Biogeosciences Discuss., 9, C8676–C8685, 2013
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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Air-sea exchange of CO_2 at a Northern California coastal site along the California Current upwelling system" *by* H. Ikawa et al.

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I prepared 3 figures (Figure C1-C3).

Response to referee 1:

(1) The authors studied air-water exchange of CO2 in a coastal upwelling site by using eddy covariance technique in summer 2007 and fall of 2008, and a SAMI-pCO2 sensor in November 2010 and March to July in 2011. They then discussed the effects of upwelling on CO2 fluxes, and concluded that the coastal area off the Bodega Bay was a source of CO2 to the atmosphere. Overall, the paper is very well written and the discussion seems to be reasonable. Unfortunately, I am struggling to find any major

contributions of this paper to the study of carbon cycle except for the method of using eddy covariance technique to study air-water exchange of carbon dioxide itself. Traditionally, air-water exchange of carbon dioxide is calculated based on the gradients of pCO2 between the atmosphere and the water, and the gas transfer velocity. The problem is that even though pCO2 gradients can be determined reasonably accurately, the gas transfer velocity has to be derived roughly based on wind speeds. The eddy covariance technique, on the other hand, is more of a direct CO2 flux measurement. It calculates vertical turbulent fluxes based on parameters within the atmospheric boundary layers. However, after reading the manuscript, I have a feeling that this technique will bring about large uncertainties. As an oceanographer who knows little about eddy covariance technique, I hope the manuscript will also be reviewed by at least one atmospheric scientist, to make sure the authors get an unbiased review. Reply to (1): Source of CO2 during early upwelling and subsequent sink during relaxation are well-known. However, their balance (ie, annual net CO2 flux) is not well understood. Our paper quantified the balance of sink and source dynamics with the uses of the bulk method and eddy covariance over the coastal upwelling system. Our study also employed eddy covariance for the first time for coastal upwelling zones. Eddy covariance is an only direct method; however, it is well known that the use of eddy covariance to measure ocean flux may introduce some uncertainty with respect to the facts that (1) CO2 flux might be too small to be detected and (2) contamination introduced by sea spray might introduce an erroneous reading of the infrared gas analyzer, which is known as a cross-talk of CO2 and H2O signals. There would be the effect of distortion as well. However, all possible issues that we are aware of were carefully evaluated. The data are still scattered likely owing to the fact that the footprint changes time to time due to the ocean current, however the overall trend should show the average sea state. Also, although the bulk technique with pCO2 has been widely used, the estimate of the gas transfer coefficient over the coastal seas is still questionable, and it is important to approach the measurement with multiple methods rather than a single method.

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Other comments: (2) SAMI-pCO2 sensors have some known issues and suffer from biological fouling. Please provide more details of the calibration and its uncertainties. Reply to (2): The SAMI-pCO2 sensor was cross-calibrated with another pCO2 system (headspace equilibrator with LI-840, LI-Cor) (Ikawa and Oechel, 2012). Here, I attach the graph (Figure. C1) (not published). For this calibration, in fact, seawater collected near the study site (nearshore water off Bodega Bay) was used. The measurement in this study was often out of the calibration range as stated in the manuscript. However, the measured high pCO2 was possible as stated in the manuscript, although we are not able to justify the accuracy of the sensor at those high pCO2.

(3) Derivation of DIC based on pCO2 that was calculated from SST, salinity (ignoring the biological processes), and pH that was measured in another area (Central California coast) will bring about tremendous uncertainties. Ideally, the two parameters that are used to calculate DIC need to be measured accurately, and from the same body of water.

