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

Anonymous Referee #1

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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 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.

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

(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.

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.

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.

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

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.

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