nature portfolio

# Peer Review File

South China Sea records Late Miocene reorganization of western Pacific deep circulation

**REVIEWER COMMENTS**

Reviewer #1 (Remarks to the Author):

The study by Liu et al. provides new seismic interpretation and sediment log evidence for the spatial and temporal extent of deep water contourite deposits in the South China Sea and elsewhere, and uses this to reconstruct major changes to deep water circulation since the Oligocene in particular related to ocean gateway openings and closings. The topic is very interesting and important, and there is still a large knowledge gap in understanding changes to deep water circulation in this region over this time period. Overall the paper is written well (although there are some grammatical issues in places, particularly the final paragraph), the figures look very nice, and the main finding it seems – that contourites initiated from 23 Ma – appears to be very clear from the seismic interpretations in Fig. 2. However, the main issue for me is the extremely vague sediment core lithological changes that are apparent (Fig. 3) compared with the very specific seismic interpretations. As I outline below, it looks to me like the very clear and precise ages provided for the changes to water masses and gateway closures are not supported by the sediment logs, which are rather incomplete temporally (looking at the large time gaps in Fig. 3) and do not show abrupt changes associated with the seismic units. I do think that a lot more nuanced discussion is needed to bring out the rationale for the broad scale changes interpreted, and that the uncertainty in interpretation due to the unclear logs should be fully discussed. A major discussion on age dating is needed too and I would like to see biostratigraphic tie points on the logs if they exist. It seems crucial to me that the seismic horizons are dated for these claims to be made of regional changes at 23, 15.5, 10, and 5.3 Ma. Additionally, lot of the cited published information seems to tell a coherent story (certainly for Pliocene onwards). As I am unsure what is really new here in terms of major clear findings, I would recommend corrections to the interpretations and discussion in particular and likely publication in a specialist journal.

• Abstract – I find this a little vague – what does ‘decode' mean specifically, what methods were carried out, and what is new here as opposed to previous knowledge?
• The labelling of ocean drilling sites is confusing and could be tightened up, I spent a long time cross referencing – how about adding the site names/numbers directly onto Fig. 1C (like is done in Fig. 1A in white text)?
• Fig. 3 – add reference for DODO core to figure caption
• Need to cite Gourlan et al. (2008), I was surprised this was missed out and I do think it should be incorporated in the introduction fully
• "contourite drifts only appeared since the earliest Early Miocene (23 Ma) in the deep South China Sea" (line 142) – The only sed log that goes through this time period is 1501C, and this does not show a change to contourites until 19 Ma (T51). Although Fig. 2e and f show contourites from 23 Ma (T60), as there are no logs through here I would like to know how this has been interpreted and how robust this interpretation is. The text needs to be a lot clearer in the main paper. I note that Ref 12 and Site 1148 suggest deep CDW from 23 Ma, but this finding needs to be based on data evidence from this study I would have thought.
• "Scott Plateau (NW Australia; Fig. 1c)" (line 152) – needs to be properly marked on the figure
• Line 153 – ‘evidences' needs to be ‘evidence' throughout
• "were connected through some oceanic seaways" (line 162) – I think you need to list these seaways more specifically
• "After 15.5 Ma, most contourite features in the South China Sea developed…" (line 166) – I think this sounds very precise in terms of age, but in actual fact the uncertainty in age is very large and the changes are also gradual as mentioned elsewhere. I think it needs to be made clear throughout the text (and abstract) that these are very approximal ages.
• Although there are gradual changes after ca. 15.5 Ma at 1501C, I don't see any changes to sedimentation at 15.5 Ma in 1504A. How does this fit into the broader claim that "After 15.5 Ma, most

contourite features in the South China Sea developed mounded geometries and U-shaped moats" (line 166)? How is T50 (15.5 Ma) therefore determined in the seismic profiles (Fig. 2)? Tis needs to be a lot clearer in the main text.

• The same can be said for 10 Ma – this is given as a sharp timeline in Fig. 2, but is not apparent in the sediment log in Fig. 3c. 1501C, 1500 and 1431D-E go through 10 Ma, but none show any special change over this interval. It doesn't fit with this sentence "large-scale contourite drifts abruptly ceased at 10 Ma" (line 175)
• Line 177 – also refer to Fig. 1c which is where the site is shown on a map
• Fig. 3c – capital letter for Benthic d13C on the axis label
• Fig. 3b probably should have some labelling on it that annotates what the changes mean that are mentioned in the text (e.g., 1438 shift to indicate more SCW in Philippine Sea, 1457 shift and closure of CAS, what the gray bar represents)
• Line 200 – should read "since the late Pliocene"

Reviewer #2 (Remarks to the Author):

This paper explores contourite sedimentary features in the South China Sea using seismic data and drillings from ODP and IODP sites. The analysis reveal several oceanographic changes from the Oligocene to End-Miocene, which the authors link to paleogeographic changes of the Indonesian Gateway and the Indian/Tethys Seaway. Generally I think this study is interesting, the paper is clear and well written, and it could potentially be of interest to this journal. However, I find the discussions around potential causes of changes in the deep ocean circulation a bit too weak in its current state, and the link to the the Tethys/Indian gateway need to be explained more. I also think the paper would improve by adding comparisons to paleo ocean circulation models, and would benefit from including a broader discussion on the climatic changes of the Miocene and how they might influence circulation.

I have added my major comments below, followed by a few minor comments.

The link to the closure of Indian Gateway/Tethys seaway need to be explained in more detail. My main issue is that while this seaway likely re-opened and closed in the Miocene, the depth during these times was on the order ~100 m, which could play some role for surface circulation, but how this signal would transmit to the deep ocean need to be better explained. Could this be linked to changes in upwelling in the Indian Ocean following circulation changes for example? See e.g. Sarr et al. 2022, or Hamon et al. 2013?
As this discussion is written now, I would think more local changes, or large scale global circulation changes (perhaps related to climatic changes) could also cause your observed changes in the Mid Miocene, and this would be good to include in the discussion.
Also, the "circum equatorial circulation" is perhaps something that would be more expected in the Early Eocene? Especially if one considers deep ocean circulation. I am not convinced this was active in the Miocene considering the Tethys (and also likely the Central American Seaway) were shallow.

