

Responses to reviewer 1.

Review of the manuscript “Modeling the biogeochemical impact of atmospheric phosphate deposition from desert dust and combustion sources to the Mediterranean Sea” by Richon et al.

General comments

The present manuscript “Modeling the biogeochemical impact of atmospheric phosphate deposition from desert dust and combustion sources to the Mediterranean Sea” proposes an analysis of the impact of phosphorus atmospheric deposition comparing different sources: namely desert dust (Pdust) and combustion sources (Pcomb).

As I stated previously in my opinion the idea is very interesting and useful because the two sources are, in principle, characterized by different spatial and temporal distributions.

In my opinion still remains the problem of the uncertainty related to the coarse resolution of the LMDz-INCA global model therefore an effort to give some sort of quantification of such uncertainty in the results would be useful.

Nonetheless given the fact that Authors declare that it is not possible to integrate the simulations with a proper high resolution model (e.g. ALADIN-Climat) in my opinion the present work represents what is currently possible to perform. A possible way to give some estimates of the inter-annual variability would be to consider the monthly mean forcings available.

Specific comments

Pg 4 line 120: “The estimations of riverine fluxes are not available after 2000. Therefore, we use the riverine fluxes from the year 2000 in our study.”

Why not using at least the 1995-2000 average value ? Is year 2000 representative?

We chose to repeat the 2000 river forcing because it is the method used in other studies performed with the same model (see Richon et al. 2017, prog. Ocean.).

The 1995-2000 average river discharge of PO₄ and NO₃ are respectively 796 ± 279 ktPO₄/year and 11500 ± 2100 ktNO₃/year. The 2000 values we use are respectively 456 ktPO₄/year and 9820 ktNO₃/year. The NO₃ value is in the variability range of the 1995-2000 period. The PO₄ value is slightly below this range. However, data from Ludwig et al. (2009) indicate a decreasing trend of PO₄ river discharge over the Mediterranean that this value is representative of.

Pg 5 line 161-164: “We considered that given the high spatial and temporal variability of atmospheric deposition fluxes, a monthly resolution of deposition, as available for other years (Wang et al., 2017), would be a too strong and unnecessary limitation in simulating the biogeochemical response.”

In my opinion given the fact that not all the results are discussed at high (daily) frequency in the present manuscript, also monthly forcings (that, if I understand correctly, are available for other years than 2005) could be acceptable and therefore used to carry on a multi-year simulation and obtain more solid results. I would suggest to keep the 2005 year simulation, with high frequency forcings but also to add, if technically possible, a simulation with monthly fields in a multi year simulation framework. For example looking at Table 2 the dust deposition of the ADIOS period simulated by the LMDz-INCA appears quite different from the LMDz-INCA (2005) estimates, so it appears worthy to consider also the inter-annual variability, in order to evaluate the variance.

We provide in Figure 2 in the article the annual variation of Pcomb deposition over the Mediterranean for the 1997-2012 period derived from monthly deposition fields. This Figure shows that the inter-annual variability of Pcomb deposition is low.

