally, the data from core MD20-3592 can be used to provide a climatic context for the ~ 120-50 ka time interval during which evidence of behavioural complexity in humans appears in South Africa. The general climatic trends inferred from the MD20-3592 PC1 record are as follows: 1) Initial humid conditions at ~120 ka become progressively more arid from ~117 to ~102 ka; 2) More humid conditions return between ~98 and 88 ka, after which a pronounced period of aridity centred on ~83 ka occurs; 3) Conditions become progressively more humid between ~ 83 and ~72 ka, after which progressive aridification occurs until ~60 ka; 4) Arid conditions persist until ~50 ka." If that's as deep as the authors want to dive into the archaeology, I don't see why MSA archaeology is the main hook of the paper—i.e., the archaeology angle appears to be little more than window-dressing. Can we not write paleoclimate papers for the sake of learning more about paleoclimate? Why frame this great new record around an archaeological problem that you aren't going to address?

The solution here is to (1) frame this paper (in the abstract/introduction) around paleoclimate—it's fine to mention the archaeology where appropriate, but that isn't what this paper is about; or (2) engage meaningfully in the implications of this record for the MSA archaeological record in the southern Cape. But if you go with option 2, just please, please avoid wiggle-watching and hand-waving!

→The suggestion of reviewer 1 in this paragraph makes sense, and we chose to pick option (1). The introduction and abstract mention more of the archaeology than required. We edited the sections accordingly.

Abstract line 12-13: The first sentence of the abstract is rather clunky
→Agreed, it appears vague at some parts. It has been rephrased:
→"During the late Quaternary, the climatic conditions in southern South Africa experienced significant fluctuations, notably in temperatures and precipitation. These fluctuations were related to changes in the atmospheric and oceanic circulation systems from the subtropics and mid-latitudes, which are themselves affected by changes in orbital parameters."

Abstract line 15: replace “exhibits” with “preserves”
→Done. (Now line 17).

Line 17: “during the MSA” I have been scolded many times for referring to the MSA as a period of time. It’s a technological industry that starts and ends at different times in different places. Reword to “…can provide climate constraints for the region’s MSA record”
→Changed as suggested (Now line 20).

Line 18: ~60 ka is early for the end of the MSA in southern Africa. I would say ~40 ka or even ~30 ka is reasonable.
→. Changed as suggested. (Now line 22).

Line 25-26: do you need to list the statistical analyses provided here? What about “Our results reveal that…”
→The statistical analyses are listed there to include notable parts of the methodology in the abstract. However, for brevity, the list of statistical analyses has been replaced by “spectral analysis” (Now line 30).

Line 50-51: It’s also complicated by a lack of a robust mechanism for transmitting climatic cause to the proposed evolutionary effect (Behrensmeyer, 2006).
→This makes sense, we addressed this angle of the problem. (Now line 67-70).

Line 57: “in the eastern Cape province…” Here and elsewhere, please be precise with your use of geography and place names. There is no Cape province anymore. There is an Eastern Cape province, but the paper cited here concerns a record from the Western Cape province.” Southern Cape would still be appropriate to use here.
→Agreed, we use “Southern Cape” here and revised the other names used for geographical locations in the text.

Line 70: “…the African climate…” Beware of referring to the African climate. It is not a monolithic thing.
→This mention of the “African climate” has been cut.

Line 123: Once again, be precise with geography and place names. “southern Cape coast and parts of the Western Cape.” A good part of the southern Cape coast falls within the Western Cape (province). So what parts of the Western Cape are you referring to here?
→This portion of the sentence has been cut (now lines 299-301)

Line 133: geography again. The WRZ extends well north of the Western Cape (assuming you mean Western Cape the province).
→I rephrased to “is located along the western coast of the Western Cape province, the Northern Cape province, and southern Namibia” (Now line 347-348).

Line 148: move the references cited here to the end of the sentence
→This sentence is now modified (Now lines 433-437).

Line 181-187: Do we need all the details about how a PCA works? A similar level of detail isn't provided for other analyses.
→These lines were removed as suggested.

Line 198: remove comma after "determine"
→Done (Now line 556).

Line 208: … "and Fe/K" add "is shown in Figure 2g.
→This sentence has now been modified (Now line 645).

Figure 3: Not sure if this is required for the main text. Might be better off in the supplement.
→This is now in the supplementary material (Fig. S6).

Line 299: Italicize G. ruber
→Done (Now lines 364, 378, 619, 627-628).

Line 316: remove comma after "reversals"
→This sentence has been cut, as it overexplained the methodology for using "undatable".

Table 2. Perhaps italicize or put in quotes "Undatable"; caption: insert comm after "calibration"
→Done. Now Table 1.

Figure 5 caption: "but here an age" to "but here the age".
→This figure has been merged with figure 6, and is now figure 2. The correction has been made for the caption.

Figure 8 caption: PC1 is referred to as PCI on two occasions here.
→This is now figure 6. Corrected to PC1 in the caption.

Line 434-435: for ease of reading, put the i.e. statements in parentheses: "…calcium input (i.e., marine productivity)…"
→ The corrections were made. (Now line 1203).

Line 463-465: It's fine to mention the model, but you should also consult the terrestrial records that speak to wetter conditions and/or the intensification of winter rainfall in the region (e.g., Chase et al., 2017; Chase et al., 2018; Faith et al., 2019; Faith et al., 2024; Sealy et al., 2016; Sealy et al., 2020)
→Agreed, the suggested references and what they indicate is now weaved in the text. (Now line 1307-1310).

Line 548: "For the Cango Caves, a composite speleothem record named "Cape Fold composite" was produced by Chase et al. (2021)". The Cape Fold Composite is not just from Cango Caves and it's not just from speleothems, as it also includes hyrax midden records. Refer to it in the text

(e.g., on lines 554, 555, 561, 622) as the Cape Fold Composite rather than the Cango Cave speleothem record.
→Agreed, this now corrected throughout the text.

Line 615: insert comma before “Collins”
→This section has now been re-written.

Line 631: Again, it would be appropriate to refer to some of the terrestrial records (e.g., Chase et al., 2017; Chase et al., 2018; Faith et al., 2019; Faith et al., 2024; Sealy et al., 2016; Sealy et al., 2020). There’s no need to rely solely on a climate model when there are empirical data at hand!
→Agreed, the suggested references and what they indicate were weaved in the text. (Now lines 1590-1595).

****

Reply to Reviewer 2

Review of egusphere pre-print 2024-2499 of Purcell et al. entitled “Climate variability off Africa’s southern Cape over the past 260,000 years”
Purcell et al. present XRF data from a study core MD20-3592 from off the coast of South Africa and apply a number of data analysis techniques (principal component analyses, filtering) to derive the dominant mechanisms driving sediment composition, primarily precipitation in nearby river catchments and terrigenous sediment supply, marine productivity changes as well as adjustments in wind circulation. The authors compare their data to speleothem records in South Africa and other sediment records around South Africa. They find a dominant impact of precession on continental aridity and precipitation patterns via its impact on land-ocean temperature contrasts, river discharge, and hence sediment composition at their study sites. This appears to be consistent, as the authors argue, with earlier findings based on sediment cores nearby.

The study addresses an important aspect of the climate system, namely the interplay of orbital forcing, local wind patterns and land-ocean interactions via sediment supply in South Africa and the adjacent ocean. These findings are not fully new, but I find the comprehensive comparison of local records spanning speleothems and sediment cores bears strong value.
The study is well-motivated and well-illustrated. I do, however, find that the study is not streamlined enough, lacks clarity, and is longwinded in parts. Also, I find that a clear scientific hypothesis is lacking. The link to hominin evolution is oversold and irrelevant because the study does not provide a clear comparison of suggested climate evolution and robust evidence from archaeological sites for mobility, technological advances of Homo species. I also recommend major restructuring of some parts of the study.

