General comments

Karatsolis et al. present a new high-resolution sedimentological and geochemical record from IODP Site U1564 on Gardar Drift spanning approximately 6.2-5.0 Ma. The study extends the record of this North Atlantic contourite system into the latest Miocene and earliest Pliocene, complementing the younger record from the same site presented by Sinnesael et al. (2025). The authors combine XRF-derived elemental ratios, a calibrated CaCO3 record, sedimentation and carbonate accumulation rates, and a newly constructed astronomical chronology to investigate changes in bottom-current activity across the late Miocene cooling–warming transition and the Miocene-Pliocene boundary.

I find the dataset valuable. High-resolution records of North Atlantic deep-current behaviour across this interval remain relatively scarce, and Site U1564 provides an important record from Gardar Drift within the modern depth range of ISOW-related deposition. The manuscript also makes a useful effort to integrate the Site U1564 observations with records from ODP Site 982 and with broader climatic and gateway changes in the North Atlantic. My main concern is not the importance of the record, but how far some of the interpretations extend beyond what is directly constrained by the proxies. In several places, changes in sediment accumulation and elemental composition at Site U1564 are interpreted in terms of changes in ISOW-related bottom-current activity and subsequently discussed in the context of deep-water formation and broader Atlantic overturning. These processes are related, but they are not equivalent, and the present dataset constrains them at different levels of confidence.

I think the manuscript would be stronger if it more clearly distinguished the observed sedimentological and geochemical changes at Site U1564 from their interpretation in terms of local bottom-current and depositional conditions, inferred changes in ISOW-related influence, and possible implications for broader North Atlantic overturning.

I therefore recommend major revision.

We thank the reviewer for recognizing the value of the presented record and for taking the time to provide detailed and justified recommendations. We have now made considerable effort to tone down the interpretations and decouple observations from claims about the large-scale changes in ocean circulation.

Major comments

1. Interpretation of bottom-current activity and ISOW

The interpretation of the U1564 record in terms of ISOW activity is central to the manuscript. The combination of sedimentation rates, Zr/Rb and Ti/K provides reasonable evidence for changes in bottom-current-related deposition, and the location of U1564 on Gardar Drift provides a basis for discussing these changes in relation to ISOW. However, I think the manuscript sometimes moves too directly from these sedimentary signals to changes in ISOW strength.

In particular, sedimentation rate within a contourite system is not a direct or necessarily linear measure of current strength. Accumulation can respond to current velocity, but also to sediment supply and focusing, winnowing, erosion, bypass and migration of the current core. Likewise, Zr/Rb provides information about hydrodynamic grain-size sorting, but does not directly quantify the volume transport of ISOW. The manuscript itself shows some of this complexity: during Interval II, for example, low Zr/Rb and Ti/K are interpreted as reflecting “reduced current strength and/or sediment

supply," whereas the subsequent Discussion more specifically attributes the change to reduced ISOW influence.

The authors later acknowledge that the apparent reduction in ISOW activity could partly reflect lateral migration of drift depocentres, short-lived changes in bottom-current pathways, or localised weakening of an ISOW segment. This is an important qualification, but it does not yet seem fully integrated into the preceding interpretation. I suggest carrying this distinction more consistently through the Discussion, Abstract and Conclusions: the proxies provide evidence for changing bottom-current/depositional conditions at Gardar Drift and can reasonably be interpreted in terms of changing ISOW-related influence, whereas changes in total ISOW transport or basin-scale overturning are less directly constrained.

McCave and Hall (2006; https://doi.org/10.1029/2006GC001284) would be a useful additional reference here. Their discussion is particularly relevant because current-sensitive sediment properties primarily constrain local depositional flow conditions and can also be affected by source and winnowing.

We thank the reviewer for expanding on the potential controlling mechanisms and nuances of the observed sedimentation rate and XRF record patterns. We agree that in certain places we move quickly to the favored interpretation of these records. We have now rephrased these in several instances and made a clearer distinction between observations and interpretations. We have done that by restructuring the discussion, and removing and/or modifying several sentences from the abstract, discussion and conclusion such as:

"The record supports the periodic presence of deep currents since the latest Miocene, as well as a distinct ISOW weakening that coincided with the global warming trend, and the severe restriction on Mediterranean Outflow Water, just before the Miocene-Pliocene boundary."

became:

"The record supports the periodic presence of deep currents since the latest Miocene, as well as reduced ISOW influence at Site U1564 that falls closely to the global warming trend and the severe restriction on Mediterranean Outflow Water, which occurred just before the Miocene-Pliocene boundary."

We also refer to the study from McCave and Hall 2006 regarding possible controlling mechanisms of deposition.

