Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-242-AC2, 2017


# *Interactive comment on* "Modeling the biogeochemical impact of atmospheric phosphate deposition from desert dust and combustion sources to the Mediterranean Sea" *by* Camille Richon et al.

**Camille Richon et al.**

camille.richon@lsce.ipsl.fr

Received and published: 13 October 2017

*General comments:*
*In this work, the authors assess how modeled phosphate deposition output from dust and combustion aerosols can affect the phosphate fluxes into the surface waters of the Mediterranean Sea. The oligotrophic Mediterranean is phosphorus stressed, limited, or co-limited in certain regions/species, and atmospheric deposition may be an important source of this nutrient. Given high anthropogenic impact on aerosols in this region, and potential future enhancements in surface water stratification, this is*



*a topic worthy of study. The methodology in this paper was good in most cases, and some of the important uncertainties were discussed very thoroughly. I have pointed out in the specific comments several places where the manuscript requires further explanation of the methodology. My main issue is that, in my opinion, the importance of this study was overstated, and that a few key uncertainties in the findings were downplayed too much (e.g., nutrient co-limitation, the influence of soluble organic P in deposition, non-Redfieldian marine biogeochemical dynamics, and some important model uncertainties). Because of this latter concern, I suggest the authors proceed in one of two ways: 1) Scale back the conclusions substantially, to focus on the differences between model estimated Pcomb and Pdust deposition and their potential implications in a (more clearly-emphasized) highly-simplified Redfieldian ocean, or 2) Maintain the scope that the authors do now, but also present results from non-Redfield experiments with prognostic biogeochemistry (this would probably be a lot more useful for the community than option 1, but would of course be more work). We thank the reviewer for these comments. It is true that the Mediterranean is likely to be a non-Redfieldian Sea and modeling this behavior would give interesting insights. However, the present version of the biogeochemical model PISCES we use is in a redfieldian configuration. A new version of the PISCES model is being developed with non-redfieldian ratios (Aumont, in prep). This non-redfieldian version of PISCES has been developed to treat the global ocean, and qualifying it for the Mediterranean Sea will likely take a few years more to come up with satisfactory results for the region. Specific comments In some cases, the manuscript methodology could benefit from further explanation. For example: I was very confused about how PO4 was handled in the model. On P.4 l. 108 it is stated that, "The model is run in off–line mode like in the studies performed by Palmiéri et al. (2015), Guyennon et al. (2015), Ayache et al. (2015, 2016a, b) and Richon et al. (2017). PISCES passive biogeochemical tracers are transported using an advection–diffusion scheme. . ." What was meant by the model being run offline? Of the references above, only Guyennon et al. and Richon et al. looked at biogeochemical processes – the others looked at processes involving*

*actual passive tracers that do not behave like nutrients in the real ocean. In Guyennon et al., they said, "the coupling between the hydrodynamic and biogeochemical models is offline, i.e., biological retroaction on the physics is not taken into account" – but it appeared to me that biogeochemistry was prognostically calculated in that reference but not in this paper. Even if passive nutrient tracers follow deep-sea observations very well based on an offline model, how can one assess the biogeochemical changes caused by P deposition at the surface as the authors do here, if biogeochemistry is not calculated prognostically? Please clarify. In the Richon et al., 2017 text, this uncertainty was not discussed. Also, if P is a passive tracer, how can it affect Chl a as discussed in section 3.4? Please clarify this point in the text as well, and address any associated uncertainty and implications of the method in the text.*
We changed the following sentence that brought confusion about the way PISCES treats biogeochemical tracers: "PISCES passive biogeochemical tracers are transported using an advection–diffusion scheme." Into "PISCES biogeochemical tracers are transported using an advection–diffusion scheme ". . . In PISCES, PO4 is one of the nutrients necessary for plankton growth; it is not a passive tracer. Phosphate concentration, as well as the 4 other nutrients represented in PISCES (NO3, NH4, Si and Fe), are used to calculate the nutrient limitation terms (that have a Michaelis-Menten formulation). These limitation terms allow calculating the productivity terms based on the use of each nutrient. The phytoplankton biomass, which is linked to chlorophyll production, is then derived from the productivity terms. All equations are in Aumont et al. (2015).
Offline models, in contrast to online or coupled models are run thanks to the use of climatological values of physical and biogeochemical boundary fluxes. In our case, the physics of the ocean is described by the model NEMO, and the biogeochemical cycles are represented by the PISCES model. NEMO allows calculating the movements of water masses using climatological values (forcings) of atmospheric and physical conditions such as winds, runoff or precipitations. The biogeochemical model PISCES calculates the biogeochemical state of the Mediterranean (nutrient and tracers con-



centrations) using the physical state from NEMO and biogeochemical conditions from climatologies such as nutrient inputs from rivers. The biogeochemistry in PISCES is calculated in the same way as in Guyennon et al. To clarify, we add the sentence "Biogeochemical variables are prognostically calculated and not read from forcing files."

