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Biogeosciences Discussions

Interactive comment on "Long-term atmospheric nutrient inputs to the Eastern Mediterranean: sources, solubility and comparison with riverine inputs" *by* M. Koçak et al.

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Response to Reviewers

We would like to thank the editor and the reviewers for their comments that have helped us to prepare this final version. All suggestions have been taken into account and all raised issues are answered one by one. References have been included as proposed. The section related to sea-water solubility has been exempted from the manuscript as suggested. Minor comments have been also taken into account. Below is a point by point answer to the reviewer's comments (by Italics).

Reviewer1 Q:Long term atmospheric nutrient inputs to the Eastern Mediterranean:

Sources, Solubility and Comparison with Riverine Inputs. This manuscript is an interesting study which should be published. I have made a number of suggestions including the fact that I think their calculated atmospheric fluxes may be incorrect.

A: There is no difference between two approaches. Please see calculations below.

Q:My other main comment is that this manuscript compares the atmospheric fluxes and riverine fluxes of nutrient to the Northern Levantine basin. This needs to be explicit in the title. The authors should explicitly compare their data with Ludwig et al., riverine flux data which exists for both the region and the basin as a whole and with Krom et al., (2004) and (2010) which looks at similar nutrient budgets for the entire EMS and then draws conclusions about regional processes.

A: Comparison has been added as suggested.

My detailed corrections are based on the line numbers that I have on my print out. In a previous review that was a problem. I would be happy to post the authors a copy of my corrections if that is also true here. However I would ask that the journal finds a better way of allowing the reviewers to identify exactly where they are making suggestions for change.

Detailed suggestions: Title:

Q: You manuscript is about the nutrient inputs to the Northern Levantine Basin and not to the EMS as a whole. The title should reflect this.

A: The title has been adapted as suggested.

Q: Line 11 Abstract replace were with have been

A: 'Were' has been replaced with 'have been'.

Q: Line 47 Krom et al and Turley are not good references for the effect of anti-estuarine circulation on the EMS. However I cannot easily find better ones. The authors should look.

BGD

7, C3928–C3934, 2010

A: Indeed, aforementioned references are hardly suitable for the effect of anti-estuarine circulation on the EMS. Hamad et al., 2005 ha been used as reference for this purpose.

Q:Line 47 Remove As a result of this peculiarity. Add at the end of that sentence a reference to Krom et al., (2004) and Krom et al., (2010).

A: Changes have been done as recommended.

Q: Ludwig et al (2009) does indeed argue that the water flux has decreased but they also show that the total nutrient flux has increased – see tables in their paper.

A: Without a doubt, Ludwig et al. (2009) have shown increase in riverine nitrogen fluxes despite the decrease in water fluxes. However, this is not the case considering phosphorous flux via rivers (please see aforementioned paper pages 209 and 213, Fig.5b; Table 8). These authors have demonstrated that P inputs in 1990s diminished to about the values of the early 1970s. On the other hand, sentence has been removed from the manuscript in order to prevent any further confusion to the reader.

Q:After (117:1) add 'combined with regionally low denitrification rates (Krom et al., 2010).

A: Suggested sentence has been added into the manuscript.

Q: Hypothesized not hypnotized.

A: 'Hypothesized' has corrected as 'hypnotized'.

Q: Silica is never a zero in the EMS and thus there is always enough silica to allow diatom growth to occur. The main reason it does not happen is because the system is so oligotrophic that eukaryotes (large plantain such as diatoms) are out competed by nano and pico plankton.

A: Indeed, Silica is never a zero in the EMS off shore water. Our recent studies at the coastal sites of Cilician Basin have shown that Si concentrations even can be lower than 200 μM (below the detection limit). Taking into account both diatom dominated phytoplankton community and such low Si values (on average Si/N 0.5) it might be suggested Silicate deficiency at coastal sites is liable on seasonal bases in such coastal environment.

Q: As a general point the Si that you have measured in this study is actually dissolved silicate (SiO42-) or SiO2. Silicon is aerosols is found within the aluminosilicates in rock such as clays and feldspars. In general that is simply insoluble. It is also found as opaline silica which is the same material that diatoms are made out of. This would not be relevant except that certain sources of mineral dust such as the Bodele depression in Chad are actually made of diatomite and the dust derived from them have a significant but unknown amount of freshwater diatom frustules within them. The silica you are measuring is some combination of silica released from weathering of rocks and opaline silica dissolving.

Q: Line 86 add us after allow

A: 'Us' has been added after 'allow'.

