

Interactive comment on “Effects of floating (solar PV) platforms on hydrodynamics and primary production in a coastal sea” by Thodoris Karpouzoglou et al.

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Reviewer 1.

The basic concept underpinning this analysis of the ecosystem impact of floating photovoltaic (PV) platforms is sound. However, I raise three concerns: (1) Some aspects of the presentation make the results inconclusive. (2) The analysis is not very detailed and does not prove the mechanisms proposed for the apparent effects. (3) The implementation is not unambiguously described.

To expand on these points: (1) The vertical profile of light in the ocean is driven by the

C1

scattering and absorption of optically active constituents (algal particles and sediment) in the water, and the water itself. Strong scattering in all directions leads to a diffuse light field. Light from open water areas between the PV platforms will be scattered into the waters beneath the PV platforms. Any SCUBA diver knows that beneath a ship it is not totally dark. There is a fundamental length scale – the horizontal dimension of the PV platforms – that is omitted from any consideration here. If the PV array is comprised of relatively small units (order several tens of metres horizontal extent) then there will still be considerable light available for photosynthesis beneath them. If they are massive, of the dimension deployed in some lakes, the effect will be more substantial. To engineer a network of PV platforms in the marine environment with the probability of high sea state conditions it seems unlikely that these will be massive rigid units with little mechanical flexibility to endure large waves. I am speculating here that the platforms are likely modest in size, but that is certainly the case for the prototype units that have been described in engineering publications and press releases. The authors have wholly neglected the lateral scattering of light in this analysis and consequently the results are an extreme worst case for the decrease of the available light.

ANSWER: The 1D model takes an area-averaged approach, and cannot resolve horizontal variations such as the size of platforms and related light diffusion. With the current model, we can only account for the 'area-averaged light deficit' introduced by the platforms, i.e. the amount of light that does not enter the water column due to the presence of the platforms. Then, for real field implementations, the well mixed conditions of the water column, and the horizontal propagation of phytoplankton due to tidal currents, suggests that the total amount of light available for each phytoplankton cell is fairly resolved by the present light model (as long as the horizontal size of the power plant is of the order of several tidal excursion lengths, page 16 line 10, otherwise indeed the present approach overestimates the impact). Light attenuation is calculated in the vertical, accounting for 1) absorption by clear water, 2) coloured dissolved organic matter (CDOM) 3) suspended mineral sediment, 4) chlorophyll, and 5) suspended organic matter (detritus). Hence the results should be taken as a first estimate of the potential

C2

effects. Modulation of these results by processes acting at the platform scale should be considered for further work. We acknowledge that 'shadow' is a misleading term in the current context, and have replaced this by 'area-averaged light deficit' or an equivalent term. We will also include more detail about the light extinction calculations, and add a remark on platform-scale light propagation effects to the suggestions for further work.

1.1) Changes to manuscript: We have added 'large scale' and 'from a water-column model' to the title. We have removed the term 'shadow' in relation to the 1D model throughout the manuscript and figures. We have included in the abstract that, for spatial homogeneity to hold, phytoplankton need to remain underneath a farm throughout several tidal cycles. We have added a paragraph to Section 2.2, including equations, describing the light-extinction method used by the model. We have added to the abstract that for small farms the effects are likely to be smaller.

(2) The order of sections 3.2.2 and 3.2.1 should be reversed.

ANSWER: We chose this order deliberately to present the over-all result first and subsequently delve into the components. The other reviewers do not seem to have a problem with this, so we will keep it as it is.

Changes to manuscript: None.

In section 3.2.1 on page 10 it is claimed that sediment concentration drives decreased irradiance due to lowered eddy viscosity due to decreased surface turbulence due to the presence of the platforms (page 10, line 12). The reader is asked to accept this without being shown the eddy viscosity profiles to prove that changes originate at the surface. Or somehow it is left to the readers to deduce this themselves from inspection of Figure 5a,b which shows the effect but not the cause. In Section 3.2.2 some attempt is made to explain the dynamics, so this should come first so that the regional differences (presently in 3.2.1) can be understood. This said, I don't think the present section 3.2.2 adequately explains the dynamics. The possibility that reduced bottom stress decreases the sediment resuspension rate, and water column turbulence, is not

C3

considered. We should be shown the different vertical profiles of velocity (and hence shear that is important in the turbulence closure), the different profiles of vertical eddy viscosity (or vertical turbulent sediment flux), and the modified light profiles, not just summary results in terms of the percent change for the different scenarios. A more nuanced look at the model results is essential to justify the suggested mechanisms by which the presence of PV platforms drive the effects observed. As stated in the text these are not proven conclusively.

