Review of Shoonover et al preprint titled “FEOTS: A New Offline Code for the Fast Equilibrium of Tracers in the Ocean”

General Comments:
This well-written manuscript explores the challenge of applying transport matrix technology to a higher-resolution parent model (nominal 0.3-degree, 100 depth levels), where there is even more to be gained than in the typical 1-degree model. There is a greatly enhanced technique for generating the IRF stencils such that a minimal number of tracers are needed to capture the IRF results. The use of a set of regional transport matrices to analyze the circulation in the Argentine Basin highlights the use of the transport matrix technique to develop a regional model for in-depth analysis of a particular area of the ocean. This facilitates gaining meaningful insights at relatively low computer cost.

While this is a useful exploration, in the example, the transport matrices are simply run forward for a few years; there is no “fast equilibrium” solution attempted. Therefore, the paper should be retitled to better describe its actual content, and what was accomplished.

Specific Comments:

@ line 50
Rephrase this to reflect what is accomplished, and to distinguish it from future plans.

@ 62
Convergence rates? Perhaps you mean volume transport rates? (So as not to be confused with the convergence rates to an equilibrium solution.)

@ 93 Section Graph Coloring approach to Operator Diagnosis
This would be easier to comprehend after having read Section 2.4 Parent Model, where the advection scheme is discussed, and the motivation for the graph coloring approach becomes clear.

@ 125
State which advection scheme (Table 1) was actually used in the offline transport matrix model for the example in this paper.

@ 150 and forward
Somewhere (Section 2.4?) please indicate the timestep used, and the relationship to the CFL for both the parent and the transport matrix model.

@ 164 and forward
From this it is assumed that there is NO horizontal diffusion term, as none is generated by the online parent model. Please restate the offline equation (eq. 10) with the missing horizontal diffusion term, and clarify which of the options in Table 1 was used.

@175
A 63-year spin-up seems short for examining deep water masses; the basin extends to 6000m. A comparison with observational temperature, salinity, and flow statistics as was done in Weijer et al, 2020 would be a good addition.

@177
Is the data volume given for IRF-output and transport operators for the entire ocean, or for the region to be studied?

@ 178 and forward
Clearly state when you are running the global model versus the regional model.

@ 200 and forward
This is a clear and useful discussion of the constant preservation issue, which is frequently overlooked until strange results show up. An additional useful criterion is the rate at which total tracer quantity is gained or lost. This will impact the ability to get a physically meaningful equilibrium solution.

@ 203
The use of single precision for the parent model will limit the usefulness of the results. It also makes it difficult to distinguish the sources of errors in the analysis.

@212 this paragraph
Clarify that the error data is from running the global or regional version of the offline model. There appears to be an assumption that these errors are not present in the parent dynamic model. It would be instructive to run this test on the dynamic model, in order to compare the error level inherent in the parent dynamic model.

@ 222
…"with and without mixing": vertical mixing? Please clarify.

@233
How does the 1000m division relate to the mixed-layer depth of the model?
In Figures 4 and 5, Dye 4, especially, raises the question about the depth of the mixed layer in this vicinity.

Figure 4:
The scales on the figures are not all the same. This creates the visual impression that the small contributions are more important than they are, compared to the major contributors.

Figure 5:
Interesting set of graphs.

Again, the scales on the graphs are very different, creating a visual that is out of proportion for the more slowly arriving contributors.

@340
Supply a reference for the statement : …"reduces to a first order upwind scheme".

@361
Need to add finding the equilibrium solution under future work.

@368
Your conclusion that the analysis "highlights the limitations of the IRF approach", considering the limitations of this particular implementation, might be more appropriately stated as "highlights the challenges of the IRF approach".