BGD Manuscript:
On the potential causes of the recent Pelagic Sargassum blooms events in the tropical North Atlantic Ocean

In this manuscript, Djakouré and co-authors investigate the possible causes of the increasing frequency of stranding episodes of *Sargassum* sp. in the tropical Atlantic Ocean. The authors have used different sources of data to tackle their scientific question, such as river discharge observations, models to estimate the riverine nutrient discharge to the tropical Atlantic, ocean/climate indexes such as the North Atlantic Oscillation, and ocean biogeochemistry model (Mercator Green) outputs. Their results suggest that since 2011 a positive SST anomaly (warmer waters) in the tropical Atlantic, together with increasing trends in riverine nutrient inputs could contribute to the observed *Sargassum* sp. biomass in the region.

Despite the relevance of the theme to the Biogeosciences scope of publications, at its present version, the manuscript is not acceptable for publication. This manuscript is very short, having the format of an "extended abstract", and some of the methods are very quickly explained. Another issue is a full lack of a section describing the general oceanographic settings of the regions where Sargassum biomass is more frequently found. The results are not presented in an organized way, and the reader gets confused with different time scales, anomalies etc. The results would be more robust if there were data on the amount (biomass) of *Sargassum* sp. , or on the area covered by the macroalgae mats every year. Is it possible to achieve via satellite imaging? Otherwise it is difficult to "believe" that the increasing nutrient loads, the positive SST anomalies etc could be associated to the increasing macroalgae extension in the tropical Atlantic. Please do not take this comment badly, but a scientific paper in this subject should always be very rigorous with the methods and the quality of the observations.

I strongly suggest the authors to carefully re-write and reorganize the text, and resubmit the manuscript. Here below follow a suite of general and specific comments intending to improve the manuscript quality for re-submission in the future.

General comments:

The paper lacks a general map (a scheme?) on where and how the *Sargassum* sp. banks are found normally, and where the strandings are occurring more frequently. The authors have described the observations on the increasing of the strandings, but a general oceanic setting of the area also is missing. The authors start straight describing the methods used to calculate the indexes.

There is some concern on the use of the terms nutrient (or DIN and DIP) yield (Eq. 3 – 4, and well as to a transformation of DIN and DIP to nitrate and phosphate. This is not recommended, as in coastal areas, where the rivers meet the ocean, many processes may occur and change the oxidation state of inorganic nitrogen. This section should be carefully re-written, and the terms checked for the correct denomination and units. There are a few suggestions for reading in order to help clarifying this issue (please refer to the specific comments).

The authors should also avoid discussing the results in the "Results" section. A few times, for instance when reporting the results for the Amazon river discharge variability, the authors have included a few comments on how "this or that" would relate to the *Sargassum* sp. strandings. This start at page 7 and continues all over the results section. The authors are strongly suggested to

present the results obtained from the available river water discharge data (HYBAM, etc), estimates of nutrient inputs (using the formulas from Smith et al. (2003) and Araujo et al. (2014)), estimates/variability of the climate indexes (NAO etc), MERCATOR Green model outputs/anomalies, orderly, for the 1993-2015 period. Only then they should start discussing the results. The mix between results and discussion and the extra discussion section make the manuscript very difficult to understand, despite its short length.

The discussion should be separate from the conclusions. In this version, the text reads more like a long conclusion. This should all be re-written and re-organized.
Have the authors tested any type of correlation among the studied parameters in this manuscript?

The manuscript should go through a very careful revision for English grammar. Some paragraphs are too long and not clear. A suggestion for the re-writing is to always put the important, relevant information in the beginning of the sentence/paragraph. This normally helps writing shorter and clearer sentences/paragraphs. Along the specific comments there are a few hints on how the English grammar should be corrected, but it is beyond the scope of this document to highlight all grammar mistakes in the manuscript.

Specific comments:

1) The authors should review the whole text and check with the publication rules of BGD on how to refer to genus and specie: normally one uses *Sargassum natans* (in italic), and only *Sargassum* sp. when referring to a certain species or to the genus Sargassum, that includes the species “natans”, “fluitans” etc.

2) There are several parts of the text that need a bit of re-organizing. When reading manuscript, one has the feeling it has been hastily written, and the text and grammar didn’t receive much attention. Here the most important parts were highlighted, but there are many others.

Page 2, lines 5 – 10: Which important consequences to marine and coastal ecosystems?

Page 2, lines 13 – 20: Here you could synthesize the information, i.e. instead of starting the sentence with something vague like “some periods of the year”, you could say already 2From July to September...”. This information appears only at the end of the sentence.

Page 2, lines 22 – 26: Again, the information here needs to be re-organized. Among the papers cited, which one refers to increasing SST as a possible cause to increasing cases of *Sargassum* sp. strandings? And which ones to the other causes cited in the paragraph?

Page 3, line 4 and the whole paragraph: Please be careful with the wording: it is not “African atmospheric dust”, but the “dust inputs from continental west Africa” or “from the Sahara region”. You should also use “nutrient inputs” instead of “nutrients inputs”.

