We would like to thank Mario Krapp and the anonymous referee for their constructive comments. Below we respond to the remarks and suggestions, following the order in which they were presented to us.

General comments

The aim and approach of this study

It follows from this main comment that the Referee was expecting a different type of approach to the problem. We now understand that we have to be more explicit about our aims and the reasons for the approach we adopt.

Perhaps it is best to explicitly state that our aim is *not* the reconstruction of the Miocene Mediterranean Sea/Paratethys. Instead, we wish to gain physics-based insight into one of the factors we expect played a key role: the gradual closure of the Indian gateway. This is why the closure was mentioned in the second part of the title. Adding "The role of…" or "Sensitivity to…" would perhaps emphasize this more.

Our focus on changes brought about by the evolving gateway is the first reason why we think it is not necessary to use Miocene boundary conditions (atmospheric forcing and conditions of the open oceans). The second reason for preferring not to do so is that these Miocene conditions are not certain. While we agree that there are several global-scale climate models that we could, in principle, turn to, close inspection of these model studies shows large differences in results. Also, many of these models differ in their geometry (most notably: gateway depth) to such extent from our regional-scale model that linking to them is questionable. Thus, while we have explored the approach suggested by the Referee in earlier work (see Karami, 2011) we here choose to start from idealized boundary conditions based on the present day.

Of course this approach brings with it the question: would the shoaling gateway have a similar effect if conditions were different? This is the reason we already investigated the sensitivity to the latitudinal variation of sea surface temperature and the distribution of net evaporation. We have now added to this an investigation of the role of conditions in the sponges. In this experiment we tried to reproduce a scenario in which the Atlantic is not only colder than the Indian Ocean - as it was prescribed in the experiments performed until now - but also saltier as suggested by many global climate models (i.e., Krapp and Jungclaus., 2011; Herold et al., 2012; Hamon et al., 2013). From the latter simulations (Figures B and C), in which the Atlantic salinity is set to 36, it can be appreciated that no substantial changes in the circulation pattern are found with respect to the equivalent experiments with previous box conditions (see Figures 4c/4d and 5c/5d). Once more we conclude that a shallow Indian Gateway can be considered effectively closed from an ocean-circulation perspective. The insight gained from this experiment will be included in a revised version in which we will also explain more explicitly our aims and approach.

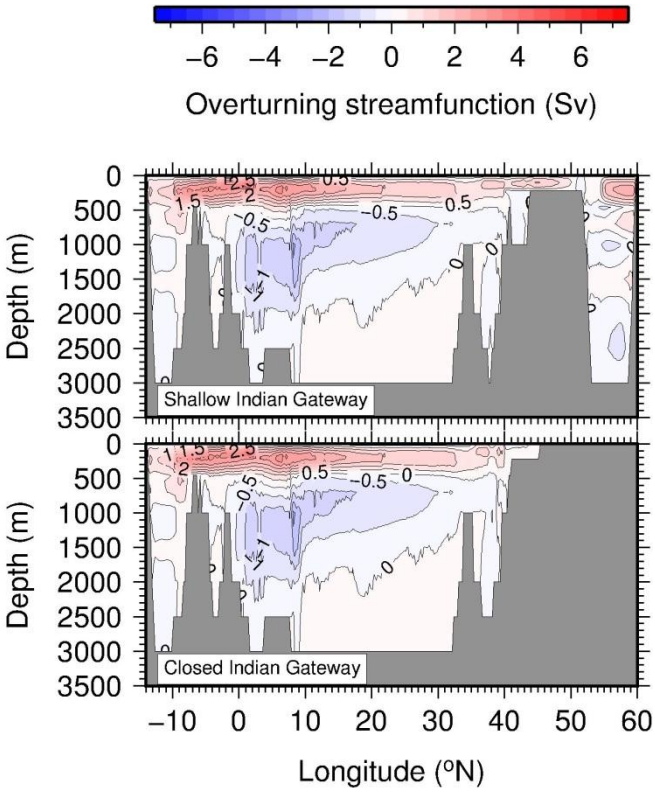

Figure B. Zonal overturning streamfunction for a shallow and closed Indian Gateway with new box conditions (Atlantic and Indian salinities set to 36 and 35)

