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Interactive comment on "Quantifying thermohaline circulations: seawater isotopic compositions and salinity as proxies of the ratio between advection time and evaporation time" *by* Hadar Berman et al.

Anonymous Referee #1

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In this work, the authors derive a new parameter (gamma) to quantify what they call the "strength" of the thermohaline circulation that would be valid for all regions where the flow in the surface layer is weak and driven by evaporation (L#90-91). According to L#117-118, gamma would be related to changes in salinity of the inflow water due to evaporation (??), if I understood correctly. This "strength" concept is not well defined in the paper and I have a lot of doubts about it. I suggest the authors work harder to give a clear physical definition for what they call "strength". There are bits and pieces spread throughout the MS.

Despite the fact that the subject could be of interest for highly evaporative, semi-

enclosed basins such as the Red Sea, most assumptions made to derive gamma are certainly not valid for open-ocean basins such as the western Indian Ocean. The science behind the MS, and its presentation are not what I would expect to see in a high-quality journal. The MS has several conceptual problems. For example, the concept of thermohaline circulation is not accurate. In the first line of the introduction, the authors state that evaporation minus precipitation drives the thermohaline circulations in general. While this statement may be somewhat acceptable in relation to the Red Sea, it is certainly wrong for the Atlantic Ocean, for example. I strongly suggest the authors read Wunch's paper in Science (Wunsch, C., 2002: What is the thermohaline circulation? Science, 298, 1179-1181).

My overall impression is that the authors were thinking about the Red Sea (i.e. A semi-enclosed basin), and generalized their concepts as being valid everywhere, including the open-oceanâĂŤbut such sloppy language induced statements that are simply wrong.

The authors also show a complete unfamiliarity with the hydrology, circulation and modern literature related to the southwestern Indian Ocean. This region is dominated by the westward-flowing South Equatorial Current (17S-18S), the eastward-flowing South Indian Countercurrent (23S-26S), characterized by strong eddy activity. There is also an anticyclonic cell centered east of Madagascar as shown by altimetric and hydrographic data. Besides the fact that the assumptions made by the authors are not valid in the southwestern Indian Ocean (There are a lot of good papers about this region published in the last ten years), the authors use only eight measurements to compute statistical relationships, which is misleading at least.

I also noticed the lack of important recent references about evaporation in the Red Sea. For example, Bower and Farrar (2015) show two-years of in situ evaporation measurements taken in the northern Red Sea (Bower and Farrar (2015), Air–Sea Interaction and Horizontal Circulation in the Red Sea. In: N.M.A. Rasul and I.C.F. Stewart (Eds.), The Red Sea,âĂÍ Springer Earth System Sciences, DOI 10.1007/978-3-662-45201-

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Broadly speaking, the MS is rich in problems, lack new findings, and no innovations. Additionally, the title is quite obscure. I do not want to overwhelm the authors with such negative comments. My suggestion is to focus on the Red Sea, where some of the assumptions may be valid, and delete the western Indian Ocean part that compromised the MS with a large amount of errors. Instead of that, why not look at other semi-enclosed basins, such as the Mediterranean Sea? I believe the authors can find data there, maybe with contributions from other researchers. I would re-write completely the abstract and the introduction. It is a scientific manuscript, and therefore accurate statements are vital.

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