

Interactive comment on “Seasonal, daily and diel N₂ effluxes in permeable carbonate sediments” by B. D. Eyre et al.

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This paper presents a very useful new dataset detailing denitrification rates in permeable carbonate sands. Although the work is somewhat outside my field of expertise, I am satisfied both that the approach is sound and that the results are of potential (global) significance. My main suggestions for improvement of the manuscript relate to a better contextualization of the study in the introduction, some restructuring of the discussion, and a better definition of the parameters derived from the raw flux data. I wish the authors success with their revisions.

Comments (Page 17XXX, Line)

438, 16-17: Insert ‘denitrification’ between ‘shelf’ and ‘may’.

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439, 1-5: The opening sentence is awkwardly constructed. I suggest to include one statement about the importance of nitrogen for system production, followed by another about denitrification being responsible for the removal of nitrogen from a system. There are a few multi-clause sentences throughout the manuscript. I will highlight some of these but maybe the authors can read through again from the perspective of a non-native speaker and simplify a few things.

439, 23-24: Given the importance of advection to denitrification rates, I would suggest to expand a little here on the mechanism of this relationship. I realize this is dealt with in Eyre et al. (2008) and included at the end of Section 4.3 but an earlier elucidation would assist the non-specialist reader.

Introduction (general): The study uses multiple flux measurements to estimate not only denitrification rates but also (aerobic) respiration rates and the efficiency of coupling between respiration, nitrification and denitrification in carbonate sands. The introduction should provide some context for these further aspects of the study.

I would also find it very useful to see a simple table detailing the potential reactions and transport processes involving N species in these sediments (N₂ fixation, ammonification, nitrification, denitrification, nitrogen storage by bacteria, advective pore water flow) which could be referred back to in the discussion. The table could include an indication of which processes are expected to be active by night and by day.

Section 2.3 (general): This section describes some calculations used to convert flux data to rates of sedimentary redox reactions, but it is incomplete. I would like to see a more logical and comprehensive introduction here of several concepts discussed later in the manuscript. In particular, a single clear description of all ‘second order’ parameters derived from the data is crucial for the logical progression of the paper. Presently, Gross Primary Production (GPP) and Net Primary Production (NPP) are defined in the caption of Fig. 1, while ‘remineralization ratios’ (451, 2) and ‘denitrification efficiency’ (452, 22) are only defined in the discussion. I suggest to include a table with a system

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of equations showing how measured fluxes are converted into reaction rates, and how these are then combined to estimate efficiencies etc.

Furthermore, I am not fully aware of the conventions in this field, but I would usually expect to see parameters such as GPP, NPP and respiration reported in terms of moles of carbon, rather than oxygen. If this was done, it would make the subsequent discussion of substrate C:N ratios easier to follow.

446, 20: Try to avoid referring forward in the manuscript if possible.

447, 3: Insert 'by' between 'measured' and 'Eyre'.

448, 5-10: Is it possible to illustrate graphically the relationship between light availability and NPP?

450, 1-17: The opening sentence of this paragraph is too long. I would suggest to deal with one potential interpretation at a time, rather than listing the full set. Similarly, I suggest to break paragraphs between the discussion of different processes, e.g., a paragraph break can be inserted on Line 11. This principle can be applied throughout the discussion.

450, 18 and 453, 9: The system is described first as an 'oligotrophic coral reef environment' then as a 'shallow productive environment'. I suggest to rephrase with more consistent terminology.

451, 2/22: By N-N₂ I think the authors mean N₂-N, or nitrogen in the form of dinitrogen. Is that correct? In any case, I suggest to remove these definitions from the discussion and include as clearly as possible in the methods section (see above).

452, 22: In this definition, 'DIN' is used in place of NH₄ + NO_x. Again, this needs to be more consistent and all acronyms need to be clearly defined.

453, 21-22: As no data are presented for nitrogen fixation, or uptake of nitrate/ammonium by benthic microalgae, it is not possible to state that one is more likely

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than the other as a mechanism for explaining the low dinitrogen effluxes during day-time. If the authors believe benthic nitrogen fixation to be a real possibility, are there any references which can be used to support this (e.g. by indicating which benthic species may be responsible)?

455, 20: The last sentence sounds incomplete to me. Maybe add 'should be investigated'.

Figures: I would urge the authors to check the labeling and captions of all figures after restructuring the methods section as suggested above. E.g. Figure 1 panel 2 should report either O₂ fluxes (in mol O₂ per area per time) or, in processed form, net benthic primary production (in mol C per area per unit time).

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