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Interactive comment on "Physical Modulation to the Biological Productivity in the Summer Vietnam Upwelling System" *by* Wenfang Lu et al.

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General Comments

Most of the scientific papers published about the South China Sea (SCS) are dealing with remote sensing or numerical modeling. Only few papers present in-situ observations, which are ignored by the authors. Unfortunately this is also one of the major problems within this paper, because a couple of relevant papers, which deal with this problem, were ignored. See reference list at the end.

The fact that higher wind speed causes a stronger upwelling and a higher nutrient flux into the euphotic zone, which is connected with higher primary production, is not new. It is text book knowledge. This process has been quantified in the SCS upwelling area

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for normal and post El Nino years inside the upwelling area and offshore (Voss et al. 2006, Bombar et al. 2010). Hence the finding of the authors is not new. It is known since more than 10 years!

In the abstract the sentence “The elevated kinetic energy and intensified circulation can be explained by the separation of the upwelling system” is the same misinterpretation as in the Liu et al. (2002) paper. The opposite is true. The stronger monsoon intensifies the circulation. If the velocity reach a critical value a jet is detached from the coast. This is the classical Gulf Stream detachment problem discussed by Haidvogel et al. (1992) and Marshall & Tansley (2001). A similar detachment modulated by the ITCZ occurs in the SCS and is described by Dippner et al. (2013). Hence, this aspect is also not new.

The model application is not well posed and the validation is rather problematically. The discussion is a mixture of trivial statements and speculations. These aspects are outlined below. In addition, I have problems with the presentation. There are many Chinese references, however, I miss the fundamental theoretical papers on upwelling (see references) as well as the classical papers on upwelling observations, which are given e.g. in the references of the review by Mittelstaedt (1986).

To conclude: I cannot find any aspect, which merits publication. The paper is a mixture of textbook knowledge, physical misinterpretation, trivial statements, speculation and improper referencing. Therefore, I recommend the editor to reject the paper.

Specific Comments

The motivation of this paper, the so-called “contradictory conclusion”, is funny. There is no contradiction. Both papers, the Hein et al. (2013) and the Liu et al. (2002), are correct. The conclusions in these papers were different, because different years were considered. The observations in the Hein paper were made in 2003, whereas the observation in the Liu paper were made in 1992, 1998 and 1999. The authors may have a look to the Multivariate ENSO Index. Drifter observations in 2003 indicated a lateral transport (Dippner et al. 2011) and the physical mechanisms behind the offshore transport and the transport parallel to the coast were explained by Dippner et al. (2013). So, what is the scientific question of the paper and what are the hypotheses?

The model has a resolution of 1/10 degree. The width of the upwelling are is 42 km. That means that the upwelling area is resolved with less than 4 grid points. Such a resolution of the upwelling area is not sufficient for any conclusions on dynamical processes. Therefore, I recommend to remove the word “upwelling” from the title.

After spin-up, the model runs from 2002 to 2011 and the period 2005 to 2011 was analyzed. The seasonal signal was filtered and from the inter-annual variability composites of high and low chlorophyll were constructed. However, it is not clear how the “normal year”, the “no advection” or the “El Niño” were constructed.

The model considers picoplankton, diatoms and coccolithiphorids as functional groups. These functional groups are not representative for the SCS. Dinoflagellates, Phaeocystis spp. and nitrogen fixing bacteria are not considered, although they play a major role in the SCS phytoplankton (Bombar et al. 2011, Doan-Nhu et al 2010, Loick-Wilde et al. 2017).

No information is given on initial conditions of the biogeochemical model. Without a sensitivity analysis, the statement that the ecosystem model is insensitive to initial conditions is not serious.

The authors used HNA and LNA as criteria for the construction of composites. This is rather problematic HNA and LNA are not robust variables. NPP is far away from any similarity with observations. Seasonal variability is much higher than inter-annual variability. There is no serious reason to use HNA- and LNA-composites Strong and weak monsoon would be much better criteria for the construction of composites.

