Review of "Air-sea exchanges of CO_2 in world's coastal seas" by Chen et al.

This is a very well written and thorough review of literature pertaining to gas transfer and CO_2 fluxes in estuaries and the coastal shelf. The authors have compiled a vast amount of published CO_2 air-sea flux data and supplemented it with important new data from SE Asian waters. Readers should find this a valuable resource.

I think the paper would benefit from a few minor revisions. I also have some questions that I think need to be clarified prior to publication.

Introduction -

The introductory review of carbon fluxes from the land through the estuaries then through the coastal shelf zone is succinct and informative. I was a little surprised that Tranvik et al's (2009) estimate of riverine C flux to the ocean was not covered as I think this is one of the more definitive analyses from freshwater scientists of the composition and quantity of river fluxes.

The main thrust of the paper is the presentation of air-sea CO_2 flux data. I assume most of the reported values compiled from the literature are computed using the thin boundary layer (TBL) method (aka stagnant film method), i.e. multiplying a wind-speed dependent gas transfer velocity by the air-water ΔpCO_2. Given the essential importance of the TBL method, I think a little more introductory discussion of the calculation is warranted, especially the uncertainty associated with the gas transfer velocity - wind speed relation (time of day of measurement, wind speed averaging periods, etc). Relevant literature has been cited but I think in a review of this sort it is helpful to more directly present this material.

Calculations and data presentation -

Not enough information is provided in the paper to allow assessment of the computed air-sea CO_2 fluxes and the authors' interpretation of them.

Please include the equation used to compute the gas transfer velocity. Wanninkhof 1992 is cited as the source but that paper makes clear a couple of things: 1) the choice of a quadratic function is arbitrary - not based on any underlying theoretical considerations; 2) flux enhancement at low wind speeds is important and temperature-dependent. Furthermore, plots in Wanninkhof (1992) suggest a very large low bias at low wind speeds which may have a major bearing on the author's conclusion that globally, fluxes are less than previously reported because of the addition of new data from Asian waters where average wind speed is ~ 1.6 m/s. I would have thought the cubic equation proposed by Wanninkhof et al (2012) and fit to the GasEx-2001 data set would have been a better choice for the present work. If Wanninkhof (1992) Eq 3 has been used without adjustment then I would expect calculated fluxes in low wind areas to be biased low by a factor of 3 to 4.

Please include the source of the wind speed data used in any calculations. I am a little uneasy about using global wind potential as a surrogate for u^2 because a number of sptially variable considerations other than wind speed are factored into Zhou et al's (2012) calculation. Also, my understanding is that wind power potential varies as u^3 and I note that Zhou et al do not provide the equation they used for their calculation (I am relying on lecture notes for wind power calculations). Zhou et al's Fig 1 is reproduced with permission as Fig 5 in this paper. More generally, I did not find Fig 5 particularly helpful because the relevant wind speed information (estuaries and coastal shelfs) tends to get lost in the land mass when, in fact, winds along the coast are the relevant consideration for estuarine and coastal shelf flux calculations.

It was not clear to me which fluxes in Tables 1 and 4 were reproduced from the cited literature and which involved new calculations. Are all values recomputed from reported pCO2 and using a consistent wind speed data source?

I would find a table listing the pCO2 measurements very useful. Perhaps as supplementary material.

p 5045 line 20 - I think the discrepancies between continental shelf area estimates is exaggerated a bit. If one discounts the 36 x 10^6 value of Liu et al (2000) as an outlier all the other results fall by and large within 26 ± 1, which is really pretty good.

p 5026 line 23 - Jaing et al. 2008 citation - is this 2008a or 2008b?

Exchanges in estuaries -

p 5047 - The roles of light and nutrients vis a vis biological production is discussed. Perhaps a comment about the role of residence time would be in order as well. Primary production in inland waters requires light (controls the rate of growth) and nutrients (sets the maximum achievable biomass) and the amount of production depends on the time available to grow, i.e. short, fast rivers will experience less C transformation than long, slow rivers given the same light and nutrient availability. Residence time is also relevant to leaching of organic matter to produce DOC and microbial transformation of DOC.

Is the role of tidal forcing on estuarine mixing worth a comment as this may have bearing on sediment burial, etc.

p 5048 - Flux results are categorised on upper, mid and lower estuaries. Can you provide an functional definition used to delineate estuarine systems? For example, is the criterion simply a specified salinity. If so, what values were used?

River plumes outside of estuaries are neglected. Is it possible to estimate their relative contribution to the overall estuary + shelf flux to justify their exclusion, i.e. are they sufficiently rare as to be unimportant (although I imagine when they occur they must carry a fair amount of C with them).

Not much seasonality is reported for the fluxes yet individual estuaries in Table 1 for which values are reported for all 4 seasons show large differences between seasons. Has the reported lack of seasonality been influenced by averaging all available data or were only data from estuaries with data for all seasons considered? The discussion left me feeling that seasonality isn't a big deal, but when I look at individual estuaries it seems to be highly relevant on a case by case basis. Can you reassure me that the lack of seasonality is 'real' rather than being an artefact of the statistical method applied? I would not want the reader to come away thinking that they could measure the flux at a single time of year and assume that is representative of the mean annual flux for the estuary.

p 5049 - Can you quantify how small the contribution of small estuaries is? What is the definition of a small estuary? Does the difference between northern and southern hemispheres reflect the much larger terrestrial catchment area supplying organic matter in the northern hemisphere?

I am quite concerned that the quadratic wind speed function, rather than real changes in gas transfer velocity, is responsible for the low fluxes reported for Asian low latitude regions. I know from lots of personal experience that it is actually penetrative convection prior to sunrise that governs mixed-layer depth in low wind speed lakes and reservoirs. I presume the same might be the case for tropical estuaries. The implication of this is that the turbulent velocity scale in the surface layer of the water column computed from heat transfer considerations is often greater than that computed from wind speed. Vachon et al. (2010, Vachon, D., Prairie, Y., T., & Cole, J., J. (2010). The relationship between near-surface turbulence and gas transfer velocity in freshwater systems and its implications for floating chamber measurements of gas exchange. *Limnology and Oceanography*, *55*(4), 1723-1732.) have shown that water turbulence is a much better indicator of gas transfer than wind speed. Could it be that the calculation of gas exchange in low-latitude Asian waters has a strong low bias because wind speed is not the correct parameter to be considering for such calculations? (I recommend, also, Schladow, S. G., Lee, M., Hurzeler, B. E., & Kelly, P. B. (2002). Oxygen transfer across the air-water interface by natural convection in lakes. *Limnology and Oceanography*, *47*(5), 1394-1404.)

p 5050 line 7 - Again, I think it is important that the reader understands that the choice of a quadratic function is arbitrary and has no basis in theory. Because it is simply a curve fitting technique, it is important that the equation used for this work fits the available field data on gas transfer velocities well at low wind speeds. I just need some reassurance that this is the case.

p 5050 line 19 - What is the criterion for defining a similar region? Is it based on area, latitude, terrestrial catchment area, etc?

p 5056 lines 1-7 - This discussion depends on the definition of the system, i.e. where the boundaries are drawn and the time scale over which the assessment is made. Presumably, cooling effects leading to undersaturation in the surface layer are offset by warming effects in the surface layer at other times of day or year? Do you have an example of such a system? How frequently does it occur?