

# ***Interactive comment on “Coccolithophore fluxes in the open tropical North Atlantic: influence of the Amazon river and of Saharan dust deposition” by Catarina V. Guerreiro et al.***

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The authors are most grateful to the Anonymous Referee #3 for his overall positive feedback on the manuscript, and for the helpful specific comments, several of which will be taken into account for the new updated version of the paper.

RC#3: The paper uses some colloquial non-scientific language and rambles on a bit too much.

CG: The new version of the manuscript will take this comment into account.

RC#3: When reading the title, I thought that the work would mainly deal with the impli-

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cations of Amazon River and Saharan dust input on the distribution of coccolithophores, but this topic only comes in the last few pages and there is precious little written about this in the introduction. I think either change the title to a more general one or really emphasize the Amazon and dust contribution from the start and reduce all the other material (which I would suggest because it would make for a much better read).

CG: After a long process of several tries in organizing the presented data, it is our conviction that the present structure is the more efficient in “telling the story” of our study. But the referee is correct when arguing that the title does not reflect the interesting result we have shown, for example, for the LPZ flora. To address this important remark, the title will be updated to: “Coccolithophore fluxes in the open tropical North Atlantic: influence of thermocline depth, Amazon water and Saharan dust”

RC#3: One way to start would be to deemphasize the statistical aspect. I say this because the primary coccolithophore species reside in the lower photic zone while the environmental parameters are taken from second party satellite information which does not say much about the water column below the surface. Much of the statistics information are mostly broad conjectures. If the authors want to include the Statistica 13 data they should really state why it is so relevant.

CG: The authors agree when the referee argues that coccolithophores in highly stratified ocean conditions preferably thrive in deeper levels of the euphotic zone. Nevertheless, we don't have access to time-series data regarding the hydrological features of the euphotic zone in the study area for the sediment trapping period. Hence, it is our conviction that the presented satellite data provide invaluable insight into several environmental processes which were reflected in terms of coccolith fluxes and species composition, and provide a more reliable and integrated interpretation of the coccolith flux data. Despite of the limitations of using statistical analyses including environmental data that only represent the atmospheric conditions and the surface of the ocean (of which we are fully aware), we have obtained a fairly good correspondence between surface satellite data and the trap records. A remarkable example of this regards the

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drastic salinity decrease and associated increase of Chl-a at the surface, during times of enhanced fluxes of dust, organic matter, *G. oceanica* and *E. huxleyi* (not to mention total mass fluxes and biogenic silica, as presented by Korte et al., 2017). On the basis of this we agree with the referee that this fair correspondence between surface satellite data and the trap records, as well as the rather satisfying result of 63% of explained variance should be (and will be) more stressed in the discussion of the paper.

RC#3: There should be much more information on the seasonal change in the water column.

CG: We acknowledge the importance of having more information regarding the upper water column conditions for a more accurate disclosure of the seasonal dynamics of the euphotic zone and related hydrological parameters. However, such water column monitoring was not performed for the studied period and hence we don't have such possibility.

RC#3: I don't think there is much insight from the satellite data. Much more interesting is actually the Amazon and dust story as the title suggests. The authors should move section 5.2.2 to the beginning of the discussion.

CG: The authors kindly disagree with the referee, as mentioned above.

RC#3: Another issue is that the authors start by pointing out the similarity between the M2 and M4 trap data. Section 5.1 is dedicated to this. Later they go on at length about the differences (p20 lines 5-7 and figure 10). I suggest that the paper be reorganized and shortened, emphasizing the influence of the Amazon and dust.

CG: The authors would like to stress that one of the most interesting aspects of this study is precisely in the seasonal similarity vs. spatial dissimilarity between the two stations, in terms of meteorological and hydrographic forcing, which is reflected by the coccolith fluxes and coccolith settling assemblages. On the basis of this, it is our conviction that this aspect is actually one of the main findings of our study, and

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hence, should be properly highlighted. Nevertheless, the authors acknowledge that some parts of the manuscript could benefit from some textual shortening whereas the influence of the Amazon and dust could be more highlighted in the introduction.

Reply to specific comments: Page 1 line 17: North Atlantic did not collect –misplaced modifier please reword

CG: OK

P1 line 20: “were yearlong dominated” >dominated throughout the year

CG: OK

P1-33: “Still> Nevertheless

CG: OK

P1-36: “seemingly similar” > not very scientific

CG: “seemingly” will be replaced by “apparently”

P1-36: If they “differ greatly” they can’t be “seemingly similar”

CG: Answered in the line above

P2-2: “low productivity” throughout the water column or just at the surface?