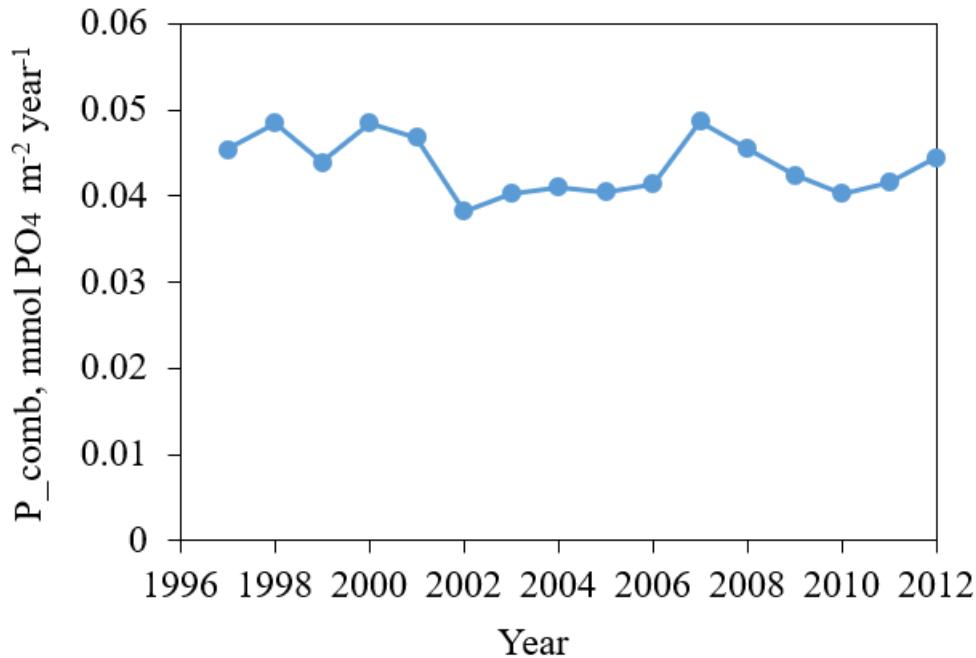


Figure 1: Inter-annual average *Pcomb* deposition over the Mediterranean

Also, Rong Wang (pers. Comm.) computed the average deposition, monthly standard deviation and standard deviation to average ratio of *Pcomb* deposition based on monthly forcings for the 1997-2012 period (Figure 3 of the article and hereafter). These figures show that, over the Mediterranean, deposition variability is very low. Therefore, these results seem to confirm that 2005 is not an exceptional year for *Pcomb* deposition.

We added these figures in the article with the following lines (197-202) : « In order to assess inter-annual variability of the *Pcomb* deposition, Figure 2 shows the annual average *Pcomb* deposition over the Mediterranean for the 1997-2012 period. Figure 3 shows the standard deviation to average ratio of *Pcomb* deposition over the Mediterranean computed from LMDz-INCA for the 1997-2012 period. These figures show that *Pcomb* deposition has low inter-annual variability. Therefore, 2005 can be considered as a study year because both *Pcomb* and *Pdust* deposition are close to the inter-annual average for the 1997-2012 period. »

