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Discussions

Interactive comment on "Effects of storms on primary productivity and air-sea CO_2 exchange inthe subarctic western North Pacific: a modeling study" *by* M. Fujii and Y. Yamanaka

M. Fujii and Y. Yamanaka

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Responses to reviewer’s interactive comments on “Effects of storms on primary productivity and air-sea CO2 exchange in the subarctic western North Pacific: a modeling study” by M. Fujii and Y. Yamanaka

First of all, the authors greatly appreciate the constructive review on our manuscript. We have revised our manuscript, basically according to the reviewer’s comments in such ways as described below.

Responses to major comments

Because the model used in this study is vertically one-dimensional, it does not re-

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produce effects of horizontal advection. The authors have excluded the data from high-salinity (>33.2 psu) which are considered to be influenced by subtropical water in our discussion for model-data comparison. This is the same procedure introduced in Tsurushima et al. (2002). Using this procedure, the model results have demonstrated better performance than before, in simulating the mixed layer, net community production, NDIC and pCO2sea.

There are very limited data for column-integrated net community production in the western North Pacific. Although the modeled net community production is relatively higher than the net community production by Imai et al. (2002), it is not relatively high when comparing other observational data by Shiomoto et al. (1998) and Shiomoto (2000)) in the adjacent oceanic region. In the revised manuscript, the authors have added the data by Shiomoto et al. (1998) and Shiomoto (2000) to the figure for reference, which shows the net community production by total phytoplankton is as high as in spring.

The authors have estimated in situ pCO2air at Station KNOT from Tsurushima et al. (2002). The data has been shown in Figure 2e in the revised manuscript for reference. The data are very similar to those at Mauna Loa (Keeling et al., 1982; Conway et al., 1994), and the authors have kept using the pCO2air data for calculation in this study. Please see below and an attached figure for the similarity in the pCO2air in detail.

Responses to moderate comments

The authors have replaced Tsurushima (2002)’s raw data in the previous manuscript with the data in Tsurushima et al. (2002). As a result, the number of data has been consistent for NDIC and pCO2sea. The authors have also removed the data which are considered to be influenced by the subtropical water, as described above.

The authors have mixed up the meaning of “compartment” with that of “state variable” in the previous manuscript. To avoid confusions, the authors have clarified this by describing “16-compartment (16-state variable)

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marine ecosystem model” in Section 2 and Figure 1 caption in the revised manuscript.

The works of Bates et al. (1998a) and Wanninkhof et al. (2007) have been discussed in Section 3. Interestingly, the conclusion from the two works is quite opposite with regards to the role of storms in interannual CO2 flux, and our conclusion is closer to that of Bates et al. (1998a). The authors have also mentioned this as follows: “Our model results also suggest that storm events could potentially have a large effect on interannual air-sea CO2 flux variability globally, which is consistent with Bates et al. (1998a). However, Wanninkhof et al. (2007) induced an opposite conclusion of minimal influence of storms on annual CO2 flux. Therefore, long-term monitoring of the air-sea CO2 flux is required in various oceanic regions to assess quantitatively the role of storm events in the interannual air-sea CO2 flux.”

The authors have added ranges of the wind speeds and highest wind speeds during the storm events to the beginning of Section 3, as follows. “Most of storm events last for no more than one day, and there are no storm events that last for more than five days (Table 2). All the storms have the wind speed of more than 10 (m s-1) with the highest wind speed of 26.3 (m s-1) for the 19 years. However, the storm events which has the wind speed of more than 25 (m s-1) are very limited (Table 3), which is different from situations in the subtropical regions in which typhoons and hurricanes have higher wind speeds more than 30 (m s-1) (e.g. Bates et al., 1998 a, b; D’Asaro and McNeil, 2007; Wanninkhof et al., 2007).” To provide supplemental or more detailed information, Tables 2 and 3 have also been added to the revised manuscript. Following the reviewer’s useful comments, the authors have reviewed the suggested three works of Ho et al. (2006), McNeil and D’Asaro (2007), and Fongohr and Woolf (2007). Considering the range of wind speed in our study, Ho et al. (2006) can be used for the wind with <20m s-1 and McNeil and D’Asaro (2007) can be applied for the wind with >20m s-1. However, temporal resolution and the wind used for simulation in our study is an hour, it is eligible for the modeling study to use

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continuous wind speed/gas exchange parameterization in calculating the air-sea CO2 flux with the wind speed of 26.3 m s-1at the highest. Considering that the gas transfer velocity is similar between Wanninkhof et al. (1992) and Ho et al. (2007) for the range of wind speed in our study (different by not more than 5-10%), the authors have decided to keep using Wanninkhof et al (1992)’s parameterization in this study. Modeling studies with various wind speed/gas exchange parameterizations, along with using various wind data (such as QuickSCAT winds), are our ongoing works (Fujii et al., in prep.).

