

Interactive comment on “Variability of air-sea gas transfer velocities in the Baltic Sea” by Leila Nagel et al.

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This paper presents new and reprocessed older field measurements using the active flux thermography technique. These are very interesting observations - the improved analysis/processing methodology is clever and should be much more robust than earlier approaches. In addition, recognition of variable Sc number dependence based on lab studies provides improved interpretation of the measurements. The data are a welcome addition to the literature on in situ air/sea gas flux studies and should be published. This paper is also very clearly written and nicely organized, which is much appreciated by this reviewer. There are a few scientific comments below which I would like the authors to consider in their interpretation (or at least discussion) of the data.

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There seems to be a rather strong bias against eddy covariance in this paper - the only comment about more than a decade of new work in that area is rather dismissive and citation-less. So, a reader new to the field would imagine that eddy covariance is not generating insight into air-sea gas transfer (which I think most would agree is not the case). The fact that eddy covariance data are often binned seems like an odd criticism, especially when the dual tracer method (which requires long averaging) is held up as the "gold standard". The uncertainty in a single DMS eddy covariance measurement under favorable conditions is on the order of 25% and one could easily imagine interesting results from simultaneous eddy covariance and active thermography measurements.

I was surprised that so much emphasis in this paper was placed on the dual tracer method because active thermography captures only interfacial flux. Bubble-related transfer is very important for CO₂ so if active thermography agrees with dual tracer at intermediate and high winds, then it would seem that some assumption in the interpretation of these methods is wrong. Active thermography should be more similar to eddy covariance measurements of DMS than to a dual tracer fit meant to mimic CO₂. Eddy covariance studies of DMS and CO₂ clearly show that CO₂ fluxes at intermediate and high winds are enhanced by bubble transfer relative to dms (which is controlled mostly by the interfacial flux; for example, Bell et al., 2013; Blomquist et al., 2017). There is a conundrum here - if the dual tracer method gets kco₂ right (which it seems to), it must be bubble-enhanced also. So one would expect active thermography to diverge from the dual tracer results at intermediate and higher wind speeds.

Several studies suggest that interfacial gas transfer appears to be limited at higher winds. This is attributed to wave shielding and other wave-related effects demonstrated in the laboratory by Mueller and Veron (2009) and incorporated into gas transfer models by Fairall et al., 2011 and Donelan and Soloviev, 2016. Such processes could be salient here in relating active thermography to gas transfer. This is not to say that the arguments in the paper about fetch and surfactants etc. are not very well founded. I

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think they are. But the overall premise that the dual tracer and active thermography measurements should measure the same thing seems open to debate. I think this should be considered by the authors and perhaps addressed in the manuscript.

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