Response to Referee #2 comments

The authors are grateful to anonymous Referee #2 for her/his very helpful comments and suggestions, which have added great value to the manuscript and have helped improve it significantly. A detailed point-by-point reply to the general and specific comments follows below. In the following, referee comments are ***slanted and bold***, while author comments are highlighted in blue.

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 CO_2. They use a coupled physical-biogeochemical model to determine the net air–sea exchange of CO_2 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 CO_2 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 studies 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?).

We have included a table (Table 1 of the revised manuscript) summarizing 11 different studies – including ours – which have contributed to the quantification of the air–sea CO_2 fluxes in the Northeast Pacific region and in particular in the CalCS. We refer to all of these studies in our introduction.

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 CO_2 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?

We have included in Section 6.2 suggestions on how the current observational network could potentially be strengthened:
"Without a full Observing System Simulation Experiment (OSSE), we are not in the position to make accurate recommendations with regard to how the current network would have to be expanded to capture the mean flux and its variability with good confidence. Nevertheless, we can make some qualitative, general statements, based on our model-based experience. First, the presently available observations are likely sufficient to estimate the domain-wide climatological annual mean air–sea CO_2 flux, as indicated by the relatively good agreement between the most recent estimates. Second, the current network is with good confidence insufficient to determine variability in time and space around this mean flux. In order to achieve this, the network would mainly need to be expanded in the first 100 km, where the short temporal and spatial decorrelation length scales require a denser coverage of pCO_2 and air–sea CO_2 flux measurements. It would furthermore be highly desirable to have a more complete latitudinal coverage of the nearshore area of the entire US West Coast, whose current observational coverage is at best fragmentary. To this end, alongshore underway cruises, rather than moored stations, may provide the most adequate means of measuring pCO_2 within this extended area of interest."
We would like to emphasize that more quantitative and established suggestions from our side would require a substantially more detailed study of the temporal and spatial decorrelation lengths of pCO_2 and the associated air–sea CO_2 fluxes. To this end, we would like to refer to a future study.

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).

This comment has been addressed with the editor. For the final manuscript version, we will make sure that the figures follow the instructions to the authors[1]. We have also enlarged Figs. 1, 2, 8 and 11 for better resolution.

[1]http://www.biogeosciences.net/submission/manuscript_preparation.html

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.***

   This sentence has been removed and the abstract has generally been shortened to focus more on the major methodological aspects and findings.

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

   This question has been addressed directly with the editor. According to the instructions to the authors, it is accepted by BGD to cite manuscripts in preparation. A quote from BGD's web site states: "Papers should make proper and sufficient reference to the relevant formal literature. Informal or so-called "grey" literature may only be referred to if there is no alternative from the formal literature. Works cited in a manuscript should be accepted for publication or published already. These references have to be listed alphabetically at the end of the manuscript under the first author's name. Works "submitted to", "in preparation", "in review", or only available as preprint should also be included in the reference list."
   Based on these instructions, we have decided to keep the reference.

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.***

   Done. Table 1 of the revised manuscript summarizes 11 different studies – including ours – which have contributed to the quantification of the air–sea CO_2 fluxes in the Northeast Pacific region and in particular in the CalCS. We refer to all of these studies in our introduction.

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 pCO_2 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.

Based on the Takahashi et al. (2009) global climatology of air–sea CO_2 fluxes we have included an estimate for the CalCS in our introduction and in Table 1 of the revised manuscript. The Takahashi et al. (2009) climatology is available at a resolution of $4° \times 5°$. So as to be comparable to our analysis domain, we chose an area between 30–46°N and 120–135°W to average over (includes 9 grid boxes). In terms of spatial extent, this region is most comparable to our far-offshore domain, as the more nearshore areas are not taken into account in the Takahashi et al. (2009) climatology. From this climatology, we computed a mean air–sea CO_2 uptake flux of about –0.8 mol C m^{-2} yr^{-1} for these 9 grid boxes. Based on our model simulations, we estimate a nearly neutral CO_2 flux of 0.05 mol C m^{-2} yr^{-1} for the far-offshore domain, which is substantially different from the estimate based on Takahashi et al. (2009). This discrepancy is likely due to the fact that we include upwelling in our model, which could lead to a higher overall pCO_2 value and hence a tendency towards more neutral or slightly outgassing fluxes in the far-offshore region.

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)?***

Thank you for pointing this out: we realized that our number of 2 021 binned and averaged pCO_2 observations was incorrectly calculated.
The total number of 5km × 5km grid boxes in our analysis domain is 55 257. In the annual mean, and after binning and regridding all the available observations to match our ROMS grid, 52 668 of these grid boxes have observations in them. However, if we apply our elimination criteria, this leaves us with a total of 38 477 grid boxes in the annual mean (reduction by about 27%). Similarly, for our seasonal analysis, the criteria that we apply is that we use only grid boxes with at least two observations taken from two different months within a season.
The captions of Figs. 2 and 3 have been adapted accordingly and the text has been modified to read:
"For this analysis, we used only those data that fulfilled the following criteria: (i) for the annual mean analysis, only grid boxes containing at least two observations from opposite seasons were considered (i.e., DJF/JJA or MAM/SON) and (ii) for the seasonal analysis, only grid boxes with two observations taken in two different months within a season were retained. This reduced the number of grid boxes considerably, particularly in the nearshore region in winter and spring and offshore of 100 km. For the annual mean analysis, the number of available grid boxes is reduced by about 27% to a total of 38 477 grid boxes with averaged pCO_2 observations in them."

