Review of the manuscript egusphere-2025-3407:
Evaluation of coupled and uncoupled ocean-ice-atmosphere simulations using icon_2024.07 and NEMOv4.2.0 for the EURO-CORDEX domain
by Vera Maurer, Wibke Düsterhöft-Wriggers, Rebekka Beddig, Janna Meyer, Claudia Hinrichs, Ha Thi Minh Ho-Hagemann, Joanna Staneva, Birte-Marie Ehlers, and Frank Janssen

The manuscript describes the evaluation of a new atmosphere-ocean coupled model and the two respective atmospheric and oceanic stand-alone models on the ERA5/ORAS5 period, on the EURO-CORDEX domain. The setup of the coupled model is well described. The results are often compared to observations, showing that they are reasonably realistic, and that the coupled model can later be used to perform historic and scenario simulations. The evaluation is more detailed on the oceanic part, which is very interesting per se, but I think it could be a bit enlarged on the atmospheric part.
I find the manuscript well written. The most serious restriction I have about the study concerns the period, which is often inconsistent with the 1979-2021 one mentioned on the first line of the abstract, and the 1979-2020 one on line 216, without any explanation. Indeed the other periods are:
- (1) 1979-2020 on the overview of the experiment (Line 216)
- (2) 1983-2020 for SST from ROAM-NBS except for marine heat waves (MHW from 1989 on)
- (3) 1981-2020 for SST from NEMO-NBS except for MHW (I understand 1981 is chosen for the comparison to Copernicus data, but the 1rst years could be shown at least on fig. A4)
- (4) 1989-2021 for MHW (1989 coinciding with observations)
- (5) 1979-2020 for atmospheric variables
- (6) 1979-2020 in fig. 7 with also the skin temperature
If (2) is an effect of ocean spin-up (and even 1984 seems out of range in fig. 3 and A4), then the atmospheric variables should not be shown before.
(4): There is no reason to add 2021 for the MHW only, without showing this year for the other diagnostics.
So to my opinion, one option is to explain why the first years of ROAM-NBS and NEMO-NBS are not shown, and then only show the same period for the atmospheric ROAM-NBS; the second option is to announce a 1983-2020 evaluation simulation.
With this minor revision, I believe the article will be ready for publication.

Please find below some more specific remarks, suggestions and corrections:
- abstract: it should be precised that it is an ERA5/ORAS5 simulation;
- L12: in the abstract and other parts in the manuscript, the authors say that in the coupled model, the SST bias leads to biases in the ocean-atmosphere fluxes: I don't agree with that. A coupled model develops it's own state of equilibrium, SST and atmosphere-ocean surface are linked.
- L28: the delay behind global simulations is also true for regional downscaling of the atmosphere, because they are also forced by global simulations;
- L57: in the manuscript one can find either SI3 or SI^3: it should be normalized;
- The new CORDEX Task Force on Regional Climate Projections (https://cordex.org/strategic-activities/taskforces/task-force-on-regional-ocean-climate-projections/) could be mentioned in the Introduction.
- L98: add Northern to "its adjacent seas" (there is no Mediterranean or Black Sea);
- L137: I think that the tuning mentioned in L262-263 should be presented here;
- L138: "For ROAM-NBS as well as for UDAG" : add "and ICON-CLM"
- L138: change icon-2024.07 for icon_2024.07 (as in the title and L56)
- L145: two ((
- L147: the lateral resolution is 2nm?

- L148: could the authors precise more the vertical distribution of the 50 layers? Which is the depth of the first ones?
- L149: 2 “chosen”
- L151: the reference is Madec et al. and not Gurvan et al. (to be corrected in the bibliography as well);
- L182: Craig et al. is the reference for OASIS3-MCT_3.0: the manual of OASIS3-MCT_5.0 by Valcke et al. 2021 could be added;
- L224: is it the same ocean restart for ROAM-NBS?
- L245 and followings: replace sea surface temperature by SST;
- L247: precise the Copernicus data period;
- L253: the authors chose the same seasons as in atmospheric studies, but for information the seasons usually used for ocean variables are JFM, AMJ, JAS, OND;
- L263: cf. L137 above
- L267-268: this parametrisation should be discussed in §2.2 or 2.4;
- L289: don’t put “surface air temperature”: it is confusing with surface temperature above;
- Figure 6: the “S” of seasonal is missing;
- §3.2.2: I think LongWave fluxes should be shown as well; and compare to the observations, not only RAOM-NBS to ICOM-CLM, which can be biased as well;
- Fig.7 and in the text: I don’t understand how the authors choose now the skin temperature: do they have it as an output of NEMO4 (and from 1979?)? The SST comparison to ERA5 SST (which is the one imposed to ICON-CLM) is enough, as it was compared to Copernicus before.
- L316: I don’t like the formulation “the skt difference determines the sign of the flux…”; as I said before they are linked in the coupled model; besides they qualify their statements on L325;
- L389: “which” will be shown;
- L393: a space is missing between “evaluation,” and “the bias”;
- L413-414: indeed the sea surface salinity is highly linked to the E-P-R flux; concerning the runoff, there are options in NEMO which for example propagate the runoff through the vertical, and also to enhance the vertical mixing at the river mouths: the choice here could be precised in § 2.2 or 2.4, or it could be discussed here;
- L425: the restart of the simulation can sometimes also explain some biases in the deeper layers;
- L433: add fig.8 for Cuxhaven;
- L435: add “nearly” at “a higher correlation at all station” because it’s not the case for all;
- L444: I’m not a specialist, but is “wind surge” appropriate here when the authors show only the SSH?
- Fig. 13: replace “blue” by “left” and “green” by “right”;
- Fig. 14: what are the isolines for?
- L465: it would be of interest to explain what is the “normal” situation of the inflow in the Baltic;
- Fig. 16 c: add ICON-CLM?
- L517: I guess Lighthouse Kiel is also Leuchtturm Kiel of fig.8: choose the same name;
- Fig. 17: Orange stars, not green; it would be interesting to show the MHW from the beginning of the simulations, even if there are no observations;
- L523: idem L517, and add Cuxhaven of fig. 8 for UFS German Bight (if I’m right?)
- L536: the authors must compute the correlations to say that;
- Chapter 4: at the end of this chapter it would be nice to add a conclusion;
- L582: the imposed runoff comes from observations, so do the authors think they might be too strong? cf. Remark for L413-414, and also there could be a discussion about coupling the runoff with a hydrological model: it is the best solution for future scenario simulations. Besides the E-P budget is also of much importance in the surface salinity;
- L593: indeed the spin-up period is very important for the ocean, but also for the coupled model to reach its equilibrium, and as I said before concerning the period of the study, the authors must explain if they think that the 1rst years of the ROAM-NBS simulation is in a spin-up phase.

- L607: the authors don’t show the computed correlations;
- L633: add Fig. Before 6a
- Fig. A5: replace “left, mid, right” by “a, b, c”
- Fig. A7: replace “blue” by “left” and “green” by “right”