

Interactive comment 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

Anonymous Referee #2

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GENERAL COMMENTS

The authors present a 1D physical and biogeochemical model for station KNOT in the subarctic western Pacific. The authors examine the effect of storms on biogeochemical functioning and present simulation results for primary production and air-sea CO₂ fluxes. The authors conclude that storm events have some impact on primary production and a strong impact on air-sea CO₂ fluxes.

Although the approach is interesting and the conclusions could be useful, I have the strong impression that the model has been poorly validated (see comments hereafter). Hence, this paper can be seen as a theoretical exercise, since the results of the ap-

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plication of the model (quantification of the effect of storms on primary production and air-sea CO₂ fluxes at station KNOT) make little sense, because they are based on a model with a poor ability to reproduce CO₂ dynamics and primary production at station KNOT.

MAJOR COMMENTS

The model does not reproduce correctly the data : Fig. 2 shows that the model systematically over-estimates NTCO₂ from 10 to 50 μM . Fig. 2 shows that the model under-estimates the pCO₂ from 10 to 30 μatm from January to August and over-estimates pCO₂ from September to December from 10 to 30 μatm . Fig. 2 shows that from June to late November the model over-estimates primary production up to 400 mgC m⁻² d⁻¹.

The model does not reproduce correctly the timing of the data : Data in Fig. 2 (and Fig. 4 of Imai et al. (2002)) show that primary production peaks in May and steadily decreases from late May to December/January. Fig.2 shows that the modelled primary production peaks in August.

The model is not internally consistent : Fig. 2 shows that the model tends to over-estimate primary production during most of the year, while on the other hand the model tends to systematically under-estimate NTCO₂.

MODERATE COMMENTS

In Fig. 2 there are more data points for NTCO₂ than for pCO₂. Why ?

In text some information is given on the number of 'ecosystem compartments' but not on the number of state variables.

The work by Bates et al. (1998) on the effect of hurricanes on pCO₂ should be useful in the discussion.

The recent work by Wanninkhof et al. (2007) on the effect of storms on pCO₂ in the Caribbean Sea should be useful in the discussion.

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The authors used the Wanninkhof (1992) relationship to compute the gas transfer velocity. The recent work by Ho et al. (2006) shows that the Wanninkhof (1992) relationship probably over-estimates the gas transfer velocity at moderate to high wind speeds.

The authors do not mention anywhere in the manuscript the range of values of wind speeds and the highest wind speed values used in the simulations. This has important impacts on the computation of the gas transfer velocity. If wind speeds are below 20 m/s, the Ho et al. (2006) relationship could be used. If wind speeds are above 20 m/s then the parameterisation of McNeil and D'Asaro (2007) could be useful. At extreme wind speeds bubble formation becomes an issue in gas transfer: the recent work of Fangohr & Woolf (2007), and references therein, could be useful.

The authors use the atmospheric CO₂ at Mauna Loa. However, the pCO₂ in seawater is fairly close to atmospheric equilibrium, hence, the use of atmospheric CO₂ data can be an issue, since it is well established that there are strong latitudinal gradients in atmospheric CO₂. Hence, the Mauna Loa data are probably not representative of atmospheric CO₂ in the North Pacific. The authors must check for a pCO₂ measuring station as close as possible to their study area from (for instance) the CMDL/NOAA database (<http://www.esrl.noaa.gov/gmd/>).

The authors model primary production and use primary production data. However it is unclear from text if the data are net or gross primary production. Also, it is unclear if the model results correspond to net or gross primary production, or net community production.

It is unclear from text if sea surface temperature was modelled or if the Reynolds climatological data were used as forcing data in the model. The authors need to clarify this, and if sea surface temperature was formally modelled, then the model results and the in-situ data must be shown. If the Reynolds data were used as forcing data in the model, then the discussion on the storm effect on sea surface temperature (and

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concomitant effect on pCO₂) does not make sense, because the Reynolds climatology does not allow to pick up the storm signals on temperature.

The authors discuss to some extent the effects of storms on pCO₂ by comparing with the data of Bates. However, there is no discussion whatsoever on the effect of storms on the other modelled biogeochemical fluxes. I did not look up what is available in literature but I'm aware of at least one paper by Soetaert et al. (2001).

The Takahashi et al. (2002) climatology of air-sea CO₂ fluxes (http://www.ldeo.columbia.edu/res/pi/CO2/carbondioxide/pages/air_sea_flux_rev1.html) indicates that station KNOT is a sink for atmospheric CO₂. The present modelling work claims that station KNOT acts as a source of CO₂ to the atmosphere. It would be useful to mention and discuss this discrepancy.