CG: low productivity throughout the water column but particularly at the surface. This will be rephrased.

P2-4: in “these” areas what does “these” refer to?

CG: The authors are referring to the centers of subtropical gyres and tropical open-ocean referred in the previous sentence. This will be rephrased into a clearer way.

P2-9: “huge” amount ..huge is a colloquial term and not scientific

CG: “huge” will be replaced by “massive” and concrete values will be referred

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P2-19: “vastness” vague how about some numbers here

CG: The authors are convinced that, for the purpose of this merely introductory sentence, it is unnecessary to provide numbers. “Vastness” will be replaced by “large surface area”

P3-9: “partially” is an ambiguous word

CG: The authors meant to say that the results from Poulton et al. (2017) agree with a large part of the results from the previous authors, but not all of them.

P4–2: “Large volumes” give numbers

CG: Ok

P4: “The upper water masses in the study area” I suggest a figure showing these water masses and the locations of the traps particularly because most of the important species are located in the LPZ.

CG: Ok

P4–12: “Further down up” could be better stated

CG: This will be rephrased to “Further down to”

P4\_22: “19 October, 19 to & November 2013” please correct

CG: The referee is correct, thank you!

P5–6: was “carried out”

CG: This will be replaced by “undertaken”

P5-17: ‘and to the total samples’ > not clear

CG: the authors meant to say “original sediment trap sample”. This will be modified in the ms revision.

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P5-22: “What about the other coccolithophore “deep water species”? there are more than 2

CG: The authors decided to exclude *R. sessilis* and only include *F. profunda* and *G. flabellatus* in this formula on the basis of the high abundance and very similar seasonal pattern between the latter two species.

P5-23 3.3 Oceanographic and meteorological data. In table II the URLs are not sufficient. They should go down to the actual page locations that were used.

CG: Thank you for this comment. The table will be properly updated (see Table attached).

P6: too many “albeits”..and also one on p 9 ..some of them not accurately used.

CG: The authors will take this comment into account for the new updated version of the manuscript.

P6-12; P6-13 try to use another word other than distinctive..which is not very scientific

CG: “Distinctive” will be replaced by “distinct”

P8 Fig 3: I don’t see dashed lines for the Shannon-Weaver Diversity Index

CG: The referee is correct. “Dashed line” will be replaced by “solid line”.

P8 -23 are you sure about *R. sessilis*? Would like to see an image.

CG: The referee is invited to look at two examples of coccoliths of *R. sessilis* in the figure attached (in the figure on the right, please note the coccolith on the upper left side)

P9 fig 5 ..the color scheme could use improvement.. Hard to read because of the color choice

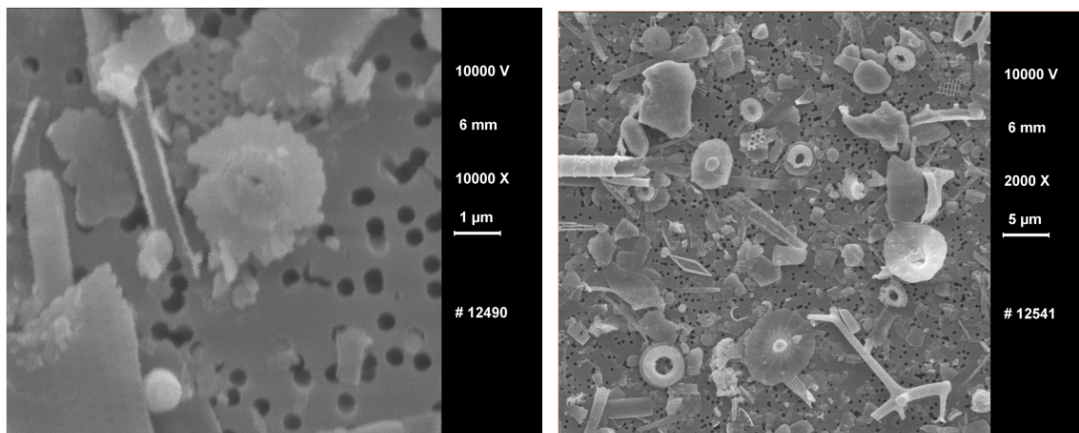
CG: Ok

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**Fig. 1.** Two images of *R. sessilis*

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