Manuscript titled " Evolution of deep-water circulation in the North-East Atlantic during the latest Miocene warming" by Karatsolis et al., et al. [egusphere-2026-3764] present an original unpublished major element data (XRF-scan) and astronomical tunning for site U1564 (Gardar drift) between 5.0 to 6.2 Ma. A large number of studies have already shown that Gardar drift is a great site for paleaoceanographic studies aimed at reconstructing the AMOC and the study period, which includes the Miocene/Pliocene transition, is a key moment when major climatic, tectonic and global thermohaline circulation changes of maximum interest took place.

The data obtained is high resolution and looks fantastic. Astronomical tunning is base in magnetostratigraphy (only two tie points) and Carbonate content obtained from major element data (Ca/Ti ratio) and shipboard-measured $CaCO_3$.

The greatest weakness of the presented study is that authors base all their work on element composition obtained by XRF-Scan (mainly Ca/Ti, and slightly Zr/Rb and Ti/K ratios). With these data, the authors have done a magnificent job but at the same it is a major limitation for the discussion and the conclusions that can be drawn. The study also is based on a previous paper at the same site by Sinnesael et al., 2025 that covers a different period, which gives greater robustness to the work presented

We thank the reviewer for recognizing the quality of the datasets. We agree with the limitations mentioned, which we now acknowledge in the revised manuscript. We also provide additional records (e.g., namely XRF ratio cross-plots and time series, micropaleontological observations, as well as sedimentation rates following a minimal tuning strategy) to support our arguments based on the reviewer's recommendations.

Despite all the issues presented, I believe that the conclusions obtained by the authors are in line with the nature of the data and the range of error of the age model. Three intervals are distinguished that affect the evolution of the AMOC and that are similar to those observed in other regions on a global scale, only for this reason I think it is worth publishing this study although we must wait a little longer to have more data (SST, oxygen isotopes in forams, a detailed study of carbonate, increase age model tie point etc...) to reach more robust conclusions, as the authors point out in their conclusions.