ANSWER: We will add these figures with accompanying text.

1.2) Changes to manuscript: We have added the requested figures to section 3.2.1, and modified the text to accommodate this.

(3) Unclear details of the configuration: Page 5 line 1 says the model is forced with a "time series of depth averaged velocities". But doesn't the profile of vertical shear evolve with the model physics in response to the imposed surface stress and the evolved stratification and velocity shear? Please explain more fully what is being done here. The bottom stress that suspends sediment is driven by the combined depth-average plus sheared velocity – they can't be considered separately.

ANSWER: This is the standard way to force GOTM. The vertically resolved solution of the model is the dynamic response to all these forcings, and the bottom shear stress is indeed the combined result. We will add some text to Section 2.2 to clarify this.

1.3) Changes to manuscript: We have added the following sentences to the first paragraph of Section 2.2: "The model uses these depth-averaged velocities to set up spatial gradients of external pressure that it uses as forcing. GOTM uses all these forcings, including bed-shear stress, to calculate the time-evolution of vertical distributions of turbulence and currents (Burchard et al., 2006). It is also possible to explicitly force GOTM with spatial gradients, e.g. to simulate salinity stratification (Simpson et al., 2002), but this was not used here."

C4

Returning to the issue of the vertical light profile: How is this computed in ERSEM? Is it strictly 1-d vertical that ignores lateral scattering, upward scattering, or perhaps any scattering at all? Maybe that has been addressed long ago by the designers of ERSEM and my concerns in point (1) can be dismissed, but the way ERSEM handles light is not documented here and it is central to the analysis.

ANSWER: We have added a more detailed description of the calculation of the vertical light profile in ERSEM to Section 2.2- essentially it's an extinction-coefficient based exponential profile, where the extinction coefficient is composed of a number of contributing factors (absorption by clear water, CDOM, suspended sediment, chlorophyll and detritus).

1.4) Changes to manuscript: Added a paragraph to Section 2.2.

In the Appendix, equation (2) implicitly assumes there is no change in the atmospheric marine boundary layer over the PV platforms – that there is just a gap in the momentum transfer from air to sea. If the platforms are small this might be reasonable, but what if they are massive? Again, the dimension of the platforms is relevant.

ANSWER: This is a valid comment and was suggested for further work (page 17 line 4, previous version). This cannot be addressed with the current model, and that requires substantial additional work involving the physics of atmospheric flow around obstacles, including details of the obstacle geometry. We would expect such processes to modulate the current results, and now mention that this is ignored, and explain better this point to the recommendations for further work.

1.5) Changes to manuscript: We have added the assumption to the text with eq. 2, and explained better this point to the list of further work in the discussion.

Similarly, equation (3) is an equilibrium assumption. In reality a modified boundary layer under the platforms will evolve from the leading edge. If the platforms are large the boundary layer might be fully developed for the majority of the distance, but what

C5

if they are small? In that case fully developed boundary layers are unlikely and this simple modified drag parameterization may be poor.

ANSWER: Similarly here, this is a valid comment, but we would regard this as a subject for further work, and have added it to the recommendations.

1.6) Changes to manuscript: Added the assumption to the text with Eq 3, and added the point to the list of further work in the discussion.

Summary comment: The agreement between model and observations (Figure 2) is spectacular, so I am confident the fundamental model is sound. The major flaw is in the simplicity of the optical model.

ANSWER: Thank you for the positive comments.

Reviewer 2

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

C6

a sensitivity analysis of the platform's roughness height. However, I have one main criticism; that is 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).

ANSWER: We think that this is an exaggeration, and that there is merit in the results. 1D models have been used before to estimate first order responses of marine ecosystems. We will, however, include in the title that we have used a water-column model. This is already stated clearly in the abstract, the last quarter of which is already devoted to caveats - we regard this as sufficient. See also the changes made in response to related comments by Reviewer 1.

2.1) Changes to manuscript: added: 'from a water-column model' to the title.

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.

ANSWER: Part of the purpose of the manuscript is to raise awareness of a potential environmental issue that requires attention, consideration and further work to advance understanding. A publication in Ocean Science would serve that purpose and reach the required audience, while a publication in GMD would most likely not. Hence we oppose this suggestion.

