Dear Editor,

Please find below a detailed answer to the comments of the three referees on our manuscript tcd-5-2765-2011. These interesting comments and suggestions helped us to improve the manuscript on several points, such as on a possible representation of the lead ensemble by the mean lead width only, on the integration of the fluxes, on the role of the power law exponent on fluxes, etc.. We would like to thank the referees for these constructive comments.

With best regards,

Jérôme Weiss and Sébastien Marcq

Review of E. ANDREAS

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.

OK, good point. We indeed forgot to cite this work in the former version of the manuscript. This is now the case at the end of section 1. Note, however, that the difference between their work and the present one is of fundamental nature: Maslanik and Key considered a (theoretical) exponential distribution for the lead widths. Indeed, in the case of an exponential distribution, the mean λ defines the entire pdf. It is therefore not surprising in this case that the ensemble of lead widths is well represented by the mean.
Our observations argue instead for a power law probability density function (pdf). As we now explain in section 3.2, this has strong consequences: For a power law exponent $a>2$ (as we observed), the mean lead width is controlled by the lower bound to power law scaling, L_0. In the case analyzed in our work (SPOT image), this lower bound has no physical meaning, as it is set by the resolution of the image (10 m). If the (unknown) physical lower bound is of the order of 1 m, the fluxes calculated for this mean lead width strongly overestimate the fluxes calculated from the entire distribution. In other words, the ensemble of lead widths is poorly represented by the mean.
Note finally that, with the sentence "a first step towards..", our aim was not to pretend that we were the first to tackle this problem, but simply that our calculations can give a first order estimate of the correction on heat fluxes needed to take into account lead width distributions.

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

OK. Done

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:

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

where 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
(2)
Hence,

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

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).

We, obviously, fully agree with this. It is indeed preferable to present the full expression (relation (3) above, now eq. (11) of the revised manuscript), especially when we discuss the role of the exponent a and the cut-off L_0 on the mean width in section 3.1.
We now also present explicitly the relation (1) above (eq. (12) of the revised manuscript), to avoid any confusion.

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.

First, we use, in section 3.1, the actual distribution of lead widths obtained from the SPOT image. Then, in section 3.4, we analyze the sensitivity of the calculated fluxes on the parameters of the (theoretical) power law pdf, the exponent a and the cut-off L_0. We tried to improve the text to explain this more clearly and to avoid a possible confusion.

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.

See also the answer to point (1). As, noted, we obtained a very different answer compared to Maslanik and Key. We performed and discussed the suggested calculations in section 3.1 and found, for a lower cut-off to power law scaling L_0 of 10 m, a reasonable agreement between the flux calculations obtained from the entire distribution and those obtained from the mean width only. However, as we discuss, this relative agreement is most probably fortuitous, as the cut-off L_0, which controls the mean width, is set by the resolution of the satellite image. For a lower(physical) cut-off, the agreement disappears (see section 3.4).

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.

OK. We have re-done the calculations, and modified the figures and the tables accordingly. This does not change qualitatively the conclusions. Note that for the Alam and Curry parameterization, this integration is still necessary. This is detailed at the end of section 2.2.

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 to3
2/5. In other words, the visual presentation is not compatible with the mathematical presentation.

OK. Done

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 Englishword. Try “fracture” or “fracturing.”

Done: 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.

OK. All done.

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

Yes. Corrected.

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.

Alam and Curry (1997) actually cite:
Bourassa, M.A., D.G. Vincent, and W.L. Wood. A sea state parameterization for low wind speeds and non-arbitrary wave age, J. Phys. Oceanogr., IN PRESS, 1997

However, although this reference was likely known by Alam and Curry at that time (as a draft ?), it was not published in 1997.
The reference: (Bourassa, M., Vincent, D., and Wood, W.: A sea state parameterization with nonarbitrary wave age applicable to low and moderate wind speeds, J. Phys. Oceanogr., 31, 2840–2851)
seems extremely close (same authors, same journal, almost same title). Therefore, we assume it corresponds to the work cited by Alam and Curry in 1997, and we kept this reference.

Review of C. LUPKES

1. abstract: number 80 % is mentioned, but a citation is not given neither in the abstract nor in the text.

Actually, taking the average fluxes given by Maykut over multi-year ice (less than 5 W/m²) and over open water (600 W/m²), and 2% of open water, one finds more than 70%. So, we now give this number in the abstract. We prefer to not add a reference in the abstract.

Probably the authors mean upward heat fluxes

Yes. Now mentioned

, since the
downward fluxes over sea ice can balance the upward fluxes over leads (L¨upkes et al., 2008b, Overland et al. 2000).