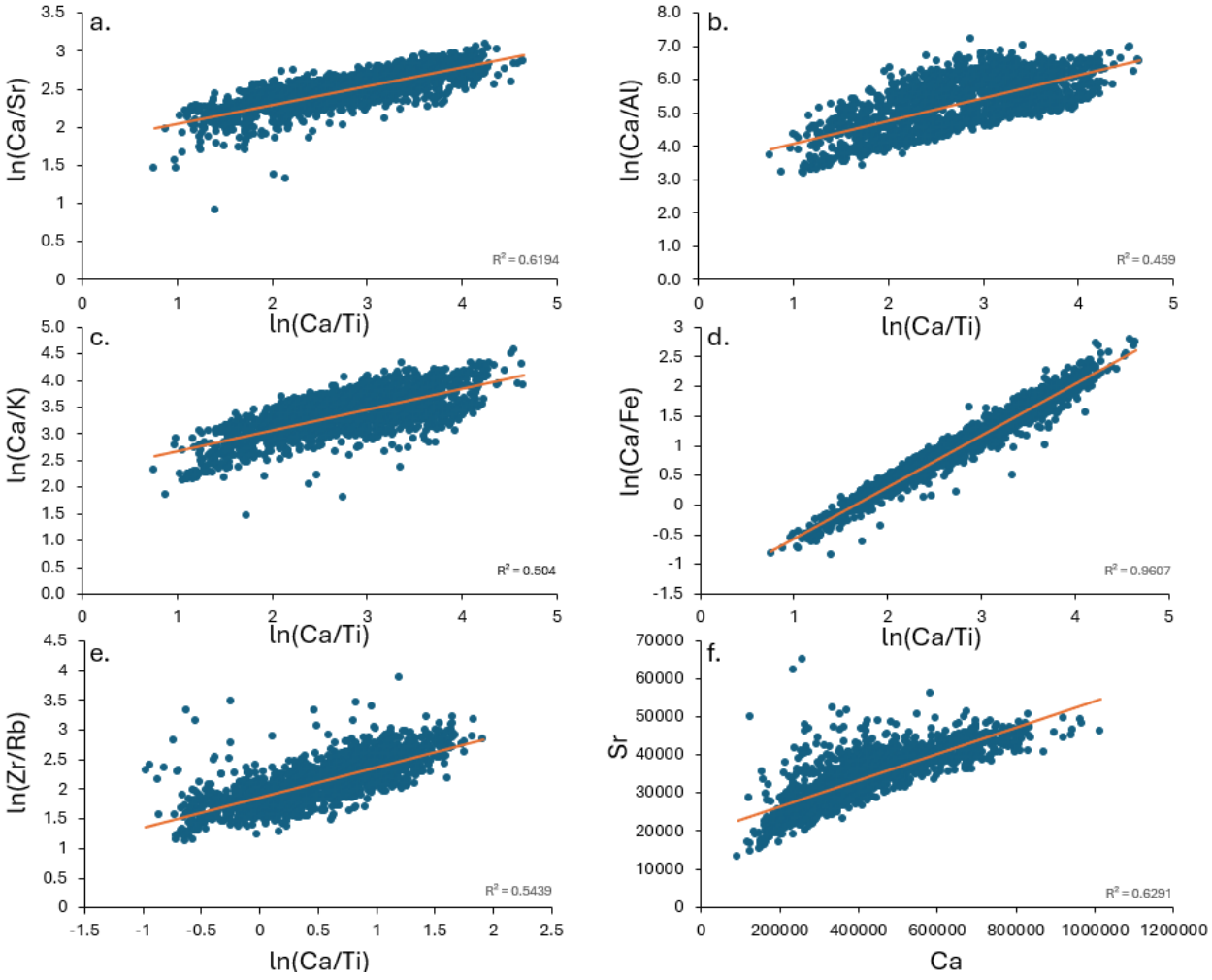
However, at this moment I can only recommend publication after minor/major revision because the whole interpretation about the presence of waters originating in the southern hemisphere that promote corrosion in carbonates seems to me too poorly supported. To add something more in this regard, having only XRF-Scan data available, the authors could explore the correlation between Sr and Ca along the control or the Ca/Sr ratio. In both cases it can give us an idea of what may be happening with carbonates. Plotting this elemental ratio could be a nice support for carbonate discussion.

We agree that interpreting the reduction in carbonate content as dissolution from a water mass sourced on the southern hemisphere is not substantially supported by the existing dataset. In line with comments provided by the other two reviewers, we have now toned down the interpretation of southerly sourced waters contributing to dissolution, and just mention this as a possibility, rather than an established climatic feature. The discussion has been restructured accordingly.

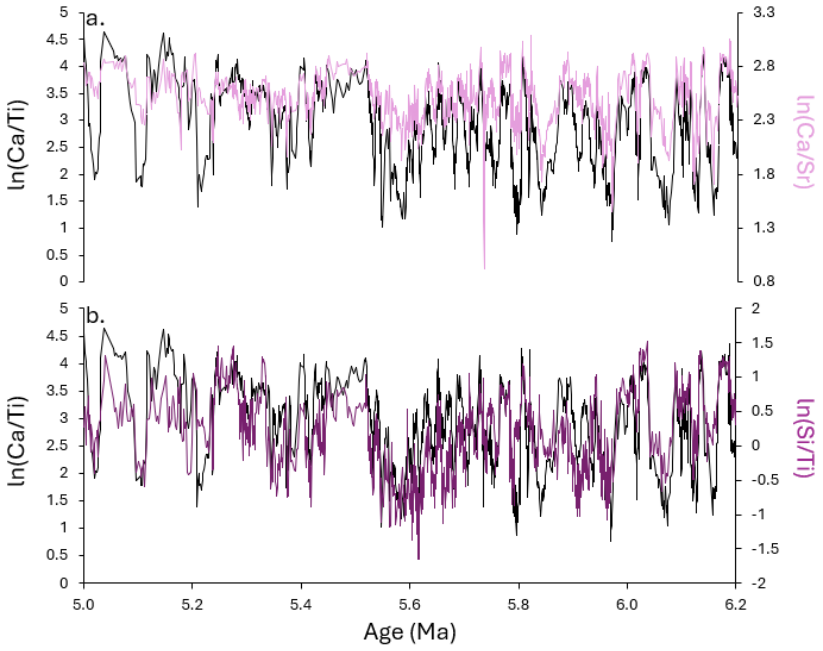
We have also calculated the Ca/Sr ratio and presented the time series of this record, as well as cross plots between Ca and Sr and ln(Ca/Sr) and ln(Ca/Ti) (the record we used to estimate $CaCO_3$ content) in Supplementary Figures S2 and S4. Overall, both the elements and two ratios show moderate to