The results are not compared to model simulations. I think one could improve the discussion by viewing your results in comparison to models simulations. See for example available miocene modeling efforts in Burls et al. 2021. Regarding this, the Miocene climatic changes could potentially have influenced the large-scale ocean circulation pattern which would also be good to include more in your discussions (as mentioned above).

Minor comments:

I think the name “Indian Gateway” is a bit Mediterranean centric, and prefer Tethys (or Eastern Tethys) Seaway. Or alternatively Mesopotamian Trough (which was the Seaway across Arabia open in the Miocene).

Figure 1: You should change the ‘rainbow’ colormap in the cross section. I recommend using a colormap that does not distort the data. See for example Crameri et al. 2020.

Best regards,
E. O. Straume

Useful references:

Burls, N. J., Bradshaw, C. D., De Boer, A. M., Herold, N., Huber, M., et al. (2021). Simulating Miocene warmth: Insights from an opportunistic multi-model ensemble (MioMIP1). Paleoceanography and Paleoclimatology, 36, e2020PA004054. https://doi.org/10.1029/2020PA004054

Hamon, N., Sepulchre, P., Lefebvre, V., & Ramstein, G. (2013). The role of eastern Tethys seaway closure in the Middle Miocene Climatic Transition (ca. 14 Ma). Clim. Past, 9(6), 2687-2702. doi:10.5194/cp-9-2687-2013

Sarr, A.-C., Donnadieu, Y., Bolton, C. T., Ladant, J.-B., Licht, A., Fluteau, F., . . . Dupont-Nivet, G. (2022). Neogene South Asian monsoon rainfall and wind histories diverged due to topographic effects. Nature geoscience, 15(4), 314-319. doi:10.1038/s41561-022-00919-0

Crameri, F., Shephard, G.E. & Heron, P.J. The misuse of colour in science communication. Nat Commun 11, 5444 (2020). https://doi.org/10.1038/s41467-020-19160-7

Reviewer #3 (Remarks to the Author):

This paper re-assesses the timing of opening and then closing of the Indonesian Gateway between the Indian and Pacific Oceans. The authors investigate these issues using seismic reflection imaging of contourite drift sedimentation tied to multiple IODP holes.

The potential influence of oceanic gateways on Earth's climate is a major question in Earth science. In principle, the way authors integrate multiple data sources to propose new conclusions about gateway opening seems to me in line with Nature family expectations.

The main advances are (1) pushing back the age of initiation of strong deep water flow in the South China Sea from middle to earliest Miocene, (2) a new hypothesis that deep water entered the region from the Indian Ocean rather than the Pacific Ocean at this time, and (3) an inference that deep water connection to the Indian Ocean was severed at 10 Ma.

Points (2) and (3) seem to me to be potentially interesting steps forward, but are not yet fully justified by the data presented here. Point (1) above seems to me more of an incremental advance.

The methodology is sound. I am happy with the analysis of the seismic and borehole sedimentology data. I do not think that the interpretations based on published chemical proxies (d13C and eNd) are justified based on the data shown, as I explain below. I therefore think that a main conclusions are not justified in this draft. The authors might be able to fix the problem with the chemical data in a revised draft.

INDIAN OCEAN INFLUENCE, AND CLOSING OF THE GATEWAY AT C. 10 MA

I am quite confused by the whole "Circumpolar Deep Water from the Indian Ocean" section. I do not think that the chemical data in Fig. 3 are sufficient to justify several of the interpretations made in the text.

A key argument is that CDW chemical signal did not reach N Pacific until c. 15 Ma (L144-146). The authors therefore infer that early Miocene contourite drifts in the South China Sea must have bee generated by CDW from the Indian Ocean (L147). I don't see how this follows. Surely the contourite drifts in the SCS could have been sourced from the equatorial Pacific, as implied by Fig. 3b (see below).

Nevertheless, it is inferred that CDW must have reached the study area from the Indian Ocean (but not from the Pacific) at 23 Ma to form the mounded drifts observed on the seismic data. A problem here is that there is no eNd data from either the eastern Indian Ocean or the South China Sea on Fig. 3b prior to 15 Ma. Thus I'm not sure there is any positive evidence presented here from eNd water chemistry in support of Indian Ocean-sourced CDW in the S China Sea prior to 15 Ma.

What Fig. 3b does show is that at c. 14 Ma the eNd composition of the South China Sea is mid-way between the compositions of the equatorial Pacific and eastern Indian Oceans. Therefore, a general reader could hypothesize that at 14 Ma the S China Sea was influenced by both Indian and Pacific waters, which mixed to give the observed composition. Yet L147 implies that the mounded drifts between 23 and 10 Ma in the S China Sea formed beneath Indian-sourced CDW only. I think it would help to recognize the interpretation that the SCS could have been influenced by CDW from both Pacific and Indian Oceans. If this is not the case, then the evidence for an Indian-only source needs to be stronger and more clearly explained.

Fig. 3b also shows that by 3 Ma the eNd composition of the S China Sea was the same as the equatorial Pacific and distinct from the eastern Indian Ocean. It is interpreted that the disconnection between the S China Sea and Indian Oceans happened at 1O Ma on the basis of the seismic data. However, the chemical data from the western Indian Ocean on Fig. 3b has similar composition to the S China Sea younger than 10 Ma, perhaps to about 8 Ma. Taken at face value, this comparison seems to contradict the inference of disconnection at 10 Ma from the seismic data. On the other hand, the W Indian Ocean is quite far from the S China Sea, and quite different from the E Indian Ocean dataset, which is closer. I also note the development of drifts post 10 Ma in the Phillipine Sea. Should we then ignore the W Indian Ocean data?

