Review of

Influence of Leads Widths Distribution on Turbulent Heat Transfer Between the Ocean and the Atmosphere (tc-2911-64)

by S. Marcq and J. Weiss

This is a concise and informative paper that considers the issue of how to parameterize the heat flux from leads in large-scale models. The central question is whether the width distribution of leads needs to be treated or whether models can succeed by doing what they are currently doing: treating only two categories in a model grid box, open water and ice. The authors raise this question because they realize that two available parameterizations for the sensible and latent heat fluxes from leads depend strongly on the lead width such that smaller leads give up more heat per unit surface area than larger leads.

I generally like this paper, but it has some factual inaccuracies and some confusing sections. Moreover, the authors seem unaware of a key previous study.

1. In their Conclusions, the authors' last sentence is "Our estimation may be a first step towards a subgrid scale parameterization . . . ." Actually, Maslanik and Key (1995, *J. Geophys. Res.*, **100**, 4573–4584) probably made the first step. They did pretty much what the current authors have done: calculate what the areally averaged sensible heat flux is for a given distribution of lead widths. They used the flux parameterization of Andreas and Murphy (1986) rather than Andreas and Cash (1999) and assumed a lead distribution function of $P(X) = \lambda^{-1}\exp(-X/\lambda)$, based on submarine sonar data, instead of the power law that the authors use; but their objectives were similar.

The authors need to consider this previous work and explain how their analysis differs from it or improves on it.

2. The title is hard to read: "Leads Widths Distribution." I'd revise it to "Influence of Lead-Width Distribution . . . ."

3. The authors' discussion of the lead-width distribution seems incomplete. They write this distribution only as $P(X) \sim X^{-a}$. That is, the distribution seems to be a probability density function. The fundamental requirement of such a function is that it integrates to one over its range of validity. The authors never make this point, and the ~ symbol above leaves unspecified the proportionality constant that enforces the limit of one.

Presumably, the authors use the distribution function to get the areally averaged heat flux (H, either sensible or latent heat) in an equation like this:

$$\overline{H} = \int_{L_0}^{\infty} H(X)P(X)dX \ , \qquad (1)$$

where $\overline{H}$ is the areally averaged flux. Because of (1), it is essential to have a true probability density function that integrates to one.

From the authors' P(X), I get

$$1 = \frac{1}{\lambda}\int_{L_0}^{\infty} X^{-a}\,dX = \frac{1}{\lambda}\left[\frac{X^{-a+1}}{-a+1}\right]_{L_0}^{\infty} = \frac{L_0^{-a+1}}{\lambda(a-1)}\ . \qquad (2)$$

Hence,

$$P(X) = \frac{a-1}{L_0}\left(\frac{X}{L_0}\right)^{-a} \qquad (3)$$

is the full expression for the distribution function that the authors use.

Notice, too, the distribution function depends on the smallest lead width considered, L_0. The authors discuss cases with L_0 of both 10 m and 1 m (page 2783).

4. Still on the issue of the distribution function, I am not sure why the authors even introduce it. In the calculations described in Section 3, summarized in Table 1, and depicted in Figures 5 and 6, for lead width, the authors use horizontal and vertical slices across Figure 3. That is, they seem to use the actual distribution of leads in their satellite image rather than the distribution function that they deduce. Please describe your approach better so we can see where P(X) fits in it.

5. Still in Section 3, the authors discuss three types of flux calculations: one, with the full distribution of lead widths; two, the calculations that CLIO makes; and three, a typical model calculation in which all the open water in a grid cell is put into one big lead. Maslanik and Key (1995) tried yet a fourth approach. Because you know the lead distribution function, from the total open water area, you could calculate the mean lead width (or the median width) and run your flux calculations just once for this width. Maslanik and Key found that the heat flux calculated for this single mean width was similar to the areally averaged flux calculated for the entire distribution. If you also find this result, the method would provide a simple but improved estimate to just using the total open water.

6. I think calculations based on equation (3) are unnecessary. At least for the Andreas and Cash parameterization, the heat flux is the areally averaged flux over a lead of width X. Trying to introduce a fetch dependence over the open water into the parameterization is redundant and a misuse of the parameterization.

7. Figure 4 is not well presented. Both axes span four decades; it would thus be better if the plot were square. Then the slope triangles shown in the figure would be in proper proportion. As they appear now, both are close to right triangles with 45° corners. They thus suggest, visually, slopes of approximately one. The notation, however, suggests the slopes are steeper are ½ to

2/5. In other words, the visual presentation is not compatible with the mathematical presentation.

8. The paper contains some errors and some language problems, as follows:

a. "Fracturation," which appears twice on page 2767 (and maybe elsewhere) is not an English word. Try "fracture" or "fracturing."

b. On page 2767, line 9, "Arctic" is spelled that way.

c. On page 2767, line 15, the authors mean shortwave or solar radiation, not "UV radiation".

d. The constructions "Alam and Curry (1997)'s" and "Andreas and Cash (1999)'s", which occur throughout the paper, are linguistic abominations. Rewrite, for example as, "the method of Alam and Curry (1999)" or "Alam and Curry's (1997) method".

e. On page 2770, lines 11–12, the text cites Makshtas and Podgomy (1991). Makshtas wrote this book himself; there is no Podgomy listed as a coauthor. Also, correct the authorship in the list of references on page 2788.

f. There should be no Pr_t in equation (10).

g. On page 2773, line 10 cites Bourassa et al. (2001) as the source of the surface roughness model that Alam and Curry (1997) used. How can this be since Bourassa et al. (2001) was published four years after Alam and Curry's (1997) work? In truth, Alam and Curry cite Bourassa et al. (1997) as the source of their roughness parameterization.

Ed Andreas
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