strong correlation (R^2=0.62), confirming that they both reflect similar processes. Some nuances based on the correlation for each interval are discussed at a later comment.

Methods: "The natural logarithms of the elemental ratio of Ca/Sr (ln(Ca/Sr)) and Si/Ti (ln(Si/Ti)) were estimated to assist with the decoupling between processes related to the changes in $CaCO_3$ preservation and paleoproductivity. The former ratio (ln(Ca/Sr)) has been previously used in North Atlantic sediments to identify changes in detrital carbonate input (relatively depleted in Sr), during which carbonate content increases but Sr doesn't (Hodell et al., 2008).



New Figure S2



New Figure S4

It is also not clear whether the Ti/K ratio is associated with changes in Iceland-sourced terrigenous input (as proposed in previous studies) or with changes in the ratio of fine to coarse material associated with the activity of the currents that give rise to the contourite. The two ratios (Zr/Rb and Ti/K) have a very high similarity (Fig. 7) during the Interval I and II, and only show some differences during interval III. The authors could include a simple figure to indicate how an intense/weak current contributes more or less material from Iceland or if, on the contrary, both ratios depend on the current intensity and therefore on the concentration between fine (Rb and K) or coarse material (Ti and Zr).

We use these ratios based on previous interpretations (provided as references in methods and discussion). Based on these interpretations, stronger currents are reflected in the Zr/Rb and northern Icelandic origin of grains in the Ti/K. With the assumption that the related currents that are linked to deposition in the Gardar drift reflect the ISOW, stronger currents and northern sources could be correlated. We now provide a cross-plot correlation for the natural logarithm of the two ratios in the supplement (Figure S2e). Moderate correlation is shown (R^2=0.54), demonstrating that these mechanisms are working in tandem, but they are not identical. However, the problem of decoupling between the two processes is more complex and remains, and decoupling between the mechanisms controlling the changes in these two proxies would need a dedicated grain size and provenance analysis, which is not provided in this study. For this reason, we do not provide a figure showing how stronger/weaker currents contribute material, since we do not have enough evidence to distinguish between provenance and current intensity.

In addition, in table 1 the authors assume that warm moments result in a higher carbonate content in the record. However, the Ca/Ti ratio in the Atlantic represents in most cases the dilution of carbonate by the detrital material (instead of paleoproductivity, even more in a contourite drift setting) and taking into account the dramatic changes that occur during the period studied, I dare to predict that when the authors have paleo-SST or stable isotope data available, it is quite possible

that the Ca/Ti peaks will change their relationship with orbital parameters along studied interval. This should not be a problem for the observations and conclusions reached and the authors themselves recognize a gap (error range) between 10 and 25 kyr. So, again, the problem is that Ca/Ti ratio is sensitive to everything (carbonate corrosion, detrital input, carbonate production, winnowing…) and it is impossible to know what is happening in the environment having this ratio alone, without further support from other proxies or observations.