Changes to manuscript: None

I recommend reconsideration for publication (possibly in GMD) after moderate revisions. ANSWER: Thank you!

C7

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).

ANSWER: The manuscript already contains numerous caveats and warnings to this extent. However, we have reviewed the text and made further warnings where appropriate.

2.2) Changes to manuscript: See response to comments by Reviewer 1.

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)

ANSWER: We recognise these issues, and have mentioned most of them in the manuscript. However, spatially resolved processes can not be simulated with an area-averaged 1D model, and most of these points must be the subject of further work. We have reviewed the current text to make these points even more clearly.

2.3) Changes to manuscript: Three separate additions to the text of Section 2.3 to stress this.

C8

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”.

ANSWER: This statement relates to the assumption of negligible horizontal gradients underlying the application of a 1D model. In that sense, the scale of a hypothetical farm must be large enough to ensure that phytoplankton do not move outside the farm by e.g. tidal currents for a considerable time. We have reviewed the text and now make this point more clearly. See also similar comment by Reviewer 1.

2.4) Changes to manuscript: of at least several hundreds of square kilometers such that phytoplankton remain underneath a farm throughout several tidal cycles.

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

ANSWER: This is not the subject of this paper, and not relevant in the 1D context used here. However, we will assess the earlier publications to see if dimensions are provided. Technical details of the planned small-scale test farm are confidential. At this stage no reliable information on the design of future installations is available.

2.5) Changes to manuscript: The manuscript now provides dimensions of a fresh-water implementation.

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

ANSWER: We disagree with the reviewer (see above). We will, however, include a statement that substantial further work is needed.

2.6) Changes to manuscript: we have added: For more detailed, spatially resolved results, and to include additional processes, substantial further work is needed.

C9

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?

ANSWER: The locations of the stations are specified clearly on p. 3, l. 31-32, and in Figure 1. We will add that we used Noordwijk 10 (throughout the manuscript). These are indeed well-studied sites. However, we have chosen to only describe the (time-series) data that we have used; providing a full catalogue of data observed at these sites is beyond the scope of this paper. Thank you for the reference on salinity stratification at Noordwijk-10. We have considered this carefully and made changes to the text when discussing this site. Our objective was to simulate shallow, well-mixed conditions. Although it is possible to represent salinity stratification with GOTM (Simpson et al., 2002), doing so requires detailed observations of spatial gradients in salinity.

2.7) Changes to manuscript: We have changed Noordwijk to Noordwijk 10 everywhere. We have changed the last sentence of the first paragraph of Section 2.1 into: Both locations are characterized by relatively strong tidal currents, high suspended sediment concentration and high primary production (van der Molen et al., 2016; <https://data.gov.uk>). The West Gabbard location remains well mixed during the entire year. The Noordwijk-10 location can stratify by combined temperature and salinity effects when river outflow is high (de Kok et al., 2001). For the purpose of this study, we ignore salinity effects at Noordwijk-10, which may lead to an under-estimation of the occasional stratification.

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

ANSWER: Yes, this is because of a reduction in suspended sediment concentration. We will add this to the text, including an additional figure illustrating the underlying

C10

causes related to a decrease in eddy diffusivity (see comment by Reviewer 1).

2.8) Changes to manuscript: We have added that this is caused by lower suspended sediment concentrations, and refer to the additional text and figure requested by Reviewer 1.

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?

ANSWER: Yes, wind shielding effect < friction effect for the well mixed areas. The introduction of platform friction results in a change in the shape of the velocity profile. A small (or zero) vertical gradient at mid depths and a large vertical gradient at the surface. This leads to high shear production (and thus turbulent kinetic energy) near the surface and low at mid depths. See also response above, and to comments by Reviewer 1.

2.9) Changes to manuscript: We have added an additional figure and text to explain this.

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.

ANSWER: We will clarify this.

2.10) Changes to manuscript: We have added that the effect of the upwards shift outweighed the light deficit induced by the platforms

You could provide numbers of light at ML depth averaged over the year for the different scenarios.

C11

ANSWER: That would not be representative as it would need to be calculated over the part of the growing season with stratification. Although it is possible to do this, we do not see how this would add to what can be read/understood from Figure 6.

Changes to manuscript: We have made no changes.

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?

ANSWER: This is the reduction in net primary production between 0 and 15 m in Figure 6b. We will add a comment on the reduced nutrient inventory.

2.11) Changes to manuscript: We have added that a thinner layer holds less nutrients.

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)

ANSWER: We will do this, also considering suggestions by the other reviewers.

