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Interactive comment on "Spatiotemporal variability and drivers of pCO$_2$ and air–sea CO$_2$ fluxes in the California Current System: an eddy-resolving modeling study" *by* G. Turi et al.

Anonymous Referee #1

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1 General comments

I have enjoyed reading this paper though it took me some time given the length. It is undoubtedly well-written and comprehensive, a very good example of using a numerical model to investigate the still controversial issue about the role of coastal systems in the global carbon balance. This manuscript investigates the processes at play in the California Current System building on previous works that have first described the hydrodynamics and the bulk biogeochemical dynamics. It is therefore a robust approach which may serve as example for other systems. This is why I would suggest the authors to make an additional effort and elaborate more on some of their findings while at

the same time shortening some parts that cannot thoroughly be investigated with their specific setup.

The paper should be accepted with minor revisions. I would like to point out that this is a personal view point as a peer scientist working in the same field and therefore I'll understand if the authors or editors have arguments against the suggested rearrangement.

- The feeling I'm left with at the end of the paper is that, despite the accurate analysis, this work does not add much to the carbon balance in the CalCS. The explicit aims of the work were to quantify the mean CO_2 fluxes of the system and to assess the spatio-temporal variability of the driving processes, separating the contributions of solubility dynamics, air-sea exchange, biological through-flow and physical transport. I think the authors are doing a good job and should streamline a bit more the conclusion that coastal regions are likely to be much more compensated in terms of carbon fluxes than conventionally thought (in the limit of the Redfield assumptions, see my specific comment below).
- If the authors have arguments to show (and I think they do) that the current observational network is inadequate to carry on estimates of carbon fluxes, I think they should state this clearly. The discrepancy with the CalCoFI line presented in Fig. 4 is rather remarkable and should be discussed more.
- The mesoscale analysis presented in Sec. 4.6 appears marginal and not as focused as the other sections. I would suggest the authors to reconsider the inclusion of this part or to make it more functional to the aim of the manuscript. As suggested by the authors in the conclusions, the study of mesoscale should be done in combination with other variables and to understand their spatial correlation.
- The caveats of the sensitivity experiments for process understanding should be

carefully outlined before being applied (see for instance the notes of caution given by Lovenduski et al in their 2007 paper). This issue is not only related to the numerics of the flux reconstruction, but also to the design of the experiments. Biology is responsible for the vertical gradient in DIC and therefore once biology is removed, it is obvious that circulation acts to restore the gradient found in the initial conditions leading to a surface ocean pCO_2 that is temporarily higher than the atmospheric value. In the longer term, without the mediating role of biological uptake, it is to be expected that DIC would equilibrate. It is trivial to consider that if the simulation would start from an homogeneous value of DIC no such effect would be seen. Disentangling the specific magnitude of each process by successive removal of the terms may lead to misleading considerations. It is an exploratory experiment but only by storing and analyzing the single terms of the dynamical equation we may hope to fully understand their dominance.

2 Detailed comments

abstract The abstract is too long. It should be more focused on the major methodological aspects and findings. As it stands, it looks more like an extended abstract of a thesis work.

P14049_L11 The NPZD model in ROMS is not an ecosystem model. It is a biomass-based biogeochemical model where plankton functional groups are treated as clouds of unicellular organisms (even in the case of metazoans) represented by their nitrogen content. It is just a portion of the ecosystem.

P14049_L21 Given the importance of the biological loop in controlling the carbon fluxes, I think the authors should consider in their discussion the limitation of using fixed stoichiometry in biogeochemical plankton dynamics (e.g. Thomas et al., 1999; Flynn, 2010). Especially in coastal systems, the decoupling of carbon

and nutrient utilization may lead to a much larger carbon uptake than the one derived just by nitrogen drawdown, which is the relationship used in this model.

P14051_L4 I have gone through the whole manuscript to find a reference on the type of forcing functions. Since I cannot believe that authors can produce any mesoscale dynamics with mean monthly forcings, I presume that the climatology has a higher temporal frequency. This is indeed described in previous works with the same model, but it should be written here as well as the period over which the climatology was derived.

P14052_L23 It is not clear how the perturbation was done. Was it done on the model domain (that is, with the whole model starting from a perturbed state) or using just the carbonate equilibrium dynamics and taking the numerical derivatives?

P14053_L10 Please consider the following questions and include relevant information in the text: 1) are the major features well represented by the degradation in resolution? 2) How long did you run these experiments? 3) Starting from the same initial conditions?