I outline major and minor criticism below and hope that the authors find them useful to improve and revise their study.

Major comments:

Motivation of the study and streamlining of the Introduction: I am missing a clear scientific question that is addressed in the study. It is therefore unclear what the goals of the PCA and spectral analysis etc. actually are. Writing “Principal component analysis (PCA) was conducted to facilitate interpretation of the XRF data.” is in my view not enough. The motivation for these types of time series analyses needs to be clear from the introduction in association with the scientific question(s) or hypothese(s). Please revise accordingly.

-->We have added the following hypothesis being tested in this work to the introduction:
“Our starting hypothesis is that regional hydroclimate variability in southern South Africa over the past 260,000 years is primarily driven by changes in local summer insolation associated with orbital precession, modulated by high-latitude climate dynamics linked to orbital obliquity and eccentricity, which together control precipitation patterns and their influence on sedimentary records” (lines 200-203 now).
-->We have made a connection in the introduction between the use of PCA and the scientific questions addressed: (line 214-222 now)
-->Spectral analyses: this term is now introduced in the abstract (line 30).
-->And the motivation for these analyses, in relation to the aims of the study, is described in the introduction as well. (lines 222-250 now)

I understand that the focus of the study is on studying sediment composition at the study site and how this is driven by precipitation changes in South Africa. However, there are a number of studies mentioned in the Discussion section that addresses these issues based on numerous cores nearby, and it is not clear how the study of Purcell et al. addresses an open question within the background of these existing studies. This needs to be emphasized in the Introduction and the scientific question(s) or hypothese(s) highlighted accordingly. In other words, the study of Purcell et al. would benefit from a clear introduction of what is known, what is debated and what remains an open question?
-->These themes have now been more thoroughly introduced in the introduction. (lines 80-200 now)

Phrases such as l. 678 “Core MD20-3592 is the first marine sediment core off the coast of southern South Africa which allows for a reconstruction of the climatic conditions over the past two glacial cycles.” should be avoided because they are clearly wrong given the numerous records. Instead, it is more valuable to highlight what kind of assessments *specifically* the record does allow, such as potential river discharge changes in Southeast African river systems due to adjustments of the hydrological cycle over the past two glacial cycles.
-->This sentence has been cut and rephrased with this text:
“The analysis of Core MD20-3592 allowed to evaluate river discharge changes in Southeast African river systems due to adjustments of the hydrological cycle over the past two glacial cycles.“ (line 1804-1805 now)

I also recommend that the authors make a clear list of XRF elemental ratios that are important for the study and elaborate in clear fashion, what this XRF elemental ratio can be used as proxy for, in what capacity in the study area it was used before, and what purpose it serves in the study.
-->This list has been put in the introduction. (line 208-214 now)

Figure 2, 4 and 9 shows a number of XRF elemental ratios although they are never properly introduced. The description of the XRF ratio Fe/K in line 424-432 is somewhat inconclusive, as variations in K are not discussed.
-->This ratio is now introduced in the introduction (line 211-214 now), elaborated further in the methodology (line 436-437 now), and the variations are discussed in the results (lines 645 -676 now), and the discussion (lines 1190-1201 now)
-->Variations in K are now discussed in the results. (line 676-677 now)

If the authors are primarily interested in the ratio of a terrigenous versus biogenic element, then it is not clear why Fe/Ca is not used.
-->We are rather interested in the precipitation occurring over the river catchments nearest to the study core. This can be indicated by the ratio of terrigenous versus biogenic elements such as Fe/Ca. However, this ratio can be affected by diagenetic processes affecting Fe in the marine sediments. Therefore, using a whole suite of elements can avoid this issue.

The importance of the analyses for the evolution of Homo sapiens in South Africa is overemphasized as no robust comparison between the marine and archaeological findings are made. The final sentence in the abstract is too vague and oversells the findings (without accurately proving the assertion). Thus, rather than elaborating at length about Homo sapiens evolution in the Introduction, I recommend to elaborate on the state-of-the-art of our knowledge of the hydrological cycle in South Africa (winds, precipitation, river discharge, etc.) and the open questions that remain and will accordingly be addressed in the study.
-->We agree, the introduction and abstract mention more of the archaeology than needed. This has been adjusted.
-->These major changes suggested have been made, with a much more condensed portion on *Homo sapiens* in the introduction, and an expanded part on the hydrological cycle and the open questions.

This link made between hominin evolution and adjustments in the hydrological cycle in the final paragraph of the Conclusions is poor and not convincing. Remove or rewrite by providing convincing figures/argumentation showing a possible relationship between the two and sufficient background of the existing state-of-the-art of the archaeological literature.
-->In the first sentence, references have been added to base that statement with state of the art archaeological literature.
-->The second sentence of this paragraph, which refers to technocomplexes, has been removed. In this section the following sentence has been added:
"The period studied here is a key period for human behavioural evolution in Southern Africa, and the data presented here may serve as a reference for future analyses of potential links between the climate and archaeological records."

Statements are often too vague in my view
--> We have tried to clarify and specify statements through the text.

Name available climate archives (l. 20 and l. 30)
-->L.20: …of available climate archives in this region (sediment cores, speleothems, and hyrax middens)
(lines 24-25 now).

Specify what exact purpose PCA serves in the study (l. 25) (stating "Principal component analysis was performed to facilitate the interpretation of the data." is not enough)
-->This is now elaborated in the introduction (lines 214-222 now)

Specify "this variability" (l. 56)
-->"One aspect of this variability in precipitation is its impact on vegetation…. (line 76 now)

Clarify preference of whom (l. 61)
-->This sentence has been cut.

Specify in what way "at coastal environments, a strong influence of the Agulhas Current on hydroclimate is apparent" (l. 64)
-->"One driver of precipitation changes along coastal environments is the strong influence of the Agulhas Current (Chase and Quick, 2018). These authors propose that the warm Agulhas Current modifies the surface heat flux and increases the flow of moist air onshore, creating localized environmental conditions near the coast during the Holocene" (lines 81-85 now)

Specify what is meant by climate signal (l. 78, -> sediment?),
-->Yes, we were referring to the sediment.
-->"The objectives are (1) to characterise the provenance of the sediment carrying the climate signal," (lines 197-198 now).

Specify what is meant by "marine proxy records provide age constraints and palaeoceanographic conditions" (l. 103, -> age constraints for what?, what conditions specifically?)
--> The sentence was removed.

Include "increased proportion of sediments from local southern African sources" (l. 118)
-->This sentence has been cut.

Link the information in the supplement by stating the figure, text and/or table to steps 1-3 (l. 204)
-->"More information on these steps can be found in the "Spectral analysis" section in the supplementary material." (lines 599-600 now)

Statement "although the general pattern displays variability" (l. 209) is too vague, please remove or specify what is the general pattern and what is meant with "variability".

-->It was removed.

