

## ***Interactive comment on “Watermasses as a unifying framework for understanding the Southern Ocean carbon cycle” by D. Iudicone et al.***

**Anonymous Referee #1**

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### General assessment

The manuscript describes the circulation and water mass transformation in the Southern Ocean of a biogeochemical ocean circulation model. The authors set out to present a "unifying framework for understanding the Southern Ocean carbon cycle", and I found it very difficult to see whether the paper comes anywhere near to this goal. First, the biogeochemical model results do not seem to agree well with observations. Although the comparison of simulated and observed DIC and alkalinity fields (Figs 5 and 6) are the only colorless contour plots in the paper, which makes them very difficult to interpret, these figures reveal substantial model deficiencies: the vertical DIC gradient is 50% too high in the model, and the vertical alkalinity gradient is far too low. As a result, simulated deep-ocean alkalinity concentrations are, in the zonal average picture,

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about 70  $\mu\text{mol/l}$  too small and in some places even smaller than simulated DIC concentrations. How can this happen? Is this a result of hugely intense remineralization? I do not know how alkalinity is computed in this model, but wouldn't this correspond to negative carbonate ion concentrations? How can one use such a model to understand the Southern Ocean carbon cycle? I get the impression that the presentation of the biogeochemical model results is not honest and tries to hide these deficiencies. First, these deficiencies would have been much more obvious had the authors used a color figure (as they did for all other figures). Second, the authors say in section 2.4 (Model configuration and evaluation) that "The model circulation and tracers fields correspond well to known features, and in many cases the agreement with observations is excellent"! This excellent agreement with observations certainly is not visible for the main variables relevant for the carbon cycle, and thus for the focus of this paper. Section 3 ("Model evaluation against observed fields and fluxes") mostly discusses simulated pre-industrial air-sea  $\text{CO}_2$  fluxes against inverse estimates (that are based on some models) or against observed (industrial)  $\text{CO}_2$  fluxes (not shown).

Section 4 entitled "Water mass analysis of the Southern Ocean  $\text{CO}_2$  flux" presents a detailed discussion of the seasonal cycle of air-sea fluxes and its driving forces. Presumably, these driving forces of the seasonal anomalies do not have anything to do with the driving forces of the mean air-sea  $\text{CO}_2$  fluxes. It is, however, the mean fluxes that are relevant for the mean carbon fluxes of section 5. The authors fail to point out why large parts of section 4 are needed in this paper.

Section 5 looks at the mean DIC transport by the overturning circulation. As indicated in section 5.1.2, variations in DIC are so small (1-10%) that the transport of DIC is roughly proportional to the volume transport. One then wonders, why one has just read through a detailed discussion of seasonal anomalies.

One conclusion of the paper is that the role of biology is significantly smaller in regulating annual  $\text{CO}_2$  fluxes than transports associated with overturning (p.3422, line 27-29). Apparently, this is based on the fact that biology does not seem to play a

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dominant role in driving linearized seasonal CO<sub>2</sub>-flux anomalies. Seasonal anomalies and annual mean fluxes are, however, completely different things (probably even in the Southern Ocean). Moreover, if the biology were not important, how could one explain the large errors in the modeled DIC and alkalinity fields? The claim that the study "sets a baseline for comparing pre-anthropogenic and recent DIC redistribution in the ocean" (p.3424, l27-28) and provides a "3-D reconstruction of the processes ruling CO<sub>2</sub> exchanges" is not supported by the results shown in the paper.

The paper is extremely long, lacks focus and is very difficult to follow. In many places there is a lack of scientific scrutiny and precision (see specific points below). There is no logical sequence of the individual parts of the paper. The investigation of what drives the seasonal cycle does not seem to be needed in later chapters, and it is not even clear whether the results would look much different if the volume transports were simply multiplied by an average DIC concentration. Taken to the extremes, this argument would erroneously imply that the biological pump does not play any role in the marine carbon cycle (nor in generating the large errors in the DIC and alkalinity fields of this model). I cannot recommend this paper for publication in Biogeosciences.

Some specific points.

p. 3396, l.8 circular argument

p. 3396, l.25 integrated, not averaged!

p.3398, l.11 How is the modeled pre-industrial CO<sub>2</sub> flux compared with the industrial observational estimate of Takahashi et al.?

p.3399, l.18. what is the interior diffusivity in the model

p.3399, l.20. Why should there be a "finite" number of water masses? Isn't it infinite?

p.3400, l.8. Is a two-week mean sufficient to conserve adiabatic processes?

p.3400, l.9-11. I do not understand what you want to say by this sentence

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p.3400, l.20, Is "t" temperature? which units? Degrees Celsius? What are the units of wind speed (and at which height is it assumed)?

p.3400, l.24, Is "T" temperature? Which units? Kelvin?

p.3401, l.9ff Why are you interested in the relative contributions? Why should the reader be interested in these?

p.3402, eq.4 What are  $G^{ZZ}$  and  $G^{MM}$ ?

p.3402, l.21 Eq.5=Eq.4? The entire section 2.2 does not add any new information.

p.3404, l.9 vs 13. Do the authors consider results of previous modeling studies as "observations"?

p.3404, l.24, and many other places. The sign is wrong. Air-sea flux describes the flux from the air into the sea, not vice versa.

p.3406, l.21 Does the Lagrangian analysis account for this significant residual eddy transport? Presumably this can be done by accounting for the bolus velocity inherent in the eddy parametrization. Has this been considered?

p.3407, l.1 What is the residence time of upwelled water at the surface? If it is not long compared to the equilibration time scale of CO<sub>2</sub>, then there is no reason to expect that upwelling and outgassing should be correlated.

p.3407, l.4. If I understand correctly, Fig.4 compares simulated air-sea exchange and observed frontal positions. What does this imply? Why not use simulated frontal positions? Are they similar to observed ones?

p.3407, l.7 "flux patterns" instead of "fluxes"

p.3407, l.16ff. This is difficult to understand. Why use winter surface density distributions in the analysis of the seasonal cycle?

p.3408, l.26, I could not find the 27.2 isopycnal in Fig.8, only 26.0 and 27.8 are shown.

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p.3409, l.7 What is meant by "resembles very closely"?

p.3410, l.10ff. How is this done? Referring to the Methods section did not help me. As I understand, the method is a linearization about some mean, so the results will depend on which mean is chosen. Is this some annual mean map, or an annual mean areal average?

p.3410, l.18. Why should the impact of alkalinity be significant only in some regions?

p.3411, l.28/19. What is meant by "in both three direction"?

p.3411, l.24. Is this about the amplitude of the seasonal cycle in the CO<sub>2</sub> flux or about the annual mean flux?

p.3413, l.18 The previous chapter discussed seasonal anomalies of the CO<sub>2</sub> flux, not the surface flux of CO<sub>2</sub> itself.

p.3414, l.19, distinguish between advective and total transport.

p.3415, eq.7,8. are the two phi the same?

p.3415, l.17ff. Where is the steady state assumption required here? How does this apply to a seasonally cycling ocean?

Fig.2 arrow air-sea flux should be from atmosphere into the ocean

Fig.4 Units?

Fig.5 pre-industrial DIC?

Fig.6 Pre-industrial DIC?

Fig.7 Units? something like "per density class" is missing

Fig.8 How were the frontal positions determined/derived?

Fig.9 what is meant by amplitude per season? Is this the amplitude of the annual cycle times the phase? This does not seem to be consistent with the fluxes shown in

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Fig.7 where, for example, winter and summer anomalies tend to have opposite signs throughout.

Fig.15 what is the meaning of red and blue lines? I do not understand the difference between panels b and c.

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