

Interactive comment on “Transfer of diazotroph-derived nitrogen to the planktonic food web across gradients of N₂ fixation activity and diversity in the Western Tropical South Pacific” by Mathieu Caffin et al.

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The manuscript by Caffin et al. addresses the important question on how much fixed N is transferred to the dissolved versus the particulate planktonic pool. Caffin et al come up with a nanoSIMS based study to not only make this distinction, but to also show that the composition of the diazotrophic community has an impact on the subsequent channeling of N in the Ocean, and they could identify that *Trichodesmium* promotes a transfer to the dissolved phase, while UCYN-B would promote transfer to non-diazotrophic plankton (mostly picocyanobacteria, followed by heterotrophs). In-

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triguingly, a higher share of the N pool was transferred to higher trophic levels when *Trichodesmium* dominated, however, an overall high transfer efficiency was observed in UCYN-B dominated environments. The manuscript is, to my knowledge, one of the first to address the channeling of N through the food web, with that it critically advances the understanding of N₂ fixation in the Ocean. I thus highly recommend publication after addressing the following general and specific recommendations.

General comments:

Overall, the manuscript seems to need a bit of streamlining. I see, this is not an easy job to do and I appreciate the thorough introduction and methodological explanations, as well as the detailed description of the results. However, it seems a bit of an overkill given the obvious key results of the two modes of DDN channeling and its subsequent transfer to higher trophic levels. I recommend to reduce the length of the text in order not to dilute your findings.

In the context of the discussion of DDN transferred to zooplankton, either directly or indirectly, I would like to see a link to export production, which may be extremely important in the context of enhanced CO₂ uptake through certain ecosystem compositions.

I am a bit worried about two things: first, some share of what you measured may be an artefact due to *Trichodesmium*'s sensitivity to mechanical stress, second, samples were taken using two different methods, i.e. from Niskin bottles and from a pump system, the latter of which is suspected to disrupt cells. Please address those concerns.

Specific comments:

p.1

I. 15: What do you mean with atmospheric- I assume dust input? In a way N₂ fixation is atmospheric.

I.16: Which technical limitations- such as tracing the isotope fractionation? That's possible at least to a certain degree

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l. 25: this is somewhat difficult to understand as it seems contradictory to the previous sentences. Please clarify that you are referring to the pool that is transferred to plankton

l. 30: Please add an explanation, here, otherwise it seems contradictory to the previous statements

p.2

l.9: Add the study by Duce et al, 2008.

l. 14: I identified some archaea being important in the Pacific, feel free to add the reference (or even not, Löscher et al, 2014 in ISMEj)

l. 31 N₂, 2 has to be in subscript

p.4

l.16, l.21: 15N, 15 in upper case

l. 20: Why would Trichodesmium be toxic?

p. 5

l. 15 onwards is largely the exact same text as in ' In depth characterization of diazotroph activity across the Western Tropical South Pacific hot spot of N₂ fixation' by Bonnet et al. As there is no point to repeat that, I would recommend to refer to this manuscript instead of having such a strong overlap.

p.8

l.17, l.23, p.9, l.11: please mind the upper and lower cases

p.10

l.28: I would like to see the rates as per day

p.11

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I.1 under the form of DON- sounds awkward, please rephrase

I.17 Sentence sounds awkward, please rephrase

I. 19 What bacteria? I assume, non-phototrophic ones. . .please clarify.

I. 29 down to what?

p.11

I.7: I don't quite get this conclusion.

p.12

I.5 + in upper case

I.27 This is actually worrying, thus all of it may be an effect of how Trichodesmium is treated during the experiments

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