Interactive comment on "Winter atmospheric nutrients and pollutants deposition on West Sayan mountain lakes (Siberia)" *by* Daniel Diaz-de-Quijano et al.

Anonymous Referee #1

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Review of Diaz-de-Quijano et al. for Biogeosciences

In the paper "Winter atmospheric nutrients and pollutants deposition on West Sayan mountain lakes (Siberia)" Diaz-de-Quijano and coauthors determined the nutrients (nitrates, total phosphorus, and sulphate) and the pollutant spheroidal carbonaceous particles (SCPs) in snowpacks of a remote, poorly known mountains in Siberia (West Sayan) only during the snow period. Then, they estimated using two approaches (time-weighted and precipitation-weighted) the annual deposition of nutrients and SCPs in the region. The ultimate goal is to know if this region is out of relevant nitrogen precipitation but submitted to climatic warming. Finally, they assessed the relevance of these inputs of N and P on lake nutrients and chlorophyll-a. I found the paper too extended in some parts and very speculative in other ones. I have several comments/concerns that I details below.

REFEREE #1 COMMENT 1:

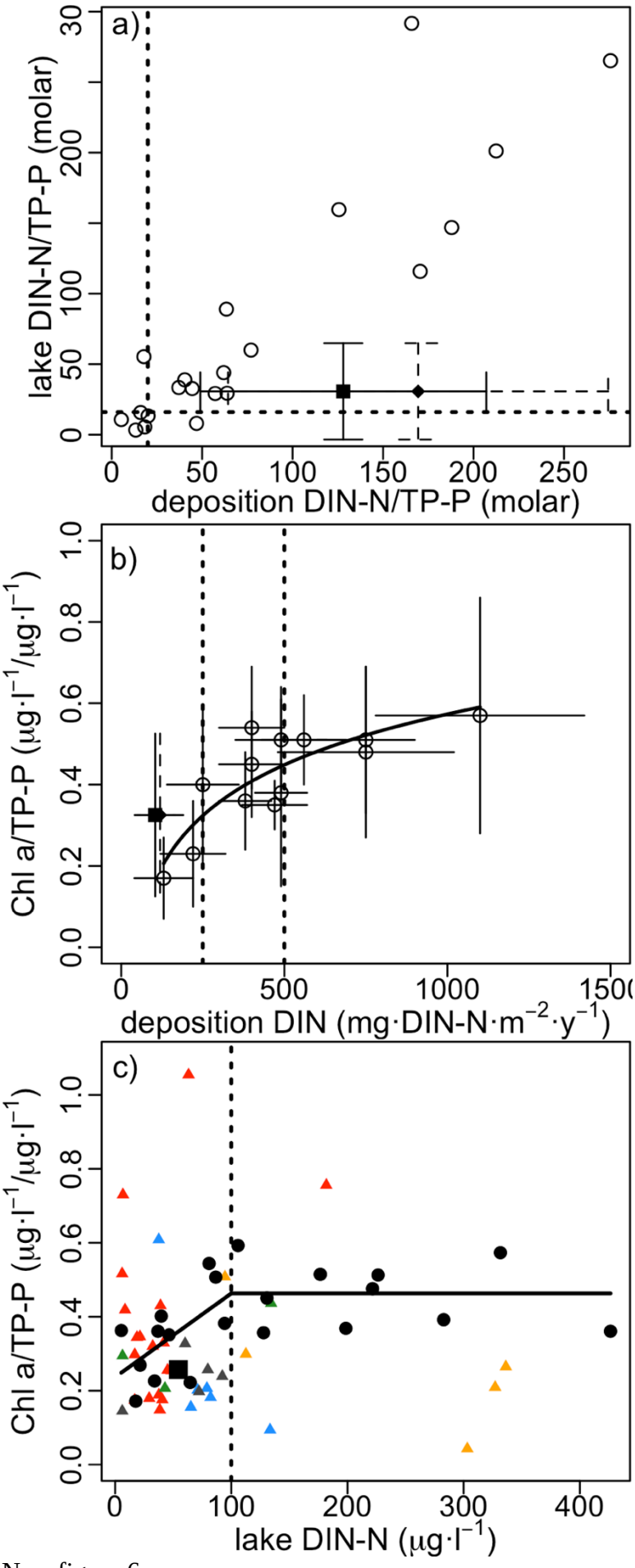
Main concerns: - I think the calculations to obtain the annual atmospheric deposition are too speculative and a focus in the real numbers could have been more productive, accurate and direct. - The consequences of the atmospheric deposition of nutrients and pollutants for the lakes are poorly evaluated.

