Referee comments for:

Ocean–atmosphere turbulent flux algorithms in Earth system models do not always converge to unique and physical solutions: analysis and potential remedy in E3SMv2

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Submitted to Geoscientific Model Development Discussions
10.5194/egusphere-2025-5430

This manuscript examines ocean–atmosphere turbulent-flux formulations which are widely used in oceanic and atmospheric numerical models to compute surface forcing. It approaches the subject from an applied-mathematics perspective and offers clear, practical recommendations for a state-of-the-art model (E3SMv2) in a way that remains accessible to Earth system modellers. In that sense, I consider it a valid and very welcome submission to *Geoscientific Model Development*.

After the introduction, the authors describe the E3SMv2 implementation of this parametrisation, with useful attention to semantic distinctions that are often overlooked (for example, the difference between a *parametrisation* and an *algorithm*). The manuscript then examines in detail two issues that affect mathematical well-posedness: a discontinuity and an artificial limiter in the formulation of the stability parameters ζ. The proposed remedies are broadly convincing, though in my view some methodological weaknesses remain. Finally, the authors assess the implications under realistic conditions through a full 3D model integration. The paper concludes by suggesting that similar mathematically focused analyses of other parametrisations could ultimately improve the robustness of Earth system models.

Overall, the manuscript is strong, well structured, and offers an original perspective on a well-known class of parametrisations. That said, I think some conclusions are stated too strongly. First, the analysis considers only one air–sea parametrisation, namely Large and Pond (1982), and this limitation is only hinted at in passing. Second, some important literature appears to have been overlooked. Third, I noted a few methodological issues that, in my view, temper the strength of the conclusions.

These concerns do not diminish the value of the manuscript for the GMD readership. Its mathematical perspective on a familiar problem makes it both relevant and refreshing, and I hope it will be taken up by the community. However, I do think the paper should be toned down and revised in several important places. I therefore recommend major revisions and would be happy to review a revised version that addresses the comments below.

Major comments

1) *On the specificity and framing*

In my view, the manuscript should be framed as an in-depth mathematical analysis of one specific parametrisation in one specific implementation. At present, it is presented more broadly than that, including in the title. Alternative air–sea flux parametrisations exist, most notably COARE, but they are treated as a minor detail. A clear example appears at the start of Section 2: Large and Pond (1982) is introduced, and the authors note that the conclusions may also apply to non-E3SM models that use it, but they do not clearly acknowledge most models use different, more modern parametrisations. My suspicion is that many of the conclusions would extend more broadly, but the manuscript should state explicitly that the present investigation was carried out for only one parametrisation.

2) *Overlooked literature*

I believe several key references have been overlooked, namely

- Louis (1979) presents a direct, non-iterative method for computing air–sea fluxes.
- Large (2006) arguably provides a more up-to-date reference for the CORE bulk formulas, following up Large & Pond, 1982.
- Brodeau et al. (2017) examine the implications of several assumptions used in bulk formulas. Had the manuscript used the generic AeroBulk software, which emulates several such formulas, its conclusions might have been stronger.
- Torres et al. (2019) briefly discuss the impact of thresholds and limiters on the stability parameter ζ (see the first paragraph of their Section 4). Although this is not the focus of that paper, such details are rarely treated explicitly in the literature, as the present manuscript itself notes.
- Kara et al., (2000), (2005); Pelletier et al. (2018), in different contexts, both propose regularised versions of bulk air–sea formulations. Our 2018 paper in particular shares several concerns with the present submission, even if the proposed remedies differ; see, for example, Section 2.4.
- Schüller et al., (2025) address a different topic (coupling and convergence) but the broader perspective is strikingly similar. The authors analyse convergence in a simplified air–sea coupling problem and regularise selected parametrisations (convection and sea-ice albedo in their case) to improve regularity and convergence. This would be worth citing, for example in the conclusion.

3) *Continuity as a necessary cause for fix-point existence*

Around lines 215–222, the logic is somewhat convoluted. In my view, the authors' point only holds under additional assumptions about the asymptotic behaviour of $x \mapsto f(x, \xi)$. The example based on Eq. 18 is therefore somewhat misleading. The manuscript partly acknowledges this by noting that some discontinuous mappings do have fixed points, but some continuous mappings also have none, for example $x \mapsto x + 1$. There are many fixed-point theorems, and I suspect that applying one rigorously to these bulk closures would be beyond the scope of the paper.

Intuitively, continuity and fixed-point existence do seem related in the case of bulk closures, but the manuscript does not yet provide convincing mathematical evidence for that claim. This a difficult issue. I therefore think the tone should be more cautious here.

Continuity is likely a desirable property, but until its necessity is established, that remains an informed expectation rather than a demonstrated result.

4) *On the efficiency of the "adaptive" stability limiter*

As the authors themselves note, limiters are often poorly documented, and sometimes not documented at all, while being presented as small practical helpers needed to ensure convergence. That framing is misleading, and the manuscript does a good job at covering this. Scientifically, adding a limiter means modifying the problem being solved. I would encourage the authors to state this more plainly in places where the manuscript currently alludes to it indirectly:

- Adding a limiter means solving a different problem. Whatever the rhetoric, the limiter modifies the mapping f.
- Relatedly, if a parametrisation behaves well only because of a limiter, then the unmodified formulation does not behave well in the first place.

