We thank for the criticisms and constructive suggestions. The comments are fully considered in the revised manuscript accordingly. Below are our response to the comments one by one.

A modelling study is presented to investigate the factors controlling bloom initiation in the region of the Taiwan Strait. The mechanism proposed is that relaxed winds trigger cold, fresh, nutrient-rich water to veer off the Chinese mainland coast, stabilizing stratification and promoting bloom onset. There may be some interesting science here but the presentation and articulation of the work is poor and I found the ms a frustrating and incoherent read. As such it is, in my opinion, nowhere near the standard required for publication. I have several high-level criticisms:

Q-1. The Introduction does not adequately set the context. The paragraph on p. 14687 beginning "With respect to the physical controls ..." starts to introduce the basic concepts. Rather, what is needed is a succinct description of general bloom initiation theory, focusing on key processes. This then needs to proceed to the concepts associated with the chosen area of study, emphasising novel aspects, and with reference to the literature for similar situations elsewhere. The reader should be given an indication of whether the chosen scenario (Taiwan Strait) is likely just a one-off case or whether the principles are more widely applicable and therefore of general interest to biogeochemical modellers.

Response: The introduction section is rewritten in the revised manuscript according to your comment.

The general bloom initiation theory should be the ***Irradiance-Mixing*** (IM) regime proposed by Nelson and W.O. Smith (1991). Namely, in the well-mixed and nutrient-rich upper ocean, the light decreases exponentially with the depth, and the phytoplankton's photosynthesis is inhibited by light in the lower layers. Strong mixing will transport the phytoplankton into lower layers inhibiting the phytoplankton increase, while weak mixing can let the phytoplankton aggregate in the upper layer, thus triggers the bloom. The regime has been widely applied to interpret the bloom scenario in the many places.

In the TWS, the bloom appeared during the relaxation of northeastern monsoon in 1998 and 2001, respectively. In the study, it is found that the direct cause of the bloom in the TWS is the reduced mixing, which follows the IM regime also.

Actually, we don't try to propose a new mechanism/regime for the bloom. The new idea we found is the off-shore MZCW contributes to the stratification, which predominantly inhibits the water mixing and triggers the bloom, while in most reported cases (e.g. Wiggert et al.,2000, Taylor and Ferrari, 2011) the it is directly caused by the reduced turbulence input.

Q-2. Following from the above, an apparently new hypothesis for bloom formation is proposed (p. 14687, line 17): "the relaxation of the northeast monsoon, which reduces the turbulence input at the surface and causes the fresh MZCW flow to veer off the western shore by geostrophic adjustment (Liao et al., 2013), enhancing the coastal stratification." The hypothesis as posed is insufficient. The link between stratification and bloom formation also has to be made, e.g. in terms of critical depth theory, or turbulence theory.

Response: Accept. The link between stratification and bloom formation is the IM regime. The stratification inhibits the vertical mixing and then triggers the bloom initiation. The Sverdrup's Critical Depth (Sverdrup, 1953) and Critical Turbulence (Huisman et al., 1999;Taylor and Ferrari,

2011) are two types of model, which follows the IM regime.

Q-3. I find section 2 (in situ and satellite observations) thoroughly unconvincing. When I first encountered this section, I assumed it would be for the purposes of model validation. In fact, the observations are instead used in their own right to try and address the central hypothesis of bloom formation. With rather flimsy evidence, the authors present some sort of correlation between wind speed reduction and increased chlorophyll concentration, and use it as supporting evidence for their hypothesis. It is wholly unconvincing and I believe section 2 could be completely removed from the ms.

Response: Accepted. The Section 2 is removed and replaced by the Model Evaluation section in the revised manuscript. We re-run the model with real wind forcing and compare the model result with in-situ observation in Fig 1.