AUTHORS ANSWER 1:

We agree that our manuscript combines an empirical (snow period) and a speculative (snow-free period) component regarding atmospheric nutrients and pollutants deposition. Probably it would have been a faster and more simple option just to show snowpack chemistry and stop there. Nevertheless, we think that this speculative exercise is legitimate and valuable for two reasons. First of all, because it is clearly and honestly separated from the empirical measurements. Secondly, because only the estimated yearly load allowed us to compare our study site with other lake districts in the literature, in terms of the relationship between atmospheric nutrient deposition and lake phytoplankton limitation regime. By doing that, we could get to interesting general conclusions.

At this point of the answers to the comments of the reviewers we have to say that during the revision process we found two mistakes that have changed the lake water chemistry dat aset in a way that conclusions have changed, too. Nevertheless, the yearly load estimation remains necessary to identify the location of the phytoplankton of the studied lakes in the nitrogen to phosphorus limitation gradient and its relationship to atmospheric nutrient deposition.

After making the necessary corrections, figure 6 and table 3 will be changed into the following correct versions:


a)
lake DIN-N/TP-P (molar)
0
50
100
200
30
0
50
100
150
200
250
deposition DIN-N/TP-P (molar)
b)
Chl a/TP-P (µg·l⁻¹/µg·l⁻¹)
0.0
0.2
0.4
0.6
0.8
1.0
0
500
1000
deposition DIN (mg·DIN-N·m⁻²·y⁻¹)
c)
Chl a/TP-P (µg·l⁻¹/µg·l⁻¹)
0.0
0.2
0.4
0.6
0.8
1.0
0
100
200
300
400
lake DIN-N (µg·l⁻¹)

New figure 6

Year	month	Lake	DIN-N/TP-P (mol/mol)	TN/TP (mol/mol)	Limiting nutrient
2011	early June and August	Oiskoe	4.3		N
		Svetloe	4.3		N
		Raduzhnoe	16.2		N-P
		Karovoe	6.5		N
		mean	7.9		N
2012	early June and August	Oiskoe	109.2		P
		Svetloe	49.5		P
		Raduzhnoe	42.1		P
		Karovoe	29.6		P
		mean	57.6		P
2015	early September	Oiskoe	3.7	11.6	N
		Raduzhnoe	10.5	30.9	N
		Tsirkovoe	23.6	82.8	P
		mean	12.6	41.8	N
2017	late August	Oiskoe	6	56.6	N
		Karovoe	25.8	61.9	P
		Tsirkovoe	97.9	164.1	P
		mean	43.3	94.2	P

New table 3

Sections 3.6, 3.7 and conclusions will be changed to a shorter text than in the previous version of the manuscript. Basically, the new conclusions are that the study site is a typical low atmospheric nitrogen deposition area, with lower deposition than the northern Sweden average, as it was described in the seminal paper by Bergström and colleagues where they formulated a new paradigm for phytoplankton growth limitation in oligotrophic lakes (figure 6 b). The studied West Sayan district is safely located in the nitrogen limited realm (figure 6 c). The idea that atmospheric nutrient deposition is quite unimportant for lake water chemistry and phytoplankton growth in these lakes is confirmed by the fact that DIN-N/TP-P ratios of atmospheric deposition and lake water clearly differ (figure 6 a). In conclusion, according to our data, both nitrogen and phosphorus limiting conditions occurred in the studied West Sayan mountain lakes (new table 3 and new figure 6 a) but the region as a whole would be predominantly located at the nitrogen limiting realm (new figure 6 a and c) and constitutes an excellent site to study the effects of global warming with a relative independence of atmospheric nitrogen deposition.

Minor concerns

REFEREE #1 COMMENT 2:

Abstract- line 20, the authors stated that the lakes have "a trend toward nitrogen limitation", despite the N:P molar ratio of atmospheric deposition is very high. This sentence seems to me counterintuitive.

