Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-210-RC1, 2020

Geoscientific Model Development Discussions

Interactive comment on "A simplified atmospheric boundary layer model for an improved representation of air-sea interactions in eddying oceanic models: implementation and first evaluation in NEMO (4.0)" *by* Florian Lemarié et al.

Anton Beljaars (Referee)

anton.beljaars@ecmwf.int

Received and published: 29 August 2020

This paper proposes a point wise single column approach to include feedbacks of ocean SST and current on near surface atmospheric variables in offline ocean simulations with prescribed atmospheric conditions e.g. from re-analysis. This is obviously of interest for the development of high resolution ocean models, because fully coupled simulations are expensive and running fully coupled at very high resolution, e.g. at 1km, is still not feasible at the global scale. The paper documents the infrastructure that has been built for this purpose, with elements like boundary layer parametrization,

numerical techniques, and technical aspects. The evaluation of the boundary layer scheme on the basis of LES is comprehensive and in fact impressive. It clearly shows that some versions of the closure are better than others, but also that advection is important in case of SST gradients. Although important, the handling of advection is left to future work.

Also first experiments with offline ocean simulations are presented with prescribed atmospheric forcing. Simulations with traditional forcing at 10m are compared with simulations that are forced and interactive with data over the entire boundary layer. Evaluation is limited to a qualitative comparison of correlation between wind and SST, and the relation between surface current vorticity and wind or stress curl. However, I suspect that the results on these coupling coefficients depend on the strength of relaxation coefficients. A clean and objective optimization strategy for the relaxation is not obvious and left to future work.

The paper covers a lot of ground and as a purely scientific paper, more would be needed on evaluation, comparison with observations and optimization of the relaxation. On the other hand, I very much welcome this paper as a step towards a technical infrastructure for offline ocean simulations with realistic air-sea interaction. This is very much needed not only for offline ocean simulations, but also for coupled simulations with a lower resolution atmosphere. In the latter case, the air-sea interaction at high resolution can be improved by the type of scheme that is proposed here. Furthermore, I expect these type of intermediate complexity systems to play an important role in coupled data assimilation, which is hard to do in fully coupled models. The merit of this paper is that it carefully describes the design and evaluation of a technical infrastructure that can be used in further studies. GMD is highly suitable for this type of paper, so I recommend publication, after addressing the points below.

Lines 61-71 The authors motivate the need for a comprehensive boundary layer feedback in the ocean coupling by making reference to earlier studies, which is good. Unfortunately, this paragraph is hard to read, mainly because too many aspects are put together here. It is perhaps better not to discuss currents at this point because the effects of currents can (or is) already included in ASL coupling. Also the reference to bottom drag does not help. The main point is that with boundary layer coupling, temperature and wind at 10m change when heat and momentum fluxes change.

Section 2.2 This section presents the basic idea, which is central to the paper. Ideally, one would like to have the full set of atmospheric equations to evolve the column and add the nudging term only to keep the forcing "deterministic" or reproducible. The purpose of the term is to ensure that the chaotic atmosphere does not drift off and one would like the nudging term as small as possible. As soon as the atmospheric equations are less complete, the nudging term has to work harder. The question is how accurate is the selected single column approximation? Is it possible to motivate the approximations by an asymptotic framework like in the surface layer? It would be good to say something about the relative magnitude of different terms: temporal change, advection, pressure gradient, Coriolis, and diffusion dependent on the traditional Rossby number and dependent on a dimensionless number that describes the magnitude of the diffusion term relative to the advection or pressure gradient terms. In this paper the diffusion term is considered to be the dominant term, but for the momentum equation Coriolis and pressure gradient are added. This is probably quite good for large horizontal scales, but becomes questionable for the 1km scale. The question can also be asked in a different way, namely what is the equilibrium solution of the diffusion equation with boundary conditions at the surface and just above the boundary layer? Over the ocean a steady state solution can be very accurate provide that all the forcing terms are present, namely radiative flux divergence, Coriolis and pressure gradient. The moisture equation is even simpler; without cloud and precipitation processes, only diffusion is left and for steady state there is no flux divergence. In conclusion, I think that the proposed approach describes the effects of an instantaneous response of the entire boundary layer to changes in the surface boundary condition.

Section 2.4 and line 242 Relaxation is an important ingredient of the proposed system

and has a strong influence as suggested at line 235. It was decided to scale the relaxation time scale with the model time step. This is understandable for the top of the boundary layer where relaxation is used to impose a boundary condition by relaxing to the forcing in a single time step (immersed boundary condition). However, in the boundary layer, I would have expected a more "physical" time scale, dependent on which physical process it represents, or how fast the error growth is in the chaotic atmosphere.

Section 3.3 If I understand correctly, the coupling with sea ice involves the averaging of temperatures of different categories before computing fraction weighted fluxes over open water and sea ice (lines 384-385). Alternatively, it would have been possible to extend the weighted averaging in 384-385 to all the categories (as e.g. in Best et al., 2004, J. Hydrometeor., 5, 1271-1278 for land use categories). It is not the same as averaging the sea ice temperature, because the transfer coefficients are stability dependent.

Section 5.2 Wind to SST correlation is considered here. I have seen papers by Chelton, where the emphasis is on wind to SST-gradient correlation. Is the latter correlation less realistic in the current coupling because advection is missing? Figures 10 and 11 are discussed and compared with figures in literature. The paper is already (too) long, but it should be possible to read the paper on its own, so it might be better to reduce the number of plots from 3 to 2 and add the reference figures.

Section 5.3 This section (including Table 4) describes the computer technology aspects, but I feel that it is a bit too technical for this paper. I recommend to shorten it and limit to general results on processing and IO time.

Section 6 This section summarizes the paper, and describes plans for future work. Also the inclusion of advection is mentioned. I suggest to conclude more explicitly that advection is high priority. From the experiments with an SST front it is clear that advection can not be ignored at high resolution.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-210, 2020.