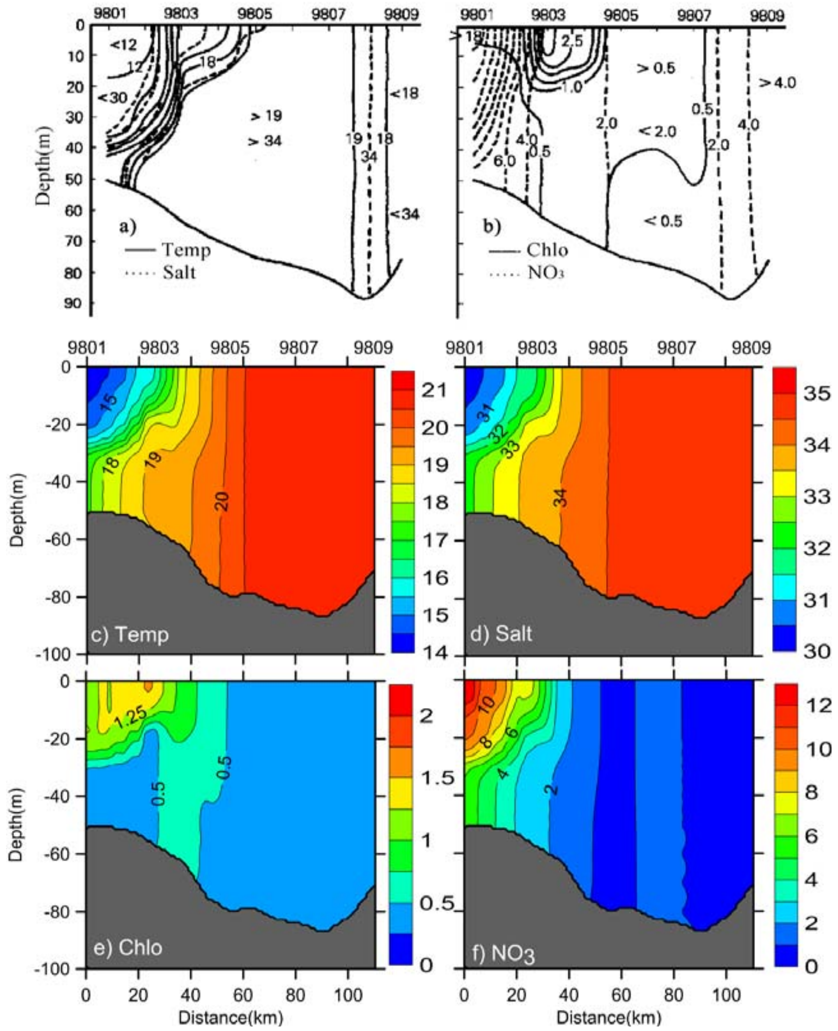

Fig 1. Comparison between in-situ observation (top panel) and the model result (middle and bottom panels) in1998.

Q-4. p. 14690. The NPZD model used is that of Fennel et al. (2006). A brief description of this model is required to convince the reader that it is appropriate. Also, what about the setting of parameter values? Were these all unchanged from the original Fennel et al. publication? If not, the authors need to say which parameter values have been changed and provide justification thereof.

Response: We give more detailed descriptions of the NPZD model and the parameters setting in the revised manuscript.

Q-5. Model forcing. I am left unclear as to whether the authors are proposing that their hypothesised mechanism of bloom formation occurs every year in the study region, or if it is specific to particular years. This is important as to whether the climatological forcing used with the model is appropriate. Worryingly, there is: "In the climatological case, the spring bloom is not reproduced due to the averaged northeasterly wind". Thus, the model does not meet the minimum requirement, to successfully reproduce the bloom. The obvious solution is to move away from climatological forcing and to select forcing data for particular years, particularly years where the hypothesised method of bloom formation is thought to occur. Instead, the authors rely on a "sensitivity experiment" in which the wind was reduced by 75%. I cannot see the justification for such a radical alteration in the forcing, especially when wind is a key component of the hypothesized mechanism of bloom formation. Unless this change can be properly justified, the whole modelling study appears to be flawed. Subsequently, the authors appear to base their case on this so-called sensitivity experiment.

Response: we accept your advice in the revised manuscript. The model is run with wind forcing from 6-hour NCEP data in 1998, and the comparison shows the modelling performance (Fig. 1). The model result (Fig. 2) resembles the result (Fig. 3) of sensitive experiment with artificial wind forcing, which represents the relaxed-wind situation. The artificial wind is linearly interpolated from climatological wind to 1/4 of the climatological wind in the TWS.