We agree that there are several processes controlling the deposition and preservation of carbonate in the ocean. However, our tuning is not based on an assumption we made but is rather based on previously established tuning strategies which we decided to follow. We consider the close resemblance in relative variations of our tuned U1564 record with the tuned $\delta^{18}O$ isotope ratio record from ODP982 (Drury et al., 2018) as a strong independent indication that the tuning makes sense, and that the main phase relationships between proxy and astronomical forcing remain similar across the studied interval. Related to that, we show the good agreement between the carbonate record and the $\delta^{18}O$ isotopes at ODP982, with the main features and stepwise changes that are visible in both records (Figure 7a). Without further evidence for the mechanistic relationship between the orbital parameters and the deposition we can't justify the use of an anti-phased relationship compared to the one used by previous studies for the tuning at ODP982 and other North Atlantic sites (see Table 1). That being said, if future evidence points at different controlling mechanisms and phase relationships between proxies, a general re-evaluation of astronomically tuned age models for the area will need to be considered.

It would also be helpful, but not mandatory, to perform Principal Component Analyses (PCA) in obtained elemental database at each interval, to see if Ca and Sr lose their correlation showing that something is changing in carbonates, or if we are moving from a more contouritic sedimentation to one dominated by more pelagic component (May be Bahr, A. et al., 2014 G3 approach could help to see how the different components of PC1 or PC2 could be associate with more or less pelagic/contourite influence).

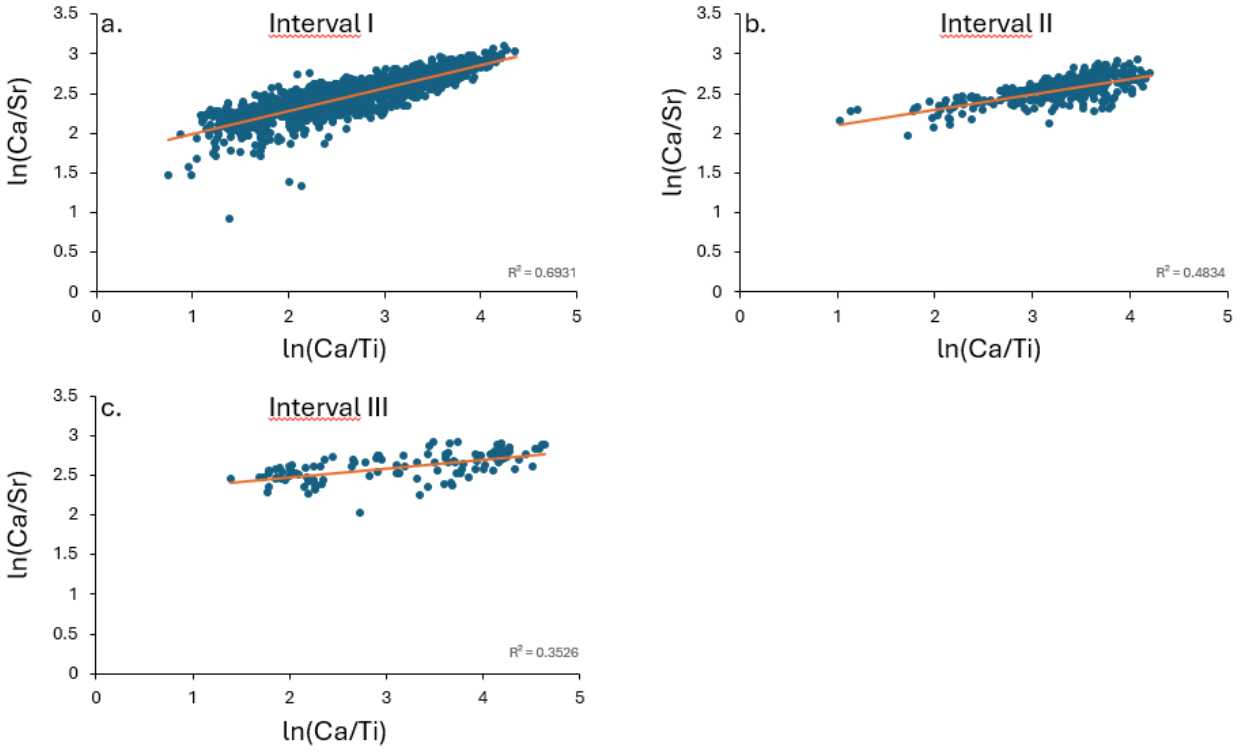
Instead of a general PCA that would include all elements, we now present a targeted comparison between the natural logarithm of Ca/Sr and Ca/Ti (carbonate content) for the three studied intervals (Figure S3). Indeed, the relationship between these proxies changed across time, and specifically weakened. Although their overall correlation has an R^2 value of ~0.62, the correlation during Interval I is ~0.7, the one during Interval II is ~0.48 and the one during Interval III is ~0.35. We now present this comparison in the revised "Results" and "Discussion" sections.

