General Comments - Christof Lüpkes

The marginal sea ice zone (MIZ) is characterized by strong surface inhomogeneity with respect to roughness and temperature. The typical scale of inhomogeneity is much smaller than the grid size of climate and weather prediction models, so that it is a challenge to parametrize turbulent fluxes close to reality. This paper attempts to study the impact of different ice floe patterns on domain averaged flux profiles over the MIZ by Large Eddy Simulation (LES).

The topic is challenging and important for polar climate modelling and weather prediction. In most parts the paper is well written, and the principal approach is adequate and can stimulate further scientific work. However, as explained below, there are some unclear points which should be considered before the paper is published. Qualitatively, the principle conclusions concerning the impact of sea ice patterns will probably not be affected by suggested modifications but their might be quantitative effects.

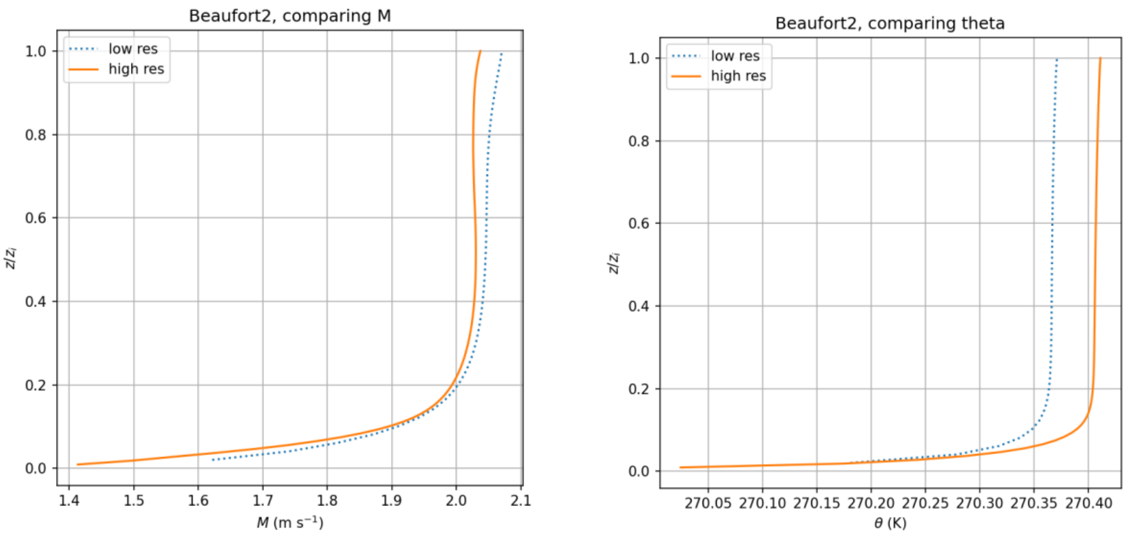
We thank the reviewer for their overall positive assessment and valuable comments and suggested edits to our article. We address the specific comments and revisions below, and how they have been resolved in the manuscript.

Major revisions

My most important concern is the used grid size of 100 m. The problem of grid spacing in LES over sea ice with open water fraction is addressed in Gryschka et al. (2023), Lüpkes et al. (2008) and especially by Weinbrecht and Raasch (2001). The latter show that in LES 2 m grid spacing should be chosen when the width of open water leads is 200 m, Lüpkes et al. (2008) used in the LES 10 m for leads of 1 km width and a similar grid spacing (20 m) is chosen by Gryschka et al. (2023). Lüpkes et al. (2008) further show that for mesoscale simulations with coarser grid (200 m horizontal grid size) a lead specific nonlocal parametrization is necessary to obtain the correct plume inclination and vertical temperature gradients on the downstream side of open leads. The considered situation might be different due to the larger open water fraction but I recommend at least one model run with a strongly reduced grid size (e.g. 50 m) to test the sensitivity of the obtained flux profiles and thus main results on the resolution.

While we could reduce the size of the domain and increase the resolution, that will sacrifice some of the large scales and circulations that we are interested in capturing at these 10 km x 10 km extents.

We concur with the reviewer that the important parameter is the number of grid points used to resolve a patch, and this necessarily will deteriorate as patches get smaller and resolving each of them with ~ 100 grid points as in the papers cited by the reviewer become impossible computationally. However, a grid sensitivity test suggested by the reviewer is included below and shows us that there is not much change in the standard atmospheric variables (potential temperature and horizontal wind, see below). This sensitivity test is actually for a real ice map (since we had them readily available, it is for panel (b) in Figure 7 in the manuscript) and is run at 50 m and 100 m resolution. As expected, the finer resolution results in more rapid warming of the atmosphere and thus higher heat fluxes at the surface as the simulations resolve the turbulence near the surface better. This then leads to a slight slowdown of the wind due to this stronger mixing.