"This could be attributed to an overall weakening of overturning circulation, as well as to the migration and/or deactivation of an ISOW branch due to modified topography or a climatically controlled change in the dominant water mass. All these factors have been shown to significantly alter Gardar drift deposition patterns (McCave and Hall 2006)."

2. Astronomical chronology and temporal uncertainty

The new astronomical chronology is an important part of the study, but I think the distinction between construction of the tuning and independent evaluation of the resulting age model needs to be clearer.

We have now restructured the paragraph to make the distinction between the construction and the evaluation of the age model clearer (see next response).

The authors tune the CaCO3 record to ET-P and subsequently note the phase and amplitude agreement between the tuned CaCO3 series and ET-P as supporting the validity and robustness of

the astronomical tuning. Because this correspondence is partly imposed by the tuning itself, I do not think it provides an independent test of the age model. The comparison with the independently tuned Site 982 benthic δ18O record is more informative in this respect. I suggest distinguishing more clearly between correspondence with ET-P as a result of the tuning and agreement with independent chronostratigraphic constraints and Site 982 as evaluation of the tuning.

The comparison with ET-P was not intended as an independent measure of evaluating the tuning. It was used to show how the earlier part of the record, which used magnetic susceptibility and obliquity as target (3.6-5.0Ma at Site U1564; Sinnesael et al., 2025), shows good correspondence with the older part which was tuned using the $CaCO_3$ content and ET-P as target (this study). The comparison with ODP982 is indeed the one that provides the independent constraint to validate the newly developed age model. We understand however that this wasn't clear in the previous version of the manuscript and we have we have now clarified this in the text.

"To evaluate the consistency with the tuning strategy of Sinnesael et al., 2025, the tuned calibrated $CaCO_3$ (wt%) time series between 3.60–6.20 Ma was plotted along with the ET-P solution in two sections/intervals (Fig. 6). The astronomical tuning from Sinnesael et al., 2025 is used for the interval 3.60–5.00 Ma (Fig. 6a) and the one from this study for the interval 5.00–6.20 Ma (Fig. 5; Fig. 6b). Figure 6 illustrates that although a different tuning target (obliquity) and proxy record (magnetic susceptibility) were used between 3.60–5.00 Ma, the $CaCO_3$ (wt%) and the ET-P solution exhibit consistent phase and amplitude alignment in both intervals, allowing the age model to be extended while preserving consistency with the younger interval of this site (Fig. 6). Additionally, for a site/depositional environment-independent metric of comparison, the $CaCO_3$ (wt%) record was compared to the benthic foraminifera $\delta^{18}O$ record from ODP Site 982 (Fig. 7a)..."

Some additional discussion of uncertainty would also be useful. The authors report offsets of approximately 10-25 kyr between U1564 and Site 982 and describe these as potentially reflecting small tuning errors or minor hiatuses. These offsets become relevant when the timing of changes at U1564 is subsequently compared with relatively short-lived climatic and gateway changes.

We now expand this section to explain what the potential reason behind this offset is, and to explain that this offset has not been quantified, but it is mostly derived by visually inspecting misalignments between the records of $CaCO_3$ (wt%) at Site U1564 and the benthic foraminifera $\delta^{18}O$ record at ODP Site 982 (Figure 7a). The estimated error is now acknowledged by providing age ranges for the transition intervals (e.g., the transition from Interval II to Interval III is now discussed as an interval between 5.56–5.5 Ma; see also next response).

Relatedly, the manuscript is transparent that the 5.55 Ma boundary is the midpoint of a gradual transition occurring approximately between 5.60 and 5.50 Ma. However, later discussion sometimes treats 5.55 Ma as a more precise onset age. I suggest retaining the transition range when comparing the U1564 change with the LMW, Mediterranean restriction and Bering Strait opening. A new age model is not necessary, but the uncertainty inherent in the existing chronology and in the definition of the interval boundaries should be reflected in these comparisons.

As mentioned in the previous comment response, the uncertainty is now acknowledged by providing age ranges for the transition between intervals.

Further explanation is provided in the results:

"Minor misalignments between the two records (offset of peaks of ~10–25 kyr) do occur in some intervals and likely reflect minimal errors in astronomical tuning at either location (precessional

cycles), minor hiatuses related to localized erosion at the deeper site, or abrupt changes in sedimentation rates that can condense or expand certain cycles. The offset estimate is solely based on the visual inspection of peak alignment in Fig. 7a and therefore a statistical analysis (e.g. phase analysis and dynamic time-warping) of the records will be needed to quantify it with a numerical approach."

3. Carbonate preservation and the interpretation of Interval III

The manuscript appropriately recognises that variations in CaCO3 can reflect several processes, including terrigenous dilution, carbonate production and dissolution/preservation. My concern is therefore not that these alternatives have been overlooked, but that the Site 982 comparison is used to narrow them more strongly than the available evidence may allow.