Results: "At the same time, these peaks were not recorded by ln (Ca/Sr), which closely resembles the fluctuations of the calibrated $CaCO_3$ (wt%) content during Intervals I and II (R^2 of ~0.7 and ~0.48 respectively; Fig. S2, S4) but loses its strong correlation to ln (Ca/Ti) during Interval III (R^2 of ~0.35; Fig. S4)."

Discussion: "The high peaks in ln(Ca/Sr) during Intervals I and II (Fig. S4) may indicate an increased contribution of detrital carbonate during periods of high $CaCO_3$ content (Hodell et al., 2008). Conversely, this detrital component could be missing during periods of low $CaCO_3$ content, resulting in the observed lows of the ln(Ca/Sr) (Fig. S4). One limitation of this interpretation is that this proxy has previously been deployed to infer detrital carbonate input associated with major ice-rafting events in glacial conditions of the late Pleistocene and it was accompanied by decreases in biogenic carbonate (Hodell et al., 2008, and references therein). At the same, the relative contribution to $CaCO_3$ content from foraminifera versus calcareous nannofossils could have also impacted the

Sr/Ca ratio. To test this hypothesis and further elucidate the mechanisms driving the observed lithological cycles, the source of biogenic and detrital and carbonate in the Gardar drift during the late Miocene, before the onset of Northern Hemisphere glaciations (McClymont et al., 2023), needs to be identified."

"During this interval, the correlation between the natural logarithm of Ca/Ti and Ca/Sr was reduced (R2 of ~0.35) and the ratio of Ca/Sr stays relatively constant, even during the lows in $CaCO_3$ content (Fig. S4). This suggests that changes in $CaCO_3$ content are not explained by the addition or removal of detrital carbonate (Hodell et al., 2008), but rather from the overall preservation and/or clastic sediment dilution of the biogenic carbonate component."



New Figure S3

Finally, in the discussion chapter dedicated to tectonics/thermohaline circulation, authors could also briefly talk about the changes in the thermohaline circulation forced by tectonics that is seen in other regions, like these affecting the AMOC associate with the closure of the Central American Seaway (Karas et al., 2017 Sci Rep.) or the Indian ocean (Antartic ice sheet expansion, Indonesian Troughflow (Tagliaro et al., 2022 Paleoceanography and Paleoclimatology)... with these planetary changes authors may explain why the relationship observed in the study area (AMOC) with CO_2 is not so obvious. In fact, the discussion on CO2 is not very clear in the current version either.

We have restructured the discussion based on the reviewer comments. We agree that the discussion chapter needs to mention more of the gateway changes that occurred during the study interval.

Discussion: “the deepening of the Australian-Antarctic Discordance (AAD; Tagliaro et al., 2022), the closure of the central American seaway (CAS; Karas et al., 2017) and the restriction of the Indonesian Throughflow (Karas et al., 2011)…

Arguably the most studied tectonic reorganization related to Atlantic Ocean circulation is the Central American Seaway closure, which has been suggested to have strengthened the thermohaline circulation and NADW during the early Pliocene (e.g., Haug et al., 2001; Haug and Tiedemann 1998). Although a wide range of ages has been proposed for the main stages of CAS closure, there is general consensus that the main restriction occurred after 4.8 Ma (e.g., Karas et al., 2017 and references therein) and therefore post-dates the ocean circulation changes we record here…”

At the same time, integrating comments from Reviewer 3, we have toned down the claim of synchronicity and preferred mechanisms, as well as further interpretations based on the records we present here. The discussion on CO_2 has been removed, since it was complicating the paleoclimatic interpretations.