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

   The differences to Hales et al. (2012) pCO_2 which we computed for our model evaluation in Section 3 and which are noted in Table 3 of the revised manuscript, are based on data that we received directly from B. Hales and which were pre-gridded on a $0.25° \times 0.25°$ grid. We then regridded his pCO_2 data to match our CalCS 5km setup and computed the difference to our model output pCO_2. As this procedure was not evident from the text, we have modified this paragraph to read:
   "To further check the model's performance, we compared our modeled surface ocean pCO_2 to pCO_2 data predicted by the neural network model of Hales et al. (2012) (Table 3). As this data was pre-gridded at $0.25° \times 0.25°$, we regridded it to match our ROMS grid."
   The caption of Table 3 was modified accordingly.

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 ± 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.***

   The calculation of our air–sea CO_2 flux error estimate of ± 3.6 Tg C yr^{-1} does not take into account any other previously published estimates or observational data. Rather it is based solely on additional sensitivity studies which we performed to determine the sensitivity of pCO_2 and the air–sea CO_2 flux to changes in the DIC boundary conditions, in the $CaCO_3$ export ratio and in two biological parameters (phytoplankton mortality and phytoplankton light sensitivity). We found that altering the DIC boundary conditions by ± 10 mmol C m^{-3} (which corresponds to the bias we found compared to DIC from the Feely et al. (2008) cruise, and which lies within the DIC uncertainty provided by GLODAP) had the most important influence, resulting in a domain-wide air–sea CO_2 flux change of about ± 0.20 mol C m^{-2} yr^{-1} or ± 3.6 Tg C yr^{-1}.
   The error estimate from Hales et al. (2012) of ± 14 Tg C yr^{-1} is based on the areally-weighted RMS deviation between predicted and observed pCO_2 and the areally-averaged air–sea pCO_2 difference, and is hence not directly comparable to our error estimate.
   We believe that the explanation of this pCO_2 difference to Hales et al. (2012) – which is most pronounced in the first 100 km – is the same as for the pCO_2 bias which we found compared to the SOCAT/LDEO/MBARI data, i.e.: this bias is mainly due to our spatially and temporally coarse wind forcing, which would lead to wind speeds being overestimated in the nearshore regions, resulting in stronger and more continuous upwelling and too high pCO_2 values (Capet et al. 2004).
   This is explained as well in the last paragraph of Section 3 of the revised manuscript.

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

   Done. A reference to Table 2 of the revised manuscript has been added as well. This sentence now reads:
   "This process-based separation based on the sensitivity studies (Table 2) reveals that the most important contributions to the spatial gradients of annual mean pCO_2 are circulation and biological production (Fig. 7a and b), both of which act upon DIC and Alk."

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.***

   Done. The sentence at the end of this paragraph has been modified to read:
   "However, as these waters "age" while they are being transported further offshore, the biological pump operates so efficiently that all nitrate is fully utilized, creating the conditions for some of the escaped CO_2 to be taken up again by the surface ocean (e.g., Hales et al. 2005; Feely et al. 2008; Pennington et al. 2010; Fassbender et al. 2011)."

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/***

    Done.

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.***

    Done.

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.***

    Done. We have rephrased the text to explain better what the Taylor diagrams de-

pict. It now states:
"A more quantitative assessment of the model's successes and challenges in reproducing the observed pCO_2 is offered by the Taylor diagrams in Fig. 3, which provide a summary of how well the observed and modeled pCO_2 patterns match in terms of their spatial correlation, their root-mean-square difference and the ratio of their standard deviations. Additionally, the diagrams show the difference between modeled and observed pCO_2 as a color-coded bias."
The caption of Fig. 3 has been modified accordingly. Please note that we redid the Taylor diagrams in Fig. 3 because of an error in the calculation of the subdomain areas and because we had mistakenly used the Spearman rather than the Pearson correlation method. The values have been adapted accordingly in the text on model evaluation in Section 3.

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

Thank you for pointing this out, as it was not clear what we were referring to. From our 5km-resolution simulations, we saved the model output at 2 different temporal frequencies:

(a) At monthly frequency: this is the output which we then average over the last 7 of a total of 12 analysis years and which is used for our annual mean and seasonal analyses in Sections 3, 4, 5 and 6.1.

(b) At 2-day frequency: as we were interested in investigating the mesoscale variability, this output was not averaged over the 7 analysis years, but rather the whole span of the 7 analysis years was used for the mesoscale analyses in Section 6.2 (and in Figs. 8 and 11).

We have rewritten this in the first paragraph of Section 2.2 to state:
"The model was started from rest and run for 12 years with monthly climatological forcing. As our model simulations require about 5 years for the spinup, we use model years 6 through 12 for analysis. For our annual mean and seasonal analyses in Sections 3, 4, 5 and 6.1, we used model output at monthly resolution and averaged this to obtain a climatology over 7 years. For the analysis of mesoscale processes in Section 6.2, we used 2-day model output and looked at all analysis years without averaging."

14. ***Table 2 – The wording in the table caption is confusing – delta pCO_2 column could just be pCO_2, 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 pCO_2 or just pCO_2 (of seawater). On a related note, perhaps you could call this column "Difference from Hales et al. pCO_2" since "bias" implies Hales et al is wrong and your results are right (which is possible but not known...).***

For more clarity, Table 3 of the revised manuscript has been changed to show pCO_2

values instead of ΔpCO_2. As our annual mean value for atmospheric pCO_2 is 370μatm, the ΔpCO_2 values could then easily be calculated using pCO_2 from this table. We have also restructured Table 3 so that the third and fourth columns now show the air–sea CO_2 flux values, while columns five and six are our pCO_2 values and the difference to Hales et al. (2012) pCO_2. The phrasing has also been changed to "pCO_2 difference to Hales et al. (2012)".

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

We wish to thank Referee #2 again for her/his helpful comments and suggestions which have greatly improved the clarity and quality of the manuscript.

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