2.12) Changes to manuscript: We have added text suggesting further work on:

- the influence of horizontal light diffusion
- the effect of platforms on the wind
- the effect of platforms on air-sea gas exchange
- the size of structures: effect on development boundary layer for friction

Minor/Technical corrections

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

OK, done.

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

C12

ok, done.

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

ok, done

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

ok, done

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

ok, done

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

ok, "buoyancy gradients" includes this as well.

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

ok, done

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

ok, done

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

ok, done

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

ok, done

Page 4, line 4: please add reference

ok, done

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

C13

ok, done

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

ok, done

Page 5, lines 6/7: "(Baretta et al., 1995; Ruardij et al., 1997; Vichi et al., 2007; van der Molen et al., 2018; ...)"

ok, done

Page 5, line 15: see van der Molen et al. (2014)

ok, done

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 ";

ok, done. We also corrected the units for benthic detritus.

I further think Tables 2 and 3 could be merged into one.

No, we want to make a clear distinction between parameters that were set, and the initial conditions that were tuned. Using one table for both would be confusing. No change.

Page 7, line 6: please specify the averaging time period: annually? Growing season?

OK, done.

Page 7, section 2.5: what is the output time step of the simulations?

Daily, now stated.

Page 8, Fig 4: the x axis labels of panels a,b,c differ from the others

Ok, done

Page 8, line 6: You should add a brief concluding statement on the general(ly good)

C14

performance of the model

ok, done

Page 10, line 5: Should it be only Fig. 5B?

Both are needed, We have improved the sentence: ' surface suspended sediment (Figure 5 b) on irradiance (Figure 5 a)'.

Page 10, line 12: comma after "mixed locations"

ok, done

Page 10, lines 15/16: Fig 5c always shows a decrease; it's only weaker for high surface coverage

ok, we have rephrased

Page 10, line 20: It only collapses for 100% coverage.

We have removed the phrase 'collapse'.

Page 11, lines 1/2: Please add how MLD was determined (e.g. maximum T gradient)

It is calculated as the shallowest layer with tke smaller than a certain value (10^{-5}), now added

Page 11, lines 7/8: remove the sentence on ecosystem collapse

Ok, changed into 'strong reduction in primary production'.

Page 12, line 4: "factor" instead of "effect"

Ok, changed.

Page 12, lines 7/8: I don't understand the statement on wind shielding and "blocked" postponement of stratification; please rephrase

we replaced "blocked" with "prevented"

C15

Page 13, lines 9/11: I don't understand this sentence. Whose impact on PP is compensated by roughness height? Please rephrase

We have added: 'the impact of the installations'

Page 15, Fig 8: the x axis labels are cut off

ok, fixed

Page 15, line 1: give a number for the "small" reduction

10%, done.

Page 16, line 8: It's not the only tidal currents but also wind-driven and/or geostrophic currents

Tidal currents should be equivalent to geostrophic currents and they are the dominant currents within the North Sea. We have added a statement that tides generate the dominant currents in the North Sea.

Page 16, line 14: "three-dimensional" instead of "3D"

ok, done

Page 17, line 13: "high-resolution"

ok, done

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?

In Fluid Dynamics kinematic viscosity is often used and called as viscosity. This is viscosity over density, thus m²/s. The source of the scalar factors: Burchard et al. (1999). We have now included units and the reference.

Reviewer 3.

C16

1 Summary

The question addressed in the paper is practical and applied, rather than of a fundamental scientific nature, but appears to lie well within the journal scope. The paper itself is exceptionally well written, structured and easy to follow. Methodology is well explained and, importantly, the limitations of 1D water column approach modelling (compared to a 3D modelling) are clearly noted and explained. Despite the constraints of a 1D approach, useful guidelines, albeit of a preliminary nature, are obtained concerning the proportion of surface area covered before significant effects on primary productivity might be expected. The application to the three sites is also very useful in giving an idea about how generic these results might be. I have a technical concern about the formulation surface friction effect that is discussed below. Apart from this, there are only a few minor suggestions for manuscript improvement. Overall, the paper is a very clearly written description of a well-motivated and executed study and deserves publication with minor modifications.

ANSWER: Thank you for these very positive comments.

