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Interactive comment on "Mechanism for initiation of the offshore phytoplankton bloom in the Taiwan Strait during winter: a physical–biological coupled modeling study" *by* J. Wang et al.

Anonymous Referee #1

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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, stabilising 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:

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.

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.

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.

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.

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 hypothesised 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.

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.

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.
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

BGD

10, C5147–C5150, 2013

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.

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.

Interactive comment on Biogeosciences Discuss., 10, 14685, 2013.