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Biogeosciences Discussions

Interactive comment on "An oceanic fixed nitrogen sink exceeding 400 Tg N a^{-1} vs the concept of homeostasis in the fixed-nitrogen inventory" *by* L. A. Codispoti

L. A. Codispoti

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Section I: General Comments

General Comments:

I thank the three reviewers (Drs. N. Gruber, J. Sarmiento and S.W.A. Naqvi) for taking time out of their busy schedules to review this manuscript. I must also apologize for the delay in my response that arose from a heavy schedule that included some hard deadlines, and some other matters beyond my control.

Two of the reviewers vote for publication with minor revision. One had significant problems with the manuscript, although he did not explicitly suggest rejection and liked

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some aspects of the paper. Thus, it seems worthwhile to seriously consider the reviewers comments, modify the manuscript where appropriate and submit a final revised version. I should note that my travels included attending a scientific meeting at the National Institute of Oceanography in Goa India where I was able to talk to two of the reviewers (Naqvi and Sarmiento).

There were generic comments from Drs. Gruber and Sarmiento relating to the "thundering" tone of the paper, the lack of new or more quantitative data, etc. I think that giving a historical background to its development may help to explain those facets of this contribution. Briefly, it was written, in response to an invitation to contribute to the volume arising from the SPOT-ON (Significant Processes, Observations, and Transformation in Oceanic Nitrogen) conference held in Warnemünde, Germany during June-July 2005. I was asked to be the opening plenary speaker at this meeting and to provide a provocative talk. Since the paper is based on that talk and has a "thundering" tone, "gets the adrenaline flowing" and can be considered a position paper as suggested by Dr. Gruber, I assume that I met the requirement to be provocative! The paper does have a point of view. While I tend to agree with the statement of Deutsch et al. (2004) that "The magnitude of sources and sinks of fixed N in the modern ocean are probably not known to better than a factor of 2." my reading and experience lead me to the conclusion that future research is likely to suggest that my estimate for the oceanic sink term ranges between realistic and too low. I agree with Drs. Sarmiento and Gruber, however, that our present state of knowledge permits disagreement. Indeed, a major goal that I have for this paper is that it excites interest in areas of research that might help to put our estimates on firmer ground. I would not be surprised if future research shows an oceanic fixed-N budget much closer to balance than the recent budgets that my co-workers and myself have presented. I would be surprised, however, if our estimates for the overall gross source and sink terms are not increased, and if we do not wind up with a much greater appreciation of the diversity of nitrogen fixation and denitrification habitats. I note that Drs. Gruber and Sarmiento are co-authors on a recent paper (Deutsch et al., 2007) suggesting a modest upward revision of the

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oceanic nitrogen fixation rate. From the philosophical point of view, I believe that it is important to have a better understanding of the sites and controls on oceanic nitrogen fixation and denitrification. From the practical point of view, I wonder if an ocean in which there is better coupling between denitrification and nitrogen fixation that would help keep the system in balance, *but* in which the N turnover rate is very high might not hold unpleasant short-term (a few human life spans) surprises if we experience a climate "tipping point". This is a major reason why I set out to make us all uncomfortable vis a vis the problem of the oceanic fixed-N budget. The budget *is* balanced within the uncertainties, but repeating this mantra too frequently may lead to a complacency that can impede understanding. For approximately 2000 years many intellectual luminaries were comfortable with Aristotle's concepts of how the natural world works, but several of his concepts do not comport with modern understanding.

Dr. Sarmiento's critique makes a case for global scale analyses/models of N_2, nitrogen isotopes, etc., and suggests that direct estimates of rates are not the best tool for addressing the question of the state of the global oceanic fixed-N budget because these direct estimates do not integrate over time and space as do the global scale analyses/models. We made similar comments about the limitations of short-term direct rate determinations in Codispoti et al. (2001, pp. 99-100). I support the types of studies that Dr. Sarmiento recommends, but they, too, suffer from limitations. For example, my review of the literature suggested that it is possible that both nitrogen fixation and denitrification are more widespread than is suggested by canonical studies that tend to segregate water column denitrification into the three major suboxic zones and water column nitrogen fixation into regions such as the N. Atlantic and the E. China Sea. To the extent that these processes occur in the same ocean volume, model "box", or model cell we may be determining *net* nitrogen fixation and denitrification not *gross* rates. This might inhibit understanding of how each process is regulated. In addition, some of the signals in the large-scale models may have been generated decades to centuries ago (e.g. a portion of the N* signal entering the Arctic via Bering Strait in the Anadyr Water), and it is not clear to me how useful these Holocene signals will be as

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society attempts to deal with the Anthropocene.

To refine the region of disagreement between myself and Drs. Gruber and Sarmiento, LET ME STIPULATE THAT I THINK THAT THE LARGE SCALE ANALYSES BASED ON ATMOSPHERIC CONSTRAINTS AND ISOTOPES STRONGLY SUGGEST HOMEOSTASIS DURING THE HOLOCENCE, but THEY ALSO SUGGEST CHANGE DURING CLIMATE TRANSITIONS. I hold with those who suggest that changing climate, population growth and human impingement on the aquatic environment are causing large changes in the ocean ecosystem and that these changes may be accelerating, with population growth multiplied by increases in per capita energy use, etc. So, if rapid change or a climate tipping point brings attempts to engineer climate to the fore, we are going to want to know a lot more about some details of how the system works. Suppose for example, we attempt to mitigate CO_2 increases by Fe fertilization, that, in turn, may reduce subsurface dissolved oxygen concentrations. Wouldn't it be nice to understand how such a reduction in subsurface dissolved oxygen concentrations would influence the fixed-N budget and N_2O? The changes would depend to a large extent on details such as the types and rate of denitrification as a function of small changes in dissolved oxygen (0-2 μM range??) that are unlikely to be unraveled by large-scale modeling and gradient analyses, alone. In our present state of ignorance, we need lots more rate estimates, more global and regional analyses, and more models.