Reply to (3): As the referee pointed out, DIC needs to be estimated based on SST and pH measured for the same water. In this manuscript, pH from the exact study area was not available and pH data estimated for a large area of central California was used. However, the effect of the variation in pH is minimal. If the amplitude of the seasonal variation in pH differ by 50 %, then the amount of DIC per unit area within the mixed layer changes by 1- 10 % (see Figure. C2). Upon estimating flux, NPP and DIC over the past few decades with the limited data set, we do not expect the estimate to be very accurate. However, we believe that the estimate is able to suggest that it is likely that the amount of CO2 transferred by air-sea CO2 flux is not negligible compared to the DIC readily available within the mixed-layer.

(4) West coast of the United States features strong CO2 sinks. The conclusion that the upwelling site is a CO2 source is not necessarily wrong, but extrapolation of data from one longitude latitude point to a large area needs to be done very carefully.

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Interactive Discussion

Discussion Paper

Reply to (4): Our manuscript cites Wilkerson et al (2006)'s study that shows the water chemistry was similar more or less along the cross-shelf distance of 50 km near our study site. We have not evaluated a representativeness of the study site over the western coast of the United States yet. Based on the past studies, I believe that our study area can represent coastal upwelling zones off the north-central California (Friederich et al. 2002, Wilkerson et al., 2006), although, coastal seas off southern California (data not published) and near Oregon (eg, Evans et al., 2011) are sinks of CO2.

(5) The conclusion that the ocean is learning more towards a source of carbon dioxide during early upwelling period than during upwelling relaxation period is nothing new. The early upwelling period is mainly a physical warming process, where the cold high CO2 deep water is warmed up in the surface, raising pCO2. The relaxation period is mainly a biological uptake process, where the high nutrient upwelled water, combined with light in the stratified surface layers, generates strong biological activities, lowering down pCO2. (6) Reply to (5): It is well known that early upwelling is a source and relaxation is more likely a sink. However, less is known about the balance between the twos.

Response to referee 2: (1) In this paper the authors estimate air-sea exchange of CO2 in an upwelling zone off the coast of California. They conclude that this coastal area represents a strong source of CO2 during upwelling events and a moderate source during relaxation of upwelling. They also report that sea surface temperature and salinity are good predictors of CO2 flux in this area. Clarifying the role of the coastal ocean in the uptake or release of atmospheric CO2 is of fundamental importance to understanding and predicting current and future states of the global carbon cycle. This study aims to increase that understanding.

Reply to (1): Thank you, we believe that the study will increase understanding and predicting current and future states of the global carbon cycle by clarifying the role of the coastal upwelling zone.

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(2) General comments I think that these results might be quite useful to a broad community, but as written the paper does not make the application clear. The English grammar is fine, but the paper is hard to follow, because the structure is confusing. In particular, the authors state in the abstract that they have found a strong relationship between the temperature and salinity of surface water and the CO2 flux. Such a relationship could be compared with results obtained by researchers in other locations, possibly leading to some sort of general empirical prediction of CO2 flux from more easily measured values. Although this is one of the main conclusions of the paper, I think that I have found it in the Method section (equations 9 and 10). In addition, it would be useful if the authors discussed why their results indicate that this upwelling zone is a source of CO2, while previous studies have found that it is a net sink. I am not familiar with the eddy covariance technique myself; it would be useful to have someone else review it who is. The work reported in this paper appears useful and could be novel. The authors mainly need to strengthen the Discussion and Summary to clarify the novelty and significance of their research and to put this study into a broader context.

Reply to (2): The most important point of our study is that the area was determined to be a source of CO2 based on the relation of CO2 flux/pCO2, salinity and SST, although it is not well understood whether upwelling zone is a sink or source of CO2. As pointed out by the referee, I admit that the importance of the study is not well emphasized in the current manuscript, particularly in abstract. I would like to revise the structure to emphasize the novelty of our study.

Detailed comments (3) There are too many abbreviations and symbols sprinkled throughout the paper which require that the reader search back a couple of pages for a definition. A table of symbols would help, but it would be better if the authors could simply reduce the number of abbreviations by writing out more terms in full.