We should note, however, that the LES simulations in this study are meant to demonstrate the need for surface analysis, given a constant ice fraction and average ice floe area. We thus do not focus on the quantitative aspect of the output. As such, we used a low resolution since it is sufficient to illustrate that configuration sensitivity. A manuscript is in progress where we continue this analysis by more closely examining the MIZ-ABL dynamics in response to surface heterogeneity – these simulations use a grid spacing of 50m.

Furthermore, in chapter 2.2 it is said that resolutions of the sea ice maps are based on much higher resolutions (2m, 10m, 20 m etc.). I cannot follow here, the maps shown in Figure 2 do not reflect these resolutions. In case of a higher resolution of the surface than of the LES one would need subgrid scale flux parametrizations, which are not mentioned here. All this needs clarification.

The different resolutions do not refer to Figure 2. The maps shown in Figure 2 correspond to Section 3 (Results: the MIZ-ABL over Idealized Configurations), where we created these configurations for the LES, at a resolution of 100 m. The description in Section 2.2, where we describe maps aggregated to resolutions of 2 m, 10 m, 20 m, etc. are real-world remotely-sensed maps that are not shown – these maps are used for the statistical analysis in Section 4 (Results: Statistical Analysis).

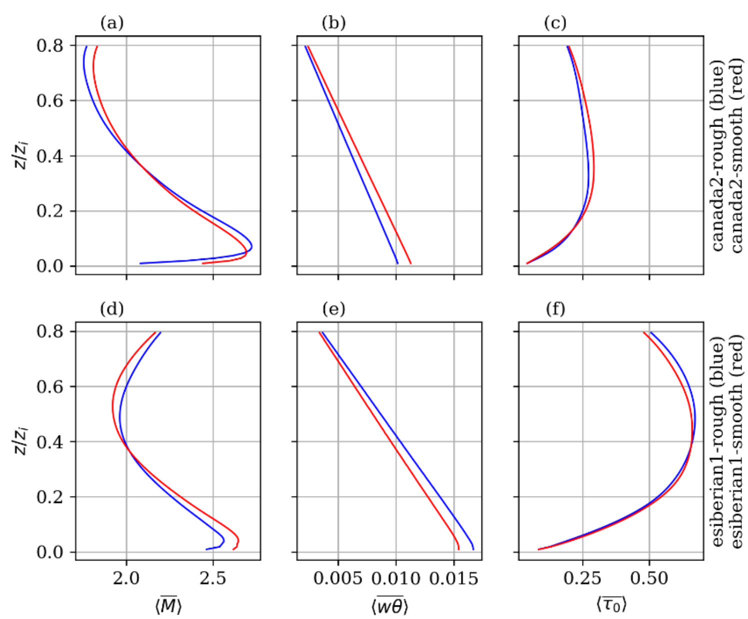
We have clarified this in the text, and furthermore, fixed the caption and content of Figure 2 to be clearer on what those five patterns are used for.

The authors use a roughness length of 1 cm for open water. This value is much too high (orders of magnitude) for open water surrounded by sea ice. More reasonable values can be found, e.g. in Andreas et al. (2010), Lüpkes et al. (2012), Lüpkes et al. (2008), Elvidge et al. (2016), and

in Gryschka et al. (2023). The latter discussed the choice of 1 cm and consequences on results. Their findings concerning the effect of too large roughness lengths must at least be discussed but new simulations with realistic roughness length would be better.

Thank you for your comment and the citations that we've added to the text. We recognize that our roughness length choices may not match what is in the current literature for the expected sea state in the MIZ. They corresponding to extremely wavy waters. Our aim, however, was to keep the roughness of ice and water equal to avoid injecting a roughness heterogeneity on top of the thermal heterogeneity. This is now clarified in the text. We may also add that the ice surface themselves can have roughness lengths that vary by order of magnitude (very smooth for flat ice and much larger for older wind battered ice).

We are in the process of preparing and submitting a new study of simulations over real ice maps. These simulations show that the roughness lengths do not make much of a difference between simulations. The thermal contrast between the ice and water have more of an effect on the results. See below for some preliminary results:



Suite	$z_{0,i}$ (mm)	$z_{0,w}$ (mm)	$z_{0,i}/z_{0,w}$	$z_{0,h}$ (mm)
Red	10	1	10	0.1
Blue	0.1	1	0.1	0.1

It is not enough to show just the flux profiles and wind. To understand the consequences of assumptions, it is necessary to show also the domain averaged profiles of potential temperature as in Michaelis and Lüpkes (2022). Also vertical cross-sections of temperature and wind at some positions and horizontal cross-sections could be helpful to understand differences in the ABL structure between model runs as well as possible difficulties of the LES. It is also necessary to mention the height averaged wind speed in the ABL. At least in conditions with high ice fraction and some open leads Lüpkes et al. (2008) found a strong dependence of the ABL development on wind speed due to their importance on plume inclination over and downstream of leads.