The discussion on the ζ_{max} adaptive limiter is difficult to follow. I would suggest moving part of the technical detail out of the main text and into an appendix. Sections 4.1.1 and 4.1.2 could likely be reduced to a short summary in the main body: depending on the precise parameter choices, and on how the $\zeta_{max} \longrightarrow 0$ limit is approached, existence and/or uniqueness may be lost. The detailed examples would be better placed in an appendix.

More importantly, I remain somewhat sceptical about the ζ_{max} adaptive procedure. By the authors' own account, the method still falls back, as a last resort, on the hard limiter the $\zeta_{max} = 10$. The statement in lines 492–494 is therefore not adequately supported. As the manuscript rightly notes in the introduction, many parametrisations are developed without explicit attention to mathematical well-posedness. The need to revert to the default hard limiter in some cases shows that the problem remains ill posed, but it does not establish *why*. It may be that the underlying physical assumptions fail in those regimes, but it is equally possible that the physics remains acceptable while the algorithmic formulation breaks down. The manuscript seems to favour the former interpretation without providing evidence, and I suspect such evidence would be difficult to provide. In my view, the fallback is an acceptable limitation and is consistent with the broader message of the paper: these parametrisations are not always well posed and often require ad hoc fixes to behave robustly.

In short, the proposed "adaptive" bound appears to mitigate the original $\zeta_{max} = 10$ "necessary evil", but not to eliminate it. Unless I missed it, the manuscript does not provide a quantitative assessment of how effective the adaptive procedure is. Figure 13 appears to be shown *without* the adaptive procedure: is that correct? A version of the same figure with the adaptive procedure, or a before/after comparison showing how often the fallback $\zeta_{max} = 10$ is invoked, would be very informative.

Minor comments

Line 93 sets the tone well, but in my view, is too verbose in some respects and not explicit enough in others:

- “Existence” and “uniqueness” are largely self-explanatory and do not require separate definitions. By contrast, “well-posedness” also includes continuous dependence on the initial state, which seems worth mentioning given the focus of the paper.
- “Equations underlying ...” does not need a separate bullet; it could simply be folded into the first item.

Line 106: does this have implications for the turbulent fluxes themselves? In other words, would the same turbulent-flux parametrisation/algorithm be used in a coupled run? Please make this explicit.

L. 130: turbulent flux algorithms determine C_D, C_H and C_E so that Eqs. (1)-(3) can be expressed in terms of large-scale prognostic variables.

Line 135: terms such as “interdependency” or “cyclic dependence” may be useful here.

L. 146: the relevant reference for (11) is (Obukhov, 1971).

L. 164: which is discontinuous with respect to ζ.

L. 179: that’s also the case in virtually all bulk algorithms, I believe.

Lines 181–183: what does the reference to Gauss–Seidel procedures add here? More importantly, the manuscript says little about how the iterative methods are initialised, even though for nonlinear problems the initial guess can be crucial for convergence.

L. 191 – 192: this is a major flaw that is worth being more underlined.

L. 240: for every point satisfying $\zeta = 0$. In practise, this only happens when $\Delta\theta = 0$, doesn’t it?

Line 245: do $\boldsymbol{x}_i$ refer to $(U_{10N},\ u_*,\ \theta_*,\ q_*)$, and $\boldsymbol{f}_i$ to the four sub-equations in (13)? Please make this explicit for the reader. Also, please make typography more consistent: $\boldsymbol{x}$ and $\boldsymbol{f}$ for the four-component vector (in bold), x_i and f_i for their scalar components (in normal font).

L. 250: near neutral, isn’t it?

Figures 3, 7 and 12: hard to understand:

- There are too many points for the scatter plot to be informative. I think having the proportion of oscillatory condition per (x, y) bins would be much more relevant.
- Histograms are not clear to me. I would put them on a separate one-row figure, with explicit x-labels and one shared [0; 1] y-label.

L. 275 – 277: this is a bit hand-wavy to me. Yes, the main non-converging points are in that $\Delta\theta$ band, which wouldn't it be possible to directly draw a map of non-convergence via directly binning with respect to geographical position (lat/lon)?

Lines 349–351: even if the algorithm converged very accurately, that would not by itself imply that the resulting turbulent fluxes are physically accurate; their parametrisation remains highly uncertain. This does not undermine the main point of the paper, but it is an important piece of context that could be stated explicitly. Of course, that still does not make it acceptable to hard code the iteration count to two!

Figure 8: the right panel would be easier to read if the x-axis showed average wall time per bulk-closure call rather than the total over the tested dataset. Arguably, this is only a relabelling change; the plot itself would remain the same.

Figure 13: could the lower end of the colour scale be given more resolution, perhaps with a nonlinear colour map? At present the lowest colour already corresponds to 10%, which is not especially low. Extending the scale down to about 1% would, in my view, be more informative. And are those actual percentages, or fractions with respect to 1?

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