Response to review 1:

The original objective of this paper was to discuss the application of the resistance parameterization over the sea ice and the deviation and parallels to the application over terrestrial areas in order to suggest a conceptual model for air-ice exchange. However it seems that our attempt failed, thus we will write this more clearly in the introduction and also expand the discussion on this subject. It is difficult to really evaluate how well this parameterization performs, since it is basically just a conceptual model. However we will discuss in more details where the largest uncertainties are, when it comes to this parameterization and of which parameters we need more detailed information.

Page 3902 Line 16: This is the TCO2 in the tank (thus they mean surface water). This will be made clear in the rewritten manuscript.

Page 3903 Line 6: We will modify the sentence to include resistance theory.

Page 3903 Line 12: We will include a sentence on how wind is influencing the ice on a larger scale.

Page 3907/8: Equation 9. The reviewer is absolutely right. There is a sign problem, but after checking carefully we found that the minus is missing in equation 4 ($F= - (c-c0)/(Ra+Rb+Rc)$) This is changed now and we thanks the reviewer for paying our attention to this.

Page 3010 Line 1-2: We have clarified that the “good agreement” refers to the study bu Sørensen and Larsen.

Page 3010 Line 5-7: We have now analyzed this further and found that the stability corrections were small, so we are now estimating the flux based on a mean flux from three different estimation techniques (eddy covariance, inertial dissipation and the Cospectra peak method, introduced by Sørensen and Larsen)

Page 3916 Line 6-9: We have expanded the conclusion to clarify especially line 8 and 9.

Page 3901 Line 17, 19, 26 is changed according to suggestion

Page 3902 Line 7: is changed to “studies addressing…” Line 11 and Line 27 is changed according to suggestions.

Page 3914 Line 12: The sentence is changed and it now says that the uptake is increased

Page 3903 Line 4-7: the sentence is changed according to the suggestion.

Page 3905 line 25: The spelling is corrected

Page 3914 Line 2, 8, 9, 13 24 are corrected according to suggestions.

Response to review 2:

In general the reviewer suggest to put more focus on the deviations from the traditional use of the resistance methodology for terrestrial surfaces (specific comment: P3903 L4-7) because the data material is too weak to confirm/reject the parameterization suggested in the paper.

The original objective of this paper was to discuss the application of the resistance parameterization over the sea ice and the deviation and parallels to the application over terrestrial areas. However it seems that our attempt failed, thus we will write this more clearly in the introduction and also expand the discussion on this subject.

It is true that we have not made an effort to show any statistic or quality assurance of our flux measurements. This off cause should have been done. We will in a new version of the manuscript expand section 3 and use not only the inertial dissipation method but also the co-spectra peak method (using high and low frequency of the sample spectra) according to Sørensen and Larsen, 2010; Norman et al, 2013 and Mørk et al., 2013 to assure the quality of our flux data and we will provide error bars to our fluxes. During our data analysis we addressed the heating issues at low temperatures and corrections for these for the Licor 7500 IR instrument, but did just not go into details in our paper. We carried out a careful inspection of the cospectra and only those cases, where a clear upward or downward flux could be identified, were used in the paper. Furthermore we will discuss detection limits to flux measurements in relation to our measurements.

P3901 L20: We did observe melting of the ice during our study, however the point of our study was not to examine what happens during ice formation or ice melting, but to suggest a parameterization for fluxes over the sea ice and to suggest and discuss which parameters (e.g. heat fluxes) could influence the exchange. The exchange over the sea ice is depending on the age of the ice. Multiyear ice is more solid and might not be exchanging with the atmosphere at all, where annual ice, which will be more abundant in a warmer climate, will exchange with the atmosphere.

P3902 L9: We will rewrite this sentence because the assumption of the sea-ice acting as a lid on the sea surface inhibiting the atmosphere to exchange with the surface is still abundant. We will also add references to models using this assumption (e.g. Toggweiler et al., 2003).

P3902 L16: Will be changed to "total inorganic carbon (TCO_2)"

P3903 L4-17: We will put more focus on the discussion of the parameterization (see our response above)

Section 2: We agree that this could be shortened if the paper was only addressing atmospheric scientist, but here we will like to keep section 2 as it is. The purpose of the paper is also to reach the ocean (including sea ice) modeling community and experimentalist who measures carbon in sea ice, to study the transport of CO_2 within the ice, and fluxes over sea ice using chamber methods. This group of sea ice researchers is not familiar with the literature on flux parameterization over terrestrial surfaces and many are not familiar with micrometeorology or micrometeorological techniques, thus we find it important to state a summary of the basic theory here.

