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Biogeosciences Discussions

Interactive comment on "Coastal upwelling fluxes of O_2, N_2O, andCO_2 assessed from continuous atmospheric observations at Trinidad,California" *by* T. J. Lueker

T. J. Lueker

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Response to reviewers

Tim Lueker

I would like to thank the 3 reviewers who, along with my colleague and friend Wei-Jun Cai took the time to evaluate my paper. I am pleased to utilize the on-line journal format to respond to their concerns, as follows.

The first concern of reviewer 1. (1) The first reviewer expresses the opinion that most of the data and conclusions in this paper were previously published in Lueker et al., 2003, and that the paper should be rewritten with additional focus on the later sections 5 and 6.

I respectfully disagree, noting that the previous publication (GRL) was contained in a short note format, and publications in that journal quite often lead to more in depth

follow up publications elsewhere. I also note that since the 2003 GRL letter an additional year of data showed a dramatic increase in upwelling events due to interannual variability that was linked to an important change in circulation with large impacts to coastal ecosystems (Grantham et al., 2004). Also newly reported in this manuscript was shipboard data showing the depth profiles of the measured O2 supersaturation corresponding to satellite recorded post-upwelling plankton bloom that highlights the thin surface nature of the signals recorded by satellite. Finally, in this paper I have included satellite SeaWiFS CHLa maps that highlight the spatial extent and temporal development of upwelling induced plankton blooms corresponding to the atmospheric data, information not included in previous publications.

Minor comments from reviewer 1: (2) The reviewer asks that a definition/explanation be given for Bakun upwelling indeces.

I state in the figure caption “Upwelling indices were computed from geostrophic winds derived from six-hourly synoptic and monthly mean surface atmospheric pressure fields provided by NOAA (http://www.pfeg.noaa.gov/products/PFEL), as offshore Ekman transport in units of m 3 s- 1 (100 meters of coastline)- 1. I added the web address for readers seeking further explanation.

(3) The reviewer notes that the caption mistakenly states there is data from 2000-2003, this is an error that I will correct in the revised manuscript.

(4) The reviewer asks for fewer citations, there seems to be a difference of opinion on this topic as others, including the editor, consider this a well referenced paper.

(5) The reviewer states that in “Figure 6 there is no word on the fact that the data presented are partly from cruises in 1990 and 1998, however, the atmospheric data cover a period from 2000-2002. What about changes in the temperature regime, maybe caused by warming of the coastal ocean? How representative are these data then? What does it mean for the error of the computations? The author should discuss this point.”

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My intention in presenting any available data suitable for this comparison was to show that while the available data are from expeditions occurring several years and degrees of latitude apart, the relationships between the dissolved gases and temperatures are consistent enough to formulate estimates of air-sea gas exchange as a function of SST observed during upwelling events. Warming of the surface mixed layer of the California current, even if it were detectable in the last decade, would probably not affect this relationship at depth. Since the main object is to determine upwelling ventilation of gases, the temperature of interest are the range of upwelling water temperatures, 7-12 °C. This also answers comment (6), as temperatures above 12 °C are not important for consideration of upwelled waters in the region near 41 °N latitude.

Reviewer 2. The main concern of Reviewer 2 was the way the author computed the air-sea CO2 fluxes. "Unlike O2 and N2O, the CO2 air-sea fluxes cannot be derived from the continuous measurements of atmospheric CO2 at Trinidad Head because of the contamination from the terrestrial signal that blurs the air-sea CO2 flux signal. Instead, the author used a relationship of pCO2 versus temperature and computed pCO2 fields from SST satellite images (the air-sea CO2 fluxes were then computed from a gas transfer velocity parameterisation as function of wind speed). There are several problems associated with this approach:"

The objective of the air-sea CO2 flux calculation was misunderstood by this reviewer. My objective was not to estimate the net regional air-sea CO2 flux, which, due to the heterogeneous nature of the coastal ocean is highly uncertain especially given the sparse coverage of the continuous underway pCO2 data that has been gathered. My objective as stated on page 344 was to evaluate the potential out-gassing of CO2 during coastal upwelling events. This is an important issue if a considerable fraction of export production is released, as I demonstrate in Table 1. By scaling the O2 uptake to CO2 release using the atmospheric records we gain a valuable estimate of upwelling CO2 flux. I would like to point out that a similar relationship between sub-surface gas pressure and temperature was used in a paper estimating coastal N2O upwelling fluxes

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published by us in Global Biogoechemical cycles (Nevison et al., 2004).

I agree with the reviewer that the SST vs. pCO2 relationship seen in hydrographic profiles does not apply to non-upwelling conditions at the surface, as can be seen in measurements by Hales et al. 2003 (see Figure 2 at http://www.skio.peachnet.edu/coop/materials/newsletter17.pdf). However, even considering all recent records of surface pCO2 obtained off the west coast of North America, given the heterogeneity of the California Current the data are too sparse in time and space to permit meaningful estimates of net CO2 flux, so methods that can utilize satellite data such as surface winds and SST are valuable for providing first order estimates. Whether the west coast ocean margin constitutes a net source or sink for CO2 remains an actively researched topic (B. Hales, pers. com 2004).

Reviewer 3 In response to the following questions/comments from reviewer 3 “Detailed comments: P337, 1st para: It would be helpful to introduce the use of N2O data more in detail. What is the advantage or what is the message of the N2O data? Is the N2O somehow related to any oxygen depletion in the water column along the shelf or is it simply used as an indicator for marine biological activity?

In this manuscript I choose not to discuss at length N2O, in part to avoid repeating the findings of our previous publications as cited (e.g. Lueker et al., 2003, Nevison et al., 2004). As reviewer 1 noted there is already substantial coverage of previously published information, and I respectfully suggest readers refer to the cited papers for further details on the N2O studies.

P339: How was the N2 /O2 ratio established? Was N2 considered to be known (and stable) or were there accompanying N2 measurements?

The N2/O2 ratio is established in the calibration gases analyzed at SIO and run at Trinidad Head as stated on page 340: “The O2/N2 measurements reported here are 4 min averages of analyzer data collected every 3 s. CO2 measurements are the 4 min averages corresponding to the O2/N2 data. Data are calibrated against compressed

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gases prepared and measured at the Scripps Institution of Oceanography, traceable to global O2/N2 and CO2 measurement programs (Keeling et al., 1998) (Lueker et al., manuscript in preparation)". Additional information of the measurement of the calibration gases is contained it the cited manuscripts.

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Interactive comment on Biogeosciences Discussions, 1, 335, 2004.