Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Analysis of the PKT correction for direct CO_2 flux measurements over the ocean" by S. Landwehr et al.

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Reviewers: John Prytherch, Margaret J Yelland.

General comments:

This paper presents air-sea CO2 flux measurements made from a research vessel in the Southern Ocean. The flux measurements were made using four Licor infra-red gas analysers, two of which used a dried airstream in order to minimise sources of error. The measurements from the other "wet airstream" sensors had an additional correction applied to remove the effects of humidity cross talk using a published method (the PKT correction, Prytherch et al., 2010a). The results obtained using the PKT correction were in poor agreement with those from the "dried airstream" sensors. The authors

then perform an analysis of the PKT correction method, and demonstrate that it does not properly correct CO2 fluxes.

At the time of publication in 2010, the PKT correction method was the only one available for open-path sensors. These sensors were used on platforms where it was not practical to dry the airstream (e.g. on buoys). The method has since been used, and the results published, by various research groups. Therefore, the authors' refutation of the PKT method is an important result and should be published.

The authors use a mathematical analysis of the PKT method to demonstrate that the method is invalid. This important result should be made as convincingly as possible. While the analysis itself is well constructed, we feel that the presentation of the results could be improved. We hope that our suggestions below will help improve the clarity and impact of the paper.

Specific comments:

a) It is not clear from the bottom panel of Figure 3 that Eq. (7) obtains similar results to the "standard" PKT correction. We compared Eq. (7) and the standard PKT results from our own air-sea CO2 flux data (previously published in Prytherch et al., 2010b), and obtained a clear 1:1 relationship (R2 = 0.98), shown in the attached Figure R1 (Measurements in all figures are from the HiWASE field campaign with quality control as described by Prytherch et al. (2010b)). The addition of a similar plot from the authors' data would be much more convincing than the current time series shown in Fig. 3.

b) The authors' analysis of the PKT correction shows that the "corrected" flux value is a product of a detrended (with respect to humidity) flux and a "beta" term. The beta term includes a factor of 0.5, which comes from a term in the original iterative PKT correction (Eq. 6), which was used to help the iteration converge by making the individual steps in the iteration smaller. This arbitrary factor was not thought to affect the convergence value, but the authors show that in fact the PKT correction is directly dependent on the

value of this factor. Modifying this value in the iterative PKT correction would provide a convincing validation of Eq. (7) (and hence the invalidity of the PKT correction). We have done this using our flux data, using a factor or 0.75 rather than 0.5 in the original PKT correction. This changes the value of beta from ~2 to ~4. The attached Figure R2 shows that the "PKT 4" results are approximately equal to 4 times the detrended flux.

Figure R3 shows that if this factor is used in the sensible heat flux test, then the converged values of the fluxes are twice as large as the actual sensible heat flux. It was a very unfortunate coincidence that the detrended sonic temperature flux was a factor of 2 smaller than the sensible heat flux, and that the factor of 0.5 used in the iteration happened to increase the detrended flux by a factor of 2.

c) Paper structure. The main (or only) result in this paper is the proof that the PKT method is not valid (which begs a minor question - why was this paper not submitted to Geophysical Research Letters as a "comment"?). However, the narrative structure used (ie "we made these measurements, then we tried this correction. The correction didn't work on our measurements, so we examined it and found it to be incorrect") leads to the authors giving a very detailed description of the lack of agreement in the results between the data from the dried sensors and those from the PKT-corrected wet-airstream sensors. Given that the subsequent analysis demonstrates that the PKT correction does not work, much of this description seems superfluous.

Technical corrections:

Is there a reason why the two open-path 7500 units were chosen to be the "dry" sensors whereas the two closed-path 7200 sensors were chosen to be "wet"? The terminology of "open" and "closed" is used rather loosely - the 7500 is an open path sensor, but in the SOAP experiment it was effectively converted into a closed path sensor. This could be explained more clearly, e.g. was the conversion to closed path done using the method of Miller et al., 2009?

Page 28286, line 8. How was the conversion to CO2 mixing ratio done? Was it done in a similar fashion to Miller et al., 2010?

Page 28287, line 7 states that no airflow distortion correction was applied, but the caption for Figure 2 states that wind speed was corrected for airflow distortion. This could be confusing; perhaps specifying that "[Flux] measurements were not corrected..." would clarify this.

Page 28288, line 2. Results from two wet IRGA are shown, so plural "analysers".