P3909 L1-20: We agree. Some of the fluxes are below or close to detection limit. This will be stated clear in the next version of the paper which will have new mean flux calculations based on three different analysis techniques, and error bars will be provided for figure 3. This will also be addressed in the discussion on especially Rc, since the small fluxes and high uncertainty on the flux direction will lead to negative Rc, however the biggest uncertainty is due to lack of knowledge of the carbonate chemistry in sea ice. Therefor we will expand on the discussion on pCO2 in the sea ice, which is calculated based on Goyet and Poisson, which is probably not adequate for calculation of pCO2 in sea ice brines, since this does not take the formation of CaCO3 in ice into account. Studies of sea ice and carbonate chemistry (Søgaard et al, 2013, Rysgaard et al., 2013; Geilfus et al., 2012, Miller et al, 2011) emphasis the importance of formation and dissolution of CaCO3 on levels pCO2 in the brines.

P3910 L11: "Equality" in this context, means, when the flux estimated by the inertial dissipation technique and the flux estimated by the covariance technique gives the same result. This will be rephrased in the next version of the paper, since this specific part will be rewritten to clarify the data treatment and the assessment of uncertainty.

P3911 L22: Jackson et al., 2013 and McGinnins et al., 2013 will be added to the reference list.

P3912 L15: The negative Rcs are due to the large uncertainty of the pCO2 (discussion on pCO2 in sea ice will be expanded) estimates and the small fluxes increasing the uncertainty on the fluxes which makes it difficult to estimate the direction of the flux. The smaller the flux the larger the Rc, but also the more difficult it is to estimate the sign on Rc. We will provide a more detailed discussion on pCO2, which is really introducing the largest uncertainty in the estimation of Rc, since we probably have under saturation of pCO2 due to formation of CaCO3 at low ice temperatures. The formation of CaCO3 will be discussed in a new version of the manuscript.

P3913 L5-11: We think the statement can be confirmed by figure 3 and 4, as we write. In figure 3 upward fluxes (and not uptakes, which will be negative fluxes) appears mainly in the beginning of the measurement period, in figure 4 we see that these are periods are associated with temperature drops. We think the reviewer has misunderstood the explanation and we will rephrase it in the updated manuscript.

P3914 L6: It is true that Rcs at times are negative (however this has changed in our new calculations based on the review from reviewer #1). However when we see negative Rc it is due to an opposite directed CO2 gradient and CO2 flux. The pCO2 in the brine is estimated to be higher than the atmospheric pCO2. This is because the production and precipitation of CaCO3 is not taken into account, when calculating the pCO2. This will be addressed in an updated manuscript.

3914 L11-15: We agree the statement seem to contradict. We meant to say that the potential for an uptake (downward flux) increase.

3915 L3-6: Rc and temperature are not independent, which we also state on 3915 L5-7.

P3915 L9: This is not regarded “ice melting season”. However the temperature in this area can have large variations, and just before we started our field measurements the air temperature was just above zero and right after we ended our field work the air temperature was 1-2 C for 2 days (Søgaard et al, 2013).

Response to review 3:

Page 3905, Lines 5-10: We will delete the details on the enclosure method.

Page 3906, We have rewritten section and corrected the figure according to the comments. We have explained in more details how we calculated Rb and redefined cs and c0.

Page 3908, Eq. 10: z0c is the surface. That will be more clear in the rewritten manuscript.

Page 3908, Eq. 11: Stanton number Bi is corrected. In the rewritten manuscript it is explained how Rb was estimated and how the variables were determined.

Page 3909, Eq.13: The mean vertical advection is normally neglected because the vertical velocity is considered to be negligible, by when measuring CO2 flux this term needs to be included since the difference in density between upward and downward moving air result in a non-negligible vertical velocity. This can be done by correction of the calculated fluxes (Webb, 1989) or the high frequent fluctuations (the raw signal) can be corrected (Sahlée et al 2008). Here we use the approach by Sahlée since we then have a corrected time series, which is needed for spectral analysis. This is described in the rewritten manuscript. The instrumentation (a Licor 7500 and a metek sonic) is also described in the manuscript.

Page 3910, L13-15: The fluxes are filtered based on a careful review of the co-spectrum and power spectrum of velocities and CO_2. A more detailed description of the flux estimations using different spectral techniques (inertial dissipation and also co-spectra peak method) is provided in the new manuscript. We also include an error analysis based on the error analysis in Sørensen and Larsen (2010).

Page 3910, L 24: We did not try to estimate the true surface pCO2 based on a vertical gradient in the ice. This paper is meant to suggest a conceptual model for CO2 air - ice exchange. However we will add that as a suggestion for future work in a more detailed study of air – ice exchange.

Page 3914, L7-8: We have looked through our analysis again and based on a longer data set from our field site (Søgaard et al., 2013), we can conclude that the largest uncertainty in our study is the calculation of the pCO2 in the ice. This is because the production and precipitation of CaCO3 is not taken into account, when calculating the pCO2. This will be addressed in an updated manuscript.

Page 3915, L14: We have removed the correlation between temperature and pCO2 from the manuscript.