Following the reviewer’s comment, the authors have compared pCO2air at Mauna Loa, Hawaii (19.54°N,155.58°W), Shemya Island, Alaska (52.72°N,174.10°E), Sand Island, Midway (28.21°N, 177.38°W), and POCN30 in the Pacific Ocean (30.00°N, 135.00°W). The pCO2air for the four stations have also been compared to the pCO2air by Tsurushima et al. (2002). The comparison result shows that the pCO2air is similar among the stations (see an attached figure). Considering the similarity, it seems okay to use the pCO2air at Mauna Loa for this study, and therefore, the authors have continued to use that. Actually, we need pCO2air data after 1970’s to calculate the air-sea CO2 flux, and Mauna Loa is the only station so far which can provide the long-term data since 1970’s.

In the previous manuscript, the authors did not clarify to distinguish terms among “net primary production”, “gross primary production” and “net community production”. In the revised manuscript, the authors have unified to the term “net community production”, following Fujii et al. (2007).

As mentioned in the manuscript, the Reynolds weekly SST data were used to drive the model (in Section 2), and the modeled SST decreased by 0.8℃ in the mid-June, 1994 (in Section 3). Therefore, the Reynolds SST data could allow to pick up the storm signal on temperature although the temporal resolution is not sufficient. Unfortunately, there do not exist in situ or remote-sensing data that validate the decrease in temperature at Station KNOT before mid-1990’s.

The authors thank the reviewer for introducing a modeling study of Soetaert et al. (2001). Along with this paper, the authors found several modeling studies which have studied the effect of intraseasonal events on ambient nutrients and chlorophyll, and have added them as references to the revised manuscript (e.g. Kawamiya and Oschlies (2004), McCreary et al. (1996 and 2001), Large and Crawford (1995) and Ridderinkhof (1992)).

The authors have mentioned about the discrepancy between this study and others (Takahashi et al., 2002 and Tsurushima et al., 2002) with regards to extent of the role of the subarctic western North Pacific as a source of CO2 to the atmosphere, in Sections 3 and 4.

One of the authors used to engage in the VOS project with Dr. Nojiri, and actually used the data as references in this study. The authors have added words to the revised manuscript mentioning that the data were used as references in this study, as well as the URL. Thank you for the comment.

Responses to minor comments

The term “TCO2” has been replaced with “DIC” in the entire manuscript.

Page 66 Line 13 in the previous manuscript: “continuous observations”  “continuous observations of the wind and CO2“

Page 66 Line 13 in the previous manuscript: “solar radiation”  ” incoming solar radiation”

Page 67 Line 3 in the previous manuscript: “In most such cases”  “In most cases”

Page 67 Line 10 in the previous manuscript: “entrainment” 

“horizontal advection”

Page 67 Line 20 in the previous manuscript: Sabine et al. (2004) have been referred in the revised manuscript instead of Tsurushima et al. (2002).

Page 68 Line 14 in the previous manuscript: “shells”  “liths”. Thank you for the useful comment.

Page 68 Line 23 in the previous manuscript: “at the sea surface”  “at 10m height”

Page 69 Line 1 in the previous manuscript: The reviewer is correct in mentioning that pCO2air is different from xCO2air. The atmospheric CO2 data are provided as a form of xCO2air, and therefore, the xCO2air should be converted to pCO2air by the following formula:

where P is the ambient sea level pressure and pH2O is the water vapor pressure at 100% humidity calculated from SST and salinity according to Weiss and Price (1980). The authors have checked possible ranges of P and pH2O at Station KNOT and have also referred to the VOS data which are mentioned above. The P can vary from ca. 0.94 to ca. 1.00, and the pH2O can change from ca. 0.006 to 0.014. Therefore, the pCO2air can be lower than xCO2air by up to 6%. In the previous manuscript, the pCO2air was not distinguished from the xCO2air. In the revised manuscript, the authors have decided to continue to use the xCO2air for calculating air-sea CO2 flux. The reason is as follows: Although the sea level pressure with higher frequency than monthly can be obtained from the NCEP objectively analyzed data, the corresponding continuous atmospheric CO2 data with higher frequency than monthly cannot be obtained. Generally, the sea level pressure is low in late winter when the pCO2sea is higher than the pCO2air in the subarctic western North Pacific. If the pCO2air is used instead of xCO2air, △pCO2 (pCO2sea minus pCO2air) gets larger, and therefore, the sea-to-air CO2 efflux should be higher as well. This is important and presumably serves as a spur to our claim that previous studies overestimated role of

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the subarctic western North Pacific as a sink of CO2. This argument has been added to Sections 3 and 4 in the revised manuscript. Thank you for the important comment.

