

## ***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

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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.

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.

Minor concerns 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.

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

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

Results and discussion- Line 174, meaning acronym SWE Line 196, delete “had” 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. Line 238 please delete “a little bit” that is too colloquial Table 2, please insert units in columns and rows Line 250, this affirmation is only true in humid climates. Dry deposition could be more relevant for instance in the Mediterranean climate. Line 254, please delete “a bit” that is too colloquial Line 258, please delete “as a rule of thumb” Lines 257 to 295, these paragraphs are too speculative. Is the P-linked to pollen available? Line 347, please delete “our primitive guess” Line 368, please delete “At a first

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glance” 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 augmented. It is too speculative and needs an experimental approach or more lake data. TP encompasses available and not available P.

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