Ocean Science
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Interactive comment on "Constraining parameters in state-of-the-art marine pelagic ecosystem models – is it actually feasible with typical observations of standing stocks?" *by* U. Löptien and H. Dietze

Anonymous Referee #2

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Summary: The authors report on a series of twin experiments with a highly simplified NPZD model where they add various degrees of noise to the synthetic data and optimize for the full parameter set and subsets. The study is rather modest in scope and very superficial in its presentation. The authors did not take advantage of and build in a meaningful way on the considerable insight into the topic of biological parameter optimization that has accumulated already in the literature. While they have cited a few key papers, they appear to have missed, or chosen to ignore, many of their main points. For example, there are systematic methods for quantifying parameter depen-

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dencies and uncertainty, several are even cited, yet the authors don't employ any of these methods and instead describe their results at a naïve level. As is, this study adds nothing new or remarkable and I can't recommend publication.

Below I'm listing some of my points of criticism.

Abstract: "The respective MM constants, along with other model parameters, are usually tuned by trial-and-error exercises where the parameters are changed until a "reasonable" similarity with observed standing stocks is achieved." This is a naïve and inaccurate characterization of common practice. Formal parameter optimization is a standard approach in biogeochemical modelling, not trial-and-error.

(page 233, lines 9 - 20) The attempt to redefine parameters in order to "simplify" and "guide" the optimization algorithm and to make the optimization "computationally more efficient" is complete bogus. As the authors state on line 17-18, the same number of free parameters is optimized. Hence, nothing is gained by combining the three parameters into the new mu_net (there are still three interrelated parameters being optimized). This is just smoke and mirrors and doesn't serve any purpose other than to potentially confuse the readers. The authors should instead stick to optimizing mu, m and m_PD and be straight about it. The same arguments hold for g_net.

On page 234 (lines 9-10) the authors speculate that although their model set-up is not directly comparable to 3D biogeochemical ocean models, their conclusions are so "generic" that they "probably apply even more so" to the realistic models. This is rather unsatisfactory. The results presented in section 3.1 do most likely not apply to realistic models, and the discussion in the following sections does not provide insights that are new or directly applicable to more realistic models.

The discussion in section 3.2 is very naïve. The issue has been discussed at length and in much more depth already in the parameter optimization literature. The same comment holds for section 3.3. That NPZD model parameters are dependent is well known.

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Page 239 (lines 5 - 7) "In order to test whether the optimization algorithm got trapped in a local minimum, each experiment comprises an ensemble of five identical parameter optimizations." It doesn't make much sense to repeat the identical optimization 5 times. Instead the initial parameter set should be perturbed in the replicate optimizations. A few lines later it becomes clear that the authors actually did this. Page 241 "If it would, the noise added to our synthetic "observations" would induce a cost of 0.086 mmol N m-3" It's not clear at all where this number is coming from.

Interactive comment on Ocean Sci. Discuss., 12, 227, 2015.

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