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Ocean Science
Discussions

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

Anonymous Referee #1

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The authors study the impact of inflow of Pacific waters into the Indian Ocean through the Indonesian passages on the circulation in the Agulhas region. They use a series of methodologies, including a global OGCM, an idealized setup and linear theory, to demonstrate that the transports in the Agulhas region are subject to change in response to an opening of the Indonesian Passage. The degree of Agulhas waters reflected back into the Indian Ocean, however, is not dependent on an opened or closed Indonesian Throughflow (ITF).

The topic of the study is interesting and timely; the connection between ITF and Agulhas leakage (AL) is an interesting piece in the discussion of the global thermohaline circulation. I find the manuscript well written and comprehensible. Apart from the inclusion of the linear theory (see general comment below) I have only minor comments

and would like to see this study appearing as an important contribution to the scientific discussion on the importance of the Agulhas system.

General comment:

Although I appreciate the development of the linear theory the comparison with the models cuts too short. The inconsistency among the most major points, (1) the variation of R wind stress (strong change in HIM, Fig. 9, no change in the theory, Fig. 12b), and (2) the dependency of R on ITF (no dependency in HIM, strong dependency, even unphysical in the theory), is not resolved by the authors. Just stating the discrepancy and arguing with the nonlinearity that is existent only in the models leaves the reader without any chance, if that is really the case or if there are shortcuts in the derivation of the linear theory. This discrepancy has to be resolved and argued more extensively.

Minor comments:

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

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

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.

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

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

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

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.

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)?

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.

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

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.

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.

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

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

Interactive comment on Ocean Sci. Discuss., 10, 353, 2013.