Biogeosciences Discuss., 10, C8324–C8335, 2014
www.biogeosciences-discuss.net/10/C8324/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Neural network-based estimates of Southern Ocean net community production from in-situ O_2/Ar and satellite observation: a methodological study" *by* C.-H. Chang et al.

C.-H. Chang et al.

shingchang@gate.sinica.edu.tw

Received and published: 29 January 2014

We would like to thank our two reviewers, Dr. Wanninkhof and the anonymous reviewer, for carefully reading through our manuscript and for their insightful comments. In addressing the issues brought about by the reviewers, we found a small mistake in our previous calculation. We have modified the text and figures accordingly. The changes in values are quite small and do not affect our discussion and conclusions.

In the following, we will address each of these issues, following the order of the review-

ers. We also provide a supplementary file, which is a merged pdf file of the revised manuscript and supporting material along with our final response.

(I) Reply to Dr. Wanninkhok (Review #1)

General comment: Chang and co-authors estimate a critical biological oceanographic parameter, net community production (NCP), from a compilation of oxygen argon ratios (O2/Ar) and extrapolation over the Southern Ocean domain using a neural network technique. They do a good job explaining the technique. They provide a comprehensive error analysis and do a nice comparison of other estimates on basin to local scales. The paper is well written with good grammar and syntax.

Our response: We would like to thank Dr Wanninkhof for the positive and constructive comment. Our reply to specific comments follows, with original comments noted by "Q:". Please refer to the revised manuscript (in the supplementary material along with this final response) for the changes noted in our final response.

Q: (1) Describe briefly how POC fields are determined. My impression is that the [remote sensing] techniques to do so are fairly rudimentary and subject to large uncertainty.

A: POC data were downloaded from the Ocean Color website (http://oceancolor.gsfc.nasa.gov/). The algorithm is based on Stramski et al. [1999, 2008]. Retrieval of POC from satellite harbors significant uncertainties, which are discussed in Stramski et al. [2008]. It is concluded that the accuracy of satellite retrieval of POC will be adequate for many applications such as the estimation of large scale or global budgets of surface POC [Stramski et al., 2008]. Considering the strong correlation between NCP and satellite POC, future studies may benefit from using more recent POC algorithms specifically derived for the Southern Ocean (e.g. Allison et al. [2010a], Allison et al. [2010b]).

References: Stramski, D., R. A. Reynolds, M. Kahru, and B. G. Mitchell (1999), Esti-

mation of particulate organic carbon in the ocean from satellite remote sensing, Science, 285, 239-242, doi:10.1126/science.285.5425.239. Stramski, D., et al. (2008), Relationships between the surface concentration of particulate organic carbon and optical properties in the eastern South Pacific and eastern Atlantic Oceans, Biogeosciences, 5, 171-201. Allison, D. B., D. Stramski, and B. G. Mitchell (2010a), Seasonal and interannual variability of particulate organic carbon within the Southern Ocean from satellite ocean color observations, J. Geophys. Res., 115, C06002, doi:10.1029/2009JC005347. Allison, D. B., D. Stramski, and B. G. Mitchell (2010b), Empirical ocean color algorithms for estimating particulate organic carbon in the Southern Ocean, J. Geophys. Res., 115, C10044, doi:10.1029/2009JC006040.

Q: (2) Since the estimates are for time scales on the order of a month the magnitudes could be expressed as mol/m2/mo rather than mmol/m2/day. However, daily values are often presented and it would require quite a bit of editing so probably not worth it.

A: We chose to present NCP in daily values because it is the most commonly used unit in the literature with which we compare, as noted by the reviewer.

Q: (3) Issue with regard to area-averaged NCP south of 50oS (3.1) It is not always clear if the entire SO is discussed (> 30 S) or only the southern part (> 50 S) (see some examples below). (3.2) Page 16937 line 25: Why is only the region south of 50 S discussed here? (3.3) Table 2. Again unclear why only >50 S is used.

