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Interactive comment on "CSIRO Environmental Modelling Suite (EMS): Scientific description of the optical and biogeochemical models (vB3p0)" *by* Mark E. Baird et al.

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This manuscript offers a description of the Environment Modelling Suite (EMS) developed over several years in Australia by CSIRO. It is based on a set of existing publications, as well as some on-line documentation. Despite the limited amount of original material included in this manuscript, I see the value of its publication, because it would offer a single point entry for new users interested in approaching such a complex suite. It would also be a supporting reference document for further scientific applications in other regions.

1 Main comments

I have some major concerns, that I would like the authors to address before resubmitting the manuscript

1. It is a very lengthy manuscript, that does not have distinct points of entry. In its current version, it needs to be read sequentially in order to appreciate the various components. I would suggest the authors to separate the pelagic from the benthic component, especially because the community of scientists is rather different, and this would also allow to clarify some aspects of the coupling with the transport processes that are a bit overlooked. This would help the referees to focus more on the original components of the model because they would be closer to their expertise, and would provide a more informed assessment.

2. In the current state, it looks more of a hybrid combination between a user manual, a scientific model description, a technical report and a summary of model applications. I would suggest the authors to better clarify their aims and decide which approach is the main one. In particular, it is not clear how the authors decided to include a full description of certain aspects, while for others they refer the reader to published literature in toto. Three unique features are listed in the Introduction (pag 6), but it is not clear how innovative they are with respect to other published works (e.g. Dutkiewicz et al., 2015).

3. I am concerned about the lack of discussion on the model science and what differentiates it from the other available open-source models. There is a cursory introduction on how the model differs from other approaches, although they are lumped into being Fasham-like, which is actually inaccurate, since several models consider stoichiometry and the internal storage of nurient and energy (for instance Lancelot et al., 1993,2000; Baretta et al., 1995; Vichi et al., 2007). Aumont et al., 2015 and Butenschoen et al 2016 are indeed referenced, but to remark the need for thorough description of models). Rather strikingly, only by looking at Table 6 the (skilled) readers understands that

the main currency of the model is nitrogen and that biomass is measured in N concentration units. I am not suggesting to have an additional lengthy discussion on the type of biogeochemical models, but just to make the reader aware of what are the peculiarities of this approach with respect to others. The scaling or geometric constraints are indeed a special feature of this modelling approach, although it should be clarified that models with multiple functional types also includes implicit size considerations in the value of the parameters . The concept that geometric description is a mean to reduce parameter uncertainties (pag 4, L1-3) would also require further clarification, especially because this model implements only two size classes with generic functions, which means it has a limited range of applications in the coastal region. If the Si or Fe cycles would need to be resolved, or specific harmful algae, then additional parameters would be forcibly needed.

2 Detailed comments

P6L14-15 There should be some description on what Fig. 3 shows. Since it is used in the introduction, I would expect some more context. However, the authors suggest me to go and read Baird et al. 2016 to understand the figure (and what is GBR4 at this stage?) Sec 1.1: This is the most important section when engaging with a manuscript of this size. However, it offers only a quick list of the upcoming sections, something akin to what is offered in shorter manuscripts. The authors state that descriptions are sorted by processes, but I would argue with this statements, since some processes are spread across various sections and there are several cross-references that interrupt the flow of the description. I would also strongly advocate against the offered solution to the reader (L26-28): to combine all the various process terms to obtain the complete differential equation. This is in my opinion what makes the manuscript more difficult to read, since there is no full appreciation of the dynamics of each state variable. This approach was also followed by Vichi et al. (2007), but in that case, a specific notation

was introduced and the full dynamics were presented.