In the d13C data in Fig. 3c, the three curves shown are from the S China Sea, Indian Ocean and a Global compilation. Rather than using a global compilation, would it not be better to show a Pacific Ocean record? Fig. 3c shows that the d13C composition of the S China Sea is different from the global average, but it is not clear whether it is different from the Pacific Ocean, since the Atlantic and Southern Oceans also contribute to the global average, and the compositions sometimes differ between these oceans.

A significant shortcoming of the d13C data in Fig. 3c is that no Indian Ocean records for younger than 10 Ma. The figure therefore seems to contradict L180-182, which states that benthic d13C in the Indian Ocean differs from that in the S China Sea younger than 10 Ma. Since a main point of the paper is to demonstrate that connection between the S China Sea and Indian Oceans was broken at

10 Ma, I think it is very important to show some d13C data from the E Indian Ocean younger than 10 Ma on Fig. 3 in support of the statements in the text.

ONSET OF STRONG BOTTOM CURRENTS

Refs 16 and 17 show interpretations of sedimentary archives on seismic data in the South China Sea back to Late Miocene and Early Miocene respectively. The present manuscript pushes that back to the Oligocene. Ref. 16 indicates onset of strong bottom currents in the S China Sea about 18 Ma with a Pacific source. The present manuscript pushes that back to 23 Ma and argues (not yet convincingly - see above) for an Indian source. I'm not sure that the revised timing counts as much earlier than previously documented examples (L206); one could argue it is an incremental advance. It would help if the authors could make clearer why this change in means a step change in our understanding.

GENERAL POINTS

Abstract doesn't make clear what new analysis has been done. Need a "Here, we ..." sentence that mentions methods as well as materials. Final sentence bland; could be appended to numerous papers.

Results go straight into statements about mounded, sheeted, plastered drifts and moats without explaining what these features are and why they are important. I think it might would help to have a paragraph between the intro and results sections that outlines hypotheses; for example, how and open and a closed Indonesian Gateway might be expected to show in terms of changes seismic geometry and sedimentology.

Contourite evolutionary stages look reasonably well justified by the data in the figs.

I think that the paragraph L175-183 belongs in the preceding sub-section "Circumpolar Deep Water from the Indian Ocean".

In Fig. 2, some of the mounded and plastered drifts look similar in morphology to me; it would help to explain how you distinguished these two morpholgies. I eventually saw that some of this information in the Supplementary Fig. 2; perhaps this could be flagged earlier.

Fig. 3. Several datasets on Fig 3b are not marked on the maps. I cannot see 05E107 (apparently in S China Sea) or D137 (North Pacific) on the maps - perhaps you could make these clearer. It would also be nice to see the actual data points for U1457.

Fig. 4. Location of D needs to be show on panels A to C of Fig. 4 - not only on FIg. 1. Sup. Fig. 3 is much clearer in this respect.

Sup Fig. 1. Missing definition / example of sheeted drifts.

Sup Fig. 2. Please use colour coding same as Fig. 2 for drift interpretations.

# South China Sea records Late Miocene reorganization of western Pacific deep circulation

Liu et al.

RESPONSE TO REFEREES

*(Line numbers in the response are from the manuscript without tracked changes)*

**Reviewer comments:**

***Reviewer #1 (Remarks to the Author):***

*The study by Liu et al. provides new seismic interpretation and sediment log evidence for the spatial and temporal extent of deep water contourite deposits in the South China Sea and elsewhere, and uses this to reconstruct major changes to deep water circulation since the Oligocene in particular related to ocean gateway openings and closings. The topic is very interesting and important, and there is still a large knowledge gap in understanding changes to deep water circulation in this region over this time period. Overall the paper is written well (although there are some grammatical issues in places, particularly the final paragraph), the figures look very nice, and the main finding it seems – that contourites initiated from 23 Ma – appears to be very clear from the seismic interpretations in Fig. 2. However, the main issue for me is the extremely vague sediment core lithological changes that are apparent (Fig. 3) compared with the very specific seismic interpretations.*

**Authors:** We are grateful for the valuable suggestions provided by the reviewer. We have incorporated a more detailed interpretation of sedimentary features, focusing on core lithology and sedimentary facies derived from IODP Sites U1505C, U1501C, U1504A, U1500A-B, and U1431D-E. Additionally, Figure 3 has been reorganized for clarity. Our analysis reveals that contourites were deposited from the earliest Oligocene (23 Ma) until approximately 10 to 5.5 Ma, exhibiting patterns consistent with seismic interpretations.

*As I outline below, it looks to me like the very clear and precise ages provided for the changes to water masses and gateway closures are not supported by the sediment logs, which are rather incomplete temporally (looking at the large time gaps in Fig. 3) and do not show abrupt changes associated with the seismic units. I do think that a lot more nuanced discussion is needed to bring out the rationale for the broad scale changes interpreted, and that the uncertainty in interpretation due to the unclear logs should be fully discussed.*

**Authors:** We have included a supplementary figure (Supplementary Fig. 3) to illustrate the complete variations in natural gamma radiation (NGR). NGR logs demonstrate significant shifts at 23 Ma, 19 Ma, 15.5 to 14 Ma, 11 to 10 Ma, 5 Ma, 2.6 Ma, and 0.9 Ma (Supplementary Fig. 3). Remarkably, these time intervals coincide with the occurrence of major seismic discontinuities (T60, T51, T50, T40, T30, T20, and T1) within the deep South China Sea (Supplementary Fig. 4). Furthermore, we have made corresponding modifications to the relevant text (Lines 79-89; 140-156).

*A major discussion on age dating is needed too and I would like to see biostratigraphic tie points on the logs if they exist. It seems crucial to me that the seismic horizons are dated for these claims to be made of regional changes at 23, 15.5, 10, and 5.3 Ma.*

**Authors:** We have updated the Methods Section (Lines 248-256; 271-278). We have included additional evidence of biostratigraphy and calcareous/planktonic foraminifers events identified at IODP Expedition 368 Site U1505 Hole C (Supplementary Tables 1 and 2). Moreover, we have incorporated correlation to seismic data (Supplementary Fig. 4).

