



EGUsphere, referee comment RC1
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Comment on egusphere-2022-393

Anonymous Referee #1

Referee comment on "Multi-platform study of the extreme bloom of the barrel jellyfish *Rhizostoma pulmo* (Cnidaria: Scyphozoa) in the northernmost gulf of the Mediterranean Sea (Gulf of Trieste) in April 2021" by Nydia Catalina Reyes Suárez et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-393-RC1>, 2022

The authors describe what was observed in spring 2021 in the Gulf of Trieste using different data sources. In this respect, the paper is rich in information and details.

My main remark is that the authors only took into account the 2021 event although a longer time series of jellyfish observations is available (Fig. 3). The 2021 event appears to be the most remarkable one because of its persistence, but other intense events occurred. For instance, in January 2020 a '3-degree' event took place suddenly after weeks of weak or no jellyfish presence. Was that event connected with the atmospheric forcing in the same way as the 2021 one?

The analysis of the whole time series of fig. 3 also poses the question if it can be considered homogeneous. As observations are provided by citizens, the number of sightings might be influenced by previous observations: When a remarkable phenomenon is observed once, then many more people are stimulated to pay attention and report their own observation.

In any case, provided that in the specific event of 2021 things went as described in the paper, can the information be generalised on the basis of observations?

I recommend a major revision.

Minor points:

Line 59: Scirocco blows from SE (ESE-SSE), SSW is approximately Libeccio (SW); I suggest 'warmer southerly wind blowing from SE to SSW directions'.

Lines 77-78: '... 2020). When the water column is stratified, the surface layer ...'.

Lines 112, 126, 127: What is 'pi:'?

Lines 113-119: Are the CTD and hydrometer data publicly available?

Line 123: I suggest to replace the '=' character with 'corresponds to', otherwise it is misleading. Please define 'ind' (individuals?). According to the definition, 1 ind m⁻² is both included in case 1 and case 2. The same occurs in fig. 3 (page 8). Please solve the ambiguity.

Line 131: Please define 'L1' (layer 1?).

Lines 132-134: 'Model and reanalysis data were used ...'. How big were the gaps in the observations?

Line 164: Please describe how the RMSE is normalised.

Table 2 (page 9): Please outline the meaning of 'corr-u', 'corr-v' and 'corr-sp' in the caption.

Lines 176-177: Please note that the variables used for the statistics have a non-normal distribution. The authors should explain how correlation was computed and how significant the results are. 'Fairly good' does not mean much.

Line 179: Here there is '0.5540' but in Table 2 it is '0.4633'. Please check.

Line 181 and 187: 'wind speed' (m s⁻¹) not 'wind intensity'.

Line 212: There are different responses of the sea to wind forcing. For instance, surface cooling near the eastern coast in case of Bora requires just a few hours. Please clarify.

Line 213: 'occupied' instead of 'filled'.

Figure 6 (page 12): The moving-average curve within the data gap has no meaning; please delete it.

Fig. 7 (page 13): 'left black dashed line' and 'right black dashed line'.

Line 239: 'north Adriatic Sea'.

Lines 243-244: Also -200 is <50 . Do the authors mean 'from -200 to approximately $+50$ W m^{-2} '? Please rephrase. Also, 50% of what? See also previous comment.