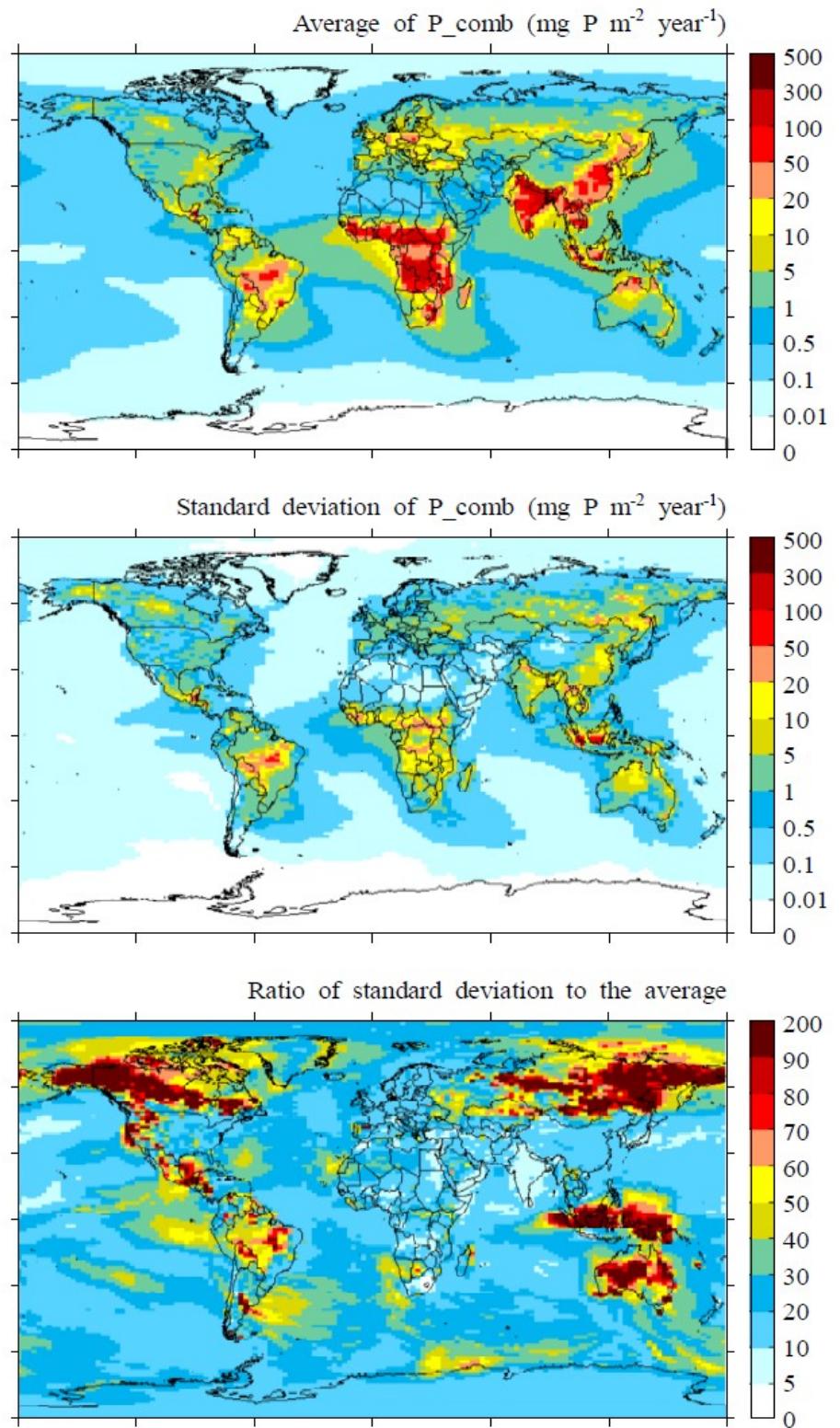


Figure 2: Maps of average deposition flux (top), standard deviation (middle), and standard deviation to average ratio (bottom) of Pcomb deposition over the entire LMDz-INCA domain. Average over the 1997-2012 period