Specify what is meant by “marine processes” (l. 216)
--> marine biological productivity.
“The Fe/K ratio has the advantage of being independent of variations on Ca concentration, which can be influenced by marine biological productivity.” (line 667-669 now)

Specify what is meant by “large” and “relatively lower amount” in l. 240 (The explanation in lines 240-242 is incomprehensive, please revise)
-->Added this information to clarify: (mean $CaCO_3$ concentration: ~25%; Fig. S1).
(line 785 now)

Specify what the type of soil has a notable impact on (l. 424)
-->on the Fe/K ratio
“The type of soil has a notable impact on the Fe/K ratio” (line 1191 now)

Specify the most important paleoclimate archives used for comparison, i.e., core name and region (Simon et al., 2015; Chase et al., 2021) (l. 681)
-->”(Core CD154-10-06P (Simon et al., 2015); Cape Fold Composite record; (Chase et al., 2021)” (line 1808 now)

Specify what exactly drives variations in terrestrial input and what is controlled by orbital forcing; “climate change” is too vague (l. 688)
-->”Variations in terrestrial inputs have shared periodicities with orbital cycles, implying that high and low-latitude forcings were affecting the hydrological cycle and hence riverine discharge of terrestrial sediment by increasing or reducing precipitation over the river basins.” (lines 1816-1818 now).

Specify what parameter shows the main frequency (l. 689)
-->We added “as indicated by the power spectrum of PC1 (Fig. 6a) and 23 ka gaussian filter of PC1 (Fig. 6b).” (line 1819-1820 now)

Specify “other mechanisms” (l. 707)
-->Mcgee (2020) found wetter conditions due to colder conditions and lower evaporation in southwestern South Africa. Sime et al. (2013) found that during the LGM, a weakened Hadley cell led to less subsidence of dry air around 30°S, which favored wetter conditions near this latitude. (line 1736-1739 now)

Specify “successive technocomplexes” (l. 711)
-->This sentence has been cut.

Specify what is meant with “elemental oxide ratios” and “calcium input” (l. 434).
Do you mean the sediment composition and the deposition of CaCO3 at the study site, respectively?

-->Yes, we refer to the deposition of the sediment of continental origin, and the deposition of CaCO3
-->We clarified by rewriting this sentence:
"As the oxide ratios are dependent on variations in the input of $CaCO_3$, (i.e. marine productivity) and preservation, as well as terrestrial input, (i.e. Al_2O_3, SiO_3, K_2O, TiO_2, and Fe_2O_3)," (lines 1203-1204 now)

Give details on "other elements".
-->We clarified by rewriting this sentence:
" the oxide ratios are dependent on variations in the input of $CaCO_3$, (i.e. marine productivity) and preservation, as well as terrestrial input, (i.e. Al_2O_3, SiO_3, K_2O, TiO_2, and Fe_2O_3)," (lines 1203-1204 now)

Elaborate on what way "this could affect the interpretation of the XRF data" (l. 438). I think you mean the postulation that XRF elemental ratios are primarily driven by riverine input of terrestrial material. Please say so in that case.
-->We intend to say that larger amounts of deposited CaCO3 from increased productivity would lower the PC1 values.
-->"this could affect the interpretation of the XRF data by lowering the PC1 values" (line 1208-1209 now)

Elaborate on why the data do not support this (l. 440)
-->"as the values from both indicate a lower proportion of $CaCO_3$ in the core at ~25 ka in comparison to ~7 ka." (line 1210-1212 now)

Rephrase "downcore variations in geochemistry" into "downcore variations in sediment composition" (l. 448)
-->Agreed, this is more clear. (lines 1250-1251 now)

Give more details on "significant sedimentation rates changes" (l. 453) and make a reference to a figure showing these changes.
--> "During MIS 2, calculated sedimentation rates were higher than in MIS 1 and 3-4, with values over 30 cm/ka at the start of MIS 2 and up to 48 cm/ka at 21 ka, and then a reduction to 20-25 cm/ka from 19 to 14 ka (Fig. 2)." (line 1269-1271 now)

Give details on the "exposed Agulhas bank", the timing and drivers of exposure. Give appropriate references. (l. 462)
-->Palaeo-Agulhas Plain (PAP). The PAP was exposed during periods of sea level lower than at present, during MIS 8, 6, and 2-4 (Cawthra et al., 2020b;Cowling et al., 2020). During the LGM, when the sea level was at its lowest, the PAP was covering 80 653 km^2 (Cowling et al., 2020). (lines 1299-1302 now)

Please revise these sentences as they are incomprehensive (l. 473-476)
-->They were revised for clarity. (line 1324-1329 now)

Use MIS to refer to time intervals (or "Late Holocene").

--> during MIS 5e and 5d and during the late Holocene (line 1325-1326 now)

Statement “This observation is confirmed with the cross wavelet transform, as indicated by the arrows in fig. S6.” is unclear (l. 516). More specification is needed in the main text.
--> “This observation is confirmed with a cross wavelet transform analysis (Fig. S8b), as indicated by the arrows, where arrows pointing towards the left indicate an anti-phase relationship.” (l. 1427-1429 now)

Specify what parameter is referred to here. (l. 555)
--> “This could be due to the Cape Fold Composite being more affected by shifts in sea level than core site MD20-3592 which records the hydrology of regional river catchments extending further inland.” (l. 1487-1489 now).

Give details on what “statistically significant amount of precessional forcing component” means (l. 577)
-->This sentence was cut.

Specify “terrestrial paleoenvironmental evidence” (l. 604)
-->We added: “rock hyrax middens” (l. 1547 now)

Specify “patterns” (l. 611).
-->This sentence was cut, and the section rephrased.

I find that key methodological information is missing or is unclear:

In section 3.1.1., specify the dimensions of the detector and the energies used? How was the core prepared (scraped clean, use of foil)? I am missing key aspects of the XRF methodology here.
-->The core preparation methods , and energies used are found in the supplementary material. The dimensions of the detector were added there as well.
-->”Detailed information on the core preparation and core scanning can be found in the “X-ray fluorescence (XRF) core scanning” section in the supplementary material.”

Rephrase “most easily interpretable signal” (“most reliable signal reflecting true sediment proportions” or similar).
-->Changed as suggested. (lines 440-441 now)

Name specifically how “previous study have used variations in sediment geochemistry” as this statement is too vague.
-->Corrected sentence: Previous studies have used variations in sediment geochemistry to infer changes in the proportion of marine versus terrestrial deposition (e.g. Ca/Fe ratio; Hahn et al., 2021a), or the proportion of physical and chemical weathering (e.g. Fe/K ratio; Govin et al., 2012; Ziegler et al., 2013), both of which can be linked , among other factors, to changes in rainfall. (l. 433-437 now)

“Rate” should likely also be rephrased into “proportion”.

-->Rate has been cut.

Section 3.1.2. specify how the sediments were digested/prepared for ICP-AES measurements? How were the samples introduced into the system? Where were the HR-ICP-MS analyses completed? How were the samples prepared for introduction into the mass spectrometer? What standards were used for the solution-based analyses? What is the accuracy/precision of the analyses? Or was a laser used throughout?
-->This information has been added. (l. 477-491)

Also, I am confused whether the first and second paragraph describe two separate ways of XRF calibrations and I recommend clarification/specification accordingly.
I also recommend that the authors describe their methods following an order of sampling, sample preparation, sample analyses, data processing/data quality check (if applicable).
-->We have amended the text to follow this order.