AUTHORS ANSWER 2:

After correcting the mistakes in the calculations for lake water chemistry none of those trends are visible anymore. We are not going to discuss about temporal trends in the upcoming version of the manuscript. Instead, we will express as we did above: "both nitrogen and phosphorus limiting conditions occurred in the studied West Sayan mountain lakes (new table 3 and new figure 6 a) but the region as a whole would be predominantly located at the nitrogen limiting realm (new figure 6 a and c"

REFEREE #1 COMMENT 3:

Introduction- Line 33, the word "paradigmatically" seems to me inappropriate Line 35, similar comments the word "paradigmatically" seems to me inappropriate Line 43, this sentence seems to be not proper in scientific, technical writing

AUTHORS ANSWER 3:

OK. We changed the sentence in lines 33 and 35 into:

"The effects of atmospheric nitrogen deposition on primary production have been documented in the usually nitrogen-limited terrestrial ecosystems (Bobbink et al., 2010; DeForest et al., 2004; Güsewell, 2004; LeBauer and Treseder, 2008), as well as in commonly phosphorus-limited lakes (Bergström et al., 2005)."

Well, our opinion is that sentence in line 43 is communicative because it shows that ecology as a scientific discipline has long disattended the importance of geographical distribution of ecological processes, but it is not important, so we also changed it into:

"Nevertheless, ecological processes are not homogeneous around the World."

REFEREE #1 COMMENT 4:

Methods- Lines 63 to 88. The description of the study site is too long. Figure 2 seems to me more appropriate to be Figure 1. The first thing to explain should be the location of the study site and then the meteorological characteristics (not climatic). My suggestion is to change the order of Figure 2 and Figure 1. Table 1- I was unable to see the site Tushkan in the map (current Figure 2). Include also the numbers in table 1

AUTHORS ANSWER 4:

Details concerning the vegetal cover at the study site (paragraph 2, lines 78-88) can be deleted, especially if we also remove the confirmation of our yearly TP deposition estimate based on pollen. This part was not in the original article but added on request of a referee in a previous submission to a different journal.

We changed figure 2 to be Figure 1.

We now say meteorological instead of climatic (lines 76 and 90)

Tushkan is visible in the map as letter C, as it is stated in the caption.

We now added an extra column in table 1 to make it easier to identify sampling sites in the map.

Results and discussion-

REFEREE #1 COMMENT 5: Line 174, meaning acronym SWE

AUTHORS ANSWER 5: Snow water equivalent. Done.

REFEREE #1 COMMENT 6: Line 196, delete "had"

AUTHORS ANSWER 6: Done

REFEREE #1 COMMENT 7: Line 217, 191+/- 35 please being consistent with the data in Table 2. The comparison here seems to me very forced mostly considering the standard deviations of the values.

AUTHORS ANSWER 7: That is true. We replaced "higher than" for "comparable to"

REFEREE #1 COMMENT 8: Line 238 please delete "a little bit" that is too colloquial Table 2, please insert units in columns and rows

AUTHORS ANSWER 8: deleted and done

REFEREE #1 COMMENT 9: Line 250, this affirmation is only true in humid climates. Dry deposition could be more relevant for instance in the Mediterranean climate.

AUTHORS ANSWER 9: OK, we added "in wet climates like that in West Sayan mountains"

REFEREE #1 COMMENT 10: Line 254, please delete "a bit" that is too colloquial

AUTHORS ANSWER 10: replaced by slightly

REFEREE #1 COMMENT 11: Line 258, please delete "as a rule of thumb"

AUTHORS ANSWER 11: replaced by "In general terms"

REFEREE #1 COMMENT 12: Lines 257 to 295, these paragraphs are too speculative. Is the P-linked to pollen available?

AUTHORS ANSWER 12: There are two different questions here. Lines 257-283 use information on seasonal deposition of atmospheric phosphorus in the literature to evaluate which of our two estimates (based on constant deposition in time and based on precipitation) would be more likely to be true. The same kind of rationale is used in the case of nitrate and sulphate in the following paragraphs in the manuscript. As we said in the answer to the main concerns, we think that such an exercise is valuable to contextualize our study site in comparison to other lake districts of the world in terms of atmospheric nitrogen deposition and lake phytoplankton limitation.

Lines 284-295 have also been criticized by the other referee. They were not present in the original manuscript but added on request of a reviewer in a previous submission to another journal. The paragraph will be deleted if the editor allows us to submit a new version of the manuscript.

