In this paper, the authors carry out a nutrient amendment experiment using the alga *Phaeocystis globosa*, and describe the patterns observed in various bulk, optical, and physiological properties. Most of the experimental details were published previously in Peperzak et al., (2011).

There are several fundamental problems with this paper which lie primarily in the interpretation of results, rather than experimental design.

First, the paper attempts to cover too much ground. In an effort to be comprehensive (presumably), the authors have measured myriad quantities, without giving too much thought as to why. For example, this is a ~1 week batch culture in which algae are subjected to rapid and extreme changes in growth conditions. The changes in nutrient conditions are well-documented (however, I have concerns about iron, Fe; see below). However, the additional change in light environment alone (due to accumulation of chlorophyll biomass during the experiment; Chl goes from ~0 μg/L to ~30 μg/L) would be enough to drive patterns in any of the physiological quantities (e.g., Chl:C). Given this, why is it surprising that none of the physiological parameters correlate with the growth rate? We know with certainty that in an acclimated culture (e.g., the classic work of Laws and Bannister, 1980), C:Chl scales linearly with growth rate. Further, recent work has challenged the idea that Fv/Fm should tell you anything about growth rate at all, and is in fact invariant with growth rate (The authors correctly cite the work of Parkhill et al. (2001) and Kruskopf et al. (2006) on this matter, but I would add recent papers by Schrader et al. (2011) and Halsey et al. (2012)). Last, the idea that the fluorescence quantum yield (ϕ_{ph} here) should tell you anything about growth rate or production seems far-fetched as well (see recent papers by Behrenfeld et al., 2009 and Huot et al., 2013).

Why is no attention given to limitation by iron? Is it assumed that the 0.2 μm FSW used is iron-replete? If this is not the case, Fe-limitation will significantly impact all of the physiological quantities monitored (C:Chl, Fv/Fm, ϕ_{ph}).

Finally, and perhaps a bit philosophically, there is in many ways, nothing new here. I don't want to downplay the amount of work involved, but if you set aside the optics for a moment, then all we are looking at is a short time-course batch culture and its response to nutrient addition. What is new and interesting? Is it that there are significant changes in biomass and physiology following perturbation? How can this information be used to better inform remote-sensing analyses? It just seems a little lacking in motivation and novelty. I do not want to be too negative, but I encourage the authors to think deeply about what distinguishes this piece of work and why the community would be compelled to read it. If, for instance, the primary conclusion is that neither C:Chl, Fv/Fm, or ϕ_{ph} correlate with growth rate, and that models using chlorophyll are inherently flawed, what should we be using to improve models of phytoplankton growth/production? If you cannot comment on this, well then, we already know most of what is presented in the results.