Page 28288 line 7. Could the authors comment on potential reasons why the range of bias was so much larger for IRGAwetA than wetB?

Page 28289, line 11. An erroneous "the" after "...scatter, but... ".

Page 28292, line 5. The fact that the bias between the "sonic flux" and the sensible heat flux is small is not really relevant: the intention of the original test was to see if the detrended flux (with a large bias) was iterated back to the correct value or not. Unfortunately it was...

Table 1. Prytherch et al., 2010b presents PKT corrected results and should be added to this list.

Figures. The general presentation of the figures needs to be improved. Some examples are listed here: Missing "-" signs in the exponents in Figures 2 to 5. Figures 8 and 9 use W/m2 rather than W m-2 as used in the other plots. Labels use wetA, dryB etc but in the figure captions and text IRGAwetA etc is used. Inconsistent use of capitalization (e.g legend of Figure 9). Missing space in x-axis label of Figure 9. "w" should be capitalized in the colour bar label of Figure 6. Labels outside legends in Figures 8 and 9.

Figure 2. What bulk formulae were used to calculate the bulk fluxes? Also, shaded areas are shown in this figure, but are only explained later on in the caption to Figure 3.

Figures 6 and 7. I wondered if some of the difference between the binned WetA and WetB results might be due to data from different periods being binned. The total number of periods from each of the two sensors only differs by 4 (174 for A, 178 for B), but it is not clear that the majority of these data come from the same periods, rather than data from WetA being accepted for one period, and WetB from another.
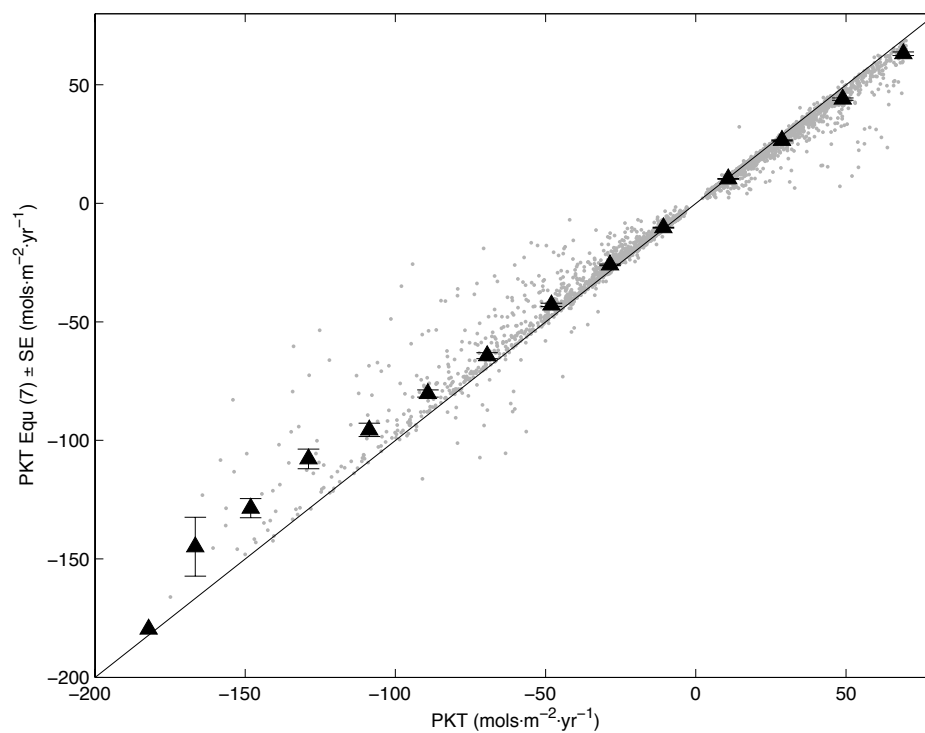
Fig. 1. Figure R1. Individual (grey) and binned (black triangles) CO2 fluxes calculated using Eq. (7), compared with the iterative PKT method (R2 = 0.98) (Prytherch et al., 2010a).
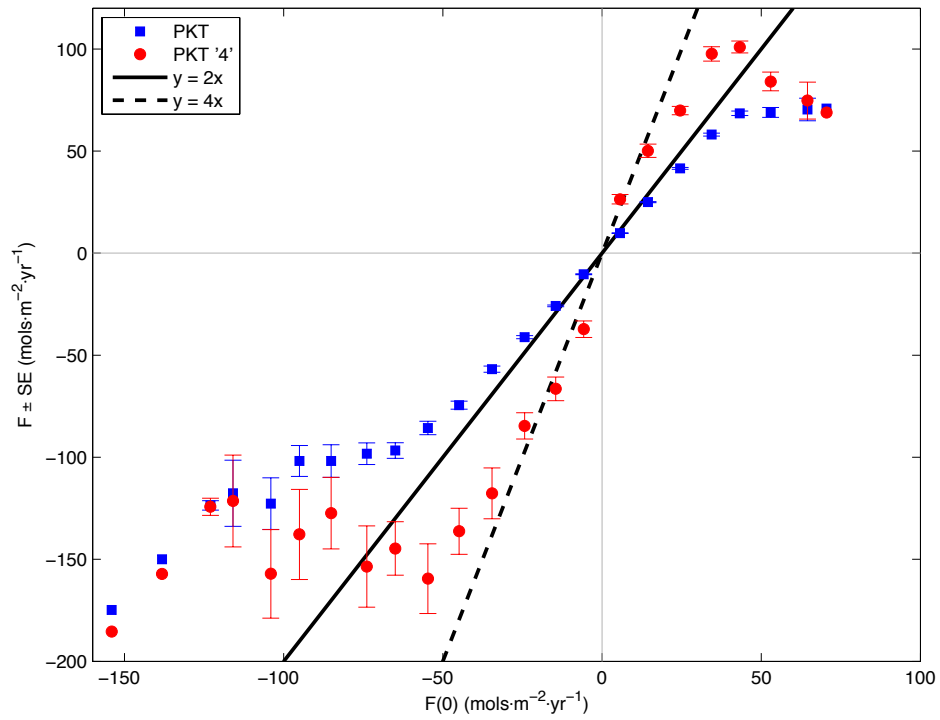
Fig. 2. Figure R2. CO2 fluxes calculated using the iterative PKT method using a convergence factor of either 0.5 ('PKT') or 0.75 ('PKT 4'), binned with respect to F(0).
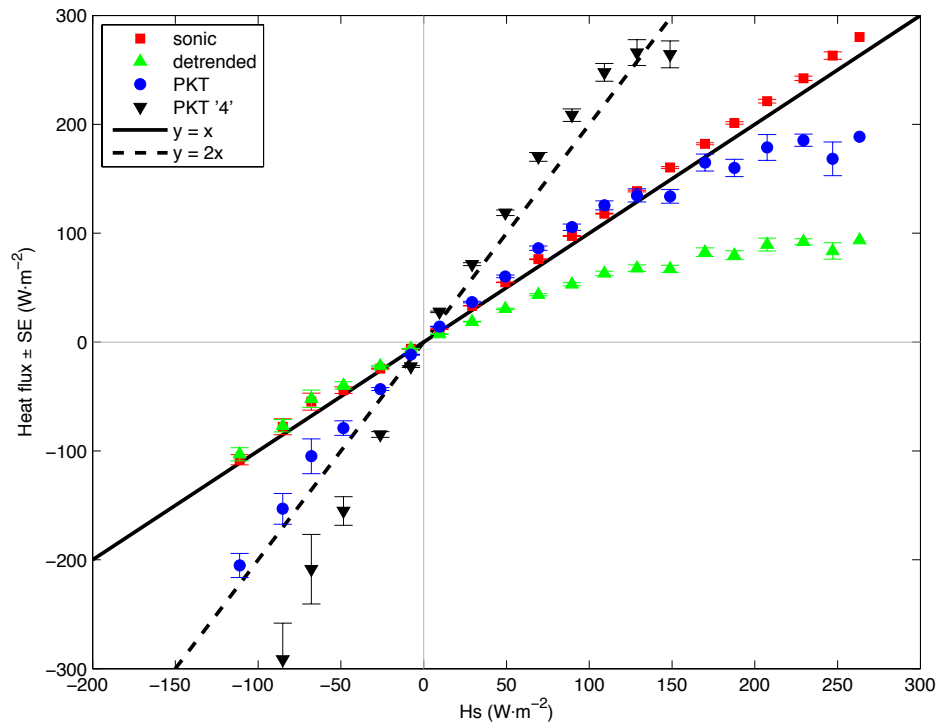
Fig. 3. Figure R3. 'Sonic', 'detrended' and 'PKT' heat fluxes binned by sensible heat flux (Prytherch et al., 2010a). 'PKT 4' uses a convergence factor of 0.75 rather than 0.5 in the PKT correction.