If "The results from VU were chosen for calibration because they included SiO2 which is the most abundant oxide in core MD20-3592." (l 237), then I am confused why the other method is included. Please clarify. The comparison of the two methods is inadequate and too short (237-239) as it is not clear what elements were compared and how the numbers given come about (here a comparative figure is needed). (at section 4.1.2)
-->We added the following sentences to explain why both data sets are included and we added a comparative table and figure in the supplementary material.
"The validity of the results obtained at VU are confirmed with the measurements done at the University of Bergen, as both methodologies show consistent results. The 18 replicate samples measured by both methodologies had results on average within 4% of each other, with an average standard deviation of 14% for the oxides discussed in section 4.1.1 (refer to Table 1, Table S1, Table S2, and figure S5)." (l. 778-782 now)

Line 156-158: These statements are very vague and unclear. Specify. What do you mean by "quantitative results"?
-->We changed the sentence to clarify: "calibrated to convert the core-scanner output to element concentrations. (l. 473-474 now)

How does this constrain "the interpretation of proxies"
and what "paleoclimatic information" is relevant?
-->These statements were not necessary and were cut.

Avoid over-describing: lines 181-190 overdescribe PCA and is redundant in the paper. The same level of detail was not applied to explain XRF scanning, ICP-AES and HR-ICP-MS analyses (and rightly so); hence, it should be removed for PCA.
-->These lines were removed as suggested.

Instead, explain in more detail what elements went into the PCA and what criteria were used to include or exclude elements.
Specify what counts as "strongest XRF counts", used to define the elements that went into the PCA.

--> “PCA was used on the normalized elemental counts of the XRF core scanning of the study core, on the elements with the highest measured average intensities (Ca, Fe, Sr, K, Ti, and Zr), as well as with Si, Rb, and Al which are associated with detrital material (Croudace and Rothwell, 2015; Govin et al., 2012).” (l. 541-544 now)

I also find lines 247-254 redundant and trivial. This is an over-description of the application of linear regressions.
-->These lines were removed as suggested.

The same applies to lines 311-319
-->These lines were cut, except for the parameters selected when using *undatable*
(The default values for the “xfactor” and bootstrapping percentage were set to 0.1 and 30%, respectively, and 10^5 simulations were used). (l. 418-420 now)

and to lines 487-490.
-->These 4 lines were condensed to 2 lines:
“Changes in eccentricity relate to the shape of Earth’s orbit, with a more elliptical orbit intensifying seasonality in comparison with a circular orbit (Maslin and Ridgwell, 2005).” (l. 1378-1380 now)

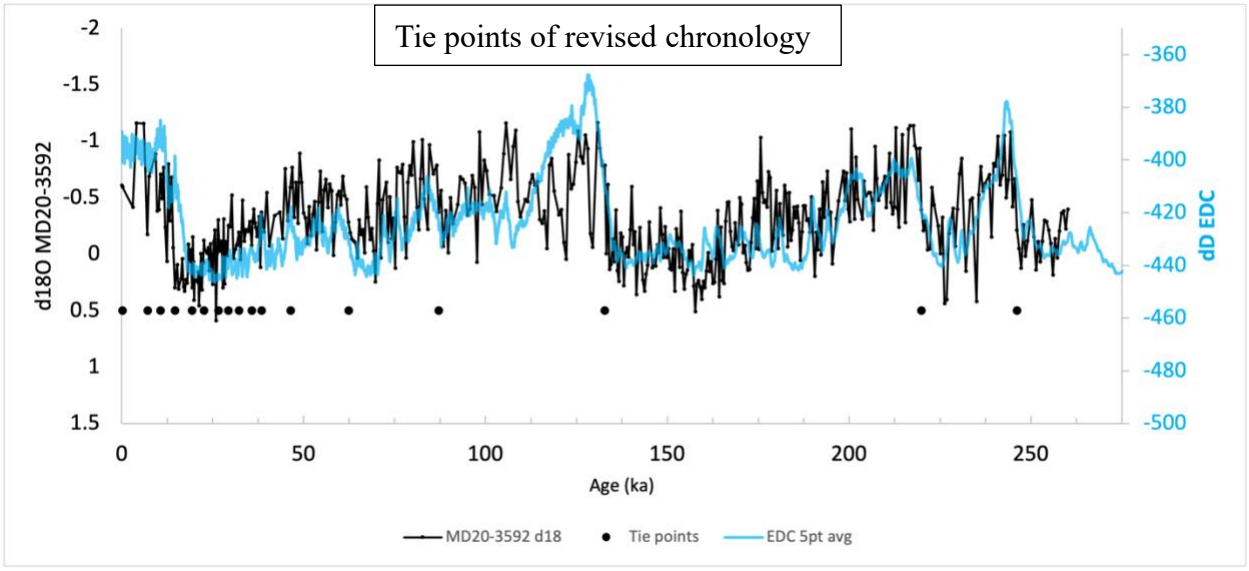
Also, the description of the orbital parameters in 517-521 and 526-529 can and should be more succinct.
-->517-521: This section has been cut
-->526-529: These 3 lines were condensed to 2 lines:
“Precession affects the distribution of solar insolation during the seasons, with precession being highly correlated with local summer insolation (Fig. S4).” (l. 1462-1463 now)

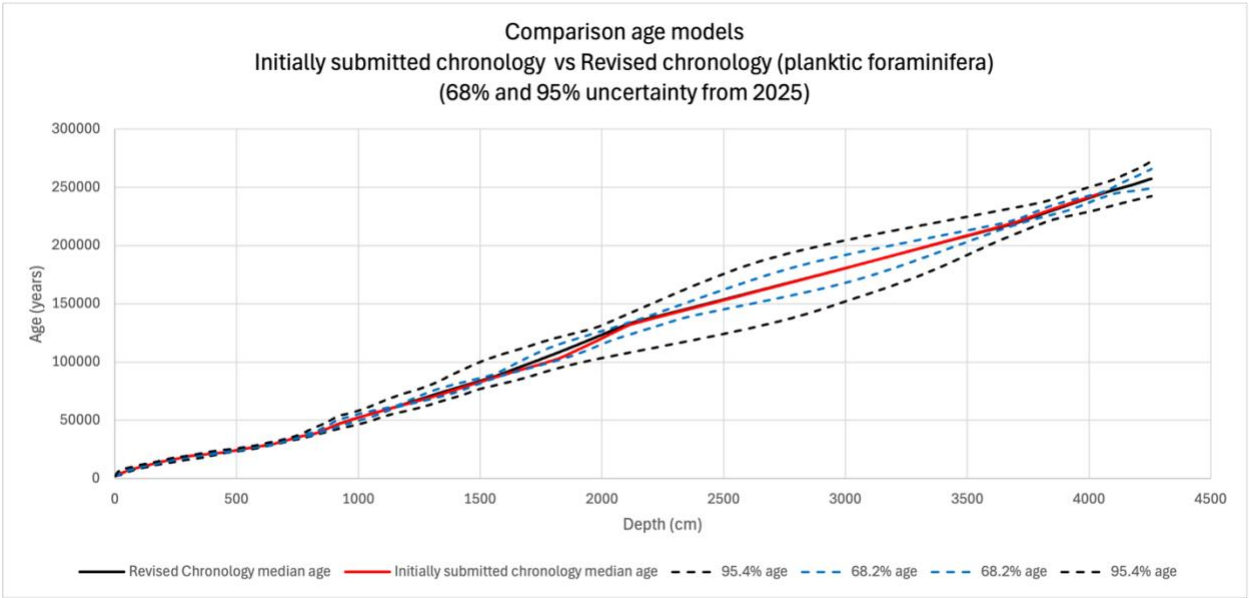
Age model:
The age model section definitely needs to go into the Methods section. It forms the basis for the spectral analysis and as such should not appear in the Results section.
-->It was moved to the methods section.

