Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-325-RC3, 2018

Interactive comment on "On biotic and abiotic drivers of the microphytobenthos seasonal cycle in a temperate intertidal mudflat: a modelling study" *by* Raphaël Savelli et al.

Anonymous Referee #3

Received and published: 5 August 2018

Savelli et al. Biogeosciences 2018 325

This is an interesting and well presented study that models the annual cycle of microhpytobenthos biomass and activity on intertidal mudflats on the Atlantic coast of France, and, using key environmental drivers of temperature and irradiance, and the biotic interaction derived from gastropod grazing (Peringia, prev. known as Hydrobia), show a three phase response over the annual cycle. The modelling is well presented and characterised, based on an extensive literature review of available sources and variables. The modelling supports a set of well accepted (if not always well described in the quantitative literature) assumptions about the main drivers of daily and annual MPB

production (light availability, temperature stress) and summer depressions of biomass due to grazing pressure. To some extent this is to be expected, as the model is constructed with a number of a priori assumptions, so providing the mathematics works, then once would expect to see the patterns that are produced. The modelling produces results consistent with earlier work, and a general acceptance that annual primary production on such mudflat systems is somewhere of the order of 120-150 g C m-2 y-1. The only outcome that I found did not fit my preconceived understanding on MPB dynamics was that Peringia grazing actually only had a significant effect on very few days over the summer. That is a surprise.

The weakness with the study is the lack of a parallel data set of primary variables (biomass, production, grazing density and pressure) with which to validate the model. There are a couple of periods when the model is shown to approximate to some corresponding field data, though there is an annual NVDI data set used to support the surface biomass aspects. The rest of the model is not validated. I think this is a problem because the extensive discussion implicitly relies on the model being correct, and then provides an interesting and well referenced discussion around various driving factors and other factors that may play a role. I think the authors need to validate their model using some other data sets, perhaps from some of the other mudflat systems that they have (and are) working on within the Atlantic / Channel seaboard, or resolved at finer temporal scales to demonstrate the robustness of the assumptions under pinning the model. After all, if the model works on one mudflat, it ought to be applicable to other similar systems, and this would really demonstrate its value to others workers in the field.

Some more specific points are made below:

Overall, what are the error terms around the modelled responses? The figures show some significant error terms in the existing field data, but no errors around the model outcomes.

P3, L15. “in the light of current knowledge….role still unclear”. I think there is a very extensive set of literature on the roles of abiotic and biotic factors for MPB dynamics, so this statement portrays a false sense of uncertainty. P5, L32. Given an extensive literature (some of which is mentioned in the discussion) about resuspension of MPB by wind/wave action, why was this not included in the model? The wind data were available, and weather-induced and tidal wash-away effects are shown to be significant in removing MPB biomass?

Figure 5 is an important figure. It needs to be made clear in the legend that this refers to S*. Why when the NVDI signal varies by over 100% in the course of the year, does the S* value only vary by at most 6-7%. Though the “pattern” looks the same (what is the correlation or correspondence between the two annual cycles?), the order of magnitude of change does not. How can this be, when they are assumed to be measuring the same thing?

P7, L9 onwards. The variable Ts is dependent on overall biomass, but then the outcomes of this seem counter-intuitive to what we know about biofilms and cell microcycling. Cells appear to spend the time they need at the surface to photosynthesise and accumulate enough carbon, while minimising their risk of photodamage. So each cell spending 54 minutes at the surface during January and August, while only 12 minutes in April, appears to be an outcome of an underlying assumption about biomass, rather than an understanding about diatom photophysiology and behaviour?

P7, L20, clarify if this is the assumed intrinsic growth rate?

P8, L14 onwards. This section appears to be saying that during the summer periods, the biofilms are light limited, because there are longer days? If this just a mathematical artefact? After all, an individual cell only needs some many quanta of light to meet its photosynthetic requirements, and with variable migration, lower biomass and longer days, why would individual cells be light limited?

P8 L30 and P15,L1 This is the one area I found surprising, given the number of published accounts of strong inverse correlations between Peringia (Hydrobia) abundance and biomass on NW European mudflats. Particularly when the authors have said in an earlier paragraph that during phase 2 light was limiting, which would make the biomass response even more susceptible to being grazed down? How convinced are the authors that this is a true situation, or is the model not capturing the real impact of grazers during this phase?

P13, L6, see Steele et al. Biofouling 30, 987 – 998 for a detailed study of EPS and desiccation on diatom photosynthetic capacity

P18, L3. What happens if a resuspension element is included in the model (Dupuy et al gives 3%, Blanchard et al 2006, in In J. Kromkamp [ed.], Functioning of microphytobenthos in estuaries: Proceedings of the microphytobenthos symposium, Amsterdam, The Netherlands, August 2003. Royal Netherlands Academy of Arts and Sciences, and Hanlon et al. 2006 Limnol. Oceanogr. 51: 79-93, provide other values, and de Jonge and van Beusekom (op. cit) provide some critical wind speeds)?

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-325, 2018.