

***Interactive comment on* “Mechanisms controlling primary and new production in a global ecosystem model – Part II: the role of the upper ocean short-term periodic and episodic mixing events” by E. E. Popova et al.**

Anonymous Referee #2

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General Comments

I very much liked this paper. This paper shows the importance of using high frequency surface forcing and an adequate mixed layer model in physical- biological ocean models, and explains why (the mechanisms in Fig. 1; excellent!). The numerical experiments are appropriate, the results clearly described and the paper well written and organized. Consequently I think this paper will become the standard reference for justifying the use of 6-hour forcing.

Specific Comments

This paper contains no major errors, and only requires light editing. Great job! Nevertheless, here are a couple suggestions.

The run intercomparison was only done for 1 year (1994). Perhaps the surface forcing somewhere on the globe was anomalous in 1994. It would have been better to compare e.g. 10-year averages, for better statistical significance.

Regarding Fig. 9, Z/P in an imprecise diagnostic of grazing pressure; perhaps a more precise term would be the grazing rate (G_p) divided by PrPr. It would be an improvement if quantifiable terms could be developed for each of the mechanisms i-v, to quantify differences in light limitation, convective input of nutrients, etc. This should be considered in future work.

Minor Comments/Technical Corrections

Abstract, l 8-10: Since many people will first only read the abstract, the key mechanisms that cause the changes in PP should be explained here. For example, "a 30% reduction in equatorial areas due to increased light limitation resulting from night-time mixed-layer deepening...a 25% reduction at moderate and high latitudes primarily due to increased grazing pressure resulting from late-winter stratification events...a 10% increase [etc]".

p 1118, l 20-21: KPP allows an "advanced representation of water column mixing", not stratification.

p 1118, l 23-24, "Compared to...thermocline.": I don't think it has been conclusively demonstrated that KPP is superior to the Mellor-Yamada 2.5-scheme. Better to just say "KPP produces a realistic exchange...".

p 1119, l 2, "(see discussion in PC06)": probably change this to "(see PC06)", to avoid confusion with "see Sec. 5 Discussion in PC06", as the Discussion section in PC06 doesn't talk much about spring shoaling.

p 1120, l 9(4): "influence on ecosystem dynamics"

p 1121, l 21(16): "night-ttime"

p 1124, l 11(6): "a light lincrease" -> "a small increase"

p 1130, l 21(16): "primary production. This overestimation"

p 1131, 2nd line from bottom: "limitation"

p 1132, last 4 lines, "Predicted primary ... of forcing.": Perhaps also because the nitracline is deeper than the maximum UML.

Fig 1: Mechanism "iii" is shown in panel (d) and mechanism "iv" in panel (c); these should be switched. In panel (b) it would make more sense (in the chain of cause-and-effect) to have the N plot above the P plot (as in panel (a)). In the Caption, under (d) "thin" should be changed to "thick"; under (e) "limitation".

Fig 7 and 9 Captions: It should be mentioned here that the units are normalized, and to see the Fig. 5 Caption for the explanation.

Fig 8: Please explain which colorbar is which simulation.

Fig 10 Caption: Please give the units. Also, I think the control run is in black and the monthly run is blue, not vice versa. I am not sure if the green and red captions were accidentally swapped also.

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