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***Interactive comment on* “Regulation of phytoplankton carbon to chlorophyll ratio by light, nutrients and temperature in the equatorial Pacific Ocean: a basin-scale model” by X. J. Wang et al.**

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The manuscript "Regulation of phytoplankton carbon to chlorophyll ratio by light, nutrients and temperature in the equatorial Pacific Ocean: A basin-scale model" by X.-J. Wang et al., uses an empirical relationship between the carbon:chlorophyll ratio, growth rate (under non-light-limited conditions), and depth (or light level) to separate the effects of nutrients, temperature and light on the distribution of chlorophyll and the carbon:chlorophyll ratio in the equatorial Pacific. The empirical parameterization has been obtained by a linear fit of the C:Chl ratio in the surface observations of Le Bouteiller (2003) to growth rate and then a linear fit of the change of the C:Chl ratio

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from its surface value to depth. This approach has some similarity to the approach of Chloern et al. (1996). The main result of the paper by Wang et al. is that temperature has only a small effect on C:Chl distribution in the equatorial Pacific, while most of the variation is due to different nutrient regimes at the surface (nitrogen and iron limitation) and to photo-acclimation with depth.

The topic of the paper is important: the conversion of satellite-derived chlorophyll values to biomass is an important intermediate step in using the vast amount of satellite data to gain information on biogeochemical fluxes in the ocean. It is therefore well-suited to a publication in Biogeochemistry.

However, I have a few criticisms on the way that the authors present their parameterization and results, and suggest to publish the paper after some minor revision.

Firstly I think that the authors do not give enough justice to other works that attempt to describe variations in phytoplankton C:Chl. Their parameterization is really quite similar to the parameterization of Chloern et al. (1996, An empirical model of the phytoplankton chlorophyll:carbon ratio - the conversion factor between productivity and growth rate, *Limnology and Oceanography* 40, 1313-1321), but neither is this work cited nor are the differences and similarities between the two parameterizations discussed. Also the authors state referring to previous parameterizations (citing explicitly Geider et al., 1998) "Most of these approaches ... prescribe the relationship between the C:Chl ratio and temperature dependence of growth rate because of a lack of observations to parameterize the nutrient dependence. Thus, the combined effects of light, nutrients and temperature on the phytoplankton C:Chl ratio are not well known, and not yet quantified". While I agree that the combined effects are not well known, at least the model by Geider et al. (1998) shows some reaction in equilibrium C:Chl to nutrient levels. The papers by Pahlow (2005, Linking chlorophyll-nutrient dynamics to the Redfield N:C ratio with a model of optimal phytoplankton growth, *Marine Ecology Progress Series* 287, 33-43) and Smith and Yamanaka (2007, Optimization-based model of multinutrient uptake kinetics, *Limnology and Oceanography* 52, 1545-

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1558) are also interesting in that respect.

Secondly it is unclear to me, whether the variable C:Chl ratios from the parameterization enter the prognostic equations for phytoplankton growth by changing the slope of the photosynthesis-irradiance curve or whether it is a pure diagnostic relationship that only affects the conversion from nitrogen units to chlorophyll after the model run. Maybe this information is contained in the cited paper by Wang et al. (2008), but it would be helpful to have that information here in this paper, since it affects the interpretation of the results.

Thirdly, while the modeled chlorophyll distributions look quite good, it would be helpful to see what effect the variable C:Chl ratio has on the distribution of the chlorophyll field, e.g. by comparing it to a chlorophyll field generated with a constant C:Chl ratio. The question is whether a variable C:Chl ratio gives a better fit to Chl observations than a fixed one. In principle this could be simply done by showing modeled phytoplankton N, converted with a constant average N:Chl ratio, i.e. without a new model run. However, if the model uses the variable C:Chl in the prognostic equation for phytoplankton, the authors might also consider re-running the model with a fixed C:Chl ratio, if that can be done without too much additional work.

Finally, the authors use a constant (Redfield) carbon to nitrogen ratio to convert from their nitrogen-based model to carbon units and then finally to chlorophyll. This is a perfectly justified approach, but since the modeled region encompasses quite different nutrient regimes, it is maybe worth a short discussion whether C:N might also vary here.

I have no other minor comments than the two other referees, except:

Figure 1: The linear fit of C:Chl ratio to depth is probably o.k. for the purposes of this study, but looking at the data one gets a hint of a systematic curvature in the data that is not present in the fit: C:Chl are slightly underestimated by the fit near the surface and at depth, while they are overestimated in mid-depth.

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