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

Anonymous Referee #1

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The manuscript presents a theoretical approach to estimate gas exchange in the seasonal sea ice zone (SIZ), which, to my knowledge, represents the first systematic attempt to deal with this issue. The bottle neck for gas exchange is always molecular diffusion within the top 0.1 mm of the water with turbulence acting as a shortcut. In contrast to the ice-free ocean, where such turbulence is primary provided by wind, one has in the SIZ several additional mechanisms. The authors address these mechanisms in detail using results from the literature. The total turbulence is then converted into a tentative gas exchange rate using a relationship from the literature. The approach yields a rational and comprehensive parameterization of the SIZ gas exchange rates and a coarse estimate of the gas exchange rates as such. From the latter, the authors present gas exchange estimates calculated on the basis of data from drifting buoys in the Arctic, which illustrate the effects of the various turbulence-producing mechanisms.

A result is that the effects contributed by the ice interactions raise the gas exchange by about 40%. The parameterization is meant to be converted into a definite formula taking into account actual gas exchange determinations in the SIZ and in the laboratory. The subject of the study is an important step forward, not the least for obtaining better CO2 uptake rates in the polar oceans, which so far are rather uncertain. The manuscript is clearly written and well structured. The cited literature is fully adequate. I recommend publication in Ocean Science after moderate revision.

Specific comments (page/line): 1. (1171/21) Is heat transfer really 100 times faster than gas transfer? Temperature conductivity is about 60 times higher, but should not the variation with D have an exponent of something like -1/2? 2. (1173/14 ff.) It is argued that gas transfer across ice is negligible, especially for columnar sea ice, which is natural because such ice is quite thick. However, there is nilas in freshly formed or coastal polynyas, which can be very thin. A remark on such a situation should be added (somewhere in the paper). 3. (1174/10) Eq. (4) gives the gas transfer rate as proportional to the Schmidt number with an exponent -1/2. I have learnt that under rather calm conditions an exponent of -2/3 is more appropriate (cf. Jähne et al., 1987). A short explanation is needed. 4. (1174/13) "may exist" or "may not exist"? I do not understand the logic. 5. (1174/14 ff.) Eq. (4) is adequate for the "surface ocean" but the dissipation in the top 0.1 mm is certainly different and far more variable. There is thus a correction needed, which may even vary. 6. (1181/24 -1182/8) I read that long-period waver are "more rapidly attenuated" and that capillary-gravity waves have the "strongest interaction with sea ice", which is sort of a contradiction. I expect that capillary waves are very quickly lost, and that a medium wave number would have the largest impact on gas transfer. Please clarify. 7. (1188/8 ff.) With a mean gas exchange rate of 1.63 m/d (1186/15) even a mixed layer only 15 m deep will have an e-folding time of 10 days, possibly allowing some transfer into the waters beneath it. A more careful argumentation may be in order. 8. As the subject of the paper is "a parameter model of gas exchange . . .", I recommend to present something like an explicit formula of it (perhaps in a generalized form), which makes the number of free parameters apparent. This should be followed by a short statement of something like a strategy what observations will be adequate to allow one to deduce the parameter values with sufficient precision. 9. (1186/17 ff.) The discussion of Figs. 9 and 10 must be enlarged. A comparison of the upper and lower panel in Fig. 9 must be added (this can be brief) and Fig. 10 is somewhat difficult to understand. More explanation in either the text or the figure caption is needed.

Technical comments: 1. Eq. (7) contains a funny sign. 2. Eqs. (10) and (11) have funny signs, probably a bracket. 3. (1172/13 ff.) Correct Section numbering. 4. (1180/21) The averaging bar is too far up. 5. (1181/11 f.) funny signs between L0 and z. 6. (1190/18 ff.) Frew et al. 2004b is missing, and Frew et al., 2004 must read 2004a (as is the case in the text). 7. (1190/20) “Station”! 8. (1191/12) page numbers are missing 9. (1192/12 ff.) McPhee citations are in wrong sequence. 10. Fig. 2: Why is the Toyota (2006) relationship partly in red and the remainder dash-dotted? Looks like the latter part was more uncertain. The legend font should be larger or the caption should contain more information. 11. Fig. 3: The stress should be “tau” rather than “t”.

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