A: (3.1) For discussion associated with spatial distributions, our domain covers regions south of 20oS, which includes the Southern Ocean on a broader definition (> 30oS). For area-averaged values, we chose 50oS as our reference latitude. We agree that there might be potential incompleteness due to such an arbitrary choice, for instance, in section 3.2 where we decide the final choice of predictor combination. In addition to the cross-validation results, we examine if temporal evolution of monthly, area-averaged NCP south of 50oS is reasonable from various possible predictor combinations. To ease this issue, in the revised manuscript, we also checked the temporal evolution of

monthly area-averaged NCP south of 40oS. We modified the text accordingly (see ln 275-279). (3.2) Page 16937 ln 25 (ln 343-344 in revised ms): We include the total area south of 50oS for unit conversion of other model estimates, mainly in Table 2 (see discussion below), to the common unit used in our study. (3.3) Table 2: The main reason we list only the area-averaged NCP south of 50oS in Table 2 along with other model estimates is because we do not have all the other model data in Table 2. We obtained some of the model estimates from the published papers. The area-averaged NCP south of 50oS seems to be the most reported values in the Southern Ocean literature.

Q: (4) As stated, the mixed layer depth is a critical parameter. A few words on differences between the ARGO derived depths and model derived depths (used) might be appropriate as many models reproduce the MLD rather poorly.

A: The OFES model is known to capture realistic upper ocean dynamics, including eddy formation and propagation as well as heat balance. It has been used to investigate the Southern Ocean dynamical variability. Careful comparison between Argo and OFES mixed layer depth (MLD) can be found in Aoki et al. [2007]. Detailed references are provided in ln 151-154. We also clarified the original temporal resolution of the OFES MLD (that is 3 days) in ln 149-150.

Reference: Aoki, S., Hariyama, M., Mitsudera, H., Sasaki, H., and Sasai, Y.: Formation regions of Subantarctic Mode Water detected by OFES and Argo profiling floats, Geophys. Res. Lett., 34, doi:10.1029/2007GL029828, 2007a.

Here we provide a correlation map between monthly Argo and OFES MLD during the growing season (Nov-Mar) for the overlapping period: Jan 2001-Oct 2009 (Figure 1). We see that the correlation coefficient (cor) is > 0.6 over most of the regions in the Southern Ocean. Figures b1∼b6 show the time series of the standardized Argo and OFES MLD at selected grid points. It is seen that the OFES is able to capture realistic MLD (represented by Argo MLD, in red) for high correlation (> 0.6) cases (Figure

2(b1)-(b2)). For low correlation cases (< -0.4), however, it is likely due to scarcity of observations (see Figure 2 (b3)-(b6)).

Q: (5) There should be mention that some of the predictor variables (e.g. Sea surface height, SSH) are smoothed due to 10(?) day repeat orbit.

A: There are two SSH data sets. One is the along-track T/P and Jason altimeters which have a 9.97-day repeat cycle. The other is the AVISO SSH data set, which incorporated all available satellite altimeter missions and has a 1/3ox1/3o spatial resolution and a 7-day temporal resolution. In our study, we use the AVISO SSH altimeter, specified in section 2.1, ln 137-139.

We clarify the temporal variability contained in the predictor data in ln 158-160.

Q: (7) Page 16937 line 19: "In addition, because the biological pump is the main mechanism that drives atmospheric CO2 into the ocean". As I recall the solubility and biological pumps are about equal in magnitude on large scale.

A: We rephrased the sentence to "the biological pump is one of the main mechanisms driving atmospheric CO2 into the ocean" (ln 337-338).

Q: (8) Page 16942 line 15: The sea-air flux is much smaller than the NCP again refuting the suggestion that the biological pump is the main mechanism of CO2 uptake.

A: The CO2 flux into the ocean is smaller than that predicted based on NCP, which implies that some of the CO2 escapes from the ocean surface, likely due to thermal CO2 outgassing, as we discussed more in the following paragraph (see ln 467-478).