For Interval II, the manuscript argues that similar average CaCO3 fluxes at U1564 and Site 982 support the absence of a major sustained change in productivity or selective preservation and therefore favour reduced terrigenous input at U1564. This is plausible, but the two sites differ substantially in water depth and depositional setting, and U1564 is a contourite drift where lateral sediment redistribution is important. I would therefore present reduced terrigenous dilution as the interpretation most consistent with the observations rather than as one that excludes changes in production or preservation.

We have now restructured this paragraph to present the reduced dilution as the primary driver of the observed changes.

The following sentence has been added:

"Considering that Site U1564 is located at the deeper parts of the basin and within the pathway of strong bottom currents in the modern ocean, reduced input of terrigenous material due to a weakening or migration of an ISOW-related branch provides the most plausible explanation for this change."

During Interval III, the divergence in CaCO3 flux between the two sites is consistent with the authors' interpretation of reduced carbonate preservation at the deeper site, but it does not uniquely demonstrate this mechanism, and identifying the responsible water mass is a further interpretative step. The manuscript already recognises the need for additional isotopic and micropalaeontological evidence to substantiate the proposed oceanographic changes. I therefore suggest keeping the interpretation of increased influence of corrosive southern-sourced water clearly as a hypothesis rather than a water-mass reconstruction demonstrated by the present proxies.

We have now restructured this paragraph to focus mostly on our observations and clarify that the presence of a corrosive water mass is still mainly a hypothesis previously proposed for the Pleistocene.

An example from the discussion:

"…at the shallower site, and the presence of a more corrosive water mass at the deeper parts of the basin."

became:

"…at the shallower site, and reduced preservation of $CaCO_3$ in the deeper parts of the basin".

4. Regional versus basin-scale circulation

The contrasting records from Gardar and Eirik Drifts provide an opportunity to strengthen the regional interpretation. The manuscript already notes that evidence from Eirik Drift indicates a different history from that inferred at Gardar Drift and considers differentiated DSOW and ISOW pathways. It also acknowledges the possibility of changes in drift depocentres and bottom-current flow paths.

I think this contrast deserves greater weight in the overall interpretation. Rather than expecting the two drift systems necessarily to record a spatially uniform strengthening or weakening of North Atlantic overturning, their different behaviour may indicate changes in the routing or relative contribution of the major overflow pathways. This would also provide a useful framework for interpreting the U1564 record without requiring every change in Gardar Drift deposition to represent a proportional basin-wide change in overturning.

The existing comparison with longer-term North Atlantic records could also be developed in this context. The manuscript notes evidence for generally stronger northern-component water production and overturning over an interval extending from approximately 6 to 2.5 Ma, while recognising that those records do not resolve the higher-frequency variability observed at U1564. The relationship between these different spatial and temporal signals could therefore be discussed more directly. In particular, the authors could consider whether the U1564 record reflects a reduction in northern-source export or a more regional change in the expression or routing of ISOW at Gardar Drift. The present dataset does not appear sufficient to distinguish these possibilities, but acknowledging them would provide a more balanced interpretation of the regional significance of the U1564 record.

We have toned down the extrapolation of U1564 observations to basin wide changes in Gardar Drift and ISOW, also in line with the comments from other reviewers. The discussion, abstract and conclusions have been restructured in several instances to reflect this approach.

Two examples from the Discussion:

“Recently, it was shown that strong and persistent ISOW current activity was established in the area after 3.6 Ma, influencing Björn and Gardar Drift sites (Sinnesael et al., 2025). In this study, our observations are limited to one Gardar Drift site, and this reduces the spatial resolution and limits the possibility to extrapolate the observed sedimentation patterns to basin-wide changes. Despite that, the 3.6 Ma transition was expressed by a characteristic ~four-fold increase in sedimentation rates at Site U1564 (from ~4.20 before 3.6 Ma to 17 cm/kyr after), supporting that this site is well situated within the area of ISOW influence.”

“Future integration of seismic data and Expedition 395 core records, including sediment provenance analysis and sites on the nearby Björn Drift, will resolve whether our inferred late Miocene reduction in ISOW activity at Site U1564 has basin-wide characteristics, or if it can be partially explained by the lateral migration of drift depocenters, short-lived changes in bottom current flow paths, or even localized weakening of an ISOW segment.”

5. Climatic and gateway controls

The discussion of the LMW, Bering Strait and Mediterranean restriction provides useful context, and the manuscript generally uses appropriately conditional language when discussing possible mechanisms. However, I think the distinction between temporal correspondence and evidence that these changes drove the U1564 transition could be maintained more consistently.

