Reviewer #1

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.)

Reviewer 2 also pointed out that this was inaccurate and we have removed this statement.

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)

We have added this statement: "In the presence of thin ice, such as grease ice, nilas or frazile ice, gas exchange may not be so slow, but solute rejection solute rejection from the ice (Killawee et al., 1998; Loose et al., 2009) makes this circumstance hard to interpret."

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.

We refer the reader to (Lamont and Scott, 1970) and (Zappa et al., 2007) for more detailed explanation of the theory behind equation (3).

4. (1174/13) "may exist" or "may not exist"? I do not understand the logic.

Please see the changes made in response to Point 5, below.

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.

Yes, this is a good point, we have added to the discussion as follows: "For the purposes of this paper, we will assume the constant of proportionality determined by Zappa et al., (2007); 0.419. It should be noted that ε varies vertically in the water column and is likely to assume distinct values at the ice water and air-water interfaces, therefore unique proportionality constants may exist for the SIZ."

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.

It is true that capillary waves decay most quickly in the absence of forcing. However, this section says "…however the gravity wave frequency distribution is variable and interacts strongly with sea ice (Wadhams et al., 1986; Hayes and Jenkins, 2007)", not that capillary-gravity waves have the strongest interaction. It is the case that longer period waves lose the most energy when they collide with sea ice. This is reflected in the statement "Energy attenuation by wave interaction with sea ice is strongest for waves with wavenumber 1 rad m^{-1} or less (Polnikov and Lavrenov, 2007)." Therefore, I don't see how this paragraph is self-contradictory.

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.

It is well established that diapycnal mixing through stratified waters, such as a seasonal pycnocline is several orders of magnitude lower than mixing in the mixed layer, e.g. 0.1 m2/s vs 10^{-4} m2/s. For this reason, the seasonal thermocline can be thought of as an effective barrier, except in the presence of entrainment events that might take place from internal wave breaking or convection that penetrates the existing mixed layer. The e-folding timescale you calculate reflects the time for the air-water gas differential to equilibrate at which point substantial net gas transfer will become negligible.

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.

This is a very good suggestion and we have tried to distill the procedure to its simplest form without overlooking significant processes. However, at this stage the procedure can not be represented by a single explicit formula. That said, we have included in the supplemental materials the Matlab code for calculating k_{eff} from shear + convection based on the parameters described in this paper.

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.

We have modified the in-text descriptions of Figure 9 and 10. The new descriptions are inserted here: "In general, *k* is a scale independent or intensive property so we would expect no strong trend between *k* and f. This lack of tendency is borne out in Figure 9 (top panel), despite the areal dependence that has been introduced through equations 8 and 14 for the shear and buoyancy budgets. However, one interesting feature does emerge near between f = 0.15 and f = 0.35, where a discernable peak when *k* exceeded 4 m d^{-1} (Figure 9, top panel). This increase in *k* is the result of shear and convection enhancements during the seasonal transition periods when

sea ice is in advance or retreat. As reported above, stratification during ice melt enhances *k* from shear by "concentrating" the net stress closer to the air-sea interface. During periods of decreasing f (e.g. Fall), the effect of convection in water that is still open also leads to an enhancement in *k*. If *k* from shear and convection is converted to k_{eff} using equation (2), we find that the effective gas exchange across the heterogeneous ocean surface is less than 1 m d^{-1} from f = 0 to 0.4, and increases almost linearly across the range from f=0 to f=0.8, with a value of ~ 2 m d^{-1} at f = 0.8 (Figure 9, lower panel).

To estimate the total magnitude of gas transfer in the sea ice zone, we have stacked *k* from the mean-squared slope relationship in equation 16 with the estimates of *k* from shear and convection (described in steps 1-6 above), displayed in FIgur 10. The $k \propto f$ line determined by the mean NCEP windspeed for the Arctic, and prior values of k_{eff} from Fanning and Torres (1991), Loose and Schlosser (2011) have been overlaid to compare how this parameter model compares with those values. The parameter model exceeds the linear proportionality, but no values meet or exceed the median estimates of Fanning and Torres (1991). On average, the magnitude of k_{eff} from ice processes is approximately 42% of the total magnitude of k_{eff} as can be observed in Figure 10."

Technical comments:

1. Eq. (7) contains a funny sign. I'm not sure which sign is funny, I don't see errors

2. Eqs. (10) and (11) have funny signs, probably a bracket.

Likewise I don't see any brackets out of place.

3. (1172/13 ff.) Correct Section numbering.