Thanks for your suggestion; we've added the domain-averaged profiles of temperature (along with wind direction, as per another reviewer) to the figure, and changed the resulting discussion. We have also added some horizontal and vertical cross sections as supplementary material.

Lines 61-63: It seems that the authors are not aware of the papers Lüpkes et al. (2008), Michaelis et al. (2021) and Michaelis and Lüpkes (2022). In all papers it is explained that a parametrization for orthogonal flow over leads in sea ice is developed and applied based on LES. Thus, although it is not LES, it is qualitatively different to other mesoscale model applications. Especially interesting for the submitted paper of Fogarty et al. is the work of Michaelis and Lüpkes (2022). They do very similar studies applying their LES-based turbulence parametrization over an ensemble of leads (see e.g. their figures 6 and 7). The main difference is that the sea ice fraction is 93 %, so much higher than in the present study and that a simpler (2D) geometry of the open water fraction is used. The new findings of the present study should be discussed considering this work.

Thank you for bringing our attention to these studies – we have added them to the manuscript (introduction section) and bolstered our discussion in the context of the findings of these papers. This helps our novelty in the sense that we are not only aiming for leads, but a mixture of leads and polynyas in the MIZ in the context of infinitely heterogeneous patches. Even if the LES accuracy could be improved with higher resolution and made more realistic with different z_0, these LES runs justify the need for the spatial analysis (which we get into in Section 4).

A Coriolis parameter is used for 90°N. This needs justification because it is not really realistic. In winterly temperature conditions prescribed in the model, a sea ice fraction of 50 % would be a rare event at North Pole. A more realistic choice would be 80°N, the typical latitude of the MIZ in the Fram Strait. What is the effect of this choice?

In the manuscript we have added the value of f_c used in these simulations (1.46×10^{-4} s^{-1}), as well as the calculated Rossby number for all simulations.

$$\mathrm{Ro} = \frac{M_g}{f_c z_i} = \frac{(2\ \mathrm{m\ s^{-1}})}{(1.46 \times 10^{-4}\ \mathrm{s^{-1}})(1000\ \mathrm{m})} \approx 13.7$$

In effect, the value of f_c is only important in relation to M_g and z_i, as they jointly determine the Rossby number. One can then say that these simulations are valid for Rossby numbers of about 13.7 – since this is a dimensionless number, the inputs themselves can change without seeing a large change in the results (see Omidvar et al. (2020) and Allouche et al. (2022) – we also discuss this in your comment below).

Omidvar H, Bou-Zeid E, Li Q, Mellado J-P, Klein P. Plume or bubble? Mixed-convection flow regimes and city-scale circulations. Journal of Fluid Mechanics. 2020;897:A5. doi:10.1017/jfm.2020.360

Allouche, M., Bou-Zeid, E. & Iipponen, J. (2023) The influence of synoptic wind on land–sea breezes. Quarterly Journal of the Royal Meteorological Society, 149(757), 3198–3219. Available from: https://doi.org/10.1002/qj.4552

The authors write always just ‘air temperature’. But I think at all occurrences, they mean air potential temperature (e.g. in equations B2, B3). This means, however, that the model is initialized with a neutral stratification throughout the atmosphere. I am afraid that this might lead to unrealistic boundary layers. Note that the usually found is for such ice fractions a convective layer that is capped by a very strong inversion somewhere between about 300 and 700 m condition (if not affected by a thick stratus layer). Such inversions cause entrainment and influence the ABL development (see e.g. Tetzlaff et al., 2015). This needs at least discussion.

Yes, when we write “air temperature,” we do mean potential temperature since this is what our LES solves for – this has been clarified in the text. We fully agree that any other initialization will result in different results and we in fact have tested that. The present initialization is aimed to match the infinite (periodic) domain, assuming the air has been flowing over this type of pattern for a long time and is near equilibrium with that surface, with minimal effect from entrainment.

We simulate a strong capping at 1000 m, as there might not always be a strong convective layer. However, this capping depth will not affect these results, since in our LES one can rescale the domain down by half, for example, and obtain almost identical results (expect for the effect of increased Rossby). That is because our Reynolds number is effectively infinite given our MOST based wall model. One can also adjust f_c to maintain a constant Rossby number. Because of this, we have expanded our discussion towards the end of the LES section to include dimensionless input parameters such as Rossby and Richardson numbers.

We also have a manuscript in progress with a principal focus on how the atmospheric dynamics and thermodynamics respond to surface heterogeneity.

