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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The influence of transformed Reynolds number limitation on gas transfer parameterizations and global DMS and CO_2 fluxes" *by* Alexander Zavarsky and Christa A. Marandino

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This paper describes modifications to water-side viscous transfer velocity,k0, parameterizations to account for reductions in transfer associated with hypothesized flow separation that is diagnosed via a wave-based scaling parameter, Retr. Retr is defined as a Reynolds number in a reference frame moving with the wave peak phase speed. If the abs(Retr)<6.7e5, then k0 is suppressed ('limited') because of flow separation on the downwind side of the peak waves. The physical mechanism is that flow separation reduces the area of strong viscous coupling on the air side and this limits the forcing

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Discussion paper

of the molecular sublayer transfer on the ocean side. The details are given in a previous paper – ZA18. The authors use these results to recalibrate a number of different parameterizations to account for the transfer suppression. For example, for DMS the transfer coefficient is assumed linear an represented as k0=a*u-b where a=3.1 and b=-5, u=u10 if ABS(Retr)>6.7E5 and u=ualt if ABS(Retr)<6.7E5. The modified wind speed, ualt, is computed by reducing u10 until ABS(Retr)=-6.7E5. The authors showed that remapping the wind speed in this manner leads to a more linear k vs u10 behavior from their two data sets. They also apply a similar remapping to the Nightingale 2000 data set so that k=c*uˆ2 where c=0.36, which is 22% higher than the unmapped fit. Note k here seems to include but viscous and bubble-mediated transfer. Finally they use this approach to compute global fluxes of DMS and CO2 with various k representations and find that their new approach reduces fluxes about 10%.

In my view a reduction on global fluxes of 10% is significant enough to justify publication. However, the paper appears to be hastily written and not carefully crafted to make it easy for the reader. It is hard to read, difficult to follow and contains a bewildering variety of coefficients, percentages, and information that is poorly organized. I have no confidence I actually know which coefficients were used where. On a side note, my own opinion is that the phenomenon they are characterizing (reduction in k in certain air-sea conditions) is almost certainly not flow separation but it is possible their use of Retr is capturing a lot of what is happening. Since ZA18 is published, I think my skepticism should not prevent publication of this paper and I don't want to argue that point here.

Here are some specific comments:

P2 line 18 Suggest identifying ZA18 here and using it throughout.

P2 Line 21 In ZA18 theta is defined as the angle between the wave direction and utr, which would be at 90 deg to what is said here. I am somewhat confused by fig D1 in ZA18 which states both that the 'wave crests are moving to the left' and that 'The wave travels from left to right'.

P3 line 14 'W14 must already have gas transfer limitation included as it is solely dependent on carbon isotopes to estimate the air-sea flux over several years'. Not sure what you mean here. Are carbon isotopes relevant to this? Do you mean that the mean flux is associated with averages that include the mean contribution of non-limited and limited conditions? I think you mean it is applicable to average wind speed conditions as is. Please clarify.

P4 line 25. You might not want to push the 'flow separation' aspect since it is not necessary to your development here.

P5 line As I interpret the mathematics, reducing the wind speed until ABS(Retr)=6.7E-5 will cause Retr=-6.7E-5. Also, I don't see why you need to iterate. Is it not true that

ualt=(cp-6.7E5*nu/Hs)/cos(th1-th2)

where th1 is the direction of the wind and th2 is the direction of the waves in the earth frame?

P5 line 13 It looks like you change the wind speed because that is in the k parameterization.

P5 line 29 'We subtract a linear dependency using the ZA18 parameterization, to account for the gas transfer limitation in k0'. I don't understand this. I think you need to provide a few equations to make it clear. Eq (13) looks wrong to me; you have removed most of the linear part and left only the bubble part. Should it be k*(u10-ualt) instead? Also, do you actually use (13) anywhere? I do not see it referred to anywhere else in the document. Is NI00 the same as N00?

P6 lines 6-7 You said that already.

P6 line 15 Change to 'plotted at the corrected..'

P6 line 17. Is ZAV17 the same as ZA18?

P6 line 23 Add ‘for Knorr11’ after ‘estimates’.

P6 line 26 The explanation about waves greater than 3.5 m/s seems to be total speculation. It is true that large swell are unlikely to have flow separation.

P6 line 31-32 Perhaps reiterate this in the conclusions.

P6 line 9 Suggest giving the formula for N00 as you did for W14.

P7 line 8 Just to be clear, k corrected = k*y(x), yes?

P8 line 23 Not sure the parameterization is ‘independent’ of events. I think you mean it includes them in a globally average sense. Your Fig. 8 implies that it would vary regionally.

P9 line 16. Since N00 applies to less soluble gases with more bubble enhancement, it seems like the limitation should be less. You argue it is ‘masked’ by bubbles.

P9 Section 4.4 Please state exactly and clearly formulas used in the computations. Suggest adding the formulas to Tables 3 and 4 with a clear rendition of their application.

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