OK. We slightly modified the text on lines 45-47 to be clearer on this point.

2. page 2768, lines 5-15 and eq. (1): The description of heat flux determination in this paper ignores wind speed U. Fluxes are proportional to the product U_T. C is not the turbulent exchange coefficient. It is the transfer coefficient for heat and its stability dependence is determined via MO theory using similarity functions for which many different formulations are available. The corresponding sentences should be modified in this way.

We agree, and modified the text and eq. (1) accordingly.

3. page 2769, eq. (2): I suggest to skip the Venkatram formula because it is constructed for a larger scale and is not used in the formulation of heat transfer used in the present work. The occurrence of the formula might lead to confusion. Figure 1 can still be shown as a general description of the process.

We agree. We skipped this section.

4. Page 2770, lines 10: As far as I understand the Andreas and Cash (1999) paper observations were obtained downstream of the leads, not over leads as mentioned in the manuscript. I find this difference important, since observations over open leads or over leads with very thin ice remain still a challenging task for the future. The fluxes downstream of a lead might be influenced by the fetch over sea ice.

We agree that measurements were not performed over water (or thin ice), but at the edges of the lead (upwind and downwind): in Andreas and Cash (1999), we read:
"The ALEX surface sensible heat flux data are based on profile measurements of wind speed and temperature, both upwind and downwind of leads, (…) Integrating the heat inputs through the sides of a control volume with the lead at the bottom and the front and back sides upwind and downwind of the lead then yielded the average surface heat flux." See also the answer to comment #6 of the review by E. Andreas.

From this, we understand that these measurements were performed as close as possible to the lead edges. Consequently, we are not sure to understand what the referee means by “The fluxes downstream of a lead might be influenced by the fetch over sea ice »

5. page 2771: The Andreas and Cash (1999) parameterization is an important step, even so I suggest to discuss uncertainties of the results of this paper related to the parameterization. One of these uncertainties is probably due to Equation (7) for the determination of the TIBL giving values which are independent on external conditions. The TIBL is about 5 m for a fetch of X_f = 200 m. One can speculate that this low value is probably due to a near-surface stable stratification of the incoming air flow. Weinbrecht and Raasch (2001) obtain by Large Eddy Simulation (LES) values of 30-40 m for TIBL under (probably weaker) stable conditions. Lüpkes et al. (2008a) show that for near-neutral inflow as sometimes occurs also in the winterly Arctic the TIBL depends on the boundary layer wind speed, the surface buoyancy over the lead and on the background mixed layer height.

We agree that a large part of the variability of these data is not explained by the empirical fit of eq. (6) (former eq. (7)). This indeed introduces uncertainties on our calculations. However, the number of data point reported on fig. 1 of Andreas and Cash (1999) is, we believe, too low to estimate correctly a residual to this equation, and therefore to correctly analyze the propagation of unexplained variability in our calculations. Note that Andreas and Cash do not precise this residual.
The TIBL values reported by Weinbrecht and Raasch (2001) are much larger than the (scarce) measurements summarized in Andreas and Cash (1999). We agree that this might be related to different external conditions. However, as the measurements or the simulations are, to our knowledge, too limited to fully quantify in details the role of the external conditions on the TIBL values, we privileged the (scarce) measurement data as the reference for this work, and we did not discuss the Weinbrecht and Raasch (2001) work.
We modified the end of section 2.2.1 to stress the limitations of the Andreas and Cash parameterization, and consequently of our work. Note, however, that both methods (Alam and Curry in one hand, Andreas and Cash on the other hand) give similar results, suggesting that the obtained numbers capture the correct orders of magnitude.

6. Line 20: In the sentence ’ ...They only calculate ...’ skip ’only’. It is called the MO similarity theory, not the theory of MO similarity.

OK. Done.

7. equation (10): the Prandtl number should not occur here. Mention that k is the v. Karman constant (which value is used?).

Done

8. Section 2.2.2: Looking into the work of Alam and Curry (1997) it seems that the main part of the parametrization is by Clayson et al. (1996). In the present paper this should be made clear.

OK. Done at the beginning of section 2.2.2

9. page 2773, line 10: In the Alam and Curry work Bourassa (1997) is used for roughness, what is improved in Bourassa (2001)? The surface renewal theory was developed by Brutsaert (1975). Clayson is only applying it. This should be made more clear.

See the answer to comment (g) of E. Andreas. Concerning the second point, see L 178 of the revised manuscript.