The group of Prof. Nojiri has obtained extensive data-sets in the area with VOS lines that could also be useful for the discussion on the model performance. Some of these data are freely available (<http://sk.soop.jp/index.html>)(<http://ah.soop.jp/index.html>).

MINOR COMMENTS

The term 'TCO₂' has been abandoned by the inorganic carbon community for some time, the term 'DIC' is preferred.

Page 66 Line 3 : 'continuous observations' of what ?

Page 66 Line 16: 'incoming solar radiation' instead of 'solar radiation'

Page 67 Line 3: 'In most such cases' is awkward, rephrase

Page 67 Line 10 : It is unclear if 'entrainment of waters with high chlorophyll-a concentration' relates to vertical mixing or horizontal transport.

Page 67 Line 20 : The work of Tsurushima et al. (2002) is based on measured pCO₂ and DIC in surface waters. This approach cannot provide insights on 'anthropogenic CO₂' uptake. For an estimate of 'anthropogenic CO₂' uptake per se in the subarctic

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western North Pacific, refer to Sabine et al. (2004).

Page 68 Line 14 : coccolithophorids do not produce 'shells' , they produce CaCO₃ 'liths'

Page 68 Line 23 : U is wind speed at 10m height and not 'at the sea surface'

Page 69 Line 1 : For the purpose of computing CO₂ fluxes, atmospheric CO₂ given in dry air as provided from databases for Mauna Loa needs to be converted into wet air. Was this done ? How ?

Page 69 Line 18: The correct phrasing is 'reproduces well' although this is not the case (see Major comments above)

Page 69 Line 23: 'psu' instead of 'pss'

Page 70 Lines 6-9: pCO₂sea is not 'the most sensitive biogeochemical parameter to the storms'. From Figure 2 pCO₂ is virtually undistinguishable in experiments 1 and 2; So the causality implied by the term 'therefore' in the sentence does not exist; Storms do 'contribute significantly to air-sea exchange of CO₂' in the simulation because of the strong enhancement of the gas transfer velocity.

Page 70 Line 23 : typo 'slihjtly'

Page 71 line 14: flux values can be rounded to the unit

Page 71 line 15 : which 'previous studies' ? The reader has not the information to check if 'previous studies' included storm effects or nor if the they are not cited.

Page 71 line 18: 'taking up atmospheric CO₂' instead of 'taking up oceanic CO₂'

Page 71 line 20: The enhancement of primary production in summer is less than 10%, this is negligible.

Page 71 line 27: The reduction of primary production in winter and spring is very low (less than 1% I would guess from Fig. 2), this is even more negligible.

BGD

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Page 72 Line 7: This is an over-statement : storms do not 'significantly' affect 'ecosystem dynamics', since the change in primary production is low (<1% to <10%).

Page 72 Line 8: irradiance was not modelled, it was a forcing variable in the simulations.

Page 72 Line 9: Nowhere in the ms are the results of nutrient simulations shown, hence this statement is not corroborated by model results.

Page 73 Lines 1-3: I fully agree with this statement because some models as the one presented here are unable to correctly reproduce the measurements.

Page 73 Lines 9-11: I'm unsure that more data would improve the present version of the model, but only further highlight the blunt short-comings of this model.

Figure 1: This is not a 'schematic view' but a 'conceptual diagram'

Figure 1: replace 'rem mineralization' by 'dissolution' for the arrow between 'CaCO₃' and 'Ca'

Figure 1: replace 'Ca' by 'Ca²⁺'

Figure 1: replace 'shell formation' by 'calcification' for the arrow between 'PS' and 'Ca'

Figure 1: replace 'shell formation' by 'frustule formation' between 'Si(OH)₄' and 'PL'

Figure 1: It seems unlikely that there is a direct arrow between 'PS' and 'NO₃' without an intermediary bacterial compartment (nitrifiers).

Figure 1: It seems unlikely that respiration by 'PL' can provide NO₃.

Figure 1: Living diatoms can sediment and this seems to have been ignored in the model.

Figure 2: if the y-axis of plot 2f was in 2 segments then the high efflux values would be visible and show the impact of experiment 1 and 2 on the air-sea fluxes.

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Figure 3 plot b): No seasonality of the atmospheric CO₂ is apparent in plot 3b. This is not the case in the real world, and hopefully the seasonality of atmospheric CO₂ was included in the simulations.

Figure 3 plot c): 'efflux' refers to a transfer of CO₂ from the water to the atmosphere. There authors use the term 'efflux' instead of 'flux'.

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