Biogeosciences Discuss., 10, C6566–C6570, 2013
www.biogeosciences-discuss.net/10/C6566/2013/

Biogeosciences
Discussions
Open Access

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 #2

Received and published: 16 November 2013

General comments: On the whole, Turi et al. present a very well-written and constructed article that will make a good contribution to the literature on the contribution of the California Current System (CalCS) to the global air-sea exchange of CO2. They use a coupled physical-biogeochemical model to determine the net air-sea exchange of CO2 across the model domain region and perform a strong analysis of the various drivers and processes controlling this important carbon cycle flux. For me, the discussion of the contributions of the drivers and underlying processes to the spatial and temporal patterns observed in their model air-sea CO2 fluxes was the highlight of the paper. Turi et al. also did a good job of putting their results into the context of prior estimates, although it was not in all cases clear to what extent the results of other stud-

ies overlapped with their study domain, and to what extent any observed differences reflected these differences in the boundaries of the study regions (clarifying this could strengthen the table – perhaps a table?).

An overall comment I will make about the paper is that, while I agree with their suggestions that it would be very difficult to constrain the air-sea flux of the CalCS through observations alone, they were a bit slim on making (any) concrete suggestions of what kinds of observations would be particularly useful in providing validation or boundary condition data sets for use in their model. They noted a case where a coastal cruise data set provided the information that they needed to identify and correct a bias in the poorly-sampled NE Pacific part of the GLODAP database that provided boundary conditions for their model. It would be great to have an idea of what they most need to validate model performance within the model domain on the temporal and spatial scales addressed in this study, as well as the follow-on studies they pointed to that would allow them to address both inter-annual and short-term variability in air-sea CO2 flux in the CalCS. I wouldn't expect a detailed discussion of this, but some brief thoughts on types of observations most needed, such as spatial cruise or underway data? Moored time series? And especially where these assets are most needed from their perspective to help constrain/improve/validate models would be helpful information?

Overall, I thought that the text, tables, and figures were very clear and appropriate. There are a few places where clarity could be improved, through specific comments I'll outline below. One thing that jumped out at me, and this may be more a comment for the editorial staff than the authors (or an issue with how these online manuscripts print), I found the scaling of the figures to be much too small. It seems like they have the resolution to be sharp when displayed larger (at least on my computer screen), but in the "print-friendly" PDF, the text and panels were so small as to make it nearly impossible to discern the figure labels as well as patterns in the figures. So please make sure the figures are scaled appropriately for printing in the final version, for those readers who may ultimately print the paper. Many of the figures are very information-

rich, and even at relatively large size, it can be hard to visually compare across panels (especially thinking of Fig. 2 here, where a difference plot would be helpful but not really reasonable, given the different distribution of observations and model results).

Specific comments: 1) Abstract lines 7-9 – this sentence is a bit awkward. I can see why it makes sense to a person after they know the story, but as written, the part after the comma seems to contradict the "virtually no bias" part. A few words could be added to clarify that these are different spatial scales. Or even just make it two sentences, so it will be easier to follow.

2) Page 14046, line 19 – the Nagai et al reference is in prep – is that allowed with Biogeosciences?

3) Pg 14047 – This section and the following paragraph or two might benefit from a table to clarify what results have been published previously, including the relevant spatial domains. Table 2 kind of gets at this, but is more detailed than I was thinking it would need to be to incorporate all studies mentioned.

4) Pg 14047 – Also, I would note that perhaps the most relevant prior study to cite for their "Far-offshore" region may be Takahashi et al. 2009 DSRII paper with the most recent global pCO2 climatology, as I believe this part of the CalCS overlaps with that open-ocean domain, though likely with a ragged boundary. Still may be worth adding to the literature context section.

5) Pg 14055, lines 25-26 – can you give a percent of coverage of observations relative to model output (sorry if I am forgetting and this was noted elsewhere)?

6) Pg. 14057, line 25 – compared to data or estimates from Hales?

7) Section 4.1 – Turi et al's mean annual flux estimate including the estimated error of +/- 3.6 Tg C/yr does not include the mean estimate from the other most comprehensive study of air-sea exchange in the CalCS (Hales et al 2012, which estimates 14 +/- $\sim$14 Tg C/yr). Do the authors have thoughts on why? I guess Turi et al are including their

400-800 km domain in their estimate, which would bring down the mean flux, but it also looks like there are considerable differences between Hales' and Turi's estimates in the two more coastal domains (in Table 2), and I can't quite see why from the table or text.

8) Pg 14062, ln 6 – Perhaps "process-based separation based on the sensitivity study" would be clearer.

9) Pg 14068, first paragraph – Fassbender et al Continental Shelf Research 2011 may be of interest to the authors, with an observational study related to this discussion from the Feely et al 2007 cruise.

10) Acknowledgements – I think you can cite the DOI for Feely's 2007 cruise. It's available on the CDIAC web site at: http://cdiac.ornl.gov/ftp/oceans/NACP_West_Coast_Cruise_2007/

11) Fig 2 – perhaps you could just put "Model" above the left column of panels and "Observations" over the right column, so that you could increase font size on the months in the individual panels and make them look a bit cleaner.

12) Fig. 3 – Taylor diagrams – for those who don't look at them every day – are very information-dense and not particularly intuitive. Can you perhaps add a sentence or two of explanation to illuminate non-experts on what these diagrams generally do? I think it is super useful, but the text lacks something like this and the caption is very detailed – a brief, clear description of what this tells the reader would be much appreciated.

13) Fig. 8 – I don't understand what "2-day output" means.

14) Table 2 – The wording in the table caption is confusing – delta pCO2 column could just be pCO2, since you are just subtracting a constant from it to get the air-sea gradient. Also, I am not clear on whether the Hales et al "bias" is from delta pCO2 or just pCO2(of seawater). On a related note, perhaps you could call this column "Difference from Hales et al. pCO2" since "bias" implies Hales et al is wrong and your results are

right (which is possible but not known. . .).

Overall – a really good paper. I enjoyed reading it and look forward to seeing the final version. Well done!

Interactive comment on Biogeosciences Discuss., 10, 14043, 2013.