One of the uncertainties related to the roughness parametrization is that it refers to open water. At temperatures of -20 to -30_C open water in leads will be covered very quickly by a thin ice layer causing a change in roughness.

OK. See L186-189 of the revised manuscript.

10. page 2773, line 10: I suggest to skip the short Appendix B and include its text in section 2.2.2

OK. Done

11. Page 2776, line 15-20. '... The contribution to heat flux from the ice ...' Probably, the upward contribution is meant ? (see revision 1).

Yes. Modified.

12. page 2779, line 10: replace 'an uniform' by 'a uniform'.

Done

13. page 2779, line 25: Is it possible to include a figure showing H_s as a function of a? This would help to understand the description at the bottom of this page. In its present version I find it difficult to follow.

Done, see the new figure 6. We slightly modified the text in section 3.4 accordingly.

14. page 2780: Are the developed parametrizations valid for leads smaller than 10 m? I would expect that the uncertainty of the observations are larger for the smaller lead sizes.

Yes, we agree and discuss this point in several places: section 2.2.1, L146; section 3.4 L 405

15. Discussion section: I suggest to address here the mentioned uncertainties of the parametrizations.

We preferred to keep (and extent) this discussion of the uncertainties in the presentation of the heat fluxes formulations (section 2.2). In section 4 (discussion), we discuss physical processes that are not taken into account in our simple approach, and which should be analyzed in more details in the future: spatial distribution of the leads, formation of clouds, downward heat transport over sea ice, etc..

Another open point is the role of refreezing of a lead which due
to figure 2 would reduce the effect of size dependence, since the air ice temperature difference would decrease in this case. On the other hand, it could be stressed that the upward heat transport over leads results in a corresponding downward heat transport over sea ice as described in Lüpkes et al. (2008b). This means that the dependence of upward heat flux on lead width would generate also a width dependence of the downward flux and stability over the ice surface.

The first point (refreezing) was already mentioned in the former manuscript. The second point (downward heat transport) is now included in the discussion (section 4).

16. There is a paper by Maslanik and Key (1995) who have also calculated heat fluxes over lead ensembles. One of their conclusions was that the ensemble of lead widths is well represented by the mean lead width. How is the relation to the present findings?

See answer to comments # 1 and 5 of E. Andreas.

Review of M. VAN COPPENOLLE

Title You may add a reference to sea ice or the Arctic in the title. It is not obvious now that you are speaking of sea ice.

Done.

Abstract l. 18 extents -> extends

Done

Body Text
p. 2767 l. 17 Here and in other instances. The authors' full name is "Morales Maqueda". Just make sure your bibtex is parameterized correctly to allow for double names.

Done

p. 2769 l.11 Specify that this is in the atmospheric boundary layer (e.g. not in the ocean)

Done

p. 2769 l. 17 Could you say in a few words what type of method they used to derive this expression.

Following the suggestion of C. Lüpkes, we skipped this reference to Venkatram, to avoid possible confusion.

p. 2770 l. 19 I would suggest to use italics for the lonelyindex "f" instead of using the present form

p. 2774 l. 21 "variations of" -> "variations in"

p. 2774 "they both" -> "both formulations"

All done.

p. 2778 "their differences are almost exactly balanced". . . which differences? be more precise in the formulation

Yes, the former sentence was unclear. We modified this section.

Appendices.
I think the appendices are slightly too concise. Make sure that all symbols are defined.

Appendix A - Say that symbols are defined in a specific appendix in the beginning.

Done

- I would recall equations (4) and (5) for readability - what is r ?

OK. For the appendix A to be « self-consistent », we now recall equations (3) to (6) of the main text in this appendix. The altitude r is now defined.

Appendix B - As it is, this appendix is not very useful. - If you redirect the reader, could you at least explain the principles of their derivation?

We finally suppressed this appendix B, as suggested by C. Lüpkes.

Appendix C Again, make sure everything is defined, in particular - the units for pressure involved in the formulae - Define k - Define psi_L - Define cshN, cleN There is a missing parenthesis in C4

Done

Tables & Figures In general very clear and relevant.
Tables could be more readable in general. The relevant information is not immediately understandable from both tables. I do not request anything here, in the end, one understands. Just make sure that this is the optimal way to carry the information as you want to. You may use a figure, or add differences, or use less numbers in the left row.

We agree that tables are not as readable as e.g. figures. However, we think that giving these numbers for different atmospheric conditions is useful. So we kept the tables.

Fig. 4 - the meaning of triangles was not obvious to me

We modified the legend to help the reader. This representation is classical in physics to indicate the slope of a power law scaling.