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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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Thank you very much for your constructive suggestions and comments. As pointed out, 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 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 inferred by 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 scatter 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.

I would like to address each of other comments:

(1) SAMI-pCO2 sensors have some known issues and suffer from biological fouling. Please provide more details of the calibration, and its uncertainties.

The SAMI-pCO2 sensor was cross-calibrated with another pCO2 system (headspace equilibrator with LI-840, LI-Cor) (Ikawa and Oechel, 2012). However, 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.

(2) 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.

Ideally, DIC needs to be estimated based on SST and pH measured for the same water. pH from the exact study area was not available and pH data estimated for a large area of central California was used. The effect of the seasonal change in pH was very minimal to the simulation; however, we will further evaluate the effect of pH on the calculation.

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(3) 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.

The paper states that the area off north California is likely an annual source of CO2, although other western coast of the United States features CO2 sinks. I will incorporate that in the text.

(4) 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.

It is well known that early upwelling is a source and relaxation is more sink. However, less is known about the balance between the twos.

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