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Ocean Science
Discussions

Interactive comment on "A parameter model of gas exchange for the seasonal sea ice zone" *by* B. Loose et al.

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This paper addresses the issue of air-water gas exchange in ice-covered oceans in an interesting way. The fundamental approach of estimating the turbulence generated by several major mechanisms and then calculating an overall dissipation rate is fundamentally sound and a simple way of addressing a complex problem. The authors have done a good job of justifying the assumptions used (with a couple of exceptions discussed below), and the resulting parameterization looks to be useful and provide reasonable numbers. Furthermore, the conclusion that the presence of ice increases the overall transfer velocity is surprising (to me anyway). The paper is well written and should be published after the authors address the relatively minor comments below.

Scientific Comments:

Parameterizing the transfer velocity, k, in terms of the turbulence dissipation rate (epsilon) raised to the 1/4 power has been proposed many times and it is reasonably clear this method works. What is subtle about these relationships is that the proportionality constant between epsilonˆ1/4 and k is a function of the depth at which epsilon is determined (and also likely the depth at which the turbulence is generated). Therefore, the proportionality constant derived by Zappa et al. for systems (mostly) where the turbulence was generated at the surface, might not be universally applicable to turbulence that is generated at depth such as for ice moving through the water. This point is discussed by Zappa et al. and it would be useful for the authors to at least point out that the scaling constant chosen for epsilon might not be a single value.

The paragraph on p 1171 starting on Line 19 is a terrible way to frame the problem. Especially since the authors reference the work of Lamont and Scott. The irony here is that Lamont and Scott start from surface-renewal theory, where there is no hypothesized viscous sublayer or molecular diffusion sublayer. Although combining of elements of boundary layer theory and surface renewal theory has become common in the literature, it should not be encouraged since the two are not compatible from a conceptual standpoint. The paragraph should be rewritten either from a standpoint of surface renewal theory (keeping the reference to Lamont and Scott), or using a boundary layer conceptual model (finding some other suitable reference discussing gas exchange in the context of molecular diffusion sublayers). The choice is somewhat arbitrary, in my opinion and the rest of the paper can be explained in the context of either conceptual model. The sentence discussing the relation between gas and heat should be deleted since it is not correct and even if it were correct it is not relevant.

Specific comments:

The Section numbers from lines 13 to 21 on Page 1172 are missing something. I think they should be 2.1, 2.2 etc.

There is a typo in Eq. 6, I think. It should be u* to the third power.

On page 1186 at the bottom there is a reference to Figure 10 that should be Figure 9.

The discussion of Figures 9 and 10 is insufficient. Figure 10 is confusing and it is not clear how it relates to the information in Figure 9. As far as Figure 9 goes, the rationale behind plotting k (ice-free transfer velocity) versus the fraction of water surface that is ice free escapes me. In Eq. 2, k is an area-averaged quantity, so plotting it versus area (which is what f represents) is guaranteed to show no correlation. I'm not sure how better to represent the dependence of k on the forcing functions, but I don't see the utility of the top panel of Figure 9. It isn't clear there is enough data at a particular value of f, but maybe by binning data into ranges one could start plotting k as a function of the various environmental parameters for specific ranges of f. That might show how at low values of f the ice drag related dissipation starts to dominate the transfer velocity, while at large values of f there is a stronger dependence on wind speed (or buoyancy, perhaps).

Interactive comment on Ocean Sci. Discuss., 10, 1169, 2013.