



EGUsphere, referee comment RC2
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Comment on egusphere-2022-547

Anonymous Referee #2

Referee comment on "Satellite-detected sea surface chlorophyll-a blooms in the Japan/East Sea: magnitude and timing" by Dingqi Wang et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-547-RC2>, 2022

Wang et al. analyzed long term variation of satellite chlorophyll in JES. Although the authors put together many kind of different data, the descriptions were fairly simplify for this complex ecosystem with large spatial variation. The discussion (many are in the results) is very poor and speculative, ignoring previous studies. Unfortunately, I cannot recommend the publication.

- The analysis of the interannual variation was only limited to the northwest coastal area for spring and deep Japan Basin for fall. Although it is based on the high variation of 1st mode of EOF, those areas are only limited areas of the JES, and I do not think it is suitable to conclude it is applicable to the whole JES.
- As the results correlations are OK, but the just correlation cannot explain the cause and effect. I think authors deeply concern about the cause and effect seriously.
- Discussion is very weak, and almost nothing was discussed about the reviews in the introduction.
- It may be a good idea to show correlations with Table(s).

Abstract

19 Delete "However".

19-21 I do not think the authors really showed PAR and stratification is more important than other processes.

21 PAT should be PAR, and it need to be defined.

21 'In addition" may not be a good connection here.

23-24 Timing affect magnitude?

24-25 In the text, it was said ENSO is not important?

25 Duration is mainly affected by initial timing?

26 Duration and initial timing have significant influence on the bloom magnitude?

1. Introduction

78 SSC "is" related

79 "based on composite analyses without test of confidence level" It is not very clear what is the problem.

89 I am not sure previous analysis really did not focus on the whole area and this study did.

90 favorable/restricting factors? You only showed the correlations, and the cause/effect can be discussed.

2. Data and Methods

100 Is WOD18 station data? Is the comparison to satellite data based on daily match-up?

103 Did you check if there is no interannual bias?

105 Are PAR and k data daily, and later make monthly composite?

119 Add "climatology" for WOA18 temporal coverage.

124-126 "To identify blooms, the threshold is about 0.55" I do not understand this sentence. Is the value spatial average?

155 What is "an adaptive data analysis technique"?

163 Was logarithmic transform not used before calculating monthly mean of SSC?

3. Results

3.1

166 "Seasonal variability of bloom magnitude" Bloom is only spring and fall, and this title seems to be strange.

185 Is the analysis with average of whole JES?

187 Is this shortwave radiation same as PAR?

188-220 I think this include too much speculations and sloppy words, and they should move to discussion and discuss carefully.

I think the dynamics depends on the regions of the JES.

202, 205 The sea ice melting effect should be important for very limited area.

204 "favors" should be "corresponds"

208 "ocean dynamics" is too broad words.

208-209 "dominate the upper layer nutrients" It is too general.

210 "in accordance with the enhanced upwelling and frontal probability" It is too general.

211-212 EKE distribution was not shown.

212-220 None of those points are shown as the results.

221-240 Is the PCA analysis was conducted with monthly seasonal climatology of the whole spatial average?

I think if it spatial average it is very difficult to understand because of the regional difference of the JES.

3.2

245 What is the criteria of "significance".

251 "JES SSC" Are these from the black boxed areas?

254 You should not know about "Photosynthetic activity".

3.3

265 Is this about northwestern coast?

275 Just correlation cannot say "controlled".

277 favorable?

277-278 I think this sentence mixed spring and fall blooms.

280-281 There is no data of increase of PAR.

282 Is this about deep Japan Basin?

288 There is no data of change wind.

290-291 It is already written.

292 Delete "The correlation show that".

4. Discussion

This is very poorly written.

5. Summary

350 "SSC bloom" should be "phytoplankton bloom".

(1) I am not sure which part gives this conclusion. At least, comparison of correlations of different processes is not shown.

354 PAR

(3)(4) It is very difficult to understand the idea from Fig. 14 (just a time series plot).

(5) "Relative to the AO" As English, it is difficult to understand the contrast to ENSO.

I do not understand how ENSO counterbalance in SSC.

Fig. 8 The areas averaged for (c) and (d) are?

Fig. 9 Is the value from the box in Fig. 8?

Fig. 10 Is the value from the box in Fig. 8?