For example, the manuscript notes that a key phase of Bering Strait opening at approximately 5.6-5.4 Ma was coeval with reduced bottom-current activity during Interval II. It similarly notes that the shutdown of Mediterranean outflow and subsequent isolation of the Mediterranean occurred close to the inferred onset of weaker ISOW. These temporal relationships are interesting, and modelling studies provide plausible mechanisms by which either gateway could influence North Atlantic circulation. They do not, however, establish which mechanism, if any, caused the transition recorded at U1564. This distinction is particularly important given that the Discussion section is currently titled "Tectonic drivers of change in deep water formation." I suggest a more neutral framing such as "Potential controls on late Miocene deep-water circulation." This would better match the level of evidence and would also accommodate the LMW itself, which is climatic rather than tectonic.

We have now restructured this part of the discussion, to provide a neutral framework of comparison between changes in drift deposition and tectonic gateway changes, avoiding claims for a direct mechanistic explanation and a cause-and-effect relationship. We now acknowledge that there is no simple and straightforward link between the Mio-Pliocene tectonic reorganizations and North Atlantic ocean circulation. The proposed mechanisms are provided as hypotheses to be tested with further constraints and climatic models.

The title has also been changed based on the suggestion.

6. Abstract and Conclusions

Following the points above, I suggest revisiting the Abstract and Conclusions so that the level of confidence in the main statements more closely reflects what is directly constrained by the record. The evidence for a substantial change in the depositional and bottom-current regime at Gardar Drift is convincing. The interpretation of this change as reduced ISOW-related influence is reasonable given the location of the site and the combined proxy evidence. However, implications for total ISOW transport and basin-scale overturning are less directly constrained, while attribution to warming or particular gateway changes remains based primarily on temporal correspondence and physically plausible mechanisms.

Similarly, the Interval III carbonate record provides evidence consistent with changing preservation at the deeper site, but the specific attribution to corrosive southern-sourced water remains a hypothesis without an independent water-mass tracer. I suggest retaining these distinctions in the final synthesis rather than presenting all stages of the interpretation with the same degree of certainty.

We have revisited the Abstract, the Discussion and the Conclusions as part of addressing the previous comments. We believe that we now provide a more realistic and conservative interpretation of the records, keeping ISOW related changes as the main proposed hypothesis, but without extrapolating the observations to suggest basin wide changes in overturning circulation. Similarly, we clarify that the presence of a corrosive water mass is not substantiated by the current evidence and remains a hypothesis, while a change in overall preservation is supported by the presented records.

Last sentences of the abstract now read:

"The record supports the periodic presence of deep currents since the latest Miocene, as well as a reduction in ISOW influence at Site U1564 that falls closely to the global warming trend, and the severe restriction on Mediterranean Outflow Water, which occurred just before the Miocene-Pliocene

boundary. Lower carbonate fluxes are observed at Site U1564 following the termination of the Messinian salinity crisis, possibly hinting to a decrease in carbonate preservation."

Example from the conclusions:

"During this interval, there are indications that corrosive, southerly sourced waters were reaching the deeper parts of the basin…"

became:

"During this interval, $CaCO_3$ fluxes at Site U1564 were diminished compared to the ODP Site 982, suggesting a change in preservation at the deeper site which could be linked to the presence of a corrosive water mass…"

I hope the above comments are helpful. Overall, I find this a valuable study that presents an important new record from Gardar Drift. I hope the manuscript will be considerably strengthened by addressing the points above, and I look forward to seeing a revised version.

We thank the reviewer once more for acknowledging the importance of the record. The comments were very useful and insightful, and we also believe that they lead to a better version of the manuscript.

References

McCave, I. N., and Hall, I. R.: Size sorting in marine muds: Processes, pitfalls, and prospects for paleoflow-speed proxies, Geochem. Geophys. Geosyst., 7, Q10N05, https://doi.org/10.1029/2006GC001284 , 2006.

Sinnesael, M., Karatsolis, B.-T., Pearson, P. N., Briais, A., Hemming, S. R., LeVay, L. J., Dunkley Jones, T., Cui, Y., Di Chiara, A., Dodd, J. P., Dwyer, D., Eason, D. E., Friedman, S. A., Hanson, E., Hochmuth, K., Ibrahim, H. E., Jasper, C. E., Lee-Takeda, S., LeBlanc, D. E., Lindsay, M. R., McNamara, D. D., Modestou, S. E., Morris, M. A., Murton, B. J., OConnell, S., Pasquet, G., Qian, S.-P., Rosenthal, Y., Satolli, S., Suzuki, T., Thulasi, T., Wade, B. S., White, N. J., Wu, T., Yang, A. Y., and Parnell-Turner, R. E.: Onset of strong Iceland-Scotland overflow water 3.6 million years ago, Nat Commun., 16, 4323. https://doi.org/10.1038/s41467-025-59265-5, 2025.