*Additionally, lot of the cited published information seems to tell a coherent story (certainly for Pliocene onwards). As I am unsure what is really new here in terms of major clear findings, I would recommend corrections to the interpretations and discussion in particular and likely publication in a specialist journal.*

**Authors:** We have substantially revised the discussion section of the paper, along with updating the abstract. Our latest finding underscores the significant influence of Circumpolar Deep Water (CDW) as early as 23 Ma in the low-latitudes of the North Pacific. CDW, originating from the eastern Indian Ocean, persistently impacted the Pacific region until approximately 10 Ma. The ultimate closure of the deep Indonesian Gateway around 10 Ma led to the disruption of deep-water exchange between the Pacific and Indian Oceans, triggering a reconfiguration of global deep-water circulation patterns. These shifts in gateway tectonics likely played a vital role in shaping Middle to Late Miocene global climate and oceanic conditions.

*• Abstract – I find this a little vague – what does 'decode' mean specifically, what methods were carried out, and what is new here as opposed to previous knowledge?*

**Authors:** We have modified the abstract.

*• The labelling of ocean drilling sites is confusing and could be tightened up, I spent a long time cross referencing – how about adding the site names/numbers directly onto Fig. 1C (like is done in Fig. 1A in white text)?*

**Authors:** We have added the labels of IODP and ODP drilling sites in Figure 1C.

*• Fig. 3 – add reference for DODO core to figure caption*

**Authors:** Done (now in Fig. 4a).

*• Need to cite Gourlan et al. (2008), I was surprised this was missed out and I do think it should be incorporated in the introduction fully*

**Authors:** Done. We also add additional discussion in the text according to the results from this reference.

*• "contourite drifts only appeared since the earliest Early Miocene (23 Ma) in the deep South China Sea" (line 142) – The only sed log that goes through this time period is 1501C, and this does not show a change to contourites until 19 Ma (T51). Although Fig. 2e and f show contourites from 23*

*Ma (T60), as there are no logs through here I would like to know how this has been interpreted and how robust this interpretation is. The text needs to be a lot clearer in the main paper.*

**Authors:** As we mentioned above, we added more evidence and interpreted IODP cores and NGR logging curves (Fig. 3; Supplementary Fig. 3).

*I note that Ref 12 and Site 1148 suggest deep CDW from 23 Ma, but this finding needs to be based on data evidence from this study I would have thought.*

**Authors:** The text in discussion section has been modified to avoid confusion and misunderstanding (Lines 118-139).

*• "Scott Plateau (NW Australia; Fig. 1c)" (line 152) – needs to be properly marked on the figure*

**Authors:** We realize that the isotope proxies maybe irrelevant in the discussion. The texts and captions have been deleted.

*• Line 153 – 'evidences' needs to be 'evidence' throughout*

**Authors:** Done.

*• "were connected through some oceanic seaways" (line 162) – I think you need to list these seaways more specifically*

**Authors:** We find out that the linkage between the Indonesian Gateway and Indian Gateway (Tethys Seaway) was not inherently related. This part of discussion has been deleted.

*• "After 15.5 Ma, most contourite features in the South China Sea developed…" (line 166) – I think this sounds very precise in terms of age, but in actual fact the uncertainty in age is very large and the changes are also gradual as mentioned elsewhere. I think it needs to be made clear throughout the text (and abstract) that these are very approximal ages.*

**Authors:** We have modified the text in Lines 144-149.

*• Although there are gradual changes after ca. 15.5 Ma at 1501C, I don't see any changes to sedimentation at 15.5 Ma in 1504A. How does this fit into the broader claim that "After 15.5 Ma, most contourite features in the South China Sea developed mounded geometries and U-shaped moats" (line 166)? How is T50 (15.5 Ma) therefore determined in the seismic profiles (Fig. 2)? Tis needs to be a lot clearer in the main text.*

**Authors:** See Supplementary Fig. 3 and Lines 79-89; 140-156.

*• The same can be said for 10 Ma – this is given as a sharp timeline in Fig. 2, but is not apparent in the sediment log in Fig. 3c. 1501C, 1500 and 1431D-E go through 10 Ma, but none show any special change over this interval. It doesn't fit with this sentence "large-scale contourite drifts abruptly ceased at 10 Ma" (line 175)*

**Authors:** Sediments recovered from Site 1500 and Site 1431 are mainly turbidites (Fig.3). The related curves could not represent contourite drift variations. But NGR logs of Sites U1505 and U1501 show a large variation during 11 to 10 Ma (Supplementary Fig. 3).

• *Line 177 – also refer to Fig. 1c which is where the site is shown on a map*

**Authors:** Done.

• *Fig. 3c – capital letter for Benthic d13C on the axis label*

**Authors:** Done (in Fig. 4b).

• *Fig. 3b probably should have some labelling on it that annotates what the changes mean that are mentioned in the text (e.g., 1438 shift to indicate more SCW in Philippine Sea, 1457 shift and closure of CAS, what the gray bar represents)*

**Authors:** We have reorganized the Nd isotope composition figure (Fig. 4a).

• *Line 200 – should read "since the late Pliocene"*

**Authors:** Done.

***Reviewer #2 (Remarks to the Author):***

This paper explores contourite sedimentary features in the South China Sea using seismic data and drillings from ODP and IODP sites. The analysis reveal several oceanographic changes from the Oligocene to End-Miocene, which the authors link to paleogeographic changes of the Indonesian Gateway and the Indian/Tethys Seaway. Generally I think this study is interesting, the paper is clear and well written, and it could potentially be of interest to this journal. However, I find the discussions around potential causes of changes in the deep ocean circulation a bit too weak in its current state, and the link to the Tethys/Indian gateway need to be explained more. I also think the paper would improve by adding comparisons to paleo ocean circulation models, and would benefit from including a broader discussion on the climatic changes of the Miocene and how they might influence circulation.

I have added my major comments below, followed by a few minor comments.

