Review of Karpouzoglou et al.: "Effects of floating (solar PV) platforms on hydrodynamics and primary production in a coastal sea"

The authors present a study of potential impacts of solar panels on primary production (PP) in the North Sea. They analyse the different factors and their individual and combined effects on PP in three different North Sea regions using a new parameterisation of floating panels in the 1D coupled physical-biogeochemical model GOTM-ERSEM-BFM. They find that up to a surface coverage fraction of 20% impacts on PP are relatively small, while PP drops significantly for higher coverage. According to their study, reduced light availability (due to surface coverage) is the main factor for PP reduction, while changes in mixing (due to wind shielding and platform friction) are comparably small. They conclude that the 1D model results likely overestimate the actual impacts due to various limitations of this type of model, and they recommend an implementation of the new solar panel implementation to a 3D model to achieve a better/more realistic assessment of likely impacts.
The general purpose and objective of the study is of high relevance, considering the importance of moving toward renewable energies and away from fossil fuel consumption. The manuscript is concise, generally well written and easy to (see comments below). The 1D model analysis is thorough and includes a sensitivity analysis of the platform's roughness height. However, I have one main criticism; that is—as also stated in the discussion/conclusion—the 1D model has significant limitations compared to a 3D model and, therefore, the study can only be considered a testbed for the implementation and sensitivity analysis of the new parameterisation, while conclusions on the actual likely impact of floating solar panels (or any other type of surface-covering platform) do not seem adequate to me. This should be made clear from the very beginning (including the title and abstract).
For the same reason, I wonder whether submitting the manuscript to Geoscientific Model Development (GMD; https://www.geoscientific-model-development.net/) would be more appropriate? However, that would require the paper to be turned into a bit more technical description of the model.
I recommend reconsideration for publication (possibly in GMD) after moderate revisions.

General points
It needs to be stated clearly in title, abstract and at the end of the introduction that the present study is only a test case and does not allow for sound conclusions on the actual impacts of solar panels on the North Sea (or at best provides an estimate of the upper limit of their impact).

There is no information on the dimensions of solar panels to be deployed in marine environments (and distances in between them) in the manuscript. That makes it hard to get an idea about the transferability of the 1D model results to a real-world application. Factors like "patchy" light availability/light scattering (depending on the size of the panels) **inside** the solar parks in combination with advection/and mixing would likely result in a weaker reduction in PP than simulated for the 1D case (and presumably than in the 3D case as well as the response of phytoplankton to light is non-linear as currently implied by reducing surface light by the coverage fraction). These information should be provided in the introduction or in the methods; and their implications for the interpretation of results need to be discussed. Depending on the size, solar panels may also have quite different impacts on waves, which are not considered in this study (e.g. wave damping)

Specific points

Page 1, lines 15-21: Given the limitations of the 1D model parameterisation (see general points), I am not convinced that the results are applicable to "very large-scale implementations of [evenly distributed] offshore floating platforms".

Page 2, lines 10-19: Information on the design of aquatic solar panels/farms should be provided here.

Page 3, lines 11-17: In this paragraph it should be stated clearly that this study is only a testbed for the parameterisation.

Page 4, line 19: How reasonably is it to assume constant S for the Noordwijk station (I assume it's Noordwijk-10 although not specified)? E.g. de Kok et al. (2001; https://doi.org/10.1006/ecss.2000.0627) show that there is quite some salinity stratification.

Page 6, lines 14-16: I am quite surprised that there are no other data sources for two of the three stations? Are these Oyster Grounds and West Gabbard (please specify in-text)? What about rosetta casts/bottle samples during earlier years?

Page 9, line 9: Can you explain that increase?

Page 10, lines 13-15: I don't understand this vertical difference in turbulence. Why is it increased near the surface (wind shielding effect < friction effect?) but opposite in mid-water?

Page 11, lines 1/2: I agree that PP shifts to the surface because of the shallower mixed layer. However, light is also reduced due to the panel shadow; so subsurface PP does not necessarily need to increase. In this particular case it does because the increase in light due to shallower mixed layer (ML) outweighs the decrease in light due to shadowing. I suggest to clarify this. You could provide numbers of light at ML depth averaged over the year for the different scenarios. The decrease in ML depth with higher surface coverage also reduces the nutrient inventory available for PP (assuming that nutrients below the ML cannot be accessed by phytoplankton). Can you comment on whether this has a measurable effect?

Page 16, lines 2-20: this discussion of the limitations of the 1D model and the applied parameterisation should be expanded a little bit (see comments above)

Minor/Technical corrections

Title: in addition to changing it (see earlier comments), "PV" should be replaced by "photovoltaic"

Page 1, line 4: "photovoltaic (PV)"

Page 1, line 6: "seasonally stratified" instead of "summer-stratified"?

Page 1, line 20: "three-dimensional" instead of "3D"

Page 2, lines 20/21: "(Trapani and Millan, 2012; Grech et al., 2016; …)"

Page 2, lines 32/33: "570,000"; riverine freshwater runoff (which produces barotropic pressure gradients) also controls hydrodynamics

Page 2, line 34: "Sündermann" (with umlaut)

Page 3, line 12: "seasonally stratified" instead of "summer-stratified"?

Page 3, lines 31/32: add degree sign (°) to geographical locations

Page 4, line 2: please add a reference for the sentence ending on this line

Page 4, line 4: please add reference

Page 4, line 16: no comma after "model"

Page 4, line 17: "one-dimensional vertical (1DV)"

Page 5, lines 6/7: "(Baretta et al., 1995; Ruardij et al., 1997; Vichi et al., 2007; van der Molen et al., 2018; …)"
Page 5, line 15: see van der Molen et al. (2014)
Page 7, Table 2: the initial detritus concentrations seem very large to me (10^5?); please specify their unit (mmol **N**/m^3?); include multiplication sign before "10^5"; I further think Tables 2 and 3 could be merged into one.
Page 7, line 6: please specify the averaging time period: annually? Growing season?
Page 7, section 2.5: what is the output time step of the simulations?
Page 8, Fig 4: the x axis labels of panels a,b,c differ from the others
Page 8, line 6: You should add a brief concluding statement on the general(ly good) performance of the model
Page 10, line 5: Should it be only Fig. 5b?
Page 10, line 12: comma after "mixed locations"
Page 10, lines 15/16: Fig 5c always shows a decrease; it's only weaker for high surface coverage
Page 10, line 20: It only collapses for 100% coverage.
Page 11, lines 1/2: Please add how MLD was determined (e.g. maximum T gradient).
Page 11, lines 7/8: remove the sentence on ecosystem collapse
Page 12, line 4: "factor" instead of "effect"
Page 12, lines 7/8: I don't understand the statement on wind shielding and "blocked" postponement of stratification; please rephrase
Page 13, lines 9/11: I don't understand this sentence. Whose impact on PP is compensated by roughness height? Please rephrase
Page 15, Fig 8: the x axis labels are cut off
Page 15, line 1: give a number for the "small" reduction
Page 16, line 8: It's not the only tidal currents but also wind-driven and/or geostrophic currents
Page 16, line 14: "three-dimensional" instead of "3D"
Page 17, line 13: "high-resolution"
Line 18, Eq (5) related description: I think the first term does not give a length scale as unit. Viscosity has Pa s as unit, i.e. kg m-1 s-1. So, the first term is in kg m-2. What's the source of the scalar factors in both terms?