Q: (9) Page 16943 line 1: Sign convention, commonly fluxes into the ocean are listed as negative

A: We agree that the sign convention for the air-sea CO2 flux is defined positive (negative) into the atmosphere (ocean) from the atmosphere's point of view. Here in section 4.3.2 and Figure 6, however, our purpose is to compare the biological drawdown of

CO2, estimated by NCP, to an independent measurement of air-sea CO2 flux [Takahashi et al., 2009]. Therefore, we chose to define the air-sea flux to be positive into the ocean. To prevent confusion, we rephrased the sentence to "the CO2 flux changes from 0.2 Pg C yr-1 out of the ocean in October to 0.2 Pg C yr-1 into the ocean in December..." (ln 472-473). We also added a notion in the caption "the air-sea CO2 flux is defined positive into the ocean" (ln 1008-1009).

Reference: Takahashi, T., et al.: Climatological mean and decadal changes in surface ocean pCO2, and net sea-air CO2 flux over the global oceans, Deep-Sea Res., Part II, 56, 554-577, 2009.

Q: (10) Page 16943 section 5: It would improve readability if discussion and conclusions were clearly separated. They are intermingled.

A: We understand the appeal of separating the discussion and conclusions, but we chose to keep the section "Discussion and conclusions" because our conclusions were established as we discussed the features of our NCP maps. The content may be repetitive if we were to open another section for conclusion.

Q: (11) All tables: it is unclear why the 95 % CI is asymmetric around the mean (?) in this study.

A: The 95% CI is not symmetric about the mean because we do not perform conventional bootstrapping calculations, which typically involve data resampling and repeated calculations of a sample statistic. In our case, we use an unconventional approach whereby we apply the full, nonlinear SOM model to the resampled observations and then calculate the distribution of the parameter from the different SOM models. This addition of applying the nonlinear SOM model before calculation of the sample statistic distribution is the source of asymmetry in the 95% CI.

Q: (12) Figure 1b. I would cut off the distribution at 250 mmol to better distinguish the distribution of the majority of the data. Also convert mmol O2 to mmol C as that is used throughout.

A: We added an additional figure to zoom in on the distribution below the outlier threshold (see Figure 1c and ln 170-171). The unit for the outlier has been changed to mmol C m-2d-1, as suggested (Figures 1b and 1c and ln 170).

Q: (13) Figure 2C. It seems odd not the specify element/compound for chl that is presented in a weight unit (mg/m3) while you do specify element/compound for molar units (molC/m3)

A: The reasons for our choice of units for POC and chlorophyll in Figure 2 are as follows. We converted POC to moles because POC is directly relevant to the biological O2 flux to the atmosphere [Cassar et al., 2014]. On the other hand, most chlorophyll data is reported in mg m-3 (see examples at NASA ocean color website: (http://oceancolor.gsfc.nasa.gov/cgi/l3?per=DAY). We followed the standard procedure used in the literature.

Reference: Cassar, N., Wright, S. W., Thomson, P., Trull, T. W., Westwood, K. J., de Salas, M., Davidson, A., Pearce, I., Davies, D. M., Matear, R. J. 2014. The relation of carbon export production to plankton community in the Southern Ocean. Submitted.

Q: (14) Figure 5. Either mention in caption that scale of panel B is 10-fold that of panel A or put on same scale

A:We added a notion in the caption to clarify the changes in contour intervals for Figures 5 (b) and (c). (see ln 1000-1002).

Q: (15) Figure 6 B axis label is PgC/a while text is PgC/yr

A: We changed the y-axis label in Figure 6b to be consistent with the caption as well as the text (see Figure 6b).

(II) Reply to Reviewer #2

Our response: We thank the reviewer for the helpful comments. Our reply to specific

comments follows, with original comments original comments noted by "Q:". Please refer to the revised manuscript and supporting material (in the supplementary material along with this final response) for the changes noted in our final response.