Minor comments:

- Throughout the text authors talk about XRF, XRF elemental ratio, XRF ratios, XRD and CaCO3… But this is not correct. XRF-scan is the technique but what is measured are the elemental compositions or intensities (major elements).

Implemented across the text. We now use XRF as the method, and “XRF-derived elemental ratios” or “compositions” for the resulting data.

- It would be helpful to put in Figure 1b or 1c the approximate location of the site.

We believe the reviewer refers to Figure 2 (map). This change has now been implemented in the revised version of Figure 2.

- It may be interesting to put the 3 phases of the MSC in Figure 7, instead of that black bar with the Mediterranean restricitions. In Rovieri et al., 2026 Earth-Science Reviews 274, March 2026, 105380. You can find a good summary of those three phases that may have some interesting correlation with the data that is presented from the Gardar drift.

This change has now been implemented in the revised version of Figure 7.

I hope these comments are helpful to the authors.

We thank the reviewer for taking the time to revise the manuscript. The comments were very helpful, and we believe that they have resulted in an improved version.

References

Haug, G. H., and Tiedemann, R.: Effect of the formation of the Isthmus of Panama on Atlantic Ocean thermohaline circulation, Nature, 393 (6713), 673–676, https://doi.org/10.1038/31447 , 1998.

Haug, G. H., Hughen, K. A., Sigman, D. M., Peterson, L. C., and Röhl, U.: Southward migration of the Intertropical Convergence Zone through the Holocene, Science, 293 (5533), 1304–1308, https://doi.org/10.1126/science.1059725, 2001.

Hodell, D. A., Channell, J. E. T., Curtis, J. H., Romero, O. E., and Röhl, U.: Onset of "Hudson Strait" Heinrich events in the eastern North Atlantic at the end of the middle Pleistocene transition (~640 ka)?, Paleoceanography, 23, PA4218, https://doi.org/10.1029/2008PA001591, 2008.

Karas, C., Nürnberg, D., Bahr, A., et al.: Pliocene oceanic seaways and global climate, Sci. Rep., 7, 39842, https://doi.org/10.1038/srep39842, 2017.

Karas, C., Nürnberg, D., Tiedemann, R., and Garbe-Schönberg, D.: Pliocene climate change of the Southwest Pacific and the impact of ocean gateways, Earth Planet. Sci. Lett., 301, 117–124, https://doi.org/10.1016/j.epsl.2010.10.028, 2011.

McClymont, E. L., Ho, S. L., Ford, H. L., Bailey, I., Berke, M. A., Bolton, C. T., De Schepper, S., Grant, G. R., Groeneveld, J., Inglis, G. N., Karas, C., Patterson, M. O., Swann, G. E. A., Thirumalai, K., White, S. M., Alonso-Garcia, M., Anand, P., Hoogakker, B. A. A., Littler, K., Petrick, B. F., Risebrobakken, B., Abell, J. T., Crocker, A. J., de Graaf, F., Feakins, S. J., Hargreaves, J. C., Jones, C. L., Markowska, M., Ratnayake, A. S., Stepanek, C., Tangunan, D., Climate evolution through the onset and intensification of Northern Hemisphere Glaciation, Rev. Geophys., 61, e2022RG000793, https://doi.org/10.1029/2022RG000793, 2023.

Tagliaro, G., Fulthorpe, C. S., Watkins, D. K., De Vleeschouwer, D., Brumsack, H., Bogus, K., and Lavier, L. L.: Late Miocene–Pliocene vigorous deep-sea circulation in the Southeast Indian Ocean: Paleoceanographic and tectonic implications, Paleoceanogr. Paleoclimatol., 37, e2021PA004303, https://doi.org/10.1029/2021PA004303, 2022.