Biogeosciences Discuss., 6, C2031–C2033, 2009
www.biogeosciences-discuss.net/6/C2031/2009/

Biogeosciences Discussions

Interactive comment on "Optical characterization of an eddy-induced diatom bloom west of the island of Hawaii" *by* F. Nencioli et al.

Anonymous Referee #1

Received and published: 16 September 2009

The dataset presented in this study illustrates quantitative and qualitative changes in the composition of particulate matter / phytoplankton with respect to mesoscale patterns (and along the vertical). It shows that optical proxies have the potential to resolve biogeochemical variables or processes that are hardly achievable through classical sampling strategies. This general topic as well as the data to support it a priori would deserve publication. Nevertheless, I presently have two main comments / interrogations that need to be properly addressed before the paper can be recommended for publication. (1) The first one deals with one objective of the paper and which is summarized in the last sentence of the abstract Âńthis is a further indication that Chla / cp might not be a good alternative to the backscattering ratio for investigating changes in particle composition with depth in Case I waters" . This sentence leads me to think that one of the basic questions of the paper was not properly addressed with respect

to the literature on the topic. We indeed are not expecting that Chl/ cp can be an alternative to bb/b ratio in case I waters. Back to Kitchen and Zanelveld (1990) and Morel et al. (1993) we know that photo-adaptation is the main driver of Chla cell-1 in tropical stratified environment. Given the strong stratification there is indeed an increase in Chla cell-1 with depth while the cp (POC) cell-1 remains much more stable, if not constant. So I am wondering if this paper is not trying to re-demonstrate, through nice observations, that much of the recorded variations along the vertical are driven by photoadaptation. At the same time bb/b varies with the size (or the refractive index) and is expected to be much less sensitive (if at all) to photoadaptation. Therefore while both proxies are very likely complementary in case II waters where photoadaptation is not important, this not expected for clear Case I waters. So I believe the general "philosophy" and orientation of the paper should be revised acknowledging what is already known and focusing only to new observations and their interpretation. (2) The second comment deals with the estimation of Chla. How is Chla calculated to draw Figure 3? Is figure 3 a subset of Figure 2? From Necioli et al. (2008), Chla is derived from calibrated Fluorescence. Is the Chla in Figure 3 derived from another calibration of the FLNTU used as part of this study? From the Landry et al. DSR II paper, the surface Chla is around 0.1 mg m-3. Your Figure shows that surface Chla concentration is more than 2 times less than this value (<0.05 mg m-3), which appears certainly too low for the NPG. Either there is a calibration problem for surface samples or there is an issue of fluorescence quenching (at what time of the day were the stations?). In any case this put some doubt on the Chla profiles and thus on Figures 9 and 5. This issue deserves to be properly addressed.

Other comments âĂć Background. I am a little bit puzzled by this section which is very important for the understanding / interpretation of what follows. A large focus is put on the core of the eddy and on the three layers (including the two diatom layers). Maybe this section should be re-included later in the discussion section. âĂć Because you use an ACS, it might be possible to extract the height of the signal of a676 as proxy of Chla biomass. See paper by Davis et al. with an AC3. This might help to retrieve

Chla profiles. It might also be that the fluorescence Chla vs a(676) Chla would be an interesting proxy for phytoplankton quality (due to package effect). Furthermore, why not use data from the ACS to get more information with respect to the qualitative nature of the assemblage and photo-adaptation processes (see Eisner et al., 2003, L &O) âĂć I am not convinced by your tentative interpretation of remineralization as responsible for high Chla / cp at depth (you do not have enough argument). Another simple explanation is that photo-adaptation still continues below the DCM (increase of Chla cell-1) while the overall biomass (cell density) and cp begin to decrease. Indeed, below the DCM there is still (admittedly little but sufficient) light for allowing maintenance of phytoplankton biomass, highly adapted to low irradiance. âĂć The bbp 700 profile match the Chla profile for cast 31. Both measurements come from the same instrument (I suppose, but see comments for Chla). At the same time the cp 650 profile is different (broader pic). Could this be a dead volume/mixing within tube related issue?

Interactive comment on Biogeosciences Discuss., 6, 8075, 2009.