Reviewer #2

This is a very nice, well-written paper that describes a continuous trait model and its application to data from two stations. Continuous trait models have some nice advantages over the traditional discrete methods that are typically used, as the authors describe nicely, and this model may provide an interesting and insightful counterpoint to standard methods when applied to 3D models, as the authors state they plan to in the future. I recommend acceptance after minor revisions.

[Response] Thank you for your positive comments on this manuscript!

I have one major criticism of the paper, which could be resolved easily enough in the Discussion section. The criticism is that the model may be sensitive to the lognormal distribution assumed by the authors. The model relies on describing the distribution of traits by the log-mean and the log-variance of volume. Making size the master trait is standard, and discussed by the authors, but they do not even mention the word 'lognormal' until page 18. This first of all leads the reader to assume that the authors are using a Gaussian distribution for volume, which would be disastrous, unless one reads carefully that they are considering the log-volume, which cannot be expected of every reader. Readers are likely to get confused without clarifying this earlier) The authors also cite a few papers (the oldest of which is from 2007; what did people think the size distribution of plankton was before 2007?), along with the standard use of Gaussian distributions in continuous-trait models, to justify their use of a lognormal. Arguably, a power-law distribution (sometimes referred to as a Junge distribution in this context) is much more commonly assumed in these situations, yet power laws are not discussed by the authors anywhere in the paper. Additionally, power-law distributions are often not well-described by their lower-order moments, which may in fact be divergent, and are instead better described by their lower cutoff and exponent. The authors do state that the use of a lognormal is a key assumption in their model, but they should also state that they are using a lognormal from the beginning, not just at the end, and they should also state that other size distributions are often used to model phytoplankton, and discuss how e.g. the model might look different if one were using a power-law distribution, and what this means for the authors' results. Don't get me wrong; if I had written this paper, I certainly would have started with a lognormal, because this is an easy, defensible place to start. It just merits further discussion that this is perhaps not the size distribution most people think of phytoplankton following, and that this may have (possibly large) implications for the results and how the model is set up.

[Response] The reviewer is entirely correct that phytoplankton size distribution is more commonly assumed to follow a power-law distribution (Sheldon et al. 1972; Gin et al. 1999; Cermeño et al. 2006). The slope of log abundance versus log size (e.g. biovolume) tends to be between –0.7 and –1 (Cermeño et al. 2006), suggesting that the slope of log biomass versus log size should be between 0 and 0.3. However, aside from fact that the power-law distribution is unrealistic in predicting phytoplankton biomass at the limit of either the largest or the smallest size, it is much more inconvenient in terms of mathematical manipulation (e.g. calculating mean and variance) compared to the normal distribution. We have included these points in the discussion in Section 4.2.1.

Relatedly, equations for Gaussian moments cited from other models are not necessarily directly applicable to log-moments of lognormals, because a 1/x can pop out from taking the derivative of a logarithm (e.g. see the equation/definition of the lognormal vs. the Gaussian). I did not check every equation in the paper for this - the authors seem to have been very careful so I do not expect this is a problem - but it is worth mentioning.

[Response] Actually, calculating the derivatives does not involve taking the derivative of a logarithm because the growth rates are directly dependent on log cell volume (see Eqn. 10), not on cell volume itself. In other words, in our model equations, we only have log volume and we do not have any calculations on cell volume directly.

Besides this criticism, the paper scores well for significance, quality, reproducibility, and presentation. I have some specific comments below:

page 3, line 7: What is meant by 'size' here? Presumably diameter, but it should be stated, especially given the importance of 'size' to the rest of the paper, where size refers to volume.
[Response] Thanks for pointing it out. We have modified it to "ESD".

page 4, line 27: Might be good to have a reference for the sentence that starts on line 25 and ends on line 27: : :

[Response] We have added the references "(Matsumoto et al., 2014; Wakita et al., 2016)".

page 5, line 10: The authors sort of 'jump right in' to the terms and equations of the model here; an overview paragraph might be useful. What is the model trying to do? How is this accomplished? This is somewhat described in the abstract and in the Introduction, but a more detailed description of the entire modeling procedure could help some readers follow the paper much better.

[Response] This is really a helpful suggestion to improve the readability and overall organization of the paper. We have added an overall summary at the beginning of Sect. 2 (Model description).

page 5, line 12: What are the units for P (and for 'fer' below)? If they are what I think they should be, doesn't that make it obvious why Pˆ2 v is the term to be considered, rather than P v, so shouldn't the sentence after next be removed?

[Response] We have added the units for P, and fer. However, we do not think that the units themselves can justify the use of Pˆ2 v instead of Pv. We have followed the pioneering work of Bruggeman (2009) to use $P\left(v + \bar{l}^2\right)$ as the tracer for variance to be transported.

page 16, line 20: This is an interesting idea - can the authors flesh it out a bit more?

[Response] Yes, we have explained it in more detail in the Discussion section 4.1.1: "In particular, the second derivative of the growth rate at mean size, $\frac{d^2\mu(l)}{dl^2}$, can be conveniently perceived as a proxy for the intensity of resource competition (The more concave is the curve of $\mu(l)$, the more intense is the competition since the fitness of suboptimal species decreases more steeply with distance from the optimal size)."