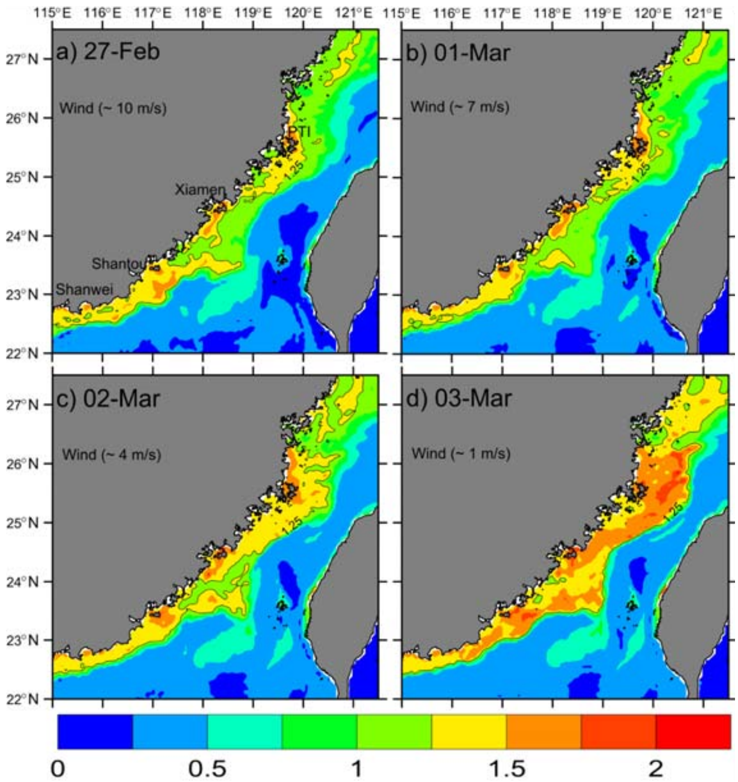

Fig 2. Simulated surface chlorophyll distributions in 1998 driven by NECP wind.

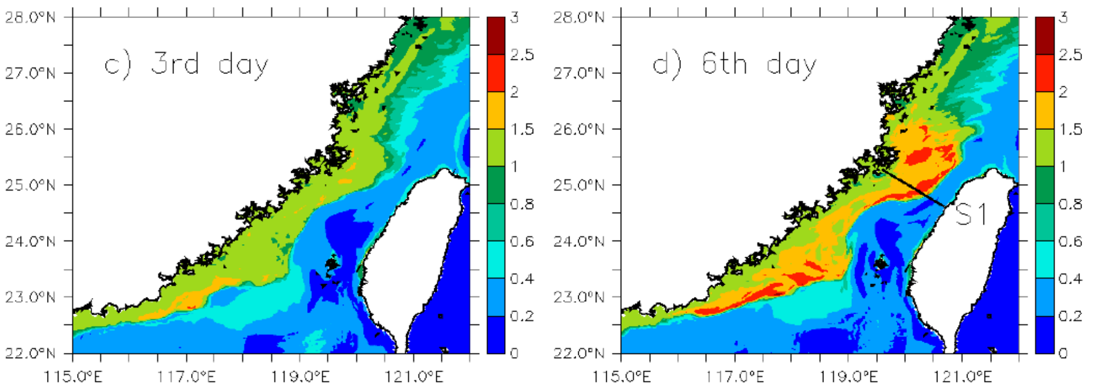

Fig 3. Simulated surface chlorophyll distributions driven by artificial wind in the sensitive case.

Q-6. The Model Results section (4) does not correctly focus on the hypothesis at hand. The model is made to reproduce the observed distributions of chlorophyll and then, by implication, the hypothesis is supposedly proven. E.g., on p. 14691: "Therefore, both the remote sensing data and model results support our hypothesis that the bloom is triggered by the relaxation of the northeast monsoon in winter". In order to address the hypothesis, it is important to drive at mechanisms, not just correlations. In fact, this is what the authors do in section 5 (Discussion). The material presented in sections 5.1 and 5.2 should have been the main material of the work, presented in the Results section and discussed thereafter in a Discussion section.