Page 3, line 25: you could split your results discussion and the conclusions of your study.

Page 4, line 3 : air-sea fluxes of what?

Page 4 and the rest of the text: please use “indexes” instead of “indices”

Page 4, l. 15: please use “river discharges” instead. Please also use the expressions “nutrient loads”, “nutrient data”, etc, throughout the text.

Page 4, l. 25 to page 5: please rewrite the paragraph. Some parts do not make much sense, such as “which were built using 165 water systems worldwide analysis” or “the five worldwide databases” What do you mean exactly here?

Page 5., l. 3: use “method” instead of “methodology”.
Page 5, text around eq. 1 – 2 : what do you mean by “discharged exportation”? Is this the riverine nutrient yield of DIN and DIP? It is a mass (in moles) per area per time. Please review carefully these equation, their units and the correct terms.

Eq. 3 and Eq. 4 do not use “Q” - river discharge. And how do you assume all DIN and DIP to be nitrate and phosphate? I strongly suggest to keep DIN and DIP. DIN includes nitrate, nitrite and ammonium, and the Smith et al. (2003) paper does not separates these three inorganic nitrogen species. Suggested reading to improve this part of the manuscript:

Seitzinger, S. P., Mayorga, E., Bouwman, A. F., Kroeze, C., Beusen, A. H. W., Billen, G., Van Drecht, G., Dumont, E., Fekete, B. M., Garnier, J. and Harrison, J. A.: Global river nutrient export: A scenario analysis of past and future trends, Global Biogeochem. Cycles, 24(4), n/a-n/a, doi:10.1029/2009GB003587, 2010.

Yasin, J. A., Kroeze, C. and Mayorga, E.: Nutrients export by rivers to the coastal waters of Africa: Past and future trends, Global Biogeochem. Cycles, 24(4), n/a-n/a, doi:10.1029/2009GB003568, 2010.

Aufdenkampe, A. K., Mayorga, E., Raymond, P. A., Melack, J. M., Doney, S. C., Alin, S. R., Aalto, R. E. and Yoo, K.: Riverine coupling of biogeochemical cycles between land, oceans, and atmosphere, Front. Ecol. Environ., 9(1), 53–60, doi:10.1890/100014, 2011.

Fekete, B. M., Wisser, D., Kroeze, C., Mayorga, E., Bouwman, L., Wollheim, W. M. and Vörösmarty, C.: Millennium Ecosystem Assessment scenario drivers (1970-2050): Climate and hydrological alterations, Global Biogeochem. Cycles, 24(4), n/a-n/a, doi:10.1029/2009GB003593, 2010.

Cotrim da Cunha, L., Buitenhuis, E. T., Le Quéré, C., Giraud, X. and Ludwig, W.: Potential impact of changes in river nutrient supply on global ocean biogeochemistry, Global Biogeochem. Cycles, 21(4), doi:10.1029/2006GB002718, 2007.

da Cunha, L. C. and Buitenhuis, E. T. T.: Riverine influence on the tropical Atlantic Ocean biogeochemistry, Biogeosciences, 10(10), 6357–6373, doi:10.5194/bg-10-6357-2013, 2013.

Page 5, line 25: which are the variables at hand? I’d suggest to call the MERCATOR GREEN Model outputs of inorganic N and P also DIN and DIP.

Page 6, l. 6: The figure does not present… you should say “… the results suggest that … (Figure 1).

Page 6., l. 25: It is preferable to cite a published and reviewed manuscript on the subject of ocean-atmosphere variability in the tropical Atlantic, rather than a link to a web site.

Page 7, l. 5: Isn't figure 3b relative to the climatology of the Amazon river discharge? It makes no sense to state that "The climatological signal (Fig. 3b) indicates that the Sargassum blooms and mass strandings in the tropical Atlantic Ocean, occurred generally during the ascending and the high flow of the Amazon River, i.e. from February to August.". Please review the whole paragraph.

Page 8: this summary in topics should not be here. Please re-write it in the format of a paragraph (or paragraphs), and when re-organizing the text, insert it in the discussion section.

Page 8, l. 20: please create a separate section for "Discussion" and "Conclusions".

Figures:

A figure with a general scheme for the occurring *Sargassum* sp. area is missing.

Figure 1:
It should be split in 2, one with the seasonal panels, and another with the SST anomaly. I suggest the authors to show the SST anomalies only for the seasons of the macroalgae massive strandings, and keep the other season to a supplementary figure, for instance. I have a question concerning the lower panel: the authors state the a positive SST anomaly could help explaining the increasing macroalgae strandings, but the in last years of the record, it seems to have been a negative (cooler?) SST anomaly.

Figure 2:
Why are the different climate indexes average for different months? How are they comparable?

Figure 4:
What unit is this: "kg mol d-1"? Please revise the units and refer to the comments concerning Eq. 1 – 4 in this document.
Are these monthly values? Please specify in the figure caption.
Please refer to the comment on using DIN/DIP and nitrate/phosphate.

Figure 5:
Please check the nutrient units in all panels. Why there aren't panels for chlorophyll and dissolved iron for the Equatorial upwelling area?