P14054_L1-5 This explanation should be expanded because it is crucial for understanding a large part of the manuscript. These kind of experiments are always intriguing because separating transport terms from with sequential exclusion necessarily modifies the concentration. Biology creates gradients and the circulation tends to restore them, therefore the order of permutations should count. Indeed, since the two major terms are biology and transport (and transport cannot be removed!), it is understandable that it makes not much of a difference. Also, permutated sequence means that, for instance, you also tested experiment S2 composed of no biology and constant solubility?

P14054_L12 If you run the experiment till adjustment than I guess that mortality terms consume all initial biomass within the first biomass.

P14054_L17 Usually salinity has no unit, but maybe the journal accepts this.

P14056_L5 I think it should be mentioned the large overestimation in the northern coastal region, particularly in spring, where the data show a clear low pCO2 while the model does not. This should be introduced in view of the analysis done in the next section on the process assessment. It is interesting that the Taylor diagram reports a weak overestimation in this season where it does not look like in the map. Is this related to the underestimation in primary production reported by Gruber et al. (2011).

P14059_L27- These comments are probably more pertinent to the final discussion. See my general comment above.

Sec4.2 This section seems like a repetition of the one before. It essentially describes Fig. 5 that has been previously discussed. What is the added value? By moving the paragraph from line 5 to 9 at page 14061 to the previous section and paragraph from 10 to 14 to the next one the paper would be streamlined and easier to read.

P14062_L4-6 See my final general comment above. Also, add a reference to Table 1 after the sentence "This process-based separation...".

P14062_L17 Please add "(not shown)" when describing alkalinity as it is not in the figure.

P14063_L1-8 The biological loop described by the authors has to be necessarily linked to the decoupling between nutrients (N in this case) and carbon uptake

P14064_L6-8 Why not using the standard deviation in the figure plots as well? It would be easier to understand the magnitudes of the processes. A possible alternative would be to use the coefficient of variation that gives and idea of the relationship with the mean.

P14064_L13 Fig 8c is in percentage while the others are absolute values. Please make this clear in the text as well.

P14065_L20-23 This sentence seems to imply that upwelling decreases the pCO_2 value during wintertime, which is not physically possible, and it is just an apparent effect of removing the annual mean from each experiment (dominated in this case by a large summertime upwelling). This is why I believe this kind of analysis should be explained with more details as the means from each process-driven experiment are sensibly different, and comparing the relative results may not be completely correct.

P14066_L6 Please specify the meaning of "somewhat different"

Section4.6 This is the weakest part of the manuscript. I would suggest the author to reconsider this section and maybe include it in a future work where the mesoscale aspects are more central. I thought the authors used a perpetual year simulation and therefore it is important that they explain what do they mean with non-seasonal component. There may be some mesoscale variability that is seasonal. The methodology described in the figure caption is not clear, and I do not understand why the authors need a smoothing of the anomalies. Also, Fig. 8c is expressed in percentage while the analysis in Fig, 11 is given with anomalies and therefore it is difficult to appreciate the magnitude of the variability associated to the mesoscale in the spatial domain.

P14068_L1-3 This remark is exactly my last point in the general comments above. I think the authors should make clear from the beginning that their exercise of sequential removal of processes is only an approximate method to estimates the magnitude of each term in the dynamical equation.

P14068_L19 I guess the authors mean "loop" and not "pump". The biological pump must be (partly) increasing as well because of enhanced nutrient availability, but the DIC upwelling is dominant.

P14071_L8-10 I don't understand this sentence. Anthropogenic emissions are independent of atmospheric CO_2 concentration (unless the authors refer to mitigation policies based on threshold-control emission reductions, but it would be a bit out of context here).

Fig.3 Taylor diagrams use the Pearson correlation because they require a correlation defined in terms of variance for the geometric relationship to hold. The Spearman correlation is non-parametric and based on rank correlation. I guess this is just a typo and the Pearson correlation was used.

Flynn, K. J. (2010), Ecological modelling in a sea of variable stoichiometry: Dysfunctionality and the legacy of redfield and monod, *Prog. Oceanogr.*, *84*(1–2), 52–65, 10.1016/j.pocean.2009.09.006.

Thomas, H., V. Ittekkot, C. Osterroht, and B. Schneider (1999), Preferential recycling of nutrients - the ocean's way to increase new production and to pass nutrient limitation?, *Limnol. Oceanogr.*, *44*(8), 1999–2004.

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