The link to the closure of Indian Gateway/Tethys seaway need to be explained in more detail. My main issue is that while this seaway likely re-opened and closed in the Miocene, the depth during these times was on the order ~100 m, which could play some role for surface circulation, but how this signal would transmit to the deep ocean need to be better explained. Could this be linked to changes in upwelling in the Indian Ocean following circulation changes for example? See e.g. Sarr et al. 2022, or Hamon et al. 2013?

**Authors:** We thank the valuable suggestions provided by the reviewer. Previously overlooked, we now acknowledge the depth variation of the Indian Gateway (Tethys Seaway). It appears that the Indian Gateway was too shallow to allow for the penetration of Circumpolar Deep Water (CDW), as it typically exists at depths of 2000-4000 meters. Consequently, the influences of the Indian and Indonesian Gateways during the Middle and Late Miocene are not directly comparable. Therefore, the related discussion has been omitted from the manuscript.

Moreover, inspired by the references cited, we have introduced the possibility of deep-water upwelling occurring in the Indonesian Gateway. Additional discussion on this topic has been incorporated into the text (Lines 199-201).

As this discussion is written now, I would think more local changes, or large scale global circulation changes (perhaps related to climatic changes) could also cause your observed changes in the Mid Miocene, and this would be good to include in the discussion. Also, the “circum equatorial circulation” is perhaps something that would be more expected in the Early Eocene? Especially if one considers deep ocean circulation. I am not convinced this was active in the Miocene considering the Tethys (and also likely the Central American Seaway) were shallow.

**Authors:** We have expanded the text in the "*Circumpolar Deep Water variations*" section to include other potential control factors for the paleoceanographic changes. Upon further consideration, we recognize that the evidence is insufficient to support a direct linkage between the tectonics of the Indonesian Gateway and the Indian Gateway (Tethys Seaway), as well as the pattern of circum-equatorial circulation. To prevent any confusion for readers, we have removed the related expressions from the text.

The results are not compared to model simulations. I think one could improve the discussion by viewing your results in comparison to models simulations. See for example available miocene modeling efforts in Burls et al. 2021. Regarding this, the Miocene climatic changes could potentially have influenced the large-scale ocean circulation pattern which would also be good to include more in your discussions (as mentioned above).

**Authors:** We add additional discussion in "*Regional and global implications*" section (Lines 194-216).

Minor comments:

I think the name "Indian Gateway" is a bit Mediterranean centric, and prefer Tethys (or Eastern Tethys) Seaway. Or alternatively Mesopotamian Trough (which was the Seaway across Arabia open in the Miocene).

**Authors:** We have deleted the discussion on this seaway.

Figure 1: You should change the 'rainbow' colormap in the cross section. I recommend using a colormap that does not distort the data. See for example Crameri et al. 2020.

**Authors:** We acknowledge the concern regarding the rainbow colour scheme. The oceanographic profile (Fig. 1b) is automatically generated by the ODV (Ocean Data View) software, and it appears that the rainbow color scheme is the default option for this software. This method of presenting oceanographic data is widely used in oceanographic research. Similar examples can be found in the references provided.

However, in response to this concern, we have modified and added a new colour map for the figures (Figs. 3 and 4) based on the guidelines outlined by Crameri et al., 2020.

Resing et al., 2015 (https://doi.org/10.1038/nature14577); Hu and Piotrowski, 2018 (https://doi.org/10.1038/s41467-018-07079-z); Penna et al., 2023 (https://doi.org/10.5194/essd-15-3513-2023).

Best regards,

E. O. Straume

Useful references:

Burls, N. J., Bradshaw, C. D., De Boer, A. M., Herold, N., Huber, M., et al. (2021). Simulating Miocene warmth: Insights from an opportunistic multi-model ensemble (MioMIP1). Paleoceanography and Paleoclimatology, 36, e2020PA004054. https://doi.org/10.1029/2020PA004054

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***Reviewer #3 (Remarks to the Author):***

This paper re-assesses the timing of opening and then closing of the Indonesian Gateway between the Indian and Pacific Oceans. The authors investigate these issues using seismic reflection imaging of contourite drift sedimentation tied to multiple IODP holes. The potential influence of oceanic gateways on Earth's climate is a major question in Earth science. In principle, the way authors integrate multiple data sources to propose new conclusions about gateway opening seems to me in line with Nature family expectations. The main advances are (1) pushing back the age of initiation of strong deep water flow in the South China Sea from middle to earliest Miocene, (2) a new hypothesis that deep water entered the region from the Indian Ocean rather than the Pacific Ocean at this time, and (3) an inference that deep water connection to the Indian Ocean was severed at 10 Ma. Points (2) and (3) seem to me to be potentially interesting steps forward, but are not yet fully justified by the data presented here. Point (1) above seems to me more of an incremental advance. The methodology is sound. I am happy with the analysis of the seismic and borehole sedimentology data. I do not think that the interpretations based on published chemical proxies (d13C and eNd) are justified based on the data shown, as I explain below. I therefore think that a main conclusions are not justified in this draft. The authors might be able to fix the problem with the chemical data in a revised draft.

**Authors:** We have incorporated new data and generated new figures depicting chemical data from various oceans in Figure 4. Additionally, we have revised the relevant sections of the text, particularly in the "*Circumpolar Deep Water variations*" section, to reflect these updates.

INDIAN OCEAN INFLUENCE, AND CLOSING OF THE GATEWAY AT C. 10 MA

I am quite confused by the whole "Circumpolar Deep Water from the Indian Ocean" section. I do not think that the chemical data in Fig. 3 are sufficient to justify several of the interpretations made in the text. A key argument is that CDW chemical signal did not reach N Pacific until c. 15 Ma (L144-146). The authors therefore infer that early Miocene contourite drifts in the South China Sea must have bee generated by CDW from the Indian Ocean (L147). I don't see how this follows. Surely the contourite drifts in the SCS could have been sourced from the equatorial Pacific, as implied by Fig. 3b (see below).Nevertheless, it is inferred that CDW must have reached the study area from the Indian Ocean (but not from the Pacific) at 23 Ma to form the mounded drifts observed on the seismic data. A problem here is that there is no eNd data from either the eastern Indian Ocean or the South China Sea on Fig. 3b prior to 15 Ma. Thus I'm not sure there is any positive evidence presented here from eNd water chemistry in support of Indian Ocean-sourced CDW in the S China Sea prior to 15 Ma.

