

Interactive comment on “Estimation of nutrient contributions from the ocean across a river basin using stable isotope analysis” by K. Nakayama

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General comments B-1 This study is important and contemporary because there is still limited knowledge about watershed-scale budget of nitrogen deriving from marine origins. This is probably the first estimate of this kind, and thus provide good scientific basis when emphasizing the importance of maintaining connectivity between ocean and the land for ecosystem health at the catchment scale. I understand that this kind of budget study requires some major assumptions. Among those assumptions, I am not really convinced about their final estimation of 22.9% as potential contribution of MDN to the land if dissolved form is also considered. This value is also clearly stated in the figure 5, but there is really no supporting evidence or information for the assumption that the contribution to dissolved nitrogen to the SOM is as the same as the

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particulate form. To me, it is misleading. Since there are many other sources of errors and uncertainty in the estimation (e.g., fractionation in the process of decomposition, land SOM potentially not very representative source of SOM exported from the river, which could be largely originating from the riparian area or in-stream sources after going through long-term and complex ecosystem processes, and so on), the fact that 23.8 and 22.9 is nearly equal is not that important. Authors should put more emphasis on the value of 3.6% as their major finding.

Reply: Thank you for your comment. The manuscript was revised by following your comment. In particular, Fig. 6 was revised and 22.9 % was removed from the figure.

B-2 Specific comments P.5541, L. 20: “chum run after considering capture rate” should be calculated as 10375 if a factor of 0.2 is used as capture rate.

Reply: Thank you. “2075” was wrong and was replaced by “3075”.

B-3 P.5545, L. 2: related to the general comment above, authors should briefly explain about the process involved in how dissolved MDN was incorporated into SOM and at the same time why it was not detected in this study; if it was really SOM then it should have been quantifiable by SIA mixing model, I think. Perhaps, the use of the term SOM was inappropriate.

Reply: Thank you for your comments. We cannot estimate the contribution of dissolved MDN to nitrogen because organic materials are only used in the SIA. Therefore, we revised the manuscript.

B-4 Technical issues P.5536, L. 25: “Hinderbrand” needs to be corrected.

Reply: Thank you. The sentence was revised.

B-5 P.5537, L. 25: “wood web” needs to be corrected.

Reply: Sorry about the typo. “food web” is correct.

B-6 Figure 2: It can not be read due to too small letters; font size needs to be enlarged

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so that it can be read. In particular, the horizontal extent of bars at each station of (b) and (c) did not make sense to me; presentation style needs to be improved. Also there is no soil sampling stations shown for (b) and (c) with green circles and this is confusing.

Reply: Thank you for your suggestions. The figure was revised by following your comments.

B-7 P.5542, L. 17: this is probably Eq.6 instead of Eq.5.

Reply: Thank you. The equation number was corrected.

B-8 Figure 3: Provide units for both axes properly and also explain what the error bars denote.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C2600/2015/bgd-12-C2600-2015-supplement.pdf>

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