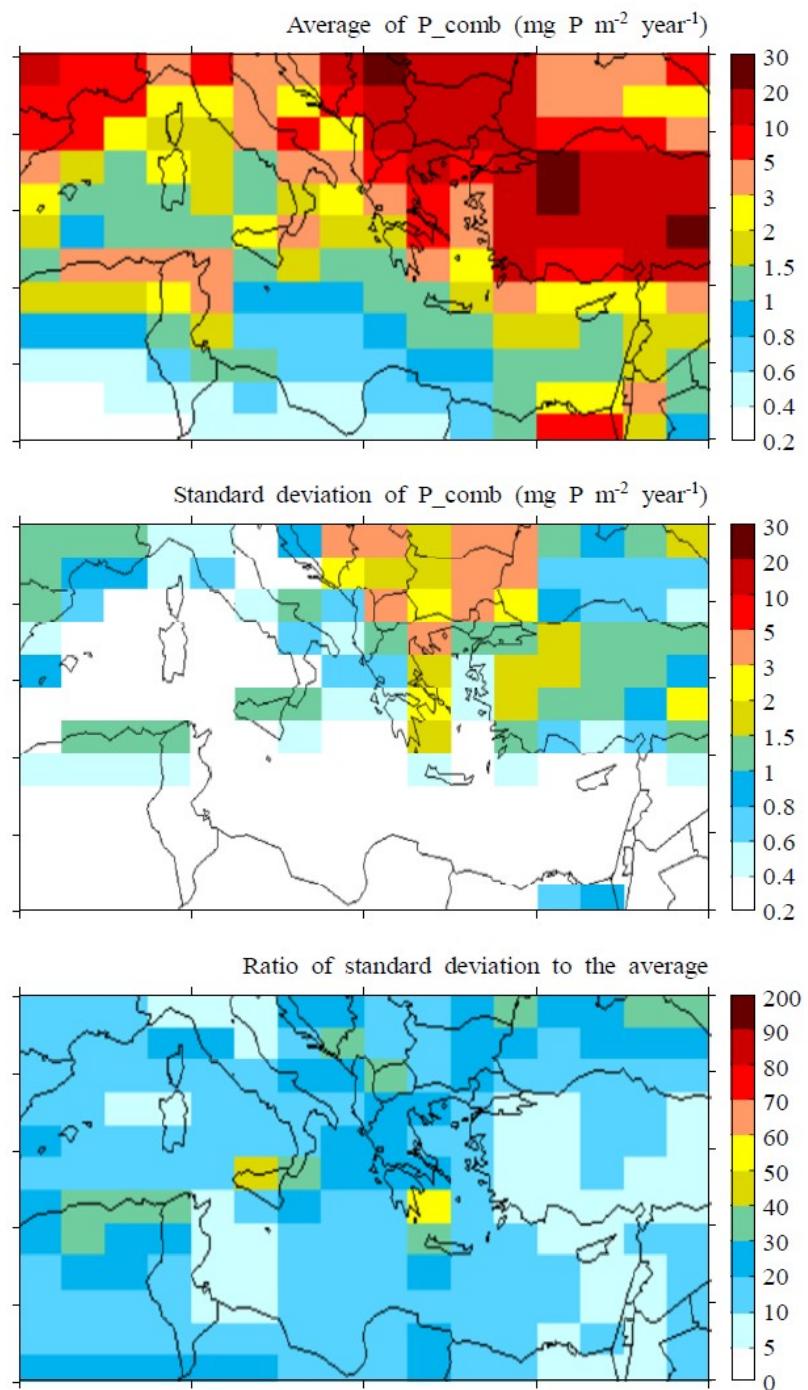


Figure 3: Same as Figure 2, zoomed over the Mediterranean Sea

Pg 17 line 559: “The relative effects of each source are maximal in their areas of maximal deposition and can induce an enhancement of up to 30 % in biological productivity during the period of surface water stratification.”

In my opinion Authors should specify that the 30 % increase in biological productivity refers to the 1-10m surface layer, and it doesn't refer to the total water column vertically integrated primary productivity. Moreover during summer a consistent part of the productivity could be located in the subsurface layers, below 10 m depth, therefore 30% could have lower relative impact considering the vertically integrated PP.

Reviewer is right, we added this precision.

Minor comment

Pg 17 line 578. 85% of of P

Changed

Responses to editor's comments

Abstract: Lines 10-11: Please clarify how this conclusion holds for the other years.

We added the sentence : « The evaluation of monthly averaged deposition fluxes variability of Pdust and Pcomb for the 1997-2012 period indicates that these conclusion may hold true for different years. »

Introduction, line 31-33: ‘... for marine biology..’ I would be more specific.

Changed to « for marine primary productivity »

Line 42: What do you mean by “constrain”?

Changed to « to better characterize »

Line 64: “. The Mediterranean Sea is also a hot-spot for climate change impacts (Lejeusne et al., 2010)” This sentence seems to be a little bit disconnected from the rest of the paragraph. I suggest that you either remove it or that you detail somehow the link between climate change and aerosols emission.

Sentence removed

Lines 67-69: please give a reference

Reference to Peñuelas et al. 2013 added.

Lines 76-80: Please synthetize in 2-3 lines the main outcome of Richon et al 2017

Sentence added : « Their results showed important impacts of N deposition on biological productivity (primary production, chlorophyll *a* production, plankton and bacterial concentrations) in the northern Ionian and Levantine basins and limited, yet significant impact of P deposition in the southern Mediterranean regions. »

Line 81: I would say: “..;by further considering the contribution of P from combustion sources in addition to that from anthropogenic...”