**Authors:** We fully appreciate the concern raised by the reviewer. Regrettably, there are currently no published Early Miocene εNd (t) values available for the South China Sea. Additionally, we lack samples of fish teeth or ferromanganese nodules to assess the related Nd isotope compositions. However, in response to this limitation, we have included more comprehensive comparisons of benthic δ13C values between the South China Sea, the western Pacific, and the Indian Ocean (Fig. 4).

Our analysis reveals that benthic δ13C proxies from the South China Sea and the Ontong Java Plateau in the western Pacific exhibit comparable trends after 8-10 Ma (Fig. 4b). Conversely, the values from the South China Sea and the Bay of Bengal in the northeastern Indian Ocean demonstrate similar patterns before 10 Ma (Fig. 4c).

What Fig. 3b does show is that at c. 14 Ma the eNd composition of the South China Sea is mid-way between the compositions of the equatorial Pacific and eastern Indian Oceans. Therefore, a general reader could hypothesize that at 14 Ma the S China Sea was influenced by both Indian and Pacific waters, which mixed to give the observed composition. Yet L147 implies that the mounded drifts between 23 and 10 Ma in the S China Sea formed beneath Indian-sourced CDW only. I think it would help to recognize the interpretation that the SCS could have been influenced by CDW from both Pacific and Indian Oceans. If this is not the case, then the evidence for an Indian-only source needs to be stronger and more clearly explained.

**Authors:** We have revised the text to reflect the updates (Lines 140-156). Nd isotope compositions from the South China Sea fall between those of the Ontong Java Plateau and the eastern Indian Ocean (Fig. 4a), indicating a significant mixing of Circumpolar Deep Water (CDW) from both the Indian and Pacific Oceans during the Middle Miocene. However, CDW from the Indian Ocean appears to have played a more dominant role in the mixing processes, as evidenced by the more comparable benthic δ13C proxies of the South China Sea and the Bay of Bengal between 10 and 15 Ma (Figs. 4b and c).

Fig. 3b also shows that by 3 Ma the eNd composition of the S China Sea was the same as the equatorial Pacific and distinct from the eastern Indian Ocean. It is interpreted that the disconnection between the S China Sea and Indian Oceans happened at 1O Ma on the basis of the seismic data. However, the chemical data from the western Indian Ocean on Fig. 3b has similar composition to the S China Sea younger than 10 Ma, perhaps to about 8 Ma. Taken at face value, this comparison seems to contradict the inference of disconnection at 10 Ma from the seismic data. On the other hand, the W Indian Ocean is quite far from the S China Sea, and quite different from the E Indian Ocean dataset, which is closer. I also note the development of drifts post 10 Ma in the Phillipine Sea. Should we then ignore the W Indian Ocean data?

**Authors:** We thank again the comments from the reviewer. We have removed the Nd isotope composition data from IODP Site 1457 in the western Indian Ocean. This decision was made to prevent any potential confusion, as this location appears to be more influenced by Atlantic deep waters, which possess εNd values that are distinct from those of the Pacific.

In the d13C data in Fig. 3c, the three curves shown are from the S China Sea, Indian Ocean and a Global compilation. Rather than using a global compilation, would it not be better to show a Pacific Ocean record? Fig. 3c shows that the d13C composition of the S China Sea is different from the global average, but it is not clear whether it is different from the Pacific Ocean, since the Atlantic and Southern Oceans also contribute to the global average, and the compositions sometimes differ between these oceans.

**Authors:** The new figure (Fig. 4) presents chemical proxies spanning different oceans throughout the geological timescale. It includes benthic δ13C records from various sites, such as IODP Site 1443, ODP Site 1148, ODP Sites 757 and 758, along with global ocean compositions. Notably, the observed changes in chemical data exhibit similar patterns when compared to the seismic interpretation (Fig. 2) and sedimentary stacking patterns (Fig. 3).

A significant shortcoming of the d13C data in Fig. 3c is that no Indian Ocean records for younger than 10 Ma. The figure therefore seems to contradict L180-182, which states that benthic d13C in the Indian Ocean differs from that in the S China Sea younger than 10 Ma. Since a main point of the paper is to demonstrate that connection between the S China Sea and Indian Oceans was broken at 10 Ma, I think it is very important to show some d13C data from the E Indian Ocean younger than 10 Ma on Fig. 3 in support of the statements in the text.

**Authors:** We add additional data in Fig. 4c.

ONSET OF STRONG BOTTOM CURRENTS

Refs 16 and 17 show interpretations of sedimentary archives on seismic data in the South China Sea back to Late Miocene and Early Miocene respectively. The present manuscript pushes that back to the Oligocene. Ref. 16 indicates onset of strong bottom currents in the S China Sea about 18 Ma with a Pacific source. The present manuscript pushes that back to 23 Ma and argues (not yet convincingly - see above) for an Indian source. I'm not sure that the revised timing counts as much earlier than previously documented examples (L206); one could argue it is an incremental advance. It would help if the authors could make clearer why this change in means a step change in our understanding.

**Authors:** We added new discussions in Lines 226-238. The onset of deep-water contourite features in the South China Sea indicate a significant influence of CDW as early as 23 Ma in the low-latitudes of the North Pacific.

GENERAL POINTS

Abstract doesn't make clear what new analysis has been done. Need a "Here, we ..." sentence that mentions methods as well as materials. Final sentence bland; could be appended to numerous papers.

**Authors:** We have modified the abstract.

Results go straight into statements about mounded, sheeted, plastered drifts and moats without explaining what these features are and why they are important. I think it might would help to have a paragraph between the intro and results sections that outlines hypotheses; for example, how and open and a closed Indonesian Gateway might be expected to show in terms of changes seismic geometry and sedimentology.