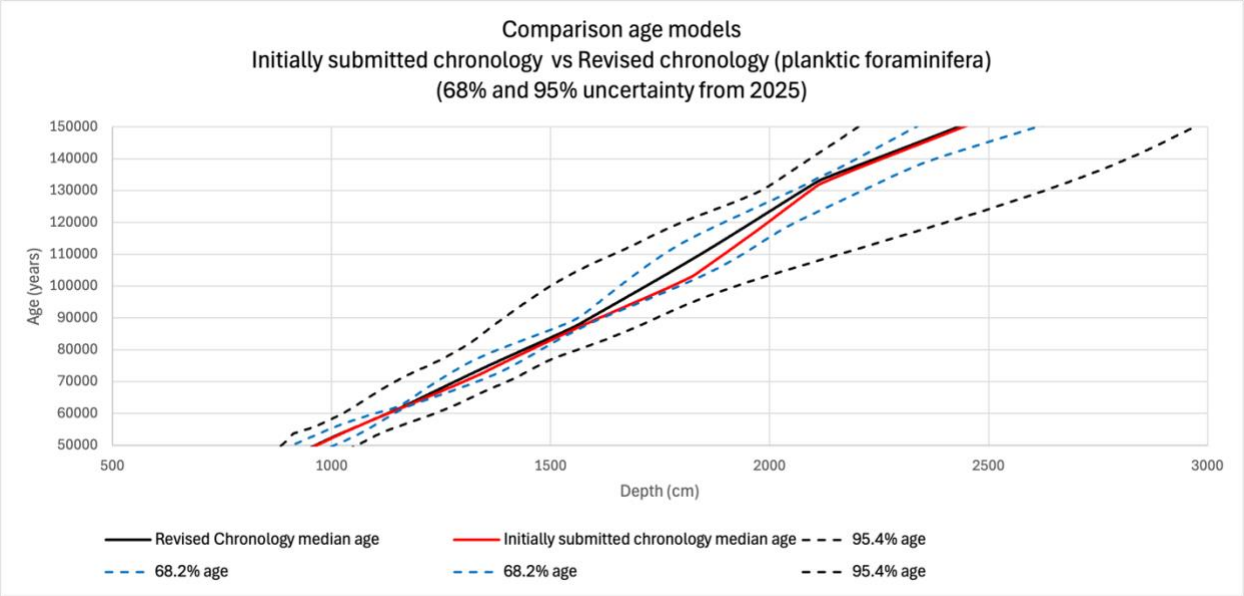
Also, this makes lengthy descriptions of XRF patterns against depth redundant as the reader is mostly interested in temporal changes in XRF elemental ratios only, as depths are difficult to link with ages for those who do not know the core inside out. This would make the paper much more streamlined and clearer, when section 4.1.1. will be shortened and the important observations will be highlighted against age.
-->The descriptions of XRF patterns against depth have been rephrased to age in section 4.1.1, and in figure 2 and 4 the depth scale has been replaced with the age scale. Both figures are also merged now.

I am also not convinced of the use of N. incompta d18O for pinpointing the transition from MIS5-4, and find the choice of different species for different intervals dubious. The N. incompta d18O data are awfully low in resolution and it is not clear why the G. ruber data is *particularly* noisy in that area and not in other areas where tiepoints are based on G. ruber d18O. Particularly, transitions do not seem to match between N. incompta d18O and G. ruber d18O within few

thousand years for instance at the MIS8-7 transition, which I find worrying. Please revise. I would strongly recommend to stick with G. ruber at the MIS5-4 transition accordingly. In addition, the G. ruber tiepoint at ~100 kyr is not convincing.
-->We agree with these comments. The chronology has been revised accordingly, with two new tie points solely based on *G.ruber* at 62 and 87 kyr. The 72 kyr tie point based on *N. incompta* has been removed, as well as the tie point at 102 kyr. The resulting age model is still within the age uncertainties of the original age model. See figures below. The first figure indicates the new tie points used. The second figure shows a comparison of the initially submitted chronology, and the revised chronology, with the age uncertainties of the initial chronology. The third figure shows a close-up between 50 ka and 130 ka to show how the age models differ.







Please specify why AICC2012 was chosen as tuning target timescale and not AICC2023 (Bouchet et al., 2023) – the updated ice age scale for EDC. Please elaborate and adjust accordingly.
-->The age model was produced before AICC2023 was available. The new age model uses the AICC 2023 chronology for EDC.

Principal component analyses (section 4.3):
Line 362: This first sentence is too trivial and should be rephrased. PC1 *always* explains the majority of the variance in the dataset.
-->Rephrased sentence: The first principal component (PC1), incorporating 60% of the variance in the XRF record (~60%), was retained for further examination. (l. 826-827 now)

I also find that the scree-plot in Figure 7a is obsolete because variances of PC1-3 are given in the text and >PC3 is not relevant in the study.
-->This figure has been cut. As well as the loadings of PC2 and PC3. It is now figure 4.

Line 364-366: I wonder why the loadings PC2 and PC3 are described, as they do not play a role in the remaining study. The text can accordingly be removed.
-->These lines were removed as suggested

Please use “positive loading” instead of “positive pole”.
-->Agreed.

Please focus here on the most relevant findings, otherwise it becomes too confusing.
The assertion that “Ratios like Fe/K display a comparable pattern with PC1, suggesting that PC1 can be used to represent terrestrial element variability.” is incomprehensive. First of all, this is not demonstrated in a figure. I therefore recommend to do a comparison of Fe/K and other

relevant XRF elemental ratios (Fe/Ca shown in Fig. 9) together with PC1 in Figure 7e in landscape as in Figure 2 and 4.
-->A figure has been created as suggested to compare the ratios with PC1, using the age scale (fig. 5).

Additionally, the use of the XRF elemental ratio Fe/K as indicator of "terrestrial element variability" is unclear to me and insufficiently explained. What is meant specifically by variability (supply? Relative proportion of different terrigenous sediment types? Please specify.)?
-->We agree that this was not clear. The quoted statement has been replaced by: "variability in and within the supply of sediments originating from land (Fig. 5)." (l. 832-833 now)

This comes back to the issue that elemental ratios are not carefully introduced and hence because Fe and K were introduced as terrigenous elements, how Fe/K and accordingly PC1 is interpreted is not clear here.
-->The elemental ratios are now introduced in the introduction.

Figure 7. Please specify what dataset the principal component analysis is based on and from what core this dataset stems.
-->this has been added: "on the normalized elemental counts of the XRF core scanning of the study core" (l. 541 now)

Spectral- and wavelet analysis (section 4.4):

Please specify on the basis of which "the power spectra of eccentricity" and "the power spectra of obliquity" was calculated. Is this based on an astronomical solution of Laskar?
-->Yes it is: the following reference was added (Laskar et al., 2004)

Line 386-399: It would also help the reader if Marine Isotope Stages are also given here rather than using "older than 175 kyr" and "starting from the bottom of the core" and "in the younger half of the core".
-->The MIS stages are also given now. (lines 1099-1111 now)

The same applies to l. 696-697,
-->The MIS stages are also given now. (lines 1829-1830 now)

l. 710-719 and
-->The relevant MIS stages and substages were mentioned. (lines 1951-1961 now)

l. 550-552.
--> This section has been re-written. The relevant MIS stages and substages were mentioned.

Line 389. Elaborate on the "notable differences in amplitude" and refer to a figure. It does not suffice to simply say that. If these differences do not matter for the later discussion, remove the statement here.

-->The statement was removed as suggested. These differences are not discussed further down in the discussion.

Specify what window size has been used to calculate the Gaussian filters.
-->The window size (bandwidth) is in the supplementary material, to avoid over-describing.

