



EGUsphere, referee comment RC1  
<https://doi.org/10.5194/egusphere-2022-430-RC1>, 2022  
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## Comment on egusphere-2022-430

Anonymous Referee #1

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Referee comment on "Ocean color algorithm for the retrieval of the particle size distribution and carbon-based phytoplankton size classes using a two-component coated-sphere backscattering model" by Tihomir S. Kostadinov et al., EGUsphere,  
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### General comment

This study attempts to retrieve from space (ocean color satellite data) information on particle size distribution and carbon-based phytoplankton size classes in open ocean waters. This significant piece of work is actually the extension of previous studies (Kostadinov et al. 2007-2022) which includes validation results. The manuscript is well organized, written and illustrated.

Unfortunately, these validation results are not convincing, most probably as several assumptions made in the methodology are not valid. The authors should carefully revise the assumptions made notably to model the particle size distributions and discuss the impact on the resulting satellite-derived products. Detailed comments are provided hereafter to clarify the methodology and discuss the validation results.

### Detailed comments

Line 45, Equation 1:

To my knowledge this very convenient power law size distribution of particles does not apply to phytoplankton particles in oceanic waters. Can you please provide relevant references to support your statement?

Line 53:

Again, probably the main/major issue in this study: phytoplankton cells in oceanic waters DO NOT follow a power-law PSD. If I am wrong please prove it based of already published quality field data.

Line 64:

"a single population of particles (approximated by homogeneous spheres)"

This is another strong assumption which definitely does not apply to phytoplankton cells in marine waters. Please discuss it and say what is the impact in your methodology.

Line 85:

Where do minerogenic particles come from in open ocean waters?

Line 93 'an initial effort of validation':

Such an effort to at least first validate the assumptions made in your recent and present studies and notably validate the PSD algorithm should have been made already, before going forward applying non-validated algorithms to satellite data and interpret the results obtained

Line 98 'backscattering are modeled using Mie theory (Mie, 1908) for homogeneous spherical particles and the Aden-Kerker (Aden and Kerker, 1951) method for coated spheres.'

Is Mie theory well adapted to your study?

What not considering also the more realistic case of non-spherical particles?

Line 138 'The two key assumptions are: 1) Phytoplankton and NAP have a power-law PSD (Eq. 1) with the same slope  $\xi'$

Once again, I do not agree for phytoplankton. Moreover why the same slope?

Tables 1 and 2:

Please justify the choice of the minimum, mean and maximum values considered here as inputs. Are your computations realistic??

Line 218:

Define LISST

Figure 8 'PSD validation results':

Thank you for showing these validation results which are not satisfactory, as could be expected considering that several assumptions made are (most probably) not valid.

While there is somehow an agreement (or at least a trend) between the satellite and situ No (number of particles), there is no correlation for the slope, therefore no validation of the satellite-derived PSD, assuming the PSD is a power-law.

These poor validation results must be discussed so as its implication on the whole methodology. What would be the results if another (more realistic) function was used to model the PSD?

Figure 10:

These validation results are more convincing. Please specify in the figure legend what you mean by 'empirical tuning'.

Figure 11.

As in Figure 8, poor validation results.