Having gone through the revised manuscript and authors' response, I can see the revision is an overal improvement over the original and the scope or purpose of (some section of) the manuscript has become more clear to me now.

I would still like to comment on some of the responses and changes, as I generally feel the major concerns have only been minimally addressed. Apologies if I did not clearly convey my concerns in the original review.

Regarding "Section 4 shortcomings"
I appreciate the rephrasing of the text and the addition of Figure 10. Your goal of revised section 4 is more clear to me now, Figure 10 adds a form of validation compared to theoretical expectations and a comparison to a coarser reference, and it's now clear to me that in-depth quantitative validation and physical understanding is reserved for separate studies.

However, "physical realism" is not substantiated (beyond energy cascade at some level in the boundary layer) and still has to be believed by the reader based on the visualization of a few data fields (figures 9, 11), relative to AROME (figure 10), or relative to ECMWF-IFS (figure 11). Referring to separate future studies for validation is not sufficient. A "Devil's advocate" example: just because Figure 11b has a lot of detail does not mean the operational IFS forecast is worse, it's really a meaningless comparison unless there is a panel 11c showing (buoy?) data of wave height in various locations - for starters.

Ultimately, I'm simply not convinced about the purpose of section 4 in the context of the goal of the study. You have ported the model partially to the GPU and are now ready to numerically run at hectometric resolution. Section 4 successfully shows this, and if that is the goal, then OK. However, section 4 in its current form does not show that the model is ready to run at hectometric resolution in terms of physical realism.

Regarding "Reproducibility"
I understand that you can't take each user's specific system into account. I was expecting more user-friendly instructions given the code's complexity, and it is hard to figure out what is going wrong if an error is in French or if a compilation issue does not give any information the cause of error.

My concern was meant in part as honest user feedback (even beyond the scope of just this manuscript) and in part for being clear that I can't fulfil my role as reviewer in terms of checking reproducibility. I suppose I could indeed contact support, but I'm afraid we've then gone beyond what can be expected of me as a reviewer given available time.

Regarding "Scaling with radiation"
Radiation was just an example of where model resolution and physical parameterizations can

go out of balance. I think my original comment was not phrased properly, but I believe it is important to address the physics of the model when going to such high resolution, and whether it makes sense to do so.

Even for just the radiation-related questions, the only change I can see is that there is now a reference to a paper that details the radiation model, which I find inadequate:

- Given the description of other model components that are listed/referenced, you could have elaborated more on what kind of radiation you are running and what its limitations are.
- At what spatial resolution do you run radiation? How does it compare to hectometric resolution simulations? How do you generally justify running an older radiation transfer model for a global (then) 15+ km resolution scale model at a 100m resolution?

I think these are important themes in physical modelling and tie back to my concerns regarding "physical realism".