Changed

Line 101: “the authors” please specify who? Hamon et al ., 2015

Changed to Hamon et al.

Line 113: I would not say that PISCES is a Monod-type model but rather that the uptake of nutrient (e.g; nitrate, ammonium, phosphate, iron and silicate) by phytoplankton is governed by a Monod-type model. Besides, I would start by a general description of PISCES (e.g. lines 118-122) then I would go to specific feature of the model (e.g. description of nutrient uptake formulation).

This part was reorganized.

Line 118: trophic level and not biological levels.

Changed

Line 135: “Biogeochemical variables are prognostically calculated instead of being read from forcing files”. This sentence is not necessary (you have a model so the model computes the state variables.

Removed

Line 136: “Biogeochemical characteristics of the latest version of the NEMOMED12/PISCES model are evaluated in Richon et al. (2017). » This sentence is similar to the next one.

Please uniformize the definition of chlorophyll a. Sometimes you use Chl a, sometimes chlorophyll a.

Sentence removed and we write « chlorophyll a »

Lines 190-191: “The Pcomb deposition fields from LMDz-INCA used here have a coarser resolution than for Pdust of 1.27° in latitude by 2.5° in longitude”. This is confusing since we are told that the study wants to have consistent atmospheric inputs provided by the same model (see lines 162-164). Please clarify.

The Pcomb deposition fields used here were obtained from a different simulation performed with the same model LMDz-INCA on a coarser grid resolution. We added these precisions in the text.

Line201: “.. which leads to a general agreement of modeled surface P concentrations with ...” to which model are you referring? I guess that this is a global estimation? Please specify.

We precised « general agreement of their globally modeled surface P concentrations... »

Lines 207-208: P surface concentrations and deposition produced by LMD model? If yes, please link appropriately with Lines 209- which refer to LMDz performances as well because as it is written we have the feeling that these two parts are not related to the same model evaluation.

These lines refer to LMDz-INCA. We added the precision.

Line 255: something is missing in the sentence. Please reformulate.

The correct sentence is : « In Table 2, the dust deposition fluxes for the period 2001-2002, corresponding to the ADIOS campaign, are based on model outputs with a lower resolution (1.27°in latitude by 2.5°in longitude) than those for the year 2005 (0.94°in latitude by 1.28°in longitude). »

Line 310: “.. available at a too coarse time resolution to perform oceanic simulations.. ”. Please justify because looking at figure 3 we do not have the feeling that Pcomb has significant variability.

Changed to « for which monthly depositon fluxes are available ». We did not run

NEMOMED12/PISCES for 16 years with monthly P deposition, but instead, we provide in Figures 2 and 3 the time series of Pcomb deposition over the 1997-2012 period and the map of standard deviation to average ratio for the same time period. These figures show that Pcomb deposition has low variability, and that 2005 can be considered a suitable study year.

Line 331: this is clearer to use Table 3 instead of This table.

Changed

Line 344: You are not really assessing the impact of atmospheric deposition on the phosphate budget but rather on the phosphate surface concentration. So I would remove budget.

Changed

Line 446: Please define PM2.5 and PM 10.I do not think that it has been already defined above.

Done

Lines 451-454: “Although total mass deposition of phosphorus from desert dust exceeds that of combustion aerosols, the latter are much more soluble than lithogenic dust. » Please clarify. In the abstract we are told the reverse. Do you mean that the higher flux in the abstract results from the use of different solubility coefficients?

In the abstract, we give soluble phosphate deposition fluxes. The term « phosphorus » should be replaced by « particulate phosphorus ».

Legend figure 5: “..; map represents the 50 % Pdust proportion limit” compared to 2005 for Pcomb? Or 1997-2012 for Pcomb as well? Please clarify.

1997-2012

Figure 9: legend: I guess that it is the reference primary production and not the reference PO4.

Yes

Table 3: “Relative atmospheric contribution (%) to total PO4 supply in different sub–basins of atmospheric sources”. I would remove “of atmospheric sources”?

Removed