

Interactive comment on “Distinctly different bacterial communities in surface and oxygen minimum layers in the Arabian Sea” by Mandar Bandekar et al.

Anonymous Referee #1

Received and published: 17 June 2016

The manuscript by Bandekar et al, entitled ‘Distinctly different bacterial communities in surface and oxygen minimum layers in the Arabian Sea’ uses 16S rRNA gene sequences to examine seasonal and depth variations in bacterial community structure in the Arabian Sea. The oxygen minimum zone of the Arabian Sea has been relatively poorly studied to date and with the expansion of these regions predicted, an analysis of the bacterial community structure and the drivers of this, makes this manuscript timely. However, I have a number of concerns that I have outlined in detail below, that need to be addressed prior to publication.

The focus of the paper seems a little confusing as it is currently structured. Based on the introduction, beginning of the discussion and last sentence of the abstract, the

[Printer-friendly version](#)

[Discussion paper](#)



authors seem to want to make this a nitrogen story, but have minimal data to support this being the focus of the paper. In the discussion of the data they only discuss denitrification for four lines and anammox is never mentioned. I think the authors should restructure the paper, to focus it on community structure and diversity and the drivers of this. Currently the auxiliary data available is not at all utilized, where it could be used for multivariate statistical analysis or something similar, to potentially reveal what variables are shaping the community, instead of relying purely on the literature.

The authors also need to emphasize how this study builds on the work by Jain et al, 2014 'Temporally invariable bacterial community structure in the Arabian Sea oxygen minimum zone', which was conducted at the same time as this study, by a number of the authors of this manuscript.

The referencing throughout needs urgent attention, a large number of references are missing and / or incorrect ones used.

I think it would be beneficial for the authors to ask a native speaker to read the manuscript for both language and grammar.

Line 16: 'zone' instead of 'region'

Line 28 to 29: This sentence seems unnecessary, your data shows no evidence for anammox (discussed more below) and denitrification is only very briefly touched on with respect to your own dataset.

Introduction: A more detailed introduction to the Arabian Sea is needed here, discussing circulation and the monsoon.

Line 34 to 36: Surely OMZs are a result of both poor ventilation and productivity fuelling the rain down of organic matter, this needs to be more explicitly addressed.

Line 36: Lisa, 2003 is not an appropriate reference here, I suggest Karstensen et al, 2008; Gruber et al, 2007 and / or Codispoti et al, 2001.

[Printer-friendly version](#)[Discussion paper](#)

Line 40 to 44: I think it is important that you emphasize here the importance of OMZs for global nitrogen loss.

Line 40: What do you mean by 'intense'?

Line 40: Stramma, 2008 is not the appropriate reference for the cycling of N within OMZs.

Line 43: Why are we suddenly discussing hypoxic conditions, this has not been defined and in the previous sentence you are discussing anaerobic pathways.

Line 51: Reference needed.

Line 51 to 53: There are a number of manuscripts that discuss both the community structure and metabolism within OMZs, for example Stewart et al, 2012; Wright et al, 2012 etc. I agree that less is known about the Arabian Sea, but surely the Jain et al, 2014 paper is relevant here.

Line 57 to 59: The way this is currently written is confusing – im assuming you are trying to say something along the lines of 'OMZs account for 20 to 40% of global N loss from the oceans, with 10 to 20% thought to occur in the Arabian Sea' Codispoti et al, 2001; Gruber, 2004 etc

Line 61: The references Newell et al, 2011 and Bouskill et al, 2012 refer to nitrification not N loss processes, so should be deleted. A large number of studies that have looked at N loss in the Arabian Sea are never mentioned Devol et al, 2006; Nicholls et al, 2007; Ward et al, 2009; Jensen et al, 2011; Lam et al, 2011.

Methods: How the oxygen and nutrient data were collected and analyzed is not mentioned, please include.

Line 79 to 80: I find the terms 'intense denitrification zone' and 'deep denitrification zone' misleading and confusing as denitrification is only discussed very briefly with respect to your data in this manuscript. Im assuming this is because you want to

[Printer-friendly version](#)[Discussion paper](#)

use the same terms as Jain et al, 2014, but upper and lower OMZ would be more appropriate.

Line 105 to 106: What does this sentence mean 'you kept the DO profile in mind'?

Section 3.1: Define your OMZ for the reader. Use values in this section, terms like 'slightly more' and 'quite low' are meaningless to the reader, unless they are relative to something.

Section 3.3: This section is very hard to read. Why in the figure are the OTUs shown as a percentage, but here you mention a number, please be consistent. Please also check all of the values mentioned in this section, as they do not all currently match the figure.

Discussion: subsections here would help the reader. Why does the discussion start with denitrification and anammox when this only takes up four lines of the whole discussion when you start to discuss your own data. Do the authors think the lack of evidence for anammox is a result of saturation not being reached at OTU or sequence level, so rarer organisms such as Planctomycetes are not being picked up? I would restructure here to focus the beginning of the discussion on community structure and diversity. It would be advantageous to compare this to your auxiliary data oxygen, nutrients, TOC, to see what variables are shaping the community.

Line 296 to 297: References needed

Line 297 to 299: This is not true, a number of studies have examined the community in OMZs and you even cite some of them later in the discussion, for example, Ulloa et al, 2006; Bryant et al, 2012; Stewart et al, 2012; Wright et al, 2012 etc

Line 302 to 317: This needs to be moved to the introduction.

Line 329 to 330: What is your evidence for this statement?

Line 338: The variation in diversity with depth in Ganesh et al, 2014 is dependent on

the size fraction looked at, this is an important point that is currently missing from this sentence.

Line 344: The difference to the Jain et al, 2014 manuscript warrants further discussion here, you sampled at exactly the same stations / depths at the same time. In general, why are not more comparisons made to the Jain et al, 2014 manuscript and how your study builds on that work?

Line 351: 'proficient'?

Line 359 to 362: You have TOC data, why are not testing this hypothesis? Why are you not using your auxiliary data to look at what is shaping the community – you never even refer to the oxygen and nutrient data in Figure 1 within the discussion.

Line 362 to 366: References or evidence for this statement is needed.

Line 375: Include references Lavin et al, 2010 and Ulloa et al, 2013.

Line 381: Reference needed.

Line 383: Surely a reference for N cycling in the Arabian Sea would be more appropriate here.

Figure 1: Use the same scale across all panels. Is it really necessary to have the same oxygen profiles in the upper and lower panels?

Figure 4 and 5: Where are the reference sequences?

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-147, 2016.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

