

## ***Interactive comment on “A 1-D examination of decadal air–sea re-equilibration induced ocean surface anthropogenic CO<sub>2</sub> accumulation: present status, changes from 1960s to 2000s, and future scenarios” by W.-D. Zhai and H.-D. Zhao***

**Anonymous Referee #1**

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I like many parts of this manuscript. The conclusions and methodology are broadly correct, but the manner of presentation is so awkward as to greatly diminish the usefulness of the paper. Basically it begins with an inappropriate Introduction of a very general nature. But on reading this through I find it might be much better presented as a step forward in a classic series of papers in which ocean scientists have periodically reviewed (re-discovered?) the ocean chemical buffer factor. This is many ways quite a beautiful history, and it seems to be updated about once every decade – so the time

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for this is ripe. It would help the reader if it was presented in this way. A casual history might find a time line of continuous improvement from Callendar (1938) who recognized alkalinity as a control but did not appreciate the buffer factor, through the 1957 Revelle and 1960 Bolin papers, the work of Dyrssen and Sillen in 1967, the extraordinary efforts of Broecker and Takahashi in the early 1970s and the paper of Whitfield in 1974, the update by Sundquist and colleagues etc etc. Seen in this way the work here is a step forward in a classic series – with each decades improvement overlaid on others. Here the novelty is the very careful use of the newly reported mapped surface fields. The authors have obviously put a lot of work into dissecting and assimilating the now large array of surface mapped data. They point to the small discrepancy between competing estimates of ocean CO<sub>2</sub> uptake rates, and they refer these back to fundamental property of (rediscovered?) a changing buffer factor. It makes sense. But the argument is so badly presented that the message is lost. The manuscript is so full of notation and jargon as to lose the non-specialist the paper should be aimed at. The endless repetition of “ $\delta\text{DIC}/\delta \text{xCO}_2\text{air}$ ” is particularly grating. For details I can't see exactly what is meant by lines 26-28 on page 9. And on page 14, lines 11-12 the comment seems odd given the large efforts of the CMIP teams. For the Figures I find Figure 3 puzzling, but Figure 4, although simple, is a classical update on a problem now at least 40 years old and still of interest and too little understood by those outside the ocean chemistry world.

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