The model validation is not convincing. In this context it is important to state that the 3D figures are not helpful. If the authors present an upwelling model, I would like to see a vertical cross section normal to the coast, which should indicate the upwelling of the isopycnals and the poleward undercurrent, which is a quality criterion of upwelling models (O'Brien & Hurlburt 1972).

The model is $\sim$1°C to cold, the modeled NPP does not fit and the estimated kinetic energy is too high. Nevertheless, the authors try to convince the reader on the reasonable well agreement. Furthermore, it should be clearly mentioned that the biannual signals were transient signals, which were not present every year. The authors mentioned that the reasons for discrepancies in validation is insufficient horizontal resolution, unrealistic parameterization etc., but these shortcomings are accepted. Why don't they use a model with a sufficient horizontal resolution, realistic parameterization etc.? Please explain.

The biogeochemical model produced results far away from reality. From observations, it is known that strong blooms in the upwelling area can be addressed to strong monsoon due to a northern position of ITCZ (Dippner et al. 2013), which causes a specific distribution of characteristic water masses (Dippner & Loick Wilde, 2011) and their corresponding specific species distributions (Loick-Wilde et al. 2017). In contrast, in the oligotrophic offshore area, production can be directly addressed to nitrogen fixing bacteria (Bombar et al. 2010, 2011).

L247: The statement "Part of the ammonium could then fuel nitrification and production . . ." is pure speculation. It is not shown.

The chapter Discussion has the character of a Results chapter. Normally in the discussion, new findings were discussed in the context of existing literature. This is not done. The paragraph on biogeochemical cycles should be skipped. The four mentioned cycles are either trivial or speculation in the sense of not shown. E.g., "upwelled water . . . stimulate high production" is a trivial statement.

The paragraph 4.2 is a collection of trivial statements. A comparison of two model runs with and without advection is not helpful in understanding dynamics.

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The statement “the more intensive separation, the larger KE in VBUS, and vice versa” in not correct. KE is not a meaningful quantity because separation occurs if the velocity (not KE) reaches a critical value. The statement “high KE is linked to accelerated biogeochemical cycle” is speculation, it is not shown. What means in this context “accelerated”. I don’t believe that KE has an influence on biological turn-around times.

The conclusion has the character of a summary. It is a repetition of previous speculations.

The statement “numerical experiment was designed to reproduce the non-separated circulation pattern, while maintaining the external monsoon forcing” documents not well posed modelling. From literature it is known that the intensity of monsoon and the connected inter-annual variability in ITCZ are responsible for the fine structure in the Vietnamese upwelling area.

Technical Comments

The ms has too much acronyms

The reference Dippner et al. (2006) was published in 2007.

Equation 1 goes back to Ekman (1905), to whom belongs the credit and not Chen et al. (2012) or Gruber et. (2011).

No information on the drag coefficient is given.

Sloppy formulation: “near-surface geostrophic current”. Skip the word geostrophic.

What is the reference level (layer of no motion) of the dynamic topography? Please explain.

The Statement “nonlinear advection is important to the separation of the coastal jet” should not been addressed to Gan and Qu (2008) or Wand et al. (2006). The credit belongs to Haidvogel et al. (1992) and Marshall & Tansley (2001).

L 165 wrong dimension, should read m2s-2.

I cannot see a magenta box.

L202 “the physical and biological parameters” is a wrong formulation. Parameters should be replaced by variable, because a parameter is a quantity, which cannot be measured and must therefore be parameterized, as the name says.

L208 Contradiction: Why ageostrophic components contribute to the kinetic energy? This is not compatible with the definition of kinetic energy. Please explain.

L220 Why a lag suggests a significant regulation of physical forcing? Please explain.

L233 What means “the current dissipates freshwater”? Please explain.

The figures are hard to read (too small legends or axes labeling) and not very informative. The main reason is the perspective view, which is surely nice to see, but the essential information remains hidden.

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