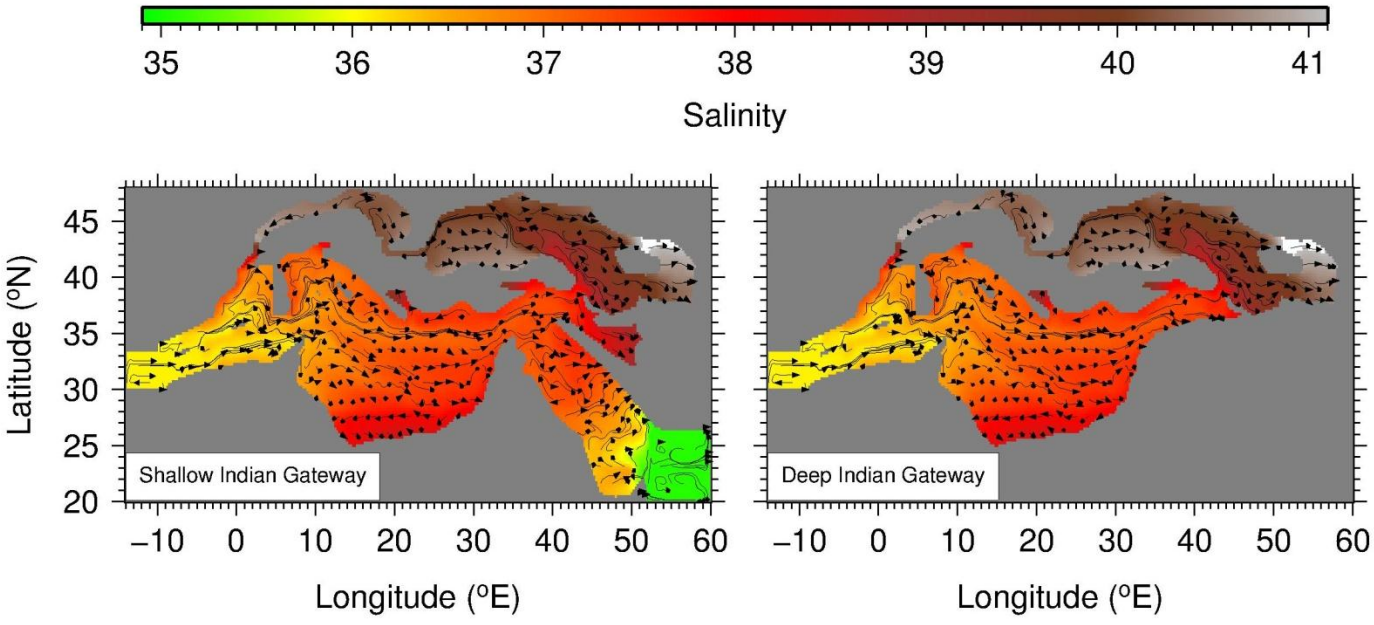

Figure C. Salinity field and trajectories of horizontal flow at 10m depth for a shallow and a closed Indian Gateway with the new box conditions (Atlantic and Indian salinities set to 36 and 35). Shown are the paths particles would travel in 80 days keeping the annual-mean velocity field constant.

Specific comments

Page 4391, lines 18-24: The authors point out at the end of this paragraph a major weak point of this study. I am aware that other authors were criticized for not having a true open boundary to the Indian Ocean. Thus, the heading of this section could also say "closed-ocean boundaries".

The heading indicates that this section is about the representation of the open ocean and does not refer to the nature of the boundary condition. We refer the reader to our response to the first point of reviewer M. Krapp for a discussion of the potential role of missing the barotropic component of gateway flow.

Page 4391, lines 25-28: The authors use a salinity of 35 from top to bottom but prescribe on the following page a more realistic temperature profile. The use of a vertical homogenous salinity distribution does not seem to be realistic. Why do they use exactly a salinity of 35?

We decided to use this value for several reasons: i) it is the ocean-average salinity, ii) the vertical variation of the salinity is not as large as that for the temperature and iii) setting the salinity to this constant value inside the Atlantic box has been proved to reproduce realistically the present-day Mediterranean circulation. However, it was not possible to set the temperature to a constant and uniform value: i) we need to prevent large temperature differences between the box and the adjacent waters with depth and ii) a match between the surface layers and the atmospheric forcing, which varies with latitude, has to be ensured.

We will explain in more detail this reasoning in a revised version. Also, note that as part of our response to the Referee's main point we have now investigated the effect of using other values than 35.

Page 4392, lines 1-5: The authors prescribe a vertical temperature profile for a paleo simulation using a present-day profile. Can we assume that the vertical water mass distribution was similar? The authors point out in their conclusions that the closure of the Indian Gateway has changed the water mass composition in the Indian Ocean meaning it must have been different from present-day boundary conditions. A citation and/or discussion could provide more insight.

The reasons for basing our boundary conditions on the present-day situation are explained in our response to the Referee's general comment. We have now investigated the role of changing the conditions in the sponges (see above).

The authors use as salinity unit psu. That unit is not supposed to be used anymore. It should simply say something like the salinity is 35.

Point taken for the revised version.