Response: Accepted. We move the some part from section 5 to section 4, focusing on proving the hypothesis.

Q-7. The authors make a play in the Introduction on their work being inspired by the turbulence convection theory of Taylor and Ferrari (2011). But section 5.1 just looks like classic Sverdrup theory to me. I found section 5.1 hard to follow and it certainly did not give any strong indication of how bloom initiation in this region actually occurs.

Response: The introduction is re-written. The Sverdrup's Critical Depth (Sverdrup, 1953) and Critical Turbulence (Huisman et al., 1999;Taylor and Ferrari, 2011) are two types of model, which follows the IM regime. The models are applied in the special oceanic situations through a series of assumptions. The Sverdrup's Critical Depth model links the bloom with mixing depth, and is generally applied to predict the vernal spring bloom in the deep ocean (Smetacek and Passow, 1990;Siegel et al., 2002;Nelson and W.O. Smith, 1991). In contrast, the Critical Turbulence model directly correlates the bloom and mixing processes, and it is suitable for the stratified seas. Taylor and Ferrari (2011) simplified the Critical Turbulence model in a two-layer water column and calculated the critical turbulence coefficient. We use the solution of Taylor and Ferrari (2011) to examine our result.

We modified the interpretation of the content in section 5.1 in the revised manuscript. The section 5.1 tries to demonstrate the IM regime from the terms in the chlorophyll equation (Eq.1) (Fig 5). The positive value in the net rate term means the chlorophyll increase, which is corresponding to the phytoplankton bloom.

$$\underbrace{dC_{hla}/dt}_{net\ \ rate} = \underbrace{K_T \nabla^2 C_{hla}}_{diffusion} + \underbrace{\mu \cdot C_{hla} - \left(g \cdot Z_{oop} + m \cdot C_{hla}\right)}_{net\ \ biological\ \ rate} \qquad \text{Eq.1}$$

where C_{hla} and Z_{oop} are the concentrations of chlorophyll and zooplankton, respectively, K_T is the diffusion coefficient, μ is the growth rate of phytoplankton, g is the grazing rate by zooplankton, and m is the mortality rate of phytoplankton.

In the strong wind (climatological wind) situation, the vertical mixing is intensified (Fig 4a) in the strait and the phytoplankton is mixed from upper to lower layers, which is corresponding to the obvious negative (positive) values in upper (lower) layers in Fig 5c. In contrast, the vertical mixing (Fig 4b) is inhibited in the relaxed wind situation, corresponding to the small diffusion rate in Fig 5d. Then, the growing phytoplankton (Fig 5b) can stay in the upper layers corresponding to the positive value in the net rate (Fig 5d).

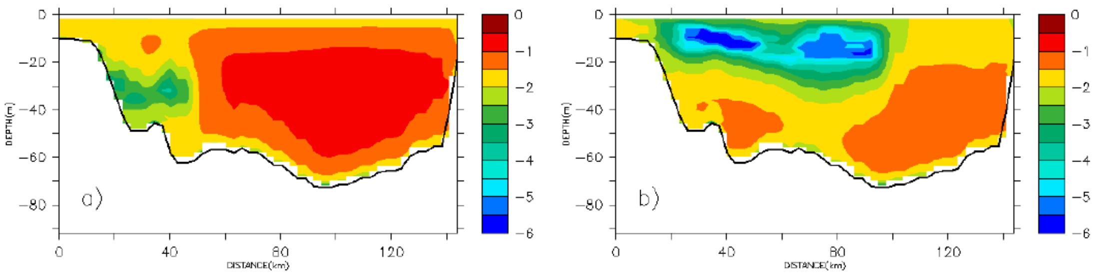

Fig 4. Logarithm of vertical diffusion coefficient (units: $m^2\ s^{-1}$) along section S1 in the climatological case (a) and relaxed-wind case (b) on the 6th day.