Yes, of course, the P linked to pollen is available in a span of articles, as we cited them in the paragraph: (Banks and Nighswander, 2000; Bigio and Angert, 2018; Brown and Irving, 1973; Doskey and Ugoagwu, 1992). Note that we used data at the genus level, not species. In any case, we will delete it.

REFEREE #1 COMMENT 13: Line 347, please delete "our primitive guess"

AUTHORS ANSWER 13: Replaced by "our literature-based estimate"

REFEREE #1 COMMENT 14: Line 368, please delete "At a first glance"

AUTHORS ANSWER 14: We replaced "At a first glance, it could seem" by "This may lead to think"

REFEREE #1 COMMENT 15: Lines 489-526, I have some concerns about phytoplankton limitation based on data of atmospheric deposition. Lake, or better phytoplankton, limitation should take in

account lake stoichiometry and corroborate phytoplankton limitation using bioassays. Taking atmospheric deposition, as a surrogate of lake limitation needs to be better aug- mented. It is too speculative and needs an experimental approach or more lake data. TP encompasses available and not available P.

AUTHORS ANSWER 15: We agree that the lake water chemistry (and stoichiometry) data set that we are using is limited in amount of observations but it is also novel for an underrepresented part of the World and, therefore, valuable. We strongly disagree with the idea that phytoplankton growth limitation only could be assessed using a combination of stoichiometry and bioassays. There are, at least, three different approaches to assess phytoplankton limitation in the environment. Each of them is supported by a vast number of published papers and have been used combined and alone. Firstly, there are enrichment experiments and calculation of response ratios that can be run at the whole lake level but also at *in situ* mesocosms, or *in vitro* in the lab. Secondly, there is a bunch of biochemical or molecular indicators of nutrient limitation in phytoplankton including enzyme activities (e.g., phosphatases, peptidases, etc.), nutrient uptake kinetics, pigment ratios, nucleic acid ratios, membrane transporters, NIFTs, etc. Finally, it is also legitimate to use a stoichiometric approach that can include ratios between different dissolved, particulate or dissolved and particulate nutrients. Of course, discrepancies exist between approaches because of empirical reasons but also because the aspects of phytoplankton growth limitation that we can assess using the different approaches are inherently different. Thus, methods assessing phytoplankton limitation at the ecosystem or cellular level can be perfectly contradictory, as particular cells can be nutrient-limited whereas other cells within the same population or other species within the same community might be not limited at all. Therefore, we stand for the legitimacy of the current approach. Moreover, we include information from previous studies that combined stoichiometry and nutrient enrichment experiments in our discussion.

The cause-effect chain presence or absence between atmospheric nutrient deposition, nutrient concentrations in lake water, and phytoplankton growth are assessed in figure 6 and the discussion associated to that figure. In fact, figure 6 b and c (discussed in lines 489-526 in the former manuscript version) consist in adding one dot corresponding to our study site to graphs representing regional or World scale studies published in very good journals, where lake stoichiometry was presented without any complementing bioassay. The use of Chl a/TP-P ratio is not our innovation but a ratio previously used in the cited articles. The fact that TP might include not bioavailable P is not a problem at all because the gist of figure 6 b and c is to show the relationship between atmospheric and lake water inorganic nitrogen and phytoplankton biomass (represented by chlorophyll a). TP is at the denominator just to remove the effects of changing P on the increase or decrease of Chla and assess the effect of N on Chla alone. Moreover, dissolved inorganic PO4 concentrations in lake water can be below the detection limit in a range of unproductive high mountain lakes like the ones included in the present and cited studies because the phosphate turnover is very fast. For that reason, TP is a more reliable measure of the phosphorus state in ultraoligotrophic lakes than dissolved inorganic phosphorus. Besides that, phytoplankton and general microplankton living in P-limited environments have developed strategies to use phosphorus forms others than dissolved inorganic orthophosphate. These arguments have been used by Brahney and colleagues (2015), Camarero & Catalan (2012) and elsewhere. A short justification will be added to the next manuscript version.

Finally, in the case of figure 6 c and the corresponding discussion, atmospheric deposition is not present at all. We use both graphs, with atmospheric N deposition (figure 6 b) and with lake water DIN (figure 6 c) namely to saw the logical chain between the atmosphere, lake and phytoplankton. Therefore, it is not true that atmospheric N was used as a surrogate of lake limitation.

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