Page 4320, lines 20-26: The authors neglect the wind field. It is know that both wind regime and also for example Monsoon.

The statement is incomplete but appears aimed at our neglect of the wind field. We decided to not incorporate winds for the reasons explained in the text (p. 4392/l. 20-26): i) it would introduce more uncertainties to the model results and the present-day winds are not appropriate, and ii) Meijer and Dijkstra (2009) - the work in which our model settings are based on - found that the 100-year average circulation and water properties of the Mediterranean Sea could be reproduced with idealized forcing ignoring winds.

Page 4394, line 22: replace "deep" by "depth".
Thanks for making this point.

Page 4394, line 22-26: The authors discuss the transport at the AG and IG. A discussion how realistic the transports are using sponge layers that are fairly close to the area of interest.
We carefully looked for the optimal position of the sponges in our work. To that end, we performed a series of experiments in which the sponges were placed gradually further away from the gateways (in which transport is monitored) and found very similar results.

Page 4395, lines 1-18: How do the authors determine a realistic inflow/out flow regime for the Miocene if they use present-day boundary conditions? Relaxation coefficients also determine the solution and should be discussed here.
This again relates to our overall approach which is not about reconstructing Miocene conditions in an absolute sense but concentrates on finding changes due to changing gateway depth. We realize this needs to be repeated in this context. Our relaxation coefficients are based on Meijer and Dijkstra (2009) and have been proven to reproduce the main features of the present-day Mediterranean circulation.

Page 4399, lines 4-22: I have already asked this question but would like to repeat that there are many studies showing that the bathymetry of the Miocene has a far reaching impact on climate and ocean circulation. Thus, I am surprised that the authors get similar results when they swap their present-day boundary conditions for idealized Miocene boundary conditions. Why wouldn't they use it from the beginning and then compare it to perhaps one present-day simulation.
This has been explained in our response to the main comment of the Referee.

Page 4404, line 7-8: correct "stablishment" to "establishment"
Yes, thanks for the observation.

Page 4408: lines 4-6: The given statement may be true but can it be concluded from a regional model? The statement that the closure of the Indian Gateway may have an effect of the buildup of the EAIS cannot be drawn from this regional study.
Point taken. We will mention in the conclusions and abstract that closure interrupted a source of relatively warm and salty waters to the Indian Ocean, but we will only speculate about its connection with the East Antarctic Ice Sheet in the discussion.

Section References: The references are to be outdated. There are newer studies and credits should be given.
We have had a look at the papers you suggest and some of them are very useful. We will certainly include them, especially in the introduction, where we are planning to make a more detailed description of previous work on the Indian Gateway closure.

Table 3: The "average temperature inflow" for AG and the "heat transport inflow" seem to go hand in hand, i.e., increase in temperature means an increase in "heat transport inflow". This is not true for "Shallow IG". The temperature hits a minimum but the heat transport stays high. Are the 16.09 and/or 3.360 the correct values?

Yes, they are. Heat transport does not only depend on the temperature, but also in the amount of water exchanged. In this case, the average temperature of the outflow decreases, but the water transport increases.

In summary, I find the study put forward interesting. However, the forcings and results should be evaluated against Miocene proxy data and modeling studies before the manuscript can be considered for publication.

This summary statement actually includes a new remark: that we should evaluate our model results against available proxy data. For this we refer the reader to our response to the last general comment of Referee M. Krapp.

References

Hamon, N, Sepulchre, P., Lefebvre, V. and Ramstein, G.: The role of the East-Tethys seaway closure in the middle Miocene climatic transition, Clim. Past Discuss., 9, 2115-2152, 2013.

Herold, N., Huber, M., Müller, R.D., and Seton, M.: Modeling the Miocene climatic optimum: Ocean circulation, Paleoceanography, 27, PA1209, doi: 10.1029/2010PA002041, 2012.

Karami, M. P.: Palaeoceanography of the Miocene Mediterranean Sea and Paratethys: regional ocean modelling of the response to closure of the Tethys Seaway, Ph. D. thesis, Utrecht University, the Netherlands, 2011. Available open acces on: http://igiturarchive.library.uu.nl/dissertations/2011-0331-200559/UUindex.html

Krapp, M. and Jungclaus, J.H.: The Middle Miocene climate as modelled in an atmosphere-ocean-biosphere model, Clim. Past, 7, 1169–1188, 2011.

Meijer, P. Th. and Dijkstra, H. A.: The response of Mediterranean thermohaline circulation to climate change: a minimal model, Clim. Past, 5, 713–720, doi: 10.5194/cp-5-713-2009, 2009.

Acknowledgements

We thank Tanja Kouwenhoven for sharing with us her insight regarding the (im-) possibilities of proxy data.