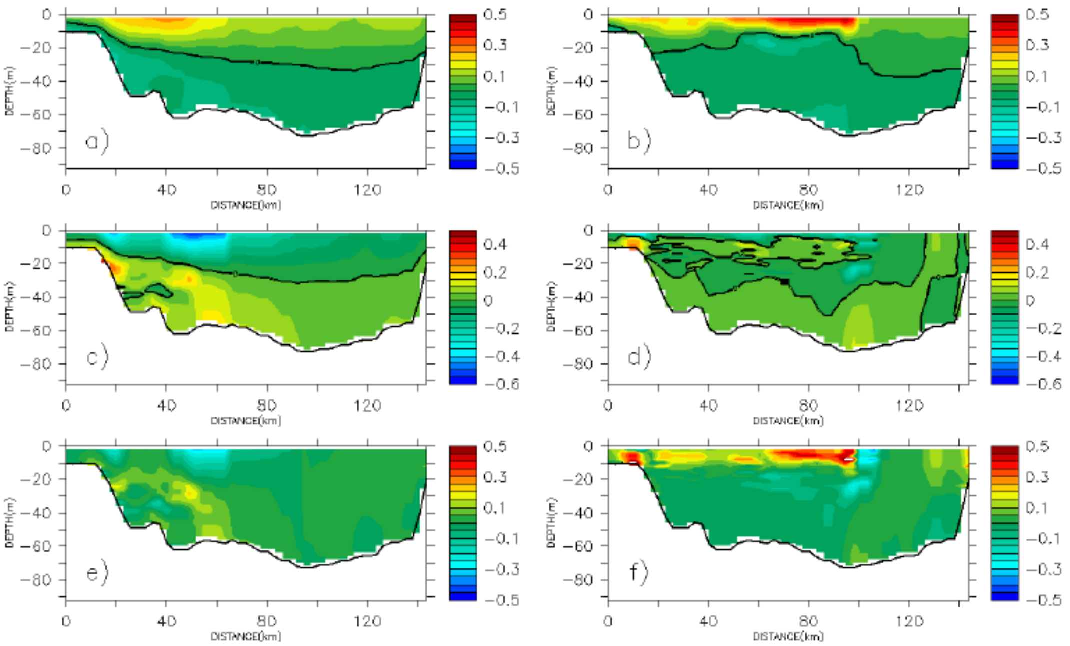

Fig 5. Distributions of the net biological rate (a, b), diffusion rate (c, d), and net rate (e, f) in Eq. (1) along section S1 on the 6^{th} day (the thick line is the zero contour). Left panel: climatological case; and right panel: relaxed-wind case, units: mg m^{-3} day^{-1}.

Q-8. So then we reach section 5.2 which is indeed "Application of typical turbulence theory". Yet section 5.2 is incredibly short and I was left wondering how this theory had been tested in the study region. The main hypothesis is articulated again in this section: "the relaxed wind triggers the cold, fresh, nutrient-rich MZCW to veer off the Chinese mainland coast". Yet I find it hard to reconcile this with the turbulence theory the authors talk about. In general, the authors just do not make the link between cause and effect as regards the bloom, which is the main topic at hand.

Response: As the response for Q-7, we quantitatively examine our hypothesis by using critical turbulence coefficient in the two-layer model (Taylor and Ferrari, 2011). We will add the discussion about the reasonability of the model application in the revised manuscript.

In the study, we confirm that the stratification in the TWS is formed by the offshore transport of the cold, fresh, nutrient-rich MZCW. Both the stratification and the direct decrease of surface turbulence input can weaken the mixing, however the model result shows the stratification plays the main role in weakening the mixing. Based on the IM regime, we agree the mechanism for the bloom initiation is inhibited mixing, which is the hydrodynamics responses to the relaxed wind, i.e, the MZCW transports offshore, leads water stratification and inhibits the water mixing.

In summary, this ms is all over the place in terms of focus and structure. I found it a frustrating read and was left wholly unconvinced regarding the modelling procedure (especially the forcing) and the consequent analysis and results.

Response: We fully consider your suggestion in the revised manuscript.

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