Ocean Science
Discussions

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Interactive comment on "Impact of the Indonesian throughflow on Agulhas leakage" *by* D. Le Bars et al.

D. Le Bars et al.

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We thank referee #1 for his useful comments on our manuscript. The main issue that he/she is raising is the use of the linear analytical model. This model appears not able to answer the question concerning the physical mechanism involved in setting a constant retroflection index when the Indonesian Throughflow (ITF) is opened. We understand and agree with this concern. The linear model results show that the large-scale constraints on the system (geometry of the basin and wind stress) are not solely responsible for the behavior of the retroflection index when the ITF is opened/closed. Although this result is interesting, we propose to put the derivation and the discussion of this model in an appendix and only refer to the main result in the main text of the paper. Instead, we will include more analysis on the HIM results to explain the behavior

of the retrofleciton index. In addition, we will add a comparison with the model results of Le Bars et al. (2012).

Answers to minor comments:

"p. 356, l. 17ff: A verb is missing in this sentence."

We will replace this sentence by "The possible effect of the ITF on the Agulhas leakage will be analyzed using two 75-yr long simulations..."

"p. 358, l. 13ff: POP and HIM are using a different vertical coordinate. Does this lead to any limitations in the interpretations?"

POP and HIM are very different models, they have different vertical coordinates but also different forcing, bottom topography and geometry. These differences indeed limit a direct quantitative comparison between the two models. However, in both models the retroflection index doesn't change when the Indonesian Throughflow is open/closed. This highlights the generality of this result. We will make this point clear in the revised manuscript.

"p. 360, l. 3: Bryden et al. (2005) are using a slanted section, which could lead to significant different when comparing to a zonal section. In addition, they restrict their transport over the top 2400m, southward velocities , and a certain time period. I would suggest to refine the comparison."

We will compare the model results and the observations in more detail concerning the Agulhas transport.

"p. 361, l. 28: What is the point here? Is the advection scheme not reasonably chosen in this POP configuration?"

We discuss here the ability of the POP model to reproduce realistically the observations of the Agulhas retroflection and try to understand why the retroflection position of POP is too far east. A high resolution is needed to model the processes involved in this

retroflection and when the resolution is increased in numerical models the choice of the numerical schemes becomes critical and model results crucially depend on this choice.

"p. 366, l. 3: Why would that be surprising, given the assumption that the retroflection is caused by local dynamics?"

Retroflection and leakage results from a subtle interplay between far field conditions and local dynamics. Local dynamics refer to the inertia and recirculation of the Agulhas Current and the associated barotropic/baroclinic instabilities. We expected that an increasing Agulhas Current transport would lead to an increase of its inertia, strengthen its instabilities and increase the retroflection index as shown in Le Bars et al. (2012). This is why the results are surprising. We understand that the term "local dynamics" can lead to confusion and will explain it better in the revised manuscript.

"p. 366, l. 20: No, there is a 50% difference, 10 Sv AC vs. 15 Sv ITF. "

We refer here to the HIM model and will make it more explicit in the revised manuscript. Indeed, the relation between ITF and AC is not as simple in POP and this is explained by the "Leeuwin Current system" (p.363, l.26 to p.363, l.4).

"p. 367, l. 2: It would help to illustrate this by adding POP to the figure, e.g. by calculating 5-10 yr averages of different wind strength. This would also add the range of natural variability to the diagram."

The adjustment time between an increased ITF and an increased leakage in POP is expected to be around 25 years (see Fig. 5). For this reason, we don't expect that the inter-annual variability seen in POP for both the ITF and the leakage to be related. POP would then provide only one point to the diagram, this value is discussed in the text.

"p. 371, l. 8: Some words on the saturating curves in Le Bars (2012) are needed: how does the linear theory compare to the barotropic and baroclinic cases in Le Bars et al.

(2012)?"

In addition to moving the linear model to an appendix we will discuss in more depth the comparison between this model and the barotropic and baroclinic shallow water models of Le Bars et al. (2012). In particular, it is interesting to notice that a very small retroflection index (R=0.1) is reached in the coarse resolution models when neither viscosity nor inertia is important enough to lead to retroflection. This relates to the linear model and explains the very small values of R.

"p. 371, l. 10: See my general comment above. How do you go on with that? This is unphysical (with AL > AC) and in strong contrast to the HIM results."

This result (AL>AC) is unrealistic but not unphysical, in fact it is also found by Dijkstra and de Ruijter (2001) (see figures 3 and 5) in a shallow-water model. It happens when a recirculation cell appears south of the African continent. We will also discuss this feature in the revised manuscript.

"p. 372, l. 8: An "et al." is missing to the reference."

This will be corrected.

"p. 373, l. 9: But Le Bars et al. (2012) show (except for the coarse-resolving barotropic case) a variation of R with wind stress."

We realize that this sentence could be misleading. Le Bars et al. 2012 explain the variation of the retroflection index by mechanisms that are not taken into account in the linear model: viscosity, inertia or turbulence depending on the dynamical regime. In this sense the results are consistent but not similar. To remove the ambiguity we propose to replace the sentence by these ones: " Results from a linear model indicate that the retroflection index is independent of the wind stress. This can be understood because it lacks all the mechanisms put forward by Dijkstra and de Ruiter (2001) and Le Bars et al. (2012) to explain the change of the retroflection index, i.e. viscosity, inertia or turbulence, depending on the dynamical regime."

"p. 373, l. 11: It *may* be consistent. However, the fact that the linear model differs from the HIM and POP results cannot be used for the conclusion that nonlinear effects are solely responsible for the discrepancy. That cuts a little too short. I would suggest using the idealized HIM setup to further close the gap between the linear model and POP. Otherwise, the discrepancy is too large and does confuse the reader."

As explained above, to avoid this confusion we will include the linear model in an appendix. However this gap can already be partly filled with the comparison of the linear model with coarser resolution versions of the HIM model, as explained above, which we will include in the revised manuscript.

"Tab. 2: Probably due to rounding the numbers do not match up. This should be noted somewhere or rounded to the first digit."

We will precise in the caption that all the numbers are rounded to 1Sv.

"What is the use of Fig. 3? This is similar to Fig. 2, and just slightly larger."

In Figure 3 we zoom on the retroflection area, which is the focus of this paper. We also change the color map to allow a precise comparison between the two models and the observations.

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