2 Shear stress formulation

If I've understood correctly, the definition of the structure surface shear stress appears to explicitly depend on layer thickness of the 1st grid cell (h in equation 4). Clearly the structure drag is a real physical quantity that cannot depend on how a particular model is set up. For example, if the user decided to refine the mesh near the sea surface then as $h \rightarrow 0$ the stress coefficient given by eqn. 4 is unbounded. It seems a real physical length scale needs to be included in the formulation - the depth of the structure below the surface would be an obvious candidate (but note, a straight substitutes of h with structure depth wouldn't work). It probably doesn't make a difference to the results obtained here as the authors show these are rather insensitive to the details of the formulation (fig. 8), but a grid independent estimate of the structure drag seems to be desirable.

C17

ANSWER: This is a good point that needs to be clarified. $h/2$ below the platform is the vertical location of the velocity u in eqn 3. U is proportional to $\ln((h/2+z_0)/z_0)$, thus $h=0$ leads to no slip conditions. Then τ is independent of h (For more information see Burchard (1999): GOTM, a general ocean turbulence model, theory implementations and test cases, chapter 3.3) We will add the equation for u .

3.1) Changes to manuscript: We have added the equation for u .

3 Suggested text amendments

Authors could mention the study is also relevant to other proposed offshore developments e.g. large-scale aquaculture/seaweed farming.

ANSWER: We intended to make this point in the very last sentence (p. 17, l. 26). We have improved this sentence to make this point more clearly.

3.2) Changes to manuscript: We have added the example of seaweed farming to the last sentence.

p line 5 16. Phaeocystis is omitted from model then suggested as explanation for peaks in chlorophyll not captured by model (p 8 line 6). Although I understand that if phaeocystis is linked to nutrient peaks from rivers it is quite legitimate to exclude it in a 1d model, nevertheless, on 1st reading of p8 line 6, it seems slightly odd to blame the mismatch on something deliberately excluded from the model. A rewording here, or on p 5 line 6, might just help the reader understand this more quickly. ANSWER: We will add this.

3.3) Changes to manuscript: We have added that inclusion led to spurious interannual variability within the 1D context on p.8.

p 6. line 8-9. How was the light extinction coefficient calibrated from observations? If I understand correctly, then the SPM at the OG causes greater light attenuation for a given concentration than at the other sites. A brief mention in the discussion might be interesting; is the difference just an artifact of calibration and not real, or is there some

C18

reason for the SPM at OG being different to the other sites. ANSWER: This is an artifact of calibration; we will mention this in the manuscript.

3.4) Changes to manuscript: We have changed the text into: "Site-specific values for the porosity of the sea bed and salinity were defined based on observations (table 1). The light-extinction factor for suspended sediment (the contribution to the light-extinction coefficient by suspended sediment is this factor multiplied by the suspended sediment concentration) was kept at the standard value for West Gabbard and Noordwijk 10, but half the standard value for Oyster Grounds as that gave better results."

p 6. detritus. Is this seabed or water column detritus? Brief explanation on what detritus is and why it is necessary to adjust its value.

ANSWER: Sea-bed detritus. This is by far the largest pool of material in the model. We will add a few words stating this.

3.5) Changes to manuscript: We have added 'benthic' to detritus in this sentence. We have also included: "Benthic detritus is by far the largest pool of carbon and nutrients in the model, so using it to set the nutrient content of the 1D model in combination with a long spin-up of more than twice the response time of the benthic system to re-distribute this content appropriately within the ecosystem is a simple and effective tuning approach."

4 Minor format issues

fig 2 Suggest put quantity name (chlorophyll, nitrate, silicate) at top of each of the 3 column of plots so reader immediately knows what is being shown rather than having to read caption.

ok, done

fig 5,6 Very minor, but I think is easier to read having the letter label before the text instead of after, e.g. "(a) irradiance at 3 meters depth", instead of "irradiance at 3 meters depth (a)"

C19

ok, done

p 15 line 2. The regime 2 text misses out on a general conclusion that the other regimes get. Suggest text be amended to something like ": : .. a substantial spread occurred between sites. Thus, no general, site-independent, conclusions can be drawn. In regime 3: : : : : :"

ok, done

âĀĀ Reviewer 4.

Overall Statements

The manuscript "Effects of floating (solar PV) platforms on hydrodynamics and primary production in a coastal sea" by Karpouzoglou et al. describes the effect of photovoltaic platforms (disposed at the sea surface) on physical and ecosystem features in the water column. In times of global warming when mitigating strategies and renewable energy production become important, such basic assessments are necessary and welcome. The overall result is that the shading effect is more important than shielding from wind and friction on currents. To my knowledge the model-system does not include 3-D scattering of light. This effect would increase underwater-light availability and thus might corrupt the central finding of the manuscript. The authors must tackle this challenge seriously, otherwise I cannot recommend the manuscript for publication.