Sec 2. I have a few problems with the organization of this section. The EMS biogeochemical model has not been introduced yet (while the next Fig. 4 and 5 are about the biogeochemical model and not the optics), but the reader is offered an initial description with no references of the science of IOP and AOP. I would suggest to invert the order and first illustrate the model structure, then highlighting the details of the optical model. Otherwise, the authors can opt for a shorter manuscript that would focus on the optical model only if they consider this the most innovative component. At Pag. 9 L19 microalgae are mentioned, but there is no mathematical equivalence neither an explanation of what small and large means. I recognize that there are tables later in the model where size classes are provided, but a set of ranges should be given from the beginning, especially because of the emphasis on geometric constraints.

P10L12 The authors should state what kind of approximation they make when considering dissolved and particulate concentrations of pelagic variables affected by transport processes. The basic approximation of fluid dynamics is the continuum hypothesis, which should also be considered for biogeochemistry (e.g. O'Brien and Wroblewski, 1973; Vichi et al., 2007). I understand that this is an aspect that was overlooked in the early works (Nihoul, 1975; Fasham et al, 1990, etc), but it is nowadays essential given the increasing resolutions of hydrodynamic models.

P10L17 I would suggest the authors to give information on whether the model has been coupled with other hydrodynamic models.

P10L22 This is one of the many cases where the authors start an explanation and then drop it abruptly referring to published papers (see my main comment 2 above) Sec 3.2. What is the difference with Sec. 2.1 and why two separate sections are needed?

P23L18 Please define a “function group”

P23L23 I think that the concept of internal reserves is an essential one to understand the equations. Nevertheless, the authors refer to Fig. 3 in another paper (see main comment 2)

P24 Table 4 caption: the authors should explain what they mean by cells here. Mean population characteristics? This is not introduced anywhere in the main text.

P24L7-8 and L10 The variable R* is introduced and used without any context. I would suggest to show an equation defining R* instead of a figure.

P25L7 Menten

Sec 3.3.3 I am struggling with this section. There is very little structure in the description. I can follow the flow because of my experience in numerical models, but to my understanding this manuscript should introduce the model to a larger audience and expand its usage beyond the group of developers (main comment 1 above). I think the authors need to make a decision on what is the narrative approach they want to have, either from the point of view of the optics or from the biogechemistry and ecosystem viewpoint. I understand that from the point of view of an optical model, the absorption cross section is independent of the physiology and definition of phytoplankton. However, treating separately eq 6 and the variable rho does make the reading more difficult. At the risk of being pedantic, I think one should first present the dynamics in eq 37 and then illustrate the various terms. I'm particularly thinking about a student user who would like to learn the model and who may not have a full understanding of the underlying physiology.

P27L20 and L28 Table 6 is referenced before Table 5, which is the one listing the state variables whose dynamics is listed in Table 6. I would strongly suggest that a full list of the state variables is given at the very beginning when the biogeochemical model is introduced. In L20, Tab 5 presents state and diagnostic variables, not equations.

P28L4 Please refer to the eq numbers and not just the table.

P30L2-3 This is an important information that should be given in the introduction, and

briefly expanded upon to clarify how this models is positioned in the context of the existing theories and methodologies. Sec 3.6. The mathematical formulation and equations are very little detailed for this component. Table 14 containing the dynamics is just referenced, and there is not a single equation describing the biomass evolution. It is also not clear from the beginning that the zooplankton variable has no variable stoichiometry (or internal reserves).

P35L13 This is a coarse over-generalization which does not pay much attention to the model development occurred in the past 30 years. Models that use preference factors and a food matrix do not have this issue (Gentleman et al., 2003). Sec 3.6.1 Grazing is actually not illustrated in this section. Table 15 is just mentioned but the specific terms not described. It is not much clear what is the food web accessed by zooplankton, apart from the first generic sentence at the beginning of sec. 3.2. How the fluxes between the state variables are actually computed is not clear. Table 12 What is the difference between variable m_n in Table 7 and variable m_B? Sec. 3.8 I suggest the authors to make specific reference to the numbered equations and not to the Table containing them (also check the typo at line 9 same page "zooplankton plankton")

Sec 3.9 I would see the section on non-grazing plankton mortality to be more pertinent to plankton dynamics, and less to zooplankton grazing. Is zooplankton mentioned at L6 because this is a loss term for all the plankton? It is rather confusing, and proper structuring would be helpful. Sec 3.10 I guess the author means gas exchange at the surface of the ocean here.