Thank you, section numbering has been corrected.

4. (1180/21) The averaging bar is too far up.

We have attempted to fix the equation on (1180/21).

5. (1181/11 f.) funny signs between L0 and z.

Looking at the typeset pdf, the symbols appear to be correctly rendered, ie "<<" much less than..

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).

The "a" and "b" are typos. There is only one Frew 2004. We have fixed the typos.

7. (1190/20) "Station"!

8. (1191/12) page numbers are missing

We substituted this for a more appropriate reference

9. (1192/12 ff.) McPhee citations are in wrong sequence.

Will confirm this with Ocean Science editorial staff.

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.

We have made the font size larger in the caption and added to the figure caption as follows: "The relationship between the floe number distribution (N) per km^2 (left axis) and the individual floe dimension, L (right axis), vs. the fraction (f) of open water area (or converse of the SIC) from Toyota et al., (2006). N follows a distinct power law distribution when floe size decreases below a threshold (Toyota et al., 2006)."

11. Fig. 3: The stress should be "tau" rather than "t".

In fact it is tau. Am beginning to suspect there was something wrong with the rendering of the PDF.

Reviewer W. Asher

Parameterizing the transfer velocity, k, in terms of the turbulence dissipation rate (ep- silon) 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 de- termined (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 turbu- lence 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.

Yes, this is a good point, we have added to the discussion as follows: "For the purposes of this paper, we will assume the constant of proportionality determined by Zappa et al., (2007); 0.419. It should be noted that ε varies vertically in the water column and is likely to assume distinct values at the ice water and air-water interfaces, therefore unique proportionality constants may exist for the SIZ."

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 hypothe- sized viscous sublayer or molecular diffusion sublayer. Although combining of elements of boundary layer theory and surface renewal theory has

become common in the liter- ature, 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 re- newal 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.

Upon further review, I haven't found any information that indicates that surface renewal theory and boundary layer theory are mutually exclusive. Instead, renewal theory appears to provide a probabilistic timescale during which the viscous sublayer in a boundary layer flow will be overcome by turbulent eddies. However, we have tried to rewrite the paragraph and maintain consistency with surface renewal theory as follows: "For moderately to sparingly soluble (i.e. most) gases, the existence of an aqueous viscous sublayer at the air-sea interface is the dominant restriction to transfer across the interface. This bottleneck ranges in length scale from 20 – 200 μm (Jähne and Haubecker, 1998); turbulent eddies impinging on the free surface can reduce or collapse the viscous sublayer, leading to "bursts" of air-sea gas transfer (Rao et al., 1971). As such, the production of turbulent kinetic energy (TKE) close to the air-sea interface is a fundamental mechanism driving the enhancement of gas exchange beyond what would exist in purely molecular aqueous kinetics (Lamont and Scott, 1970), and consequently measurement of the TKE budget are the best quantities for determining the mechanisms that drive gas exchange rates."

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.

We have removed this line and reworded the sentence as follows: "This bottleneck ranges in length scale from 20 – 200 μm (Jähne, 2010); turbulent eddies impinging on the free surface can reduce or collapse the viscous sublayer."

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.

Thank you, section numbering has been corrected.

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

We are attempting to follow the nomenclature used by McPhee (2008) and he refers to the reference friction velocity as u_{*0}, with a "subscripted *".

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

We have changed this cross reference from Figure 10 to 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).

We have chosen to plot k vs f to observe whether the fundamental independence between k and f holds because in fact area weighting has been introduced through equations 8 and 14. Therefore, the perspective that k appears essentially independent of area is worth confirming.

We also hoped to highlight the distinction between k and k_eff, which is a fundamental objective of this manuscript. We elect to maintain Figure 9 intact, but we have included further discussion on Figures 9 and 10 to help clarify their meaning: "To estimate the total magnitude of gas transfer in the sea ice zone, we have stacked *k* from the mean-squared slope relationship in equation 16 with the estimates of *k* from shear and convection (described in steps 1-6 above), displayed in Figure 10. The $k \propto f$ line determined by the mean NCEP windspeed for the Arctic, and prior values of k_{eff} from Fanning and Torres (1991), Loose and Schlosser (2011) have been overlaid to compare how this parameter model compares with those values. The parameter model exceeds the linear proportionality, but no values meet or exceed the median estimates of Fanning and Torres (1991). On average, the magnitude of k_{eff} from ice processes is approximately 42% of the total magnitude of k_{eff} as can be observed in Figure 10. "

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