Journal: BG
Title: Contribution of dust inputs to dissolved organic carbon and water transparency in Mediterranean reservoirs
Author(s): I. de Vicente et al.
MS No.: bg-2012-221
MS Type: Research Article

Dear Associate editor,

Please find below our reply to the two reviewers. The reviewers´ comments are in *italic* type and our replies are in **bold** type.

Referee#1

In his/her general stament this reviewer states: "*The manuscript describes a very special system*." …and "*For the wide readership of Biogeosciences the impact is not broad enough and the interpretations are too vague*".

In the new version of this manuscript, we have rewritten and restructured the introduction and the discussion sections.

We have explicitly emphasized that the dust export from Sahara and Sahel regions to the atmosphere is a global scale process which deposition affects to both terrestrial and aquatic ecosystems. Comparatively to the oceans, the impact of atmospheric deposition in inland waters has been poorly explored. In addition, the role of inland waters in the global carbon cycle is becoming essential to understand terrestrial carbon budgets. Therefore, we think that the study of atmospheric inputs of organic carbon to inland waters deserves more attention and is a very timely and general topic.

We think this paper presents relevant results beyond the local peculiarity. The study reservoir are very contrasting in trophic status, morphometry, etc to cover a wide range of ecosystem variability and, thus, with a broad interest for Biogeosciences readers linking dust in the atmosphere and aquatic ecosystems.

Specific comments:
At the end of the introduction objectives and hypotheses are missing. It ends with a description of the locations and summary of the analytical work.

In the previous version, the objectives of this study were embed in a long paragraph and they could be easily overlooked. In this new version, we have exposed our objectives more explicitly in an independent, last paragraph in the introduction section. We have also shortened the site description in the introduction and moved it to the methods section.

Methods:
There is need for some more details about the methods, e.g. the sampling procedures such as air volumes collected, calculation of rates. Otherwise one has to refer to the reference to know the basic conditions. What is the reason

to add ultrapure water to the samples if the volume is less than 1000 ml? Then you have to correct for this addition.

Here, we think the reviewer had a misundertanding in relation to the type of atmospheric collector that we used. The passive collectors did not filter any air volume, they collect the atmospheric deposition by gravity sedimentation (dryfall) or washout (rainfall) therefore the units are per surface (m^{-2}) and collection time (days).

To avoid this type of confusion among the potential readers and to follow the reviewer´s suggestion, we have included more details about the atmospheric collectors as well as the equation for the calculation of the deposition rates (please see paragraphs 1 and 2 in 2.2.section).

I think a figure showing the location of the reservoirs instead of the surface to depth ratio is more informative. The characteristics of the reservoirs are already well described in chapter 2.1.

The locations of the three study reservoirs are only 40 km apart, therefore we think this figure would be not very informative. However, we have included the latitude and the longitude coordinates for each reservoir in the new Table 1.

We have decided to keep Figure 1 since it reflects very well the morphometry of the reservoirs and this is a key parameter to detect the effect of dust inputs in the reservoirs.

Results:
It may be better to present the DOC data in μM instead of mM.

We think that mM is appropiate since the values for the reservoirs are high enough and the molar absorption coefficients are obtained normalizing by the DOC concentration in mM.

Page 8314, line 3-8: This is part of the discussion.

We have rewritten this sentence in a less speculative way, but we have decided to keep it in this section to show that the peaks of the WSOC inputs are concomitant with strong Saharan dust intrusions in the area. This observation is a fact, not an speculation.

The discussion should be re-organized. I think the manuscript would benefit from combining the results and discussion section. Then a well written conclusions section could pull it all together.

We have rewritten the discussion and conclusions sections

Minor comments:
Page 8312, line 19: What depths were chosen? Samples take above the thermocline, may be better than "over".

We have included more details about the samplings and changed the word "over" by "above".

Figures:
Use points instead of comma.

Done.

Referee#2

In his/her general staments the reviewer states:
….. This paper requires some important revisions to further clarify key points (see below) and to fill in some of the broader relevance that is missing...

In the revised version of the manuscript, we have rewritten and restructured the introduction and the discussion. As we have mentioned in our reply to referee 1, we have emphasized that the dust export from Sahara and Sahel region to the atmosphere is a global scale process which deposition affects to both terrestrial and aquatic ecosystems. Please, see our reply to referee one's general comments.

G1. For the Biogeosciences audience, this reviewer feels that the authors could improve the paper by discussing the wider implications of their research, beyond what is stated in the introduction. Perhaps a Conclusions section or a separate section on Environmental Implications could be added to address what these results mean for other reservoirs in light of projections of future dust transport or severity of dust events, for example.

We have rewritten the discussion and conclusions including some environmental implications.

G2. In tackling the atmosphere-water connection, the paper does well to examine both direct and indirect influences. To examine direct influences, the authors compiled WSOC mass input rates from atmospheric deposition collectors and reservoir morphometric characteristics to show that, in terms of DOC mass, the contribution from atmospheric inputs was fairly low. They also examined the influence of chromophoric compounds on the transparency of the reservoirs and found that the colored compounds in atmospheric deposition did, in fact, change the depth of the photic zone.
The greater influence of atmospheric deposition on reservoir optical properties than on DOC mass in the reservoir is an important point of discussion that could be elaborated upon. Is this effect due to the high molar absorption of the WSOC?

