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Interactive comment on "Air–sea CO_2 fluxes and the controls on ocean surface pCO$_2$ variability in coastal and open-ocean southwestern Atlantic Ocean: a modeling study" *by* R. Arruda et al.

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Point-by-Point reply to referee 2

24 August 2015

We would like to thank the referee for the careful review of our manuscript. Your comments were taken into careful consideration. Here, a point-by-point reply together with the latest changes on the manuscript is presented. We hope that the changes adequately address the reviewer's comments and that the new version is suitable for publication in Biogeosciences.

- **Comment 1** – The title (And abstract) should contain the term seasonal – as longer-term variability is not assessed here, nor could it be due to the experimental design.

- **Reply to comment 1** – Changed title to "Air-sea CO_2 fluxes and the controls on ocean surface pCO$_2$ seasonal variability in the coastal and open-ocean southwestern Atlantic Ocean: A modeling study"

- **Comment 2** – P7372: line 11 acting -> acts

- **Reply to comment 2** – corrected “acting” to “acts”.

- **Comment 3** – P7372, line 27: the reference to Takahashi et al (2002) seems to be referencing to coastal ocean, while this ref refers to the open-ocean. This should be rephrased.

- **Reply to comment 3** – Rephrased the reference of Takahashi et al (2002) concerning open-ocean estimates. Changed text to: "In the open-ocean, the South Atlantic is thought to absorb between 0.3-0.6 PgC/year south of $30°$S, while acting as a source to the atmosphere north of $30°$S (Takahashi et al.,2002)"

- **Comment 4** – P 7375, line 1-10: this seems better placed in the discussion.

- **Reply to comment 4** – As our discussion section is divided into spatial and temporal analysis, we found it more fitting to move this paragraph (tides and rivers importance) to the conclusions section.

- **Comment 5** – P7379, line 1-19: this evaluation is a bit unclear, the authors need to add a bit more information to the reader about what this means and what are the thresholds. Also please be consistent "reasonable/good" or "good/reasonable".

- **Reply to comment 5** – Added equations and a more detailed explanation of the statistical indicators (ME, CF and PB).
  Texts added: L225-236:
  "model efficiency $ME = 1 - (\Sigma(O - M)^2)/(\Sigma(O - \bar{O})^2)$ (Nash and Sutcliffe, 1970), cost function $CF = (\Sigma \mid M - O \mid)/(n\sigma_o)$ (Ospar et al., 1998) and percentage of bias $PB = \mid (\Sigma(O - M).100)/\Sigma O \mid$ (Allen et al., 2007), where M stands for modeled $p\mathrm{CO_2}$ and O for observations from SOCAT database, n is the number of observations and σ_o is the standard deviation of all observations."
  "ME relates model error with observational variability, CF is the ratio of mean absolute error to standard deviation of observations, and PB is the bias normalized by the observations (Dabrowski et al., 2014; Stow et al., 2009). Basically if $ME > 0.5$, $CF < 1$ and $PB < 20$, indicate that the model is “good/reasonable ”when comparing to observations. If $ME < 0.2$, $CF > 3$ and $PB > 40$ the model is classified as “poor/bad ”."
  Also added Taylor diagram as an additional model performance analysis. Now Consistently using “good/reasonable” and “poor/bad” to describe the model performance.

- **Comment 6** – P73780 1-3: remove satisfyingly.

- **Reply to comment 6** – removed “satisfyingly”.

- **Comment 7** – P7381, line 16: what is metabolic DIC? Clarify.

- **Reply to comment 7** – Metabolic DIC is the DIC formed by the respiration of CO2.

- **Comment 8** – P 7384, line 5 and after: I think it is actually Sea-air fluxes, not air-sea fluxes, please check this.

- **Reply to comment 8** – Both terms “sea-air fluxes” and “air-sea fluxes” can be used. We are using “air-sea fluxes” throughout the text now.

- **Comment 9** – P7385, Section 4.4 This model only deals with seasonal variability, therefore any statements about this site, and what is simulated needs to be tempered with a caveat. Also a reference to Fig 1 is needed in this section.

- **Reply to comment 9** – We agree that our statements and conclusions should be tempered with caveats, since we are working with a climatological analysis. With regards to the OOI site, a more realistic year-specific forcing and boundary conditions would be needed for a appropriate evaluation of the model performance. Here we only discuss the expected climatological behaviour at the OOI site, and use it to relate mixed layer depth with pCO2 and DIC input to the surface ocean. Added a reference to fig 1 a .

- **Comment 10** – Conclusion: in the methods a number or limitations of the modelling approach are highlighted, could the authors please make a comment on how these may modulate the results e.g. riverine input, large phytoplankton etc? Perhaps the implications of only addressing the seasonal variability also need to be considered (particularly as part of the paper deals with the Argentina OOI site – see above).

- **Reply to comment 10** – Riverine input and tides are locally important in the inner shelf of Patagonia and in the La Plata region. Therefore these limitations will affect our results only in these areas, while we do not expect a major effect in the overall larger scale study area.
  The fact that we are using a model with only one phytoplankton type is another limitation in this study. Since we are working with a large area, with various biogeochemical characteristics, a single parametrization will inevitably fail in some areas. Nevertheless, experiments were made (not shown) with different biogeochemical parameters more representative of small phytoplankton instead of large phytoplankton (as is the case of the present study, based on Gruber et al 2006). Only minor differences in the ocean surface pCO2 were found, therefore, we decided to keep the parameters as in the previous study. We anticipate that we will use more complex, multi-species biogeochemical models in future studies.
  The fixed atmospheric CO2 concentration of 370 uatm also limits a direct comparison between the model results and the sparse data from SOCAT in the region. These limitations do not affect our discussion of the drivers and processes responsible for ocean surface pCO2 variability in our seasonal analysis.
  We agree with the referee that there are implications of only addressing the seasonal variability, but more observations are needed for more detailed model evaluations.

- **Comment 11** – Table 1: If this table is from Gruber (2006), is it needed here?

- **Reply to comment 11** – Removed Table 1 – Only left in citation.

- **Comment 12** – Figure 2-3: consider a diff plot.

- **Reply to comment 12** – A diff plot would not be helpful because we are mostly concerned with the overall behaviour of the ocean surface (In equilibrium, source or sink), rather than with the absolute values. Furthermore, the only few data available are sparse throughout the years, and most observations are available in only one year, making it challenging to compare with the model climatology.

- **Comment 13** – Figure 4: EKE is shown, but not really used in the text – is this the correct figure to show, given that the analysis does not explicitly deal with eddies?

- **Reply to comment 13** – We show the figure with the EKE map to demonstrate that the areas A2 and A3 are near a maximum of EKE, and therefore present larger variability, which will possibly lead to a decrease in correlation coefficients.

- **Comment 14** – Figure 12: I don't follow the figure caption, could it be clarified?

- **Reply to comment 14** – Rephrased caption on Fig 12 (Now fig 13) to "Vertical profile at 42°S, 42°W. Upper panels showing monthly mean surface pCO$_2$ (solid black line), pCO$_2$ anomalies (dashed black line) and the contribution from T and DIC^s (red and blue dashed lines) and the contribution of biology and solubility (green and cyan dashed lines). Lower panels showing vertical profiles of DIC (a), T (b), and chlorophyll a(c), black line represents the mixed layer depth".