Q: Line 118: The information about sampling the rivers is not adequate. We need to how samples were taken, what was done to them, how they were stored, how often they were sampled. As an aside I would be really interested if they measured the particulate N and P, and the opaline silica in the particulate matter as well as just dissolved nutrients. I am happy to explain to Nikos why I think that is important. A: Indeed, the information about sampling rivers was inadequate. More information has been added into Materials and Methods section.

Q: Line 127 Great that you include detection limits but how were they defined?

A: In general, blanks were found to be less than detection limit of the instrument. Therefore, detection limit of the Aut-analyzer instrument was used.

Q:Throughout the manuscript Eilat is misspelt as Eliat.

A: Required changes have been done.

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Interactive Discussion

Discussion Paper

Q: Sentence starting line 188. The authors should also include in their table of data the results published in Carbo et al., (2005) DSR II CYCLOPS volume. That data has rather lower values of phosphate from Tel Shikmona Israel (2001-2003) and therefore there is no need to explain the 'high' values for Israel except as natural variability. Page 11 line 257.

A: Carbo et al., (2005) has also included into Table 2 and the manuscript.

Q: Do you really have one rain event per 3 days? That seems an awful lot to me for a place with a Mediterranean climate.

A: Indeed one rain event per 3 days for Mediterranean is an awful lot. This number strictly presents rain frequency in winter.

Q: Given that you have the data; it would be really interesting to compare the nutrient content of acid rains with that from non-acid events.

A: The nutrient content of acidic and non-acidic rain events have been compared. For instance, volume-weighted mean concentrations of Sidiss (1.8 μM) and PO43- (0.8 μM) for non-acidic events were 6 and 3 times higher than those calculated for acidic events.

Q: Line 322 Does not ammonia come mainly as gaseous emissions from intensive agriculture?

A: Not only as animals (cows, sheep) contribute also to NH3 emissions. However, stock-breeding is not common around the sampling site.

Q: Line 365: The experiments of phosphate and silicate dissolution with time are not designed as Ph vs. solubility experiments. They are also almost certainly misinterpreted. Silica is under saturated in seawater. If you put a sample containing particularly opaline silica in seawater, the silica will dissolve and the longer you leave it the more sill dissolve. In the case of phosphate I would assume that the dissolution of phosphate in seawater and freshwater are almost identical. But then the phosphate tends to reabsorb onto surfaces including back onto the original dust particles. This adsorption might well be pH and ionic dependant. That would explain the difference between freshwater and seawater phosphate numbers. Ammonia and nitrate do not behave this way.

Q:Line 385. You have the logic the wrong way round. Aerosol nitrate and ammonia are almost exclusively . . . therefore this might explain why they dissolve in seawater and freshwater.

Atmospheric nutrient fluxes: Q: It looks as though the authors used the known values of Vd for coarse and fine and then created a new Vd based on the fraction of coarse and fine particles and then multiplied that by the average nutrient content. If they did this, it was wrong. You need to do a separate sum for Vd fine * conc. fine + Vd coarse * conc. coarse and then sum then together to get the total nutrient flux. If they did this then they should make it clear in the text. If I am right then all the atmospheric nutrient fluxes which follow are wrong but probably not by enough to change the general interpretation.

A: Indeed, there were some mistakes during the estimation of dry depositions however; it is not related with the approach. Actually, there is no difference between two approaches during the calculation of dry deposition. Calculations of dry deposition for Si can be given as example as follow:

a) Mean CF Si 1.08 78.54 21.46 0.848232*2 0.231768*0.1 1.719641 0.54

b) Mean CF Si 1.08 78.54 21.46 0.7854*2 0.2146*0.1 1.59226 1.719641 0.54

Q: Section 3.5.2 I have requested much more detail of the sampling methods. With that detail, this section can be properly understood.

A: More detail of the sampling of rivers has been added.

Q: The authors add a sentence about the effect of Si/N ratios on microbial ecology in the EMS which is unjustified. Is there a diatom rich community in their waters? Is there any evidence of change?

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7, C3928–C3934, 2010

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A: Information has been included in the manuscript.

Q: Table 2 Add Carbo et al., (2005) data

A: Carbo et al., (2005) has been appended into Table 2.

Q: Table 8 Do you really need to give inputs in tons rather than molar units. Tons of PO4, HPO4, H2PO4? Which likewise NH3 or NH4? What is the Si dissolved you are using since as I mentioned above you are actually measuring SiO2 or silicate. If I am right then the variations in Vd in table 5 are not useful.

A: Undeniably, molar units could be more useful during comparison with literature. There is no difference between two approached during the calculations of dry deposition, besides our approach allows us to estimate Vds.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C3928/2010/bgd-7-C3928-2010-supplement.pdf

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7, C3928–C3934, 2010

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