Page 69 Line 18 in the previous manuscript: “well reproduces”  “reproduces well”

Page 69 Line 23 in the previous manuscript: “pss”  “psu”

Page 70 Lines 6-9 in the previous manuscript: As the reviewer points out, the phrase “pCO2sea is the most sensitive biogeochemical parameter to the storms” cannot be justified in this study, and the authors have removed the phrase “that pCO2sea is the most sensitive biogeochemical parameter to the storms, and therefore,” in the revised manuscript.

Page 70 Line 23 in the previous manuscript: “slihjtly”  “slightly”

Page 71 Line 14 in the previous manuscript: flux values have been rounded (244.55  245, 160.60  161)

Page 71 Line 15 in the previous manuscript: In the revised manuscript, the authors have referred to two previous studies (Takahashi et al., 2002; Tsurushima et al., 2002).

Page 71 Line 18 in the previous manuscript: “taking up oceanic CO2”  ” taking up atmospheric CO2”

Page 71 Lines 20 and 27 in the previous manuscript: The reviewer’s comments “The enhancement of primary production in summer is less than 10%, this is negligible” and “The reduction of net community production in winter and spring is very low, this is even more negligible.” are right, but the model result is for 19-year mean (1982-2000). Therefore, in some years the enhancement is much higher than 10%. The authors have emphasized that the model results shown in Figure 2 is for the 19-year mean in Table 2 and Figure 2 caption and in the main text in the revised manuscript.

Page 72 Line 7 in the previous manuscript: Even considering the authors’ response above, this seems an over-statement, so the authors have removed a word “significantly” from the sentence in the revised manuscript.

Page 72 Lines 8-9 in the previous manuscript: The reviewer is right in mentioning “the irradiance was not modeled” and “the results of nutrient simulations were not shown” in this study. The authors have removed the terms “the irradiance” and “nutrient concentrations” from the revised manuscript. These modifications do not affect purport of this study.

Figure 1 caption: “schematic view”  “conceptual diagram”

Figure 1: “remineralization”  “dissolution” for the arrow between “CaCO3” and “Ca2+”

Figure 1: “Ca”  ”Ca2+”

Figure 1: “shell formation”  “calcification” for the arrow between “PS” and “Ca2+” (and also that between “ZS” and “Ca2+”)

Figure 1: “shell formation”  “frustule formation” for the arrow between “Si(OH)4” and “PL”

Figure 1: Both flows from PS and PL to NO3 were set to zero in this study, following Fujii et al. (2007). Modeling diatom sinking is an ongoing study (e.g. Yoshie et al., in prep.). The process seems important especially at the end of the diatom bloom, and this is important because the process accelerates termination of the diatom bloom. The process is partly represented by parameterizing the diatom mortality proportional to the square of diatom abundance.

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Figure 2: Following the reviewer’s comment, Figure 2f has been modified to have two segments.

Figure 3 plot b): The atmospheric CO2 (pCO2air) has the seasonality, as the reviewer points out. The pCO2air in the previous manuscript is not correct and has been revised in the revised manuscript. The seasonality was taken into account in calculating the air-sea CO2 flux in the previous manuscript, and therefore, the model results do not change.

Figure 3 plot c): The term “efflux” has been replaced with “flux” in Figure 3(c) and the caption, and the main text which explains this figure. The term “positive upward” has been removed from Table 2 caption because it is obvious as long as one uses the term “efflux”.

Other revisions

Table 1 in the previous manuscript included some errors in number, and therefore, the numbers have been revised in the revised manuscript. The corresponding statements in the main text have also been revised as well.

Tables 2 and 3 have been added to the revised manuscript. Most of storm events last for no more than one day, and there are no storm events that last for more than five days (Table 2). All the storms have the wind speed of more than 10 (m s-1). However, the storm events which has the wind speed of more than 25 (m s-1) are very limited (Table 3), which is different from situations in the subtropical regions in which typhoons and hurricanes have higher wind speeds more than 30 (m s-1).

Along with references introduced by the reviewers, the authors have referred the following previous studies to the revised manuscript.

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