Q: (1) In their supplement document, they mentioned that the approach of determining predictor/predictand SOM clusters is quite similar to that of Telszewski et al. [2009] except for one main difference, and they combine the training and the labeling steps of map generation from Telszewski et al. [2009] into a single step. It means to me that their SOM is trained by only the data in the regions where in-situ observations have been made. However, Telszewski et al. [2009] indicates that the SOM should be trained by the whole grid data so that the SOM has been preconditioned with comprehensive, basin-wide training knowledge with regards to the relevant biogeochemical processes. In this aspect, the technique in this study is completely different from that of Telszewski et al. [2009]. I think the authors should clarify how they overcome the claim of Telszweski et al. [2009].

A: We agree that this issue requires clarification. We do not believe that our approach is fundamentally different from that of Telszewski et al. [2009] because whether or not the SOM is trained with all gridded data or just the ship track data, the fundamental requirement is that the data space spanned by the ship track data approximates the data space spanned by the gridded data. For example, if we train the SOM with gridded predictor data, as in Telszewski et al. [2009], but if some of the resulting neurons are located outside the data space spanned by the ship tracks, then those neurons will not be labeled with the appropriate NCP values, which are only derived from the ship tracks. In other words, with either approach, the data space spanned by ship tracks is still the limiting factor because the labeling data (NCP) is limited to the ship tracks.

However, the essence of the reviewer's comment is clearly valid, and it is a point that we did not address sufficiently in the original manuscript: we did not discuss whether the ship track data distribution spans the range encountered in the gridded predictor data sets (a point addressed thoroughly in section 2.4.1 in Telszewski et al. [2009]).

In response, we have calculated the percentage of gridded predictor data that falls outside of the range of the training data for each of the three retained predictors, Chl, PAR, and MLD. We now report those results in ln 23-40 of the Supporting Material, with the main calculations discussed in ln 29-33:

"For Chl, approximately 4.4% of the gridded values fall below the ship track minimum (0.04 mg m-3), and 2.0% exceed the ship track maximum (1.56 mg m-3). For PAR, 5.2% of the gridded values fall below the ship track minimum (9.63 μE m$-$2 s$-$1), and 1.3% exceed the ship track maximum (59.5 μE m$-$2 s$-$1). For MLD, only 0.3% of the gridded values fall below the ship track minimum (3.5 m), and 0.3% exceed the ship track maximum (595 m)."

Therefore, these calculations support that the predictor distribution within the ship track data is reasonably representative of the distribution within the Southern Ocean overall during this time period, with well over 90% of the gridded values falling within the range of ship track values. These calculations support the reasonableness of the generalizations to the Southern Ocean basin used in this study. However, these calculations also point out that the lower range of two predictors, Chl and PAR, are not represented as well in the ship tracks. This suggests that there may be some bias in the NCP predictions for regions of very low Chl and/or PAR, which should be borne in mind by the reader and is noted in ln 38-40 of the Supporting Material.

Reference: Telszewski, M., et al. (2009), Estimating the monthly pCO2 distribution in the North Atlantic using a self-organizing neural network, Biogeosciences, 6, 1405-1421, doi 10.5194/bg-6-1405-2009.

Q: (2) Furthermore, it would be grateful if more information about the SOM technique (e.g. how many neurons were used and how many times the rough and fine tunings were executed in the first step. etc) is described so that any researchers can follow their experiment.

A: We agree that more details would be useful. In the manuscript in ln 285-286, we note

that our final SOM consists of 12 rows and 8 columns. To clarify, we added a notion that explicitly mentions that our map consists of 96 neurons (ln 286). In addition, we have added a paragraph in the Supporting Material (ln 41-49) that discusses each of the other SOM parameter choices used in our study.