P41L3 The variable is u_{10}. This is the cubic function.

P41L18 positive

P41L31 This seems a fragment with no connection with the previous paragraph.

P42L22 Please clarify what "vertical order" means.

P43L18 Please refer where the diffusivity values have been taken. I could not find a table with the values.

Eq113 and other. I would suggest the authors to use named constant for the stoichiometric coefficients, to allow identifying which conversion is actually being done. Also, the authors should briefly illustrate the rationale for the use of this multiple minimum function, which I guess is linked to the maintenance of the constant stoichiometry in this functional group biomass.

P49L1 This is another example of the main problem I have with this manuscript (main comment 2). The same can be found at L29 w=in the case of coral processes. The authors seem to have cherry-picked what should be described and what should be left for the reader to scavenge through the literature. Please explain if there is an underlying rationale or a unified criterion.

P49L6-7 These are microalgae, so I wonder why the authors decided not to use the same dynamics described earlier.

P57 How is M computed?

P58L5 With the use of coding style, the manuscript turns towards the user manual. This is the first time that code is used in the document. I am not against it if properly explained, but makes the manuscript less coherent (see main point 2).

P58L7-8 Make reference to Table 34 where the variables are listed.

Sec 5.2.1 I am a bit confused here, because light is not an environmental variable that controls the dynamics in the sediment model. What is the difference with Sec. 4.2.2? I understand that the optical model is a major part of this manuscript, but then I would separate the biogeochemistry from the optics in two different manuscripts and make reference when needed instead of mixing the descriptions (see main point 1). Sec 5.3.1 and 5.3.2 There is no reference to equation numbers in these short sections. Sec. 6 I understand that section 6 collects all processes that are common to the pelagic, epibenthic and benthic processes. Therefore, I would expect to find here

cross-references to the other sections where they have been described, as well as the inverse (when the dynamics are first illustrated, inform the reader that a certain term is considered a common process and found in Sec. 6).

P68L9-11 I agree with this sentence, but this would deserve some more discussion. According to the Introduction, the model is designed to be generic, and the combination of physical and biogeochemical processes may lead to stiffness (this is cursory mentioned somewhere in the text or in the captions). The authors can refer to Butenschön et al., 2012 for an illustration of the problem. I would also ask the authors to clarify what do they mean with the "time step of the splitting"? Different time steps can be used for the various steps, but I am not familiar with a splitting time step. (Please explain what GBR1-4 mean in the table)

P68L13 The choice of a 5th order ODE should be justified, especially in the case of empirically-derived parameterizations. Please also say here which scheme is used and that it includes adaptive stepping. This information is given further below somewhere.

P69L2-4 This justification raises some concerns. The method is explicit, which would actually lead to instabilities that would require a time step shortening. Is this what the authors mean?

P69L16 Please explain the term "between". Is this implicit or explicit? According to Table 42, it uses the same time step as the ecology, but not clear what light environment is used for the ecology.

P70L9-10 This sentence is not clear. I understand that the manuscript is not about the coupling between the physics and the ecology, but this sentence would require more context. Please refer to the table where the number of levels in the described applications are listed.

Sec 8 I would argue that this manuscript does not offer any model evaluation beyond what has been already published. Is the assessment done here a technical check that version vB3p0 produces the same results as vB2p0 described in Skerratt et al. (2019)?

Sec 9 and 10. I am not sure these sections help the manuscript concept, and they confirm my impression of the lack of coherence in the original idea of the presentation (point 1 above). I would suggest the authors to reconsider their structure and to move some sections to the appendix or to on line material.

Sec 11 I have some concerns about the content of this discussion. I would expect Sec 11.1 in the introduction. I cannot really see any discussion here as I have indicated in my main point 3 above.

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