Line 396-399: the information on the cone of influence is in my review redundant. This is general knowledge.
-->Agreed, this sentence has been cut.

Figure 8: Specify on what core and on what dataset the timeseries analysis was conducted.
-->Correction: Frequency analysis of PC1 of core MD20-3592. This is now figure 6

PCI should read PC1.
-->Agreed and it was corrected.

AR(1) as abbreviation is not defined: please revise.
-->Correction: The first-order autoregressive red noise model (AR(1))…

"See text for explanation" is redundant and can be removed.
-->It was removed.

"It can be seen that between ~25 and ~125 ka the cyclicity centred at ~23 ka dominates, while deeper in the core the cyclicity centred at ~41 ka dominates. The 122 ka cyclicity is outside the cone of influence." is also redundant as this is explained in the text.
-->It was removed.

For Figure 8b, give reference of the orbital data.
-->Done: (Laskar et al., 2004).
This is now figure 6.

The statement "The morlet wavelet is used as it usually performs best (Hammer et al., 2001)." is odd in the figure caption as this should be clear from the Methods section and it remains unclear what "usually" means and whether this also applies to the study of Purcell et al.
-->This sentence was cut, and a new sentence was put in the methods section.
"The morlet wavelet is used as it minimizes ripple effects and retains the resolution of the original signal (Cohen, 2019)."

Avoid (lengthy) descriptions of figures in the main text. All information relevant to understand figures must become clear from the figure captions. In my view the authors should choose to link the observation and/or the finding with the figure in brackets after the statement. Please revise E.g. lines 208, 235, 246, 254, 275, 378, 394, 542-546, 584 This would make the reading flow much more efficient and the study clearer.
-->These lines have been revised.

Discussion:
Comprehensibility of the discussion: Descriptions are often not streamlined and long-winded. Often the authors attempt to explain changes seen in other records rather than focusing on the study site.
-->This has been addressed, with an increased focus on the study site.

L. 434-540: The authors argue that XRF elements of terrigenous type are primarily driven by input fluxes via rivers. However, relative changes in the proportion in biogenic carbonate and biogenic silica, changes in carbonate preservation and changes in winnowing/focusing could also play a role. The assumption of constant or low fluctuations in carbonate flux out of the euphotic zone given strong changes in alkenone fluxes (e.g., Martínez-Garcia et al., 2014) seems misleading. The arguments based on CaO percentages in the study core are weak because the data are very low in resolution and XRF Ca intensity counts could be influenced by water content variations. Here I recommend that the authors make a thorough assessment of possible variations in the export flux of carbonates, and show the CaO assessment for the full core length.
-->The following information has been added to address the comment above:
"Glacial-interglacial variability in upper ocean biogeochemistry and productivity in the southern Indian Ocean has been observed (Tangunan et al., 2021) but there is no indication of a 23 ka cycle in those processes. This excludes, based on the current knowledge, any surface water productivity changes that would drive the $CaCO_3$ content of core MD20-3592 and bias the terrigenous signal on a precessional timescale. Additionally, the core is not located in an upwelling zone such as the Benguela System where we would expect nutrients to promote high biological productivity and increase carbonate production on precession timescales (West et al, 2004)." (line 1213-1220 now)
-->the CaO assessment has been added to the supplementary material in figure S1.

Also, the authors discuss possible influences of carbonate preservation changes based on change in circulation, namely the influence from GPDW. However, NADW and AABW have likely a much stronger influence on the carbonate ion saturation of the study site (e.g., Hodell et al., 2001; Gottschalk et al., 2018), which are not addressed. More information is needed here, to exclude the effect of variations in the carbonate preservation on the sediment composition at the study site (e.g., Hodell et al., 2001; Gottschalk et al., 2018).
-->This has been added to address the comment above:
"In addition to the GPDW potentially influencing the study site, the AABW could also have affected the study site with changes in calcite preservation (Fig. 1). Gottschalk et al. (2018) and Hodell et al. (2001) found higher burial of carbonate during glacials in the deep Cape Basin. This can be explained by the findings of Rickaby et al. (2010), which indicate that during glacials, there was an increased alkalinity (reduced corrosiveness) in the deep waters of the Weddell Sea, near where the AABW forms. Thus, the enhanced carbonate accumulation recorded in the Atlantic sector of the Southern Ocean during glacials (Rickaby et al., 2010) can be explained by a reduction in dissolution of the carbonates by the AABW. Conversely, for sites bathed in NADW in the Atlantic sector of the Southern Ocean, calcite dissolution was elevated during glacials (Rickaby et al., 2010). This can be explained by the AABW reaching further norward and mixing with the NADW during glacials, causing the NADW to be more corrosive (Rickaby et al., 2010). However, both these changes in the NADW and AABW occurred on glacial-interglacial timescales, and

this suggests that the composition of these water masses is not driving the changes in the XRF data of core MD20-3592 on a precessional timescale." (line 1253-1266 now)

L. 453: How can high sedimentation rates unequivocally explain increased sediment supply by rivers? How about increased aeolian dust input, increased productivity through dust driven input, increased sediment focusing? How about ACC changes? I would expect that other factors are unequivocally excluded before making such statement.
-->We added statements addressing aeolian dust input, productivity linked with dust, sediment focusing, and ACC changes.
→"However, increases in dust inputs during glacials could stimulate biological productivity in the waters overlying the study site. This could decrease the PC1 values by increasing the proportion of Ca of biological origin in the sediment, and PC1 is indeed observed to drop towards the end of MIS 2 (between 25 and 18 ka). However, PC1 remains negative throughout most of MIS 1, which is inconsistent with this hypothesis.. Another factor that can affect the sedimentation rates at the study site is sediment focusing, which is the redistribution of sediment on the seafloor by bottom currents. Starr et al. (2025) found evidence of a faster near-bottom flow speed of the CDW, in comparison with the Holocene, between ~37-15 ka in cores MD02-2588 and IODP U1475 in the Agulhas Basin. This timing is comparable to the higher sedimentation rate estimated between ~37-15 ka in core MD20-3592. The flow speed rose up to ~10 cm/s (Starr et al., 2025), which is a speed that can induce selective deposition of sortable silts (McCave et al., 2017). This increased bottom flow speed could therefore be a factor that drives the increased sedimentation at the core site during MIS 2. However, with the current evidence available, this process cannot be disentangled from increased riverine discharge." (l. 1355-1368 now)

In the discussion section 5.2, the authors have chosen to compare their record to other sediment cores and archives, one by one. There is a lot of potential to streamline this because commonalities could be pointed out first, essentially confirming postulations made based on the study core, then followed by differences and how they can be explained. I find the one-to-one discussion of the different archives with the study core long-winded. At the moment, some of the comparisons are insufficient.
For instance, l. 580 explanation is needed why "there are differences between the study core and MD96-2048".
-->These changes have been made in the text as suggested to streamline the text (commonalities pointed out first then followed by differences and how they can be explained).