ANSWER: See also the responses to comments by Reviewer 1. Indeed, the 1D model cannot simulate shadow, shading and horizontal light diffusion, but rather considers the overall reduction in the amount of light entering the water column and available for growth (which is a real physical effect that the 1D model can represent). The current text is confusing, and we will reformulate using a term such as 'area-averaged light deficit induced by the platforms'.

4.1) Changes to manuscript: See responses to comments by Reviewer 1.

The second major difficulty of the manuscript is the method of individual spin-ups over

C20

26 years using different starting values for nutrients. Does the model system include sink terms, like burial and N₂ release during denitrification?

ANSWER: No. There is no burial below the part of the sea bed that the benthic system represents, and nitrogen released by denitrification is re-introduced immediately as nitrate by atmospheric deposition.

4.2) Changes to manuscript: We have added the following sentence to Section 2.2: Within the 1D model context, nitrogen, phosphorus and silicate are fully conserved. N₂ gas produced by denitrification processes is fed back immediately as nitrate in the form of atmospheric deposition. Carbon and oxygen are exchanged with unlimited atmospheric pools at constant concentration.

In this case the model will show a drift which should be clearly seen over these years.

ANSWER: This does not apply, see above.

Changes to manuscript: We have made no changes.

I understand that the initial conditions must be different for different places, but this very long spin-up must be justified. The initial very high detritus concentrations appear very artificial.

ANSWER: This is the first time we've ever been accused of having a spin-up period that is too long :-). The benthic system in the model has a typical response time of a decade or so. So roughly two decades is reasonable. Part of the detritus redistributes into the rest of the ecosystem during this time until it settles into a quasi-steady initial state. Using one ecosystem component in combination with a long spinup is a much more straight-forward method than tuning the initial settings of all components at the same time. We have added a few words to the text to this effect. See also comment by Reviewer 3.

4.3) Changes to manuscript: See response to comment by Reviewer 3.

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The manuscript is structured and written very well. Thus, I hope the puzzles can be clarified.

Answer: Thank you, please see above, and responses to the comments of the other reviewers.

Detailed remarks

P1 L4: Define here the "PV" abbreviation.

OK, done.

P1 L27: Also discuss possible conflicts with shipping and offshore windfarms.

ANSWER: This is a technical section on how the platforms were implemented in the model. Moreover, spatial planning is beyond the scope of this paper as a whole. We have made no changes.

P5 L24: Why do you exclude gas exchange?

ANSWER: This is a good point. We already included air-sea exchange in the recommendations for further work. We will change it to air-sea gas exchange.

P5 L30 ff: The air circulating around the platforms will be accelerated and behind the platform I expect a turbulent wind field. Can you estimate these effects?

ANSWER: We're sorry, but no. The current model is limited to the hydrosphere. However, this is a good suggestion (also raised by another reviewer), and we have added it to the list of things that may need to be looked at in further work.

P6 Table 1: Did you use SPM concentrations? Please give the corresponding values.

ANSWER: No, that is, we did not prescribe them. They are calculated dynamically by the model, as is stated clearly in Section 2.2. We have not made changes to the manuscript.

P6 L10 ff: It is not the mass of nutrients, which is conserved. It is the amount of

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nitrogen, phosphorus and silicon, which is conserved, if there is no sink and source within the water column (see overall statements).

ANSWER: Thanks, we have re-formulated this.

P7 Table 2: You mention detritus. Which element describes detritus? Is it pelagic detritus? This are very high values. In this case the shading of detritus would be much larger than the shading of the platform.

ANSWER: This is benthic detritus. See the response to the equivalent comment by Reviewer 3.

P7 L6: Fraction = 1 appears very artificial. Please mention this already here.

ANSWER: we now mention that the high end of this range was included for completeness, but may never be realised.

P8 Figure 2 abc: The arrangement of x-axis labels does not allow to identify the exact positions.

ANSWER: we have corrected this.

P12 L7 ff: This sentence is over-complex. Please rephrase.

ANSWER: we have split this sentence: "Reduced mixing resulting from wind shielding prevented a later onset of stratification and spring bloom that would otherwise be caused by the effect of platform shadow (decreased buoyancy input). It thus prevented the partly compensating effect of a later spring bloom on net primary production that occurred at the well-mixed sites."

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