Minor revisions

Line 38: the term MIZ was introduced some decades before the paper of Dumont (2022), so that more references than just this paper should be given.

True, so we've edited to say that we are just pointing to Dumont (2022) for a review on the current state of MIZ research

Line 59: it should be even if some....

Fixed

Section 2.1: More information is needed here (see above): Which lateral boundary conditions are used in the LES? How strong is geostrophic wind? What about humidity? Are these dry runs without clouds?

We've added the following details to Section 2.1:

- Geostrophic wind speed
- Horizontally periodic domain (also mentioned in Appendix A)
- Dry run with no clouds

Figure 2: What is the unit of the axes? I suggest including two vectors illustrating the geostrophic wind and boundary layer wind.

These patterns are 10 km x 10 km – updated in the figure, caption, and text.

Line 136: I am not sure if I understood Figure 2 and its relation to the different resolutions correctly. This should be better explained. It would be helpful to use kilometers as a unit for the axes and to give some distances between floes (or the width of leads).

Figure 2 does not have different resolutions associated with it; it is simply the idealized surfaces that were created for the LES portion of the manuscript. The multiple-resolution maps are used in the statistical analysis portion of the text. This has been clarified in the text, and Figure 2 has been updated to reflect the 10 km extent and geostrophic/surface wind vectors.

Line 196: It is not the geostrophic wind alone. The near-surface wind is dominating the fluxes. However, the near-surface wind direction might differ from case to case for the same geostrophic wind.

Thanks for the clarification, we've added: "and thus the near-surface wind" in the text.

Line 200: One could cite Michaelis et al (2021) in this connection (occurrence of LLJ) as well as Tetzlaff et al (2015). This would support the results.

Citations added, thanks for the suggestions

Line 245: I would not write that differences are minimal. Note that smallest and highest surface fluxes differ by about 30 % from each other, which is a lot.

We've changed the sentence to be: "the differences are not as impressive as the other variables, but can still result in a difference of up to 30\%, especially near the surface"

Line 368: The stability over only ice or water depends on many factors, especially on the air temperature and wind direction. It can happen that there is an unstable stratification over sea ice (cold-air advection) and a stable stratification over the open ocean (warm air advection).

We've added: "Although the stability over an ice- or water-dominated surface depends on many factors such as the wind direction and air temperature, for the cases where the air temperature falls between the surfaces temperatures of ice and water," at the beginning of this section.

References

Andreas, E. L., Horst, T. W., Grachev, A. A., Persson, P. O. G., Fairall, C. W., Guest, P. S., & Jordan, R. E. (2010). Parametrizing turbulent exchange over summer sea ice and the marginal ice zone. QJRMS, 136(649), 927-943.

Elvidge, A. D., Renfrew, I. A., Weiss, A. I., Brooks, I. M., Lachlan-Cope, T. A., & King, J. C. (2016). Observations of surface momentum exchange over the marginal ice zone and recommendations for its parametrisation. Atmospheric Chemistry and Physics, 16(3), 1545-1563.

Gryschka, M., Gryanik, V. M., Lüpkes, C., Mostafa, Z., Sühring, M., Witha, B., & Raasch, S. (2023). Turbulent heat exchange over polar leads revisited: A large eddy simulation study. J. Geophys. Res.: Atmospheres, 128(12), e2022JD038236.

Lüpkes, C., Gryanik, V. M., Witha, B., Gryschka, M., Raasch, S., & Gollnik, T. (2008). Modeling convection over arctic leads with LES and a non-eddy-resolving microscale model. J. Geophys. Res.: Oceans, 113(C9).

Lüpkes, C., Gryanik, V. M., Hartmann, J., & Andreas, E. L. (2012). A parametrization, based on sea ice morphology, of the neutral atmospheric drag coefficients for weather prediction and climate models. J. Geophys. Res.: Atmospheres, 117(D13).

Michaelis, J., Lüpkes, C., Schmitt, A. U., & Hartmann, J. (2021). Modelling and parametrization of the convective flow over leads in sea ice and comparison with airborne observations. QJRMS, 147(735), 914-943.

Michaelis, J., & Lüpkes, C. (2022). The impact of lead patterns on mean profiles of wind, temperature, and turbulent fluxes in the atmospheric boundary layer over sea ice. Atmosphere, 13(1), 148.

Tetzlaff, A., Lüpkes, C., & Hartmann, J. (2015). Aircraft-based observations of atmospheric boundary-layer modification over Arctic leads. Quarterly Journal of the Royal Meteorological Society, 141(692), 2839-2856.

Weinbrecht, S., & Raasch, S. (2001). High-resolution simulations of the turbulent flow in the vicinity of an Arctic lead. J. Geophys. Res.: Oceans, 106(C11), 27035-27046.