Q: (3) It should be noted how much “not well-calibrated” Chl-a concentrations obtained from the satellite differs from in-situ data. I think that SOM doesn’t need accurate values in practice, but its temporal and spatial variation is more important for the analysis.

A: We agree with the reviewer that the trends are more important than absolute values for correlation and SOM analyses. However, the studies mentioned here in the manuscript show that the new algorithm improves correlation of satellite Chl to in situ observations. As mentioned in Johnson et al. [2013], "These new algorithms improve in situ versus satellite chlorophyll coefficients of determination (r2) from 0.27 to 0.46, 0.26 to 0.51, and 0.25 to 0.27, for OC4v6 (SeaWiFS), OC3M (MODIS-Aqua), and GlobColour, respectively."

Reference: Johnson, R., P. G. Strutton, S. W. Wright, A. McMinn, and K. M. Meiners (2013), Three improved Satellite Chlorophyll algorithms for the Southern Ocean, J. Geophys. Res. Oceans, 118, 3694–3703, doi:10.1002/jgrc.20270.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C8324/2014/bgd-10-C8324-2014-supplement.pdf

---

Interactive comment on Biogeosciences Discuss., 10, 16923, 2013.

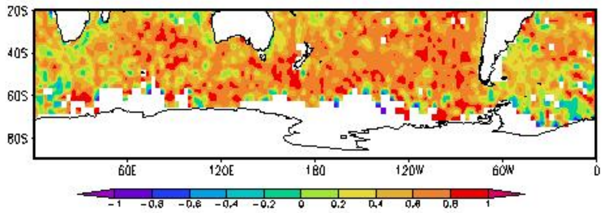

Fig. 1. Correlation map of monthly Argo and OFES MLD for Nov-Mar (Jan 2001-Oct 2009). (See ln 523-526 for details about Argo MLD.)

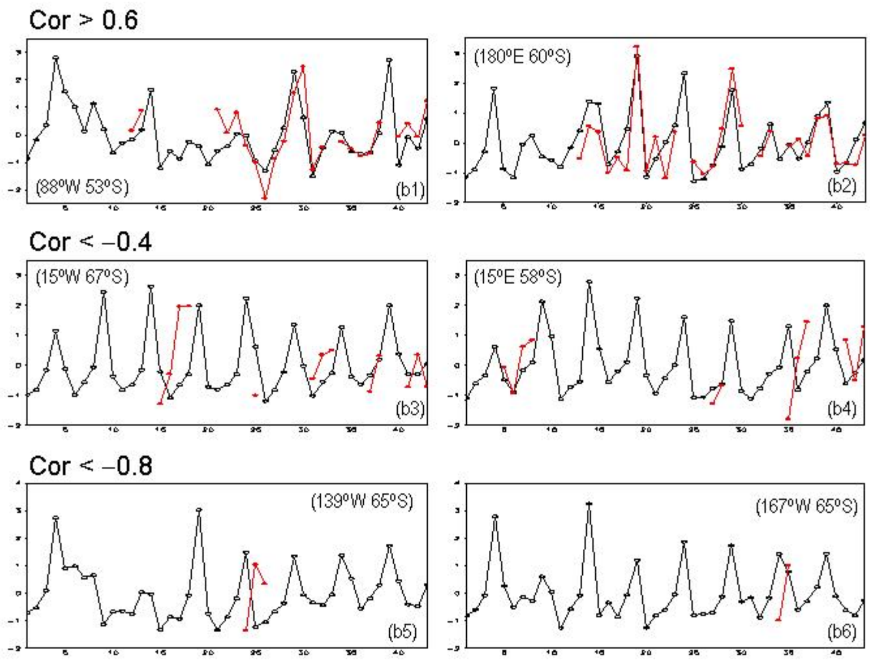

Fig. 2. Time series of Argo (red) and OFES (black) MLD at various grid points. Note that the labels along x-axis are in months. There are total 43 growing season months (Nov-Mar) between Jan 2001 and