Comments for version 2 of:
Quantifying Coupling Errors in Atmosphere-Ocean-Sea Ice Models: A Study of Iterative and Non-Iterative Approaches in the EC-Earth AOSCM

by V. Schüller *et al.*

https://doi.org/10.5194/egusphere-2025-1342

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As stated in my first report, the manuscript was already strong to start with. I think this revised version gained in clarity, thus making it even stronger. The authors' response to my and the other reviewer's comments and the related updates (or lack thereof, with justifications) are convincing.

That said, there still are a few minor points on which the manuscript could be made clearer than the current revised version. The authors' response to reviewers' comments contains valuable information that would be worth being more explicitly worded in the main body text. Therefore, I recommend publication in GMD, provided the following two minor comments (plus a typo) are addressed.

I appreciate and respect that the authors' opinions may differ from mine, but I honestly think that accounting for these last small remarks would benefit the manuscript and eventually make it an even better GMD submission.

Line numbers refer to the manuscript's version 2.

On introducing the SWR, well-posedness etc.

- L. 21 – 22: I would slightly rephrase to "if the coupled problem is well-posed, then it has a unique solution which the iteration converges to", which explicitly refers to "well-posedness", as this is a key. The initial phrasing could also have been misunderstood: the "correctly constructed" thing could have either been the coupled problem itself (which is what the authors meant), or the SWR algorithm it tries to solve.
- Line 36, I suggest adding one further sentence for insisting on the utility (and limitations) of the SWR: while SWR cannot formally prove whether a coupled problem is well-posed, it does provide a valuable stress-test on the robustness of the model formulation's robustness. This might sound like a repetition of the phrase L. 34 – 35, especially to domain experts like the authors, but I think this point is important enough and might be subtle to grasp for some of the GMD readership, so insisting might be worthwhile. And it is in line with one of the main messages of the manuscript, on the sea-ice albedo and atmospheric convection irregularity.

On the mixed ocean/ice heat boundary conditions

I still do not agree with the authors' choice not to explicitly write \$a_i\$ in Eq. 14, but I could live with it, provided the authors add one sentence explicitly saying that \$Q_i\$

already accounts for a_i. I think that point is relevant, even if the scaling is done implicitly by SI3 (and rightfully so). In their answer to my initial concern, the authors say that §2.3.2 “explicitly mentioned” this, but:

- Where is that explicit mention in §2.3.2?
- §2.3.2 is not a relevant location for this anyway, because it treats the 100% ice cover case, for which a_i does not bear much meaning. Right after Eq. 14 is a better spot, IMO.

L. 244: “the” boundary condition (not “The”)