In effect, the greater influence of atmospheric deposition on reservoir optical properties than on DOC pool is due to the high molar absorption coefficients of WSOC along with the higher susceptibility of

cromophoric compounds to environmental changes. We have expanded significatively the discussion associated with this issue.

G3. Another important result of this study is that the indirect influence of dust inputs on reservoirs may be very important in lakes with certain biogeochemical (P limitation) and morphometric characteristics. In particular, the explanation of synchrony in two of the reservoirs that may result from phytoplankton stimulation by dust (bottom of page 8316) is well thought out. The evidence of P limitation in those reservoirs, but lack of P-limitation in the third is very convincing. This section is exciting to read and could be highlighted better in its own separate section on indirect effects (see comment S3).

As it was suggested by this reviewer we have separated the discussion in two sections: 1) Direct effects of WSOC atmospheric deposition on reservoir DOC and CDOM and 2) Indirect effects of dust deposition on reservoir DOC and CDOM.

Specific comments

S1. For each reservoir, the authors perform a fairly novel calculation of the depth at which transparency is reduced by 10% (compared to the surface). This appears to be a useful and quantitative measure of chromophoric compounds in atmospheric WSOC influence lake transparency by absorbing light. However, the method is not explained in sufficient detail and it is difficult to understand how a difference could be calculated when there is no baseline for conditions without dust. For example, since the reservoirs are continuously being bombarded by dust, how are the authors able to extract the dust influence from pre-dust conditions and from other influences such as CDOM from bacteria and algae? Also, the influence of photobleaching is discussed in the context of reservoir CDOM observations. How is photobleaching accounted for in the calculations for z10%?

In this new version of the MS we have included more details and the equations for the calculations of the effects of aromatic carbon inputs on reservoirs water transparency (please see paragraph 4 in section 2.2.).

In effect, as the reviewer is wondering, it is hard to discriminate the CDOM baseline conditions in the reservoirs, since they are receiving dust continuously and are submitting to photobleching in the photic zone. More accurately our comparison (Figure 6) is between the real water UV transparency (in situ conditions) calculated using the CDOM measured in the reservoirs that is a result of all previous inputs and losses (e.g. dust and biological inputs and photobleaching losses, respectively) and the potencial water UV transparency (estimated a_{320}) when we consider the CDOM provided by the atmospheric inputs is dissolved in the reservoir´s epilimnion. These last values are estimations. We hope that now all calculations are easier to understand.

We have also compared the residence time of a_{320} atmospheric inputs in the reservoir with the a_{320} half life due to photobleaching losses (please see paragraphs 6 and 7 of the 4.1 section).

S2. The final paragraph of the introduction on page 8310 should be revised so that it contains more of a thesis statement. For example "Our goals or objectives were to: : :"

In fact, as it was also mentioned by reviewer#1, in the previous version the objectives of this study were embed in a long paragraph and they could be easily overlooked. In this new version, we have exposed our objectives more explicitly in an independent paragraph. We have also shortened the site description that was included in the introduction and we have moved it to the methods section.

S3. For clarity, one approach may be to separate the Discussion section into subsections covering 1) direct and 2) indirect influences of dust on these reservoirs. The authors do well to highlight these differences in the abstract. The indirect effect of atmospheric deposition on reservoir DOM and water transparency is mentioned in at least two places in the discussion, but it is somewhat buried and it seems that the result would have more impact if it was highlighted in a specific sub-section.

We thank this reviewer´s suggestion and we have split the discussion in two sections as mentioned above.

S4. There are many errors in English usage and grammar. Some have been noted below. Missing "the"s have been listed up to page 8309. After that, the authors should carefully comb the text for other omissions. There are also some errors in spelling or diction/word choice – eg. "synchronic" and "contrarily", which should be "synchronous" and "in contrast to", and those should be corrected throughout the manuscript. Agreement between plural subjects and plural verbs is also a problem and should be addressed.

We thank all the suggestions to correct these errors.

S5. The Study sites sub-section should provide more information about the setting.The river mentioned is in southern Spain; can it be assumed that the reservoirs are also in southern Spain? Are they near a city? Are there other DOM inputs to be aware of?

Yes, the reservoirs were located also in Southern of Spain. We have included the latitude and longitude of the reservoirs in Table 1. In the section 2.1. (Study site) we have included details of the landscape of these reservoirs. Quentar is a mountain reservoir far from urban pollution and Cubillas and Beznar are located in open-valleys with

small-moderate urban influence. During the stratification, inputs from runoff are extremely limited.

Also, why are there no molar absorption coefficients for wet deposition? This should be described in the methods, or as a footnote to Table 2.

We thank this reviewer´s observation. This was a slip. We have the data for the WSOC concentration and the absorption coefficients for wet deposition, therefore we can present the molar absorption coefficients. We have included them in the new Table 2.

S6. The methods should list the precision of analyses or some measure of error associated with the analyses.

We have included the precision of the used instruments in the method's new version.

S7. The titles of table 2 and 4 should specify that the range is given in parentheses.

Done.

Technical corrections:

All the technical corrections suggested by the reviewer has been corrected and we have included also all his/her suggestions. We appreciate very much the effort of this reviewer.