

Dear Editors,

We thank reviewer #3 for the very positive review and his comments and suggestions, which will help to improve the manuscript. In the revised version, we will follow the suggestions and clarify the issues raised. Please find below our answers (in blue). Furthermore, some new literature has been published since the reviews came in, we include relevant work by Beusen et al. (2009) and the group of Hartmann et al..

Reviewer remark:

*General: when talking about concentrations, the authors most often use the term "contents". It would be better to refer always to the same term and I suggest using "concentrations" which is unambiguous.*

Reply: we will follow this recommendation in the revised manuscript.

Reviewer remark:

*p. 1347, line 16: the reference to Conley (1998) does not refer typically to the soda extraction but to an "inter-laboratory" comparison. The original reference for the method is "Paasche (1973) modified by Nelson et al. (1989)" Paasche E. 1973. Silicon and the ecology of marine plankton diatoms. I. *Thalassiosira pseudonana* (*Cyclotella nana*) grown in a chemostat with silicate as the limiting nutrient. *Marine Biology* 19: 117-126. Nelson D.M., Smith Jr. W.O., Muench R.D., Gordon L.I., Sullivan C.W., and Husby D.M. 1989. Particulate matter and nutrient distribution in the ice edge zone of the Wed- dell Sea: relationship to hydrography during late summer. *Deep-Sea Research* 36: 191-209.*

Reply: we will replace the reference with the proposed ones.

Reviewer remark:

*p. 1350, line 28: even though the PRISRI database is mentioned in Meybeck (2003), a short description including the location of the database could be useful for the reader.*

Reply: as also mentioned in the answer to reviewer #1, the PRISRI database overall patterns of dissolved matter have been published and extensively tested by Meybeck (2003). The same data quality criteria were applied to the Glori dataset (Meybeck and Ragu 1995). The accent of the manuscript here is on the pristine character of the catchments, especially with respect to influence of dams. We will include a description of the database in the text, in line with the answers provided to the comments of reviewer #1.

Reviewer remark:

*p. 1352, last sentence of the first paragraph. An additional sentence should be useful to state how robust is the affirmation of a bed load representing 10% of sediment transport.*

Reply: bedload is generally higher in steeper headwaters and where mountainous rivers drain quickly to the oceans, but these kinds of data are seldom reported and are thought not to account to significantly in the global budget. The global 10% figure from Milliman and Syvitski (1992) seems to be the most robust number. We will adapt the text accordingly and provide more references from studies focusing on sediment transport in rivers, underlining this number (e.g., Zanke 1982).

Reviewer remark:

*p. 1353, line 13: the explanation for the abbreviation M should come earlier in the text in the abstract.*

Reply: we will do this in a consistent manner.

Reviewer remark:

*p. 1356, line 26: the reference to Table 1 for the limnic index is incorrect; there is no value of this index in the mentioned table. Probably the authors want to refer to Table 6 (?).*

Reply: this is correct, we will change this.

Reviewer remark:

*p. 1358, line 21: refer to Table 3 at the end of the first sentence, rather than at the end of the paragraph on next page.*

Reply: we will do this.

Reviewer remark:

*p. 1359, line 24: replace "... silica contents (concentrations) ..." by "... silica concentrations ..." (see my general comment above).*

Reply: we will homogenize the description of concentrations (also elsewhere in the text).

Reviewer remark:

*p. 1361, line 2: "As a rough approximation, we estimate that the additional uncertainty on extrapolated yields, using our method, yields in a total error probably not much greater than 30% and probably less in well-documented areas." This affirmation is not scientifically correct and the authors might need to give little information about their hypotheses leading to this approximation. This also holds true for the following sentence "The hypothesis of constant PSi content in river particulates induces an uncertainty of +/-15%, probably lower than the uncertainty of particulate matter fluxes." Definitely more information is needed on how these estimates are obtained.*

Reply: true. Based on extrapolation studies for dissolved silica fluxes (Hartmann et al. 2010, Jansen et al. 2010), it can be assumed that the performance of the budget approach is weaker than the 10% identified in the studies referenced. However, while it is difficult to put actual numbers on the overall uncertainties, considering available data and that the budget approach was designed to overcome gaps of data for certain local and regional areas, some aspects are better known, such as the uncertainty induced by extrapolating annual averages of concentrations (c.f. Moatar and Meybeck 2007). The number 30% has been chosen as a regional conservative estimate based on the studies cited. Locally, the uncertainty is of course higher. However, as the budget approach closes gaps in knowledge of DSi-concentrations from certain areas, a reliable and calculable number can of course not be provided for these areas. In this sense we will clarify this in the revised manuscript, also for the particulate fluxes.

Reviewer remark:

*p. 1361, line 25: give the exact references within the special issue of GBC mentioned.*

Reply: uncertainty of the Global-NEWS models is discussed in the individual papers by Beusen et al. (2005), Dumont et al. (2005) and Harrison et al. (2005), in addition to the overview paper by Seitzinger et al. (2005). We will add the references and will clarify the text accordingly.

Reviewer remark:

*p. 1366, line 27: as diatoms are not the sole organisms depositing biogenic silica, I suggest replacing "... by diatoms ..." by "... by silicifying organisms such as diatoms ...".*

Reply: we will do this.

Reviewer remark:

*p. 1370, line 26: I have difficulties understanding the sentence "The net riverine inputs to the open Pacific, even without considering the estuarine and shelf retention, could therefore be lowered by half of the gross river inputs." There is need for explanation.*

Reply: this sentence might have been unclear. What we'd like to express is that the net inputs to the open Pacific after transformation and retention processes in coastal areas are probably only half of the riverine inputs arriving at the mouths of the rivers. This is taking into account the position of the regional seas basins only. If the estuarine filter is considered, this retention might even be higher. We will clarify the text accordingly.

Reviewer remark:

*p. 1372, line 13: I would not suggest any probable range of PSi dissolution as the amount of available data is so scarce and, so, I suggest removing "... leading to a suggested probable range of 1% to 5% of dust fallout PSi".*

Reply: we will do as suggested.

Reviewer remark:

*p. 1373, line 8: replace "...(classes 1, 3, and 15% of 4, 5, 9-12 ..." by "... (100% of classes 1, 3, and 15% of classes 4, 5, 9-12 ...".*

Reply: we will do this.

Reviewer remark:

*p. 1374, line 11: replace "This retention has already been attributed to the decrease of silica in some large river basins ..." by "This retention has already been suggested as responsible for the decrease of silica in some large river basins ...".*

Reply: we will do this.

Reviewer remark:

*p. 1391, for clarity of the table, please add a blank line in between the DSi and PSi related data.*

Reply: this will be done.

Reviewer remark:

*p. 1398, inside fig. 3, DSi and PSi should be replaced respectively by YDSi and YPSi.*

Reply: OK, will be done.

Additional reference (as far as not yet cited in manuscript or referenced in response to reviewer #1):

Zanke U. (1982). Basics of sediment transport (in German: “*Grundlagen der Sedimentbewegung*”). Berlin-Heidelberg-New York, Springer-Verlag. XII, 402 pp.