**Authors:** We add introductions on contourite features in Lines 47-50. We also add the Indonesian Gateway tectonics in introduction (Lines 22-26).

Contourite evolutionary stages look reasonably well justified by the data in the figs.

I think that the paragraph L175-183 belongs in the preceding sub-section "Circumpolar Deep Water from the Indian Ocean".

**Authors:** Done (Lines 157-166).

In Fig. 2, some of the mounded and plastered drifts look similar in morphology to me; it would help to explain how you distinguished these two morpholgies. I eventually saw that some of this information in the Supplementary Fig. 2; perhaps this could be flagged earlier.

**Authors:** We move this information to the beginning of the Results and Discussion section.

Fig. 3. Several datasets on Fig 3b are not marked on the maps. I cannot see 05E107 (apparently in S China Sea) or D137 (North Pacific) on the maps - perhaps you could make these clearer. It would also be nice to see the actual data points for U1457.

**Authors:** We add the location of these drilling sites in Fig. 1.

Fig. 4. Location of D needs to be show on panels A to C of Fig. 4 - not only on FIg. 1. Sup. Fig. 3 is much clearer in this respect.

**Authors:** Done.

Sup Fig. 1. Missing definition / example of sheeted drifts.

**Authors:** Done.

Sup Fig. 2. Please use colour coding same as Fig. 2 for drift interpretations.

**Authors:** Done.

**REVIEWER COMMENTS**

Reviewer #1 (Remarks to the Author):

Having looked at the revised manuscript, the authors have made a substantial but incomplete attempt to address my original concerns.

Incorporation of U1505C is a major improvement, as it is the (only) site that goes through the entire records with no hiatuses, dating the seismic horizons convincingly and pulling the whole story together. As it is so important, I suggest moving it from Fig. S2 to Fig. 2.

In terms of tying the sediment logs to the seismic reflector horizons, this is still a little loose and I am not sure it could be described as 'remarkable' (as in the response). However, as long as the text in the manuscript is kept very vague in terms of precise ages (please give healthy uncertainties) this should be fine.

"Contourite evolutionary stages since 32 Ma"
This section for instance gives no indication of the age uncertainties. Looking at Fig. S4, where a simple straight line is drawn that goes near only some biostratigraphic tie points, there appears to be up to a ca. 1 Ma departure from this line for some tie points, giving a large uncertainty. There will be biostratigraphic-related age uncertainties too, which are not discussed, and depth uncertainties with the seismic reflectors, and so you can see where I'm coming from here. Readers should be aware that each of the seismic horizons (T1, T2 etc) are very loose in terms of age determination. This needs to be squarely discussed. Does Stage II end at 15.5 Ma for instance, or 15.5 +/- 1 Ma? This is extremely important when comparing with events like the MMCT (13.9 Ma) and NHG (2.6 Ma).

If the NGR curves are central to correlating the sediment cores to the seismic, then they should probably also be moved out of the supplement into the main text somehow.

The correlation of seismic data in Fig. S4 is a nice addition.

Line 48 – "through geological time"
Fig. 3 – 1505C shows no lithology below T60, whilst it does on Fig. S3 – correct
Fig. S3 – all colours including yellow need to be in the key
Fig. 4 – what is the yellow shading? It should be clearly marked up
Line 151 – should be Fig. 4a?
Line 153 – evidence
Line 156 – paleobathymetric
Line 159 – Fig. 5?

Reviewer #2 (Remarks to the Author):

I think the authors have addressed my concerns and I am happy with the current version of this manuscript.
I have no further comments

Best regards,
E.O. Straume

Reviewer #4 (Remarks to the Author):

This revised manuscript by Liu et al. effectively utilizes seismic data and core analysis to investigate that how closure and opening of the straits/gateways control the intricate relationship between the Pacific and Indian Oceans during the Cenozoic. The authors have addressed the comments from the previous review round (especially Reviewer 3), resulting in a significantly enhanced manuscript. The incorporation of new data and refined interpretations strengthens the overall contribution of this work to our comprehension of global deep-water circulation dynamics in response to changing oceanic gateways.
Specific comments:
(1) While the absence of Nd isotope data for the early Miocene is a limitation, the authors have effectively mitigated this issue by providing comprehensive benthic δ13C isotope records from the South China Sea, Western Pacific, and Indian Ocean. These data offer valuable insights into the potential influence of Indian Ocean waters on the South China Sea during this period.
(2) Reviewer 3 is right that approximately 14 Ma the Nd composition of the South China Sea is mid-way between that of the equatorial Pacific and eastern Indian Oceans and therefore suggest mixing of the Indian and Pacific waters. The authors have incorporated this advice. In addition to Nd composition, the authors seems right that the Indian Ocean appears to have played a more dominant role in the mixing processes, based on the more comparable benthic δ13C proxies of the South China Sea and the Indian Ocean (Bay of Bengal).
(3) Seismic-geochemical inconsistency. Reviewer 3 nicely proposed the inconsistency between the seismic data and the geochemical data around 10 Ma. The authors' explanation for the discrepancy between seismic and geochemical data around 10 Ma is reasonable. Therefore, it is smart that the authors delete the Nd isotope data from IODP Site 1457 to prevent confusion.
(4) Based on the useful comment of showing Pacific record (Reviewer 3), The authors have compiled records from the Pacific, the Indian Ocean to clearly show how critical straits/seaways (e.g., Luzon Strait, Indonesian Seaway and central American seaway) controls inter-connectivity between ocean basins. I find this figure very useful and valuable.
(5) Reviewer 3 is right that in Fig. 3c there is no Indian Ocean records prior to 10 Ma. Based on this comment, the authors have provided d13C data and thus can solve this problem.
(6) I support that the new text Lines (226-238) improves much and indicates a much earlier influence of CDW as early as 23 Ma. To further strengthen this point, it would be beneficial to explicitly confirm the consistency between the accretion direction of the contourite drift and the NE-ward flow of the CDW.