Purcell et al. assess the role of obliquity on the sediment composition in their study core. The find that "The inconsistent phasing makes it difficult to explain a mechanistic relationship between obliquity and the signal observed in MD20-3592." This is somewhat disappointing and I wonder what the scientific hypothesis was that was tested based on the filtering. Please elaborate, if possible. Maybe a more important outcome would be that obliquity did *not* have a strong control on the sediment composition at the study site via shifts its control on the southern hemisphere westerlies (Timmermann et al., 2014). Would that be a valid conclusion?
-->Response: We still think that the in-phase relationship between the PC1 and obliquity, although lagged, between 260 ka and 125 ka is an important finding that can potentially be explained by the already stated mechanism in the text line 658 onwards: " a stronger cross-

equatorial transport of moisture towards the summer hemisphere when obliquity is high (Bosmans et al., 2015; Daniau et al., 2023). Bosmans et al. (2015) observed in model experiments that during obliquity maxima, surface winds towards the summer hemisphere are strengthened, leading to an increase in moisture transport over the Indian Ocean and increased precipitation over part of the southern African summer monsoon region."
-->We added the following statement to the text: "The findings in our study core would contrast the expectations associated with the SHW influence on the region during low obliquity phases ie. stronger westerlies and storm tracks and hence more precipitation over the YRZ during periods of lower obliquity. The pattern found in MD20-3592 between ~260 ka and ~125 ka (MIS 8-5e) shows the opposite, i.e high PC1 values during obliquity maxima." (line 1429-1433 now)
-->We additionally added the following statement to the text:
"Higher obliquity could influence sea surface temperatures in the South Indian Ocean, altering the strength of the Agulhas Current. A warmer Agulhas Current may lead to enhanced evaporation and regional convective precipitation, particularly in the southern Cape (Tim et al., 2023). Caley et al. (2011) found obliquity-driven SST variability in the Agulhas Current with higher ocean temperatures aligning with obliquity maxima. These Agulhas Current dynamics on obliquity timescales may serve as an additional factor linking the obliquity signal identified in the PC1 data of the core." (lines 1433-1439)

Also, the wavelet analysis shows that the early record indicates a dominance of obliquity and the later record a dominance of precession. This is not mentioned in the discussion and remains unexplained. Please elaborate on that in the discussion.
-->This is discussed in the second paragraph of section 5.1.2.
However, we added the following sentence for more clarity:
--> The ~41 ka frequency detected in the core (Fig. 6a) is dominant from the bottom of the core up to ~128 ka (MIS 5e: Fig. 6c), while the ~23 ka frequency dominates from ~128 ka (MIS 5e) to ~40 ka (MIS 3a). (l. 1416-1418 now)

The role of the southern hemisphere westerlies is not well addressed in the discussion. The statement in l. 650 leaves a role of the SHW unclear. A better integration of the possible changes in the SHW on glacial-interglacial timescales (Gray et al., 2023; Toggweiler et al., 2006) and/or driven by obliquity (Timmermann et al., 2014) is needed in the discussion.
-->These references, and additional references, have been used in the revised discussion to expand on the role of the SHW on the hydroclimate in the study area. (lines 1598-1739)

Minor comments:
L. 72: Please revise the following statement "The hydroclimate of southern Africa is recorded in multiple terrestrial and marine climate archives (e.g., Chase, 2021). However, the southern part of South Africa is poorly resolved, especially on orbital timescales (Fig. 1). So far, to the author's best knowledge, there are no published marine sediment cores records offshore the Southern Cape which span the last two glacial cycles." It cannot be the only motivation of the study that this record is the first one right off the Cape and that spans the last two glacial cycles. There are other cores nearby that are similarly valuable although they only span one glacial cycle. Rather mention here what the open questions are that the new core allows to address!

Also, why is this the “first high-resolution and continuous record of terrestrial climate and palaeoceanographic conditions for the study region” (l. 76)? There are a number of other similarly important records in Figure 9 and 10, including Simon et al., 2015 and Caley et al., 2018 which mainly show the same sedimentary features on a precession scale. Please refrain from such statements even if it is the first covering the last two glacial cycles. Another core will be the first one to cover two and a half glacial cycles, which is trivial.
-->These statements have been revised.
“The hydroclimate of southern Africa is recorded in multiple terrestrial and marine climate archives (e.g., Chase, 2021). However, the southern part of South Africa is poorly resolved, especially on orbital timescales (Fig. 1). This study aims to extend the spatial coverage of palaeoclimatic reconstructions in the region, by providing a high-resolution and continuous record of terrestrial climate for the highlighted river catchments (Fig. 1). (lines 193-197 now)

Figure 1. Text on land (river labels) is not legible at all.
-->In the full resolution figure the text is legible.

I also recommend to list all sites from Figure 1 (sediment cores, speleothems, archaeology) with associated metadata (lat, long, depth, references, interval covered) in a table for clarity. At the moment symbols overlay each other and it is not clear what the different sites refer to (caves, archaeological sites and craters are not clearly separated).
-->This table is now in the supplementary material.

Line 124-126: statement requires reference(s)
-->(Chase and Meadows, 2007; Fig. 1). (l. 302 now)

Line 131-133: statement requires reference(s)
-->(Chase and Meadows, 2007; Fig. 1). (l. 348 now)

Figure 2. specify that the figure shows “XRF element ratios from sediment core xxx.
-->Done

What does CCSF refer to?
-->It refers to composite core below sea floor, taken from Babin et al. 2020. It was removed as the study core is not a composite record.

“Lower variability in d and e causes the running average to overlap closely with the full resolution.” in the figure caption can be removed.
-->Done.

Why does the figure not show logarithmic ratios of the elemental ratios as announced in the Methods section?
-->The simple ratio was shown as it is the starting point before calibration.

The abbreviation cps should be defined.
-->Done.

Line 245 and 338: Tables have headers and not captions.
-->The captions were moved to headers.

Line 259: Specify what “some peaks” and “general variability” refers to.
-->This sentence was cut, because after re-examining the figures, it did not represent them well.

Also, this sentence is entirely unclear: “This shows that the calibration yields effective individual elemental concentrations”. Please specify and elaborate.
-->This sentence was cut, as it was unnecessary and lacked support.

Line 277: please specify the correct name for the radiocarbon facility at ETH Zurich.
The name was changed from “ETH Zurich radiocarbon laboratory, Switzerland” to “Laboratory of Ion Beam Physics Zurich, at ETH Zurich in Switzerland.” (l. 380 now)

Line 279: specify what is meant by “nearest 5 measured points”
-->obtained by averaging the reservoir correction of the 5 measurements which are closest to the coring site (Reimer and Reimer, 2001). (l. 382-383 now)

Line 284: define the use of d18O- and dD-notation.
-->We added this sentence: The $\delta^{18}O$ values are reported relative to the Vienna Peedee Belemnit scale (VPDB), and the δD values are reported relative to the Vienna Standard Mean Ocean Water (VSMOW). (l. 392-394 now)

Line 304: µg instead of mg
-->15 and 100 µg. (l. 370 now)

Line 320: specify how the density of the sediment was determined.
-->This has been added. (l. 423-427 now)

Table 2: include CaCO3 weights for each sample
-->This has been added.

Figure 3. Why are the XRF element ratios (ln or not) not plotted against the oxide analyses? I understand from the text that this was used to calibrate the XRF data, and not the regression lines that are actually shown in Fig. 3. I must say that I am quite confused at this stage. The grey shading is not visible in the figure.
-->The results from the oxide analyses were converted to elemental ratios, for easier comparison with the XRF elemental measurements which are also expressed as elemental ratios.
-->The grey shading has been made more visible.

Figure 4. See my comments on Figure 2. Figure 2 and Figure 4 should be combined; there is a lot of redundancy there.
Figure 5 and Figure 6 e show some redundancy too. Please combine the figures accordingly.
-->These figures have been combined.

