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Interactive comment on "Winter phytoplankton blooms in the offshore south Adriatic waters (1995–2012) regulated by hydroclimatic events: Special emphasis on the exceptional bloom of 1995" *by* Mirna Batistić et al.

Anonymous Referee #1

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GENERAL COMMENT: The manuscript bg-2017-205 combines physical and biological oceanography aiming at describing the evolution of winter phytoplankton blooms in open waters of the Southern Adriatic. The topic is of great scientific interest as it tackles mechanisms for the development of phytoplankton bloom in an area generally considered as oligotrophic. This can unveil a different perception of the production of the southern Adriatic pelagic ecosystem under specific hydrologic and meteorological conditions. In doing this, authors relied on extensive scientific literature, which describes circulation regimes of the Northern Ionian Sea and Southern Adriatic, including

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regular circulation patterns and exceptional changes to these regimes over a 20-year long period (1993-2012, see Fig. 2). Authors' original contribution to the manuscript is the detailed description of winter situation of two years, 1994 and 1995, which were under the same circulation regime - the anticyclonic phase of the NIG. Another original contribution is the satellite Chlorophyll-a time series (1997-2012) along the E-W transect of the Southern Adriatic. The time-series was discussed in view of specific hydrologic and climatic conditions, which in certain years led to the development of a winter bloom, and were compared to the situation of winters 1994 and 1995 (Table 2). Again, information of comparable years were retrieved from literature. Saying this, in my opinion the title does not reflect the content of the paper properly. Firstly, the period mentioned in the title (1995-2012) does not match the sampling period (starts in 1994) nor the satellite time-series (starts in 1997). Secondly, both sampled years were assessed in the same comprehensive way, which does not justify emphasizing 1995 only. I would suggest modifying the title.

To summarize, authors' effort in describing complex hydrologic and climatic mechanisms that govern on large spatial and time scales in the Mediterranean and associate them to signals of change in phytoplankton community that occur on mesoscales in the Southern Adriatic is valuable. Moreover, observed differences in species composition in both years were supported by wide phytoplankton literature regarding the prevalence of certain species of Atlantic origin. I suggest minor changes of the manuscript on specific points, which need to be elaborated more in detail or re-interpreted.

SPECIFIC COMMENTS: 1. Material and methods (p. 3, lines 94-95): discrete sampling depths are listed from surface to the bottom of sampling stations. However, at these depths chemical parameters (oxygen, nutrients) were sampled, whereas is not specified that for phytoplankton community structure only the euphotic layer was sampled. (i.e. down to 200 m, as in the figures 9 and 10). This should be added. 2. Material and methods (p. 4, lines 109-113): provide information on the depth of surface layer of the Ocean Colour observations (i.e. surface Chl-a). 3. Material and methods (p.

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4, lines 117-119) and Fig. 2: “To track different circulation regimes in the North Ionian Gyre (NIG), we used average salinity values from 1993 to 2012 in the 200-800 m depth layer. . .” It is not clear whether the average salinity was calculated for the upper 200-800 m deep layer OR for the layer at the depth of 200-800 m. If the latter is true then it contradicts the statement in Conclusions (p. 13, lines396-400) saying that during anticyclonic years the inflow into the Adriatic Sae can be observed in the 50-200 m layer. 4. Material and methods (p. 4, lines 121-123): check the statement “Year 2012 display both 121 circulation modes: cyclonic mode which started in 2011, in the second part of the 2012 (May) 122 unexpectedly reversed to anticyclonic (Gačić et al., 2014), Fig. 2”. To me it looks just opposite; year 2011 and the first half of 2012 was in the anticyclonic mode, which in mid-2012 changed to cyclonic. 5. Results, subchapter 3.1 (p.5, lines 137-138): for non-physical oceanographers, explain more in detail (or rephrase the sentence) which conditions are unfavourable for convection. 6. Results, title of the subchapter 3.2. (p. 5, l. 140): I’d suggest changing it to “Physical and chemical properties of seawater in February 1994 and 1995” 7. Discussion (p. 8, lines 246-249): You claim that winters of 1994 and 1995 were characterised by the EMT “. . . that drove that drove nutrient-rich, lower oxygen, less saline water to mid-depths of the Adriatic. This was accompanied by a massive intrusion of Atlantic Water (AW)”. Decreased oxygen at mid-depths was observed only in 1995, whereas salinity profiles of both years show a constant increase throughout the water column. Regarding nutrients, peaks were registered at different depths in both years: roughly from 200 to 400 m in Feb 1994 and around 600 m in Feb 1995. Are all these peaks related to the intrusion of EMDW and at which depth this water enters the Southern Adriatic? Authors should also mention which are the characteristics (salinity) of the AW and at which depth can be traced. 8. Discussion (p. 11, lines 320-321): “Anticyclonic circulation characterized the NIG in 1994, 1995, 2007, and 2008 (Gačić et al. 2010; Civitarese et al. 2010; Bessières et al., 2013).” According to Fig. 2, 2007 and 2008 were in the reversal phase. Moreover, do you have data, besides satellite observations of Chl-a, for years in the reversal phase - 1997, 1998 and 1999? It would be noteworthy to in-

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clude them in Table 2 in which you summarise complex hydrologic and meteorological conditions that supported phytoplankton blooms. In reversal years, 1997-1999 different conditions governed than those presented in Table 2 but they anyhow led to the increase in phytoplankton biomass. 9. Conclusion: discussion and conclusion fairly answer the proposed hypotheses (p. 3, lines: 79-86). Nevertheless, when you are saying that winter blooms in the OSA could account for a large fraction of OSA annual production have you any indication for this statement. Can you sustain this evidence with some publish data of the inter-annual variability of primary production that could match years of winter blooms?

TECHNICAL COMMENTS: 1. Figure 1: add legend for LIW and AW. 2. Figures 5, 6, 7, 8, 9, 11: station P-1200 presented in these figures does not match the deepest station in Fig. 1 (P-1000) nor the description in the text. Intervals between labels on the salinity scale (Figs. 5 and 6) are too small. The reading of the salinity scale is unclear, as are unclear isopycnal contours especially on Fig. 5. Use a different colour. 3. Figures 10, 12: change the caption as follows: "Relative contribution (in %) of different. . ...". 4. Be consistent with the term coccolithophorid(s). 5. Table 1: Check the validity of species names (e.g., genus Ceratium) in updated databases, preferably in Algaebase. 6. Table 2: add legend for NIG and AVG. 7. References: Civitarese and Gačić (2001) and Kovačević et al. (2003) are missing in the list of references. 8. Citation (Rabitti, 1994) (p. 13, l. 389) has to be changed in (Rabitti et al., 1994) 9. Thoroughly check the style of citations, as they are not uniformly written out.

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