Overall, the authors have made substantial improvements to the manuscript based on the reviewer comments. The incorporation of new data and refined interpretations enhances the overall quality and impact of the work.

Congratulations!

Best wishes,
Xin Shan

# South China Sea records Late Miocene reorganization of western Pacific deep circulation

Liu et al.

RESPONSE TO REFEREES

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Incorporation of U1505C is a major improvement, as it is the (only) site that goes through the entire records with no hiatuses, dating the seismic horizons convincingly and pulling the whole story together. As it is so important, I suggest moving it from Fig. S2 to Fig. 2.

**Authors:** We have reorganized the supplementary figures and added a new figure (Figure 2) to the main text to illustrate the age model and the time-to-depth conversion.

In terms of tying the sediment logs to the seismic reflector horizons, this is still a little loose and I am not sure it could be described as 'remarkable' (as in the response). However, as long as the text in the manuscript is kept very vague in terms of precise ages (please give healthy uncertainties) this should be fine.

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**Authors:** The age models from other IODP sites have been added (Fig. 2c). The time-to-depth conversion is based on a combination of models from various sites. We have also revised the text to prevent any confusion for readers. Additionally, further discussions on the age model have been included in the Methods section.

If the NGR curves are central to correlating the sediment cores to the seismic, then they should probably also be moved out of the supplement into the main text somehow.

**Authors:** Done. See Fig. 2d.

The correlation of seismic data in Fig. S4 is a nice addition.

Line 48 – "through geological time"

**Authors:** Done.

Fig. 3 – 1505C shows no lithology below T60, whilst it does on Fig. S3 – correct

**Authors:** Done.

Fig. S3 – all colours including yellow need to be in the key

**Authors:** Done (Fig. 2d).

Fig. 4 – what is the yellow shading? It should be clearly marked up

**Authors:** Done (Fig. 5c).

Line 151 – should be Fig. 4a?

**Authors:** Corrected.

Line 153 – evidence

**Authors:** We change "evident" to "pronounced".

Line 156 – paleobathymetric

**Authors:** Done.

Line 159 – Fig. 5?

**Authors:** Done.

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This revised manuscript by Liu et al. effectively utilizes seismic data and core analysis to investigate that how closure and opening of the straits/gateways control the intricate relationship between the Pacific and Indian Oceans during the Cenozoic. The authors have addressed the comments from the previous review round (especially Reviewer 3), resulting in a significantly enhanced manuscript. The incorporation of new data and refined interpretations strengthens the overall contribution of this work to our comprehension of global deep-water circulation dynamics in response to changing oceanic gateways.

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Overall, the authors have made substantial improvements to the manuscript based on the reviewer comments. The incorporation of new data and refined interpretations enhances the overall quality and impact of the work.

**Authors:** We appreciate the reviewer’s comments. The absence of Nd isotope composition data remains a limitation in our study. However, we are fortunate that a paper on the εNd record (27-10 Ma) of fossil fish teeth in the South China Sea was published recently, after we submitted the last revised version of our manuscript.

**Reference:**

Shu, W., Colin, C., Liu, Z., & Dapoigny, A. (2024). Late Oligocene–Miocene evolution of deep-water circulation in the abyssal South China Sea: Insights from Nd isotopes of fossil fish teeth. Geology, 52, 620-624.

One of the most significant conclusions of our work is the Sulawesi gateway closure and its influence on palaeoceanography. The closure of the Sulawesi pathway likely occurred in two stages: (1) initial shoaling and restriction of the pathway around 14-15 Ma, and (2) the final disruption of deep-water connections between the Indian and Pacific Oceans around 10 Ma. The results from Shu et al. (2024), particularly the seawater εNd records from IODP U1499 in the South China Sea, indicate such changes on palaeoceanography, and these εNd variations strongly support the hypothesis presented in our study. Consequently, we have updated Figure 5a. These εNd records, along with sedimentary records and benthic δ13C isotope records, provide significant support for our results and conclusions.

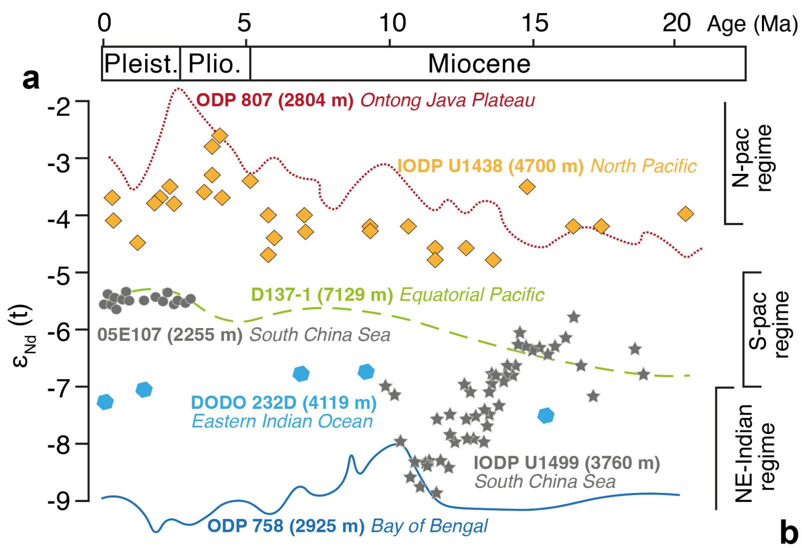


Figure 5a

**REVIEWERS' COMMENTS**

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Overall I think the authors have done a good job to incorporate the various changes, and in particular I think the new figure really improves the paper. However, the significant age uncertainties (in particular when referring to sub-Ma dates such as 15.5 and 5.3 Ma) are not fully addressed by adding 'ca' to all the dates. I suggest also moving the new text from the Methods (line 295-298) to the end of the paragraph on line 94. With that done I would recommend publication.

# South China Sea records Late Miocene reorganization of western Pacific deep circulation

Liu et al.

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