While making these general comments, I feel that I must weigh in on a conversation between the author and an anonymous reviewer of Altabet (2006, this web site). They disagreed about what was said in Codispoti et al. (2001) with respect to the possibility of a balanced fixed-N budget and how to account for isotope fractionation in the face of significant ammonium and organic-N oxidation to N_2. In more than one place in Codispoti et al. (2001), we left open the possibility of balancing the oceanic fixed-N budget by finding more nitrogen fixation. Here is a quote from that paper (page 91). ***" In this work, we are taking a more prospective view and ask: "Are we in a***

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transition state as we enter the Anthropocene in which the deficit in the oceanic fixed N budget exceeds 200 Tg N yr $^{-1}$, or are we significantly underestimating oceanic nitrogen fixation or both?" Here is another (page 100) "***We suggest that it is possible that the oceanic nitrogen fixation rate is considerably higher than present estimates."*** To me, it seems that these and other comments suggest that we leave open the possibility that the present-day oceanic fixed-N budget could be close to a balance. I feel that Dr. Altabet did not interpret what we were trying to say, the way we meant to say it. Perhaps the overall tenor of Codispoti et al. (2001) gives the wrong impression. If so, let me stipulate that I am guessing that the oceanic fixed-N budget is likely to come into more of a balance as we learn more, but I also think that the gross rates of nitrogen fixation and denitrification will be higher than in most/all existing budgets. While on the subject of the Codispoti et al. (2001) paper in relation to Altabet (2006), we did try to account for the fact that the ammonium and organic-N oxidized to N_2 might have an isotopic signal akin to sedimentary denitrification as suggested by the anonymous reviewer of Altabet (2006). I guess that at this point, it behooves me to point out that I was NOT the anonymous reviewer of Altabet (2006), do not know who the reviewer was, and only read the exchange in January 2006.

With respect to the lack of new data, quantitative analysis and blinding insights in this paper noted by Dr. Sarmiento, I plead guilty. The problem is that despite several attempts, I have not been successful in obtaining funding to continue work on this subject for several years. I have been involved in several proposals that, in the aggregate, include requests for fieldwork and modeling, but none have been successful. I am a junior author on some recent papers dealing with the Arabian Sea's denitrification regime (e.g. Bange et al., 2005, Devol et al. 2006a&b), but in the absence of funding it is hard to collect new data, etc. Although, I would like it to be otherwise, my efforts vis a vis the oceanic fixed-N budget are now mainly restricted to overviews such as this manuscript. Given this situation, I am bemused that some folks still want to know what I think about the oceanic fixed-N budget, but I was asked to contribute and I did what I could with the available resources. After the SPOT-ON meeting, I was asked to

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give another plenary talk at the June 2006, ASLO meeting, so I assume that there are some who are still interested in the results of my hobby! I also note that the discussion version of this paper has already been cited in a commentary in *Science* (Capone, 2006).

An underlying premise of this paper is "the more we look, the more we find", and it brings to mind a recent conversation that I had with Dr. Charles Yentsch who reminded me that I used to say to him quite often when he was my Director at the Bigelow Lab that there are still major oceanic features to be discovered. He looked me in the eye at one point during this conversation and said, "Lou, you didn't say it loud enough!". I guess that this paper is an attempt to say it loud enough! Certainly, the recent trends are on my side: we have found new pathways for biogenic N_2production and new sites for denitrification in recent decades, and the estimated rates for both nitrogen fixation and denitrification have increased (e.g. Deutsch et al., 2007). I think that another thought that has to be kept in mind is that advances in our understanding of microbial processes are going to open new worlds that have to be considered. Consider this quote from Caron (2005), "Collectively, recent findings from these initial attempts during the 20^{th} century and early 21^{st} century at applying molecular biological methods to characterize marine microbial communities have brought the field full circle back to a "discovery phase" that oceanography experienced more than a century ago."

I propose to meet the generic comments/criticisms outlined above by writing a foreword to the paper, explaining its history and pointing out the large uncertainties.

Nomenclature:

As noted in the manuscript when I say "canonical denitrification" I mean the following four-step reduction process; $NO_3^- \rightarrow NO_2^- \rightarrow NO \rightarrow N_2O \rightarrow N_2$ traditionally thought to be the pathway for biological N_2 production in the sea. When I say "denitrification" I mean all possible pathways for the biological production of N_2 as outlined in Fig. 1 of the manuscript. When I use the term "unfractionated", I use it loosely, to

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denote that portion of denitrification that behaves with respect to isotope fractionation of N like sedimentary denitrification which has a small fractionation factor relative to the nitrate and nitrite reduced during most water column denitrification. I include as "unfractionated" that portion of water column denitrification that involves the oxidation of organic-N and ammonium to N_2. For brevity, I will refer to Dr. N. Gruber as NG, to Professor J. Sarmiento as JS, and to Dr. S.W.A. Naqvi as SWAN.

In the following sections of this reply I will address the more specific comments of the reviewers:

Interactive comment on Biogeosciences Discuss., 3, 1203, 2006.