Reply to (3): I mistakenly used symbols in the text (eg, line 271), and I need to write them out. I will also avoid AWS and write alongshore wind instead.

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(4) Figure 2 requires more explanation.

Reply to (4): As the referee pointed out, “Cospectra of CO2 and temperature with the vertical wind speed were evaluated to assess data quality (Fig. 2).”, and following sentences may not give enough explanation about Fig. 2. Spectral analysis was used to evaluate whether the eddy covariance sensors can correctly resolve high frequency, and examine whether there was any noise in the high frequency data that increases error in flux outputs. Ideally, the logarithmic cospectra of scalar and vertical wind speed show a peak near the frequency where the most of turbulence occurs and small at low and high frequencies. The attenuation of cospectra from the peak toward high frequency follows a -4/3 slope against normalized frequency under stationary conditions (Kaimal et al., 1972). If cospectra do not attenuate at high frequency, then it is possible that noises in the sensors caused an error in the flux calculation. If the attenuation is much more than -4/3, then it is possible that the sensors did not resolve at high frequency. Because the cospectra shown in Figure 2 shows that the attenuation at high frequency follows -4/3 more or less, we concluded that the sensors operated correctly.

(5) Figure 3 and 4. Abbreviations are OK on the plot, but avoid them in the caption, except for the universal abbreviations, such as S for salinity and T for temperature.

Reply to (5): I will do so.

(6) Figure 7. Plot the regressions used to determine the predictive relationships among S, T and CO2 flux.

Reply to (6): I superimposed estimated pCO2 with the regressions on Figure 7 (Figure. C3).

(7) Figure 7. Salinity is dimensionless; avoid “psu” as a unit.

Reply to (7): I will change so.

(8) Figure 7. Discuss the two clusters of data that appear above and below S$\sim$33.

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Reply to (8): It seems S lower than 33 was often observed between December 2010 - March 2011, and the data during this period seems to compose the cluster below S33 (Figure 6). Alongshore wind tends to be negative during this period. This period with southerly wind ("reversal winds" (Garcia-Reyes 2011) leads to weak upwelling when low salinity is typically expected because of less vertical mixing.

Interactive comment on Biogeosciences Discuss., 9, 17707, 2012.

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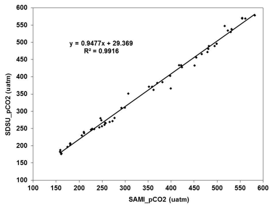

Figure C1. Comparison of pCO2 between SAMI-pCO2 and the SDSU pCO2 system.

Fig. 1.

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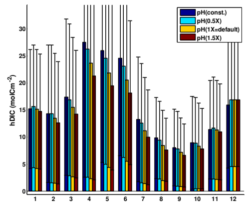

Figure C2. Simulated dissolved inorganic carbon per unit area within the mixed layer (hDIC) when there is no seasonal variation in pH (blue), when the seasonal variation in pH is 50 % less than the one reported by Hauri et al (2012) (cyan), when the seasonal variation in pH is as reported by Hauri et al (2012) (yellow), when the seasonal is 50 % more (red).

Fig. 2.

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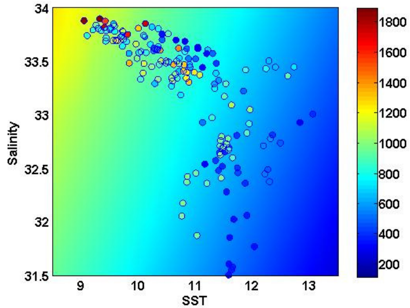

Figure C3 (Fig. 7). Distribution of daily averaged pCO_2 in relation to salinity and seas surface temperature (SST) (ºC) measured from November 2010 to July 2011. A multiple linear regression was applied for pCO_2 against both salinity and SST ($R = 0.57$, $p < 0.001$). The background color indicates pCO_2 estimated from the multiple linear regressions at given salinity and SST.

Fig. 3.