Line 331: "EDC dD values from the Antarctic EDM ice core" is inconclusive. Please correct. EDM should read EDC, but then it would be a double mention.
-->this part of the sentence was shortened to "the EDC δD values". (l. 369 now)
-->The EDC record has now been introduced above: "…with the δD record of the Antarctic ice core EPICA Dome C which comes from the Antarctic sector to the South of the coring site (EDC…" (l. 388-389 now)

Line 492: period instead of frequency
-->Done. (l. 1382 now)

Line 584-591: this paragraph contains a lot the phrase "this record". It is hard to follow. Please revise accordingly.
-->This has been addressed by referring to the Tswaing Crater record. (l. 1515 now)

This paragraph also requires a concluding sentence as the results of Engelbrecht et al. (2019) are not effectively linked with the observations.
-->After revision, the last two sentences were cut as they do not add to the discussion.

Line 595: correct MD20-3992
-->Done

Line 601-603: provide reference of the dataset in the first sentence.
-->This reference has been added: Collins et al., 2014 (l. 1541 now)

Line 626-630: add references in association with external datasets.
-->These references have been added to the re-written section: (Collins et al., 2014); (Dupont et al., 2022; (Stuut et al., 2002). (l. 1516-1542 now)

Figure 9. For the EDC dD record Jouzel et al. 2007 should be referenced.
-->Done. Figure 7 and 8 now.

Rephrase "PC1 of XRF".
-->Changed to "PC1 of core MD20-3592".

Figure 10. Specify what is compared against what in the introductory line.
-->Comparison of core MD20-3592 with global and western south African records.

Rephrase "PC1 of XRF".
-->Changed to "PC1 of core MD20-3592". (figure 7 caption, and figure 8)

For the EDC dD record Jouzel et al. 2007 should be referenced.

-->Done.

Provide the references for Figure 10c, d and e in the figure caption.
-->Done: Dupont et al., 2022, Collins et al., 2014, Stuut et al., 2002.
Figure 8 now.

The MD20-3592 data superimposed in c-e is hardly visible.
-->It was made more visible.

References:

Bouchet, M., Landais, A., Grisart, A., Parrenin, F., Prié, F., Jacob, R., Fourré, E., Capron, E., Raynaud, D., Lipenkov, V.Y., Loutre, M.F., Extier, T., Svensson, A., Legrain, E., Martinerie, P., Leuenberger, M., Jiang, W., Ritterbusch, F., Lu, Z.T., Yang, G.M., 2023. The Antarctic Ice Core Chronology 2023 (AICC2023) chronological framework and associated timescale for the European Project for Ice Coring in Antarctica (EPICA) Dome C ice core. Clim. Past 19, 2257–2286. doi: 10.5194/cp-19-2257-2023

Caley, T., Kim, J.H., Malaizé, B., Giraudeau, J., Laepple, T., Caillon, N., Charlier, K., Rebaubier, H., Rossignol, L., Castañeda, I.S. and Schouten, S., 2011. High-latitude obliquity as a dominant forcing in the Agulhas current system. *Climate of the Past*, *7*(4), pp.1285-1296.

Gottschalk, J., Hodell, D.A., Skinner, L.C., Crowhurst, S.J., Jaccard, S.L., Charles, C.D., 2018. Implications of past carbonate preservation events in the deep Southeast Atlantic (Cape Basin) for Atlantic overturning dynamics and the global carbon cycle. Paleoceanogr. Paleoclimatology 33, 1–21. doi: 10.1029/2018PA003353

Gray, W.R., de Lavergne, C., Jnglin Wills, R.C., Menviel, L., Spence, P., Holzer, M., Kageyama, M., Michel, E., 2023. Poleward Shift in the Southern Hemisphere Westerly Winds Synchronous With the Deglacial Rise in CO2. Paleoceanogr. Paleoclimatology 38, 1–23. doi: 10.1029/2023PA004666

Hodell, D.A., Charles, C.D., Sierro, F.J., 2001. Late Pleistocene evolution of the ocean's carbonate system. Earth Planet. Sci. Lett. 192, 109–124. doi: 10.1016/S0012-821X(01)00430-7

Martínez-Garcia, A., Sigman, D.M., Ren, H., Anderson, R.F., Straub, M., Hodell, D.A., Jaccard, S.L., Eglinton, T.I., Haug, G.H., 2014. Iron Fertilization of the Subantarctic Ocean During the Last Ice Age. Science. 343, 1347–1350. doi: 10.1126/science.1246848

Rickaby, R.E., Elderfield, H., Roberts, N., Hillenbrand, C.D. and Mackensen, A., 2010. Evidence for elevated alkalinity in the glacial Southern Ocean. *Paleoceanography*, *25*(1).

Tangunan, D., Berke, M.A., Cartagena-Sierra, A., Flores, J.A., Gruetzner, J., Jiménez-Espejo, F., LeVay, L.J., Baumann, K.H., Romero, O., Saavedra-Pellitero, M. and Coenen, J.J., 2021. Strong

glacial-interglacial variability in upper ocean hydrodynamics, biogeochemistry, and productivity in the southern Indian Ocean. *Communications Earth & Environment*, *2*(1), p.80.

Timmermann, A., Friedrich, T., Timm, O.E., Chikamoto, M.O., Abe-Ouchi, A., Ganopolski, A., 2014. Modeling obliquity and CO2 effects on southern hemisphere climate during the past 408 ka. J. Clim. 27, 1863–1875. doi: 10.1175/JCLI-D-13-00311.1

Tim, N., Zorita, E., Hünicke, B., and Ivanciu, I.: The impact of the Agulhas Current system on precipitation in southern Africa in regional climate simulations covering the recent past and future, Weather Clim. Dynam., 4, 381–397, https://doi.org/10.5194/wcd-4-381-2023, 2023.

Toggweiler, J.R., Russell, J.L., Carson, S.R., 2006. Midlatitude westerlies, atmospheric CO2, and climate change during the ice ages. Paleoceanography 21, 2005. doi: 10.1029/2005PA001154

West, S., Jansen, J.H.F. and Stuut, J.B., 2004. Surface water conditions in the Northern Benguela Region (SE Atlantic) during the last 450 ky reconstructed from assemblages of planktonic foraminifera. *Marine Micropaleontology*, *51*(3-4), pp.321-344.

Behrensmeyer, A.K., 2006. Climate change and human evolution. Science 311, 476-478.
Chase, B.M., Chevalier, M., Boom, A., Carr, A.S., 2017. The dynamic relationship between temperate and tropical circulation systems across South Africa since the last glacial maximum. Quaternary Science Reviews 174, 54-62.
Chase, B.M., Faith, J.T., Mackay, A., Chevalier, M., Carr, A.S., Boom, A., Lim, S., Reimer, P.J., 2018. Climatic controls on Later Stone Age human adaptation in Africa's southern Cape. Journal of Human Evolution 114, 35-44.

Faith, J.T., Chase, B.M., Avery, D.M., 2019. Late Quaternary micromammals and the precipitation history of the southern Cape, South Africa. Quaternary Research 91, 848-860.

Faith, J.T., Chase, B.M., Pargeter, J., 2024. The Last Glacial Maximum climate at Boomplaas Cave, South Africa. Quaternary Science Reviews 329, 108557.

Sealy, J., Lee-Thorp, J., Loftus, E., Faith, J.T., Marean, C.W., 2016. Late Quaternary environmental change in the Southern Cape, South Africa, from stable carbon and oxygen isotopes in faunal tooth enamel from Boomplaas Cave. Journal of Quaternary Science 31, 919-927.
Sealy, J., Naidoo, N., Hare, V.J., Brunton, S., Faith, J.T., 2020. Climate and ecology of the palaeo-Agulhas Plain from stable carbon and oxygen isotopes in bovid tooth enamel from Nelson Bay Cave, South Africa. Quaternary Science Reviews 235, 105974.