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Interactive comment on "EMPOWER-1.0: an Efficient Model of Planktonic ecOsystems WrittEn in R" *by* T. R. Anderson et al.

Anonymous Referee #2

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EMPOWER - 1.0: an Efficient Model of Planktonic ecOsystems Written in R

by T. R. Anderson, W. C. Gentleman, and A. Yool.

General comments:

This manuscripts explains the technical details of a simple NPZD model that runs in a two-layer vertical setup. The authors claim that using simple ecosystem models such the one described in the manuscript are still useful because one can run them very fast and then be able of evaluating how changes in equation formulation or parametrization affect the ecosystem dynamics. I can see the point of this argument and I somehow agree with it, although with some reservations. Personally I think that the dichotomy over "simple vs. complex" models is overstated and should not be a matter of too

much debate: in my view models are (or should be) "question-dependent" – simple models are okay to answers some questions such as biogeochemical cycles while more complex models are required to answer more ecologically relevant questions such as the effect of biodiversity on ecosystem functioning.

For those interested on community- or ecosystem-level properties (total phytoplankton or zooplankton dynamics, carbon or nitrogen cycle, etc.) using NPZD is good enough and probably better than using models that resolve phytoplankton or zooplankton diversity. NPZD have been around at least 25 years (Fasham 1990) and have proven useful to understand many aspects of ecosystem dynamics. Having said that I am not sure that simply coding another NPZD model deserves a publication in a journal such as GMD because I can't really see how this is going to move the field forward. Besides that, I find the article quite technical and therefore slightly boring. I did not find any relevant error or mistake in this work, but neither any major advance or originality. The manuscript can be seen as a very well written technical report. I leave the editor with the decision about if this work is within the scope of the GMD journal.

Minor comments:

- Page 54: The abstract should say at some point that the model is a simple NPZD configuration. It's not clear now until one starts reading the main text.

- Page 55 - Line 11: The code is "transparent" – What the authors mean by this? The simplicity of the code? No code is transparent and its simplicity is subjective anyways.

- Page 56 - Line 05: "They require expertise and time to set up". I don't find much difference between 0D and 1D models in terms of difficulty (3D are another story).

- Page 56 - Line 28: "Of course" – I think this statement is unnecessary.

- Page 57 - Line 03: "we submit" – I think this statement is unnecessary.

- Page 66 - Line 05: "density" – Do you mean plankton concentration? The density of water?

- Page 68 - Line 15: "kpar = f(bj,Cj)" – is not this parametrization too complex for such a simple model?

- Page 70 - Line 10: "Eqs(11) (12)" – I might be missing something but these equations appear to me as exactly the original Fasham parametrization.

- Page 78 - Line 09: "The NPZD model we have presented is a new one" – I honestly do not think that this NPZD can be called "new" at all. The code is new, the model is not.

- Page 80 - Line 29: "Averaging data across years ... to compare the model to data" – I do not agree with this. If the model is using climatological forcing, the data should be climatological as well. Just show average monthly outputs for the model to smooth out the bloom as well as it happens with the data. Or otherwise run the model using the MLD forcing from 1998 to 2013 and then average the model outputs to construct a climatology. The data are not measured daily anyways; usually sampling is once or twice per month.

- Page 81 - Line 04: "in this case 2006" – Why 2006 and not any other year? This is an arbitrary choice. One can then select the year or years that best fit the model output. I don't think this is a robust comparison

- Page 81 - Line 24: "varied +/- 10%" – Why such a small change? Sensitivity analysis usually perform +/- 30% or 50% change in parameter values.

- Page 83 - Line 02: "There is also a hint that ... 2006, this not particularly surprising" – This is not a valid argument (see my previous comments about climatologies)

- Page 83 - Line 12: Figure 11 uses year "2008" – Why the authors now select 2008 and not 2006? These choices look too arbitrary to me.

- Page 83 - Line 18: "grazer controlled phytoplankton in iron limited ecosystems" – The current consensus is that phytoplankton in HNLC is more controlled by iron limitation than by grazers and I personally agree with it.

- Page 83 - Line 25: "Vmax acting as a proxy for iron limitation" – This is way to crude. If the model does not resolve iron cycle it should not be compared against HNLC regions.

- Page 84 - Line 20: "It is perhaps unsurprising ... curves are similar" – Why then bother doing a sensitivity analysis?

- Page 86 - Line 09: "The sensitivity shown here is at least as great as that for the choice of P - I curve" – Which you say was quite low right?

- Page 87 - Line 12: "Many models do not include a non-linear phytoplankton mortality" – Using a squared mortality term amounts to imposing a carrying capacity.

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