

Interactive comment on “A 1-D examination of decadal air–sea re-equilibration induced ocean surface anthropogenic CO₂ accumulation: present status, changes from 1960s to 2000s, and future scenarios” by W.-D. Zhai and H.-D. Zhao

Anonymous Referee #3

Received and published: 17 November 2014

This paper considers how the surface ocean carbon system might have varied over the last few decades in response to the anthropogenic CO₂ rise, using a combination of published observational products.

Unfortunately, I must say that I struggled with this paper. First, I struggled to understand what exactly the authors had done. Second, I struggled to see how this calculation is a useful step forward.

C6824

If I understand correctly, the calculation takes all required values as ~present-day climatologies, and varies only the atmospheric pCO₂ over the time period at hand. The variable atmospheric pCO₂ enters the calculation as the global base value to which the 2-dimensional Takahashi year 2000 delta-pCO₂ is added. Thus, the 2d spatial climatology of Takahashi is held constant over time, and the values at each grid cell increase in step with the global atmospheric pCO₂.

If my interpretation of the method is correct, I am not convinced that it is useful. That's not to say it isn't useful - but the paper does not clearly make the case. It would be helpful to explain better at the introduction, and also emphasize in the conclusion just why the results are valuable. Is the idea to better understand the carbon cycle? Is it to provide an observational constraint against which to test numerical models? Is it to gauge the impacts of ocean acidification? What is the real advance here beyond the Broecker et al. (1979) view?

If we are considering the importance for ocean carbon uptake, the mixed layer is most important as the gateway between the atmosphere and the ocean. The carbon contained directly in the mixed layer is a small amount of the total oceanic uptake, and the results here do not seem to make a significant change in our quantification of this uptake. Besides, the mixed layer depth is highly seasonal, changing by hundreds of metres at high latitudes. One might ask, why does it matter how much carbon is contained in the mixed layer? Again, maybe there is a good reason we should know this - but if so, I'm not sure what it is, and the paper has not pointed it out to me.

I hope these comments will help the authors to focus their work towards a product that will be useful to the community. I also offer a few additional, more specific comments below.

p 11511: This sentence does not make sense: 'The surface ocean uptake of anthropogenic CO₂ is just the case.' Also, 'This effect, chemical buffering capacity. . .' is confusing, in that the chemical buffering capacity itself has not been introduced.

C6825

R_{equ} is not well defined. Given that this is of central importance, it should be carefully defined.

I'm not sure what is meant by the 'eloquent definition' of RF.

I don't follow what happens from the second to the third step here, please elaborate: $\delta \text{DIC} / \delta x\text{CO}_{\text{air}} = \delta \text{DIC} / \delta p\text{CO}_2 \cdot \frac{d p\text{CO}_2}{d(\text{P} - \text{pH O})} = \text{DIC} / p\text{CO}_2 \cdot \frac{d p\text{CO}_2}{d(\text{P} - \text{pH O})}$

This part of the method needs to be much better explained: 'To illuminate the atmospheric forcing on the ocean surface carbonate system, we defined "steady-state sea surface pCO₂" in a given decade by scaling Takahashi et al. (2009) data (for a reference year 2000) to the change in xCO_{air} from the corresponding time period.' How scaled? Do you mean you assumed the DpCO₂ of Takahashi, i.e. the surface disequilibrium, was constant over time, so that the pCO₂ at all points varies directly with global CO₂?

I don't like the idea of looking at local relationships between surface ocean DIC change and the atmospheric conditions immediately above the same grid cell. The air-sea exchange timescale of carbon is long compared to the circulation rate. As a result, the DIC at a given point in the mixed layer has a lot to do with where that water was before, and little to do with where it is right now. Åã

The methodology is a hodge-podge that doesn't obviously account for the most important things. For example, windspeeds vary by a large amount, and piston velocities along with them. Global wind fields are available.

The term 'potential' first appears in section 2.3 It needs to be defined, since it's not clear what it is.

This is not well justified: 'if the wintertime MLD was deeper than 100 m at any grid box, we replaced it by 100 m, as this is the approximate MLD to be fully ventilated in a decade'. The mixed layer is the mixed layer, isn't it? Why cut it off at 100 m? There may be a good reason for this, but since I don't really understand what we learn from

C6826

Requ, I don't know how it should be calculated.

I can't follow the last 2 paragraphs of section 2.3.

In general, the English needs improvement, and the writing is very dense.

Some sentences include details that do not help. For example, this sentence is unnecessary: 'During the past five decades, decadal average of xCO_{air} rises from 320 ± 3 ppm in 1960s to 331±4ppm in 1970s, to 345±5ppm in 1980s, to 360±5ppm in 1990s, and to 379±7ppm in 2000s, based on the Mauna Loa station data released by NOAA/ESRL at <http://www.esrl.noaa.gov/gmd/ccgg/trends/>.'

Is the title appropriate? It seems like a 2-D examination to me.

Interactive comment on Biogeosciences Discuss., 11, 11509, 2014.

C6827