*On a related note, how exactly was surface PO4 related to Chl a in the model? I did not see this discussed, or any of the associated uncertainties.*
To clarify, we added in section 2.1 "The concentration of nutrients is linked with phytoplankton productivity and chlorophyll-a production according to the equations described in Aumont et al. (2015). Phytoplankton growth rate is dependent on nutrient concentrations via the growth limiting factors"

*Section 3.3 and figure 5: Where does the referred-to surface PO4 data come from? From the model or from observations?*
We changed the Figure 5 legend "Map of daily maximal relative effects of total (Pdust + Pcomb) deposition in June 2005 on the surface phosphate concentration (0âĂŤ10 m) compared to the reference simulation without atmospheric P deposition."

*Section 3.1: How was P deposition estimated from aerosol concentration observations? Was a deposition velocity assumed, and if so, what assumptions were used?*
In the LMDz-INCA model, an explicit deposition scheme is implemented. It represents 3 physical processes of deposition including sedimentation, turbulent dry deposition and wet deposition (in-cloud and below-cloud scavenging). These schemes allow accounting for more complex physical processes than the simple hypothesis of a constant deposition speed.
The observations we use for model evaluation are direct measurements of phosphorus bulk deposition (see Guieu et al. 2010 mar. chem. for protocol details). No estimations from atmospheric concentrations or optical properties such as AOD are used.

*The usage of the terms "total P" and "total phosphorus" in the manuscript are confusing. In most of the literature on atmospheric P deposition, the term total P indicates the sum of all phosphorus in any form (soluble or insoluble, organic or inorganic). On p. 6 l. 172, the authors state, "We investigate the impacts of each source of PO4 by performing two different simulations: "PDUST" and "PCOMB"; they include, respectively, natural dust only and combustion–generated aerosol only as atmospheric sources of PO4. We also performed a "Total P" simulation with the two sources included." Although it is not completely clear, here the authors seem to me to imply that total phosphorus is actually the sum of phosphate only from dust and combustion sources. On p4 l. 117, the term "total phosphorus" seems to imply the same thing. Then on page 6 line 187, the authors state, "We used the times series of total P measured at 9 different stations over the Mediterranean from the ADIOS campaign (Guieu et al., 2010) and the soluble P measured at 2 stations in the South of France from the MOOSE campaign (de Fommervault et al., 2015)". Here the authors seem to distinguish between soluble and total P, as I would have otherwise expected. Elsewhere in the manuscript, the authors also use the term "atmospheric P" (which to me implies total phosphorus) to mean atmospheric soluble PO4. I suggest clarifying these different concepts, and using separate terms for each. Along those lines, I also suggest changing the title in Fig. 6 from "Total P" to something else.*

We agree with the reviewer that the use of different terms is quite confusing. We modified some sentences in the text to clarify this point:

Section 2.3: "From now on, we name "total P" the sum of bioavailable phosphate from dust and combustion (Pdust + Pcomb)."

Section 3.1 "We used the times series of total phosphorus measured at 9 different stations over the Mediterranean from the ADIOS campaign (Guieu et al 2010) and the soluble phosphate measured in the deposition at 2 stations in the South of France from the MOOSE campaign"

Section 3.3 We renamed the section "Impacts of atmospheric deposition on marine surface phosphate budgets ", and line 298: "Atmospheric deposition of phosphate



aerosols has different impacts"

"Figure 5 shows the relative impacts of phosphate deposition from the two sources (combustion and dust) on surface PO4 concentration for the month of June 2005. The relative impacts of atmospheric deposition from different sources are dependent on both the underlying phosphate concentration and the bioavailable phosphate deposition flux."

We use the term "phosphate deposition" in the text because our focus is on the deposition of this bioavailable nutrient.

*On a similar vein, P1 l.15: "We examine separately the different soluble phosphorus (PO4) sources. . ." Please keep in mind again that soluble phosphorus and PO4 are different things. Soluble P includes soluble organic P, which was not discussed much in this manuscript, except as a small note late in the paper in section 4. To avoid confusion, I recommend being clearer about this in the text.*

We thank the reviewer for this remark. We are conscious that soluble P can also describe organic P. In this manuscript, we refer to the only soluble phosphorus form in PISCES which is PO4. We replace soluble phosphorus in this sentence by "phosphate".

*The authors talk about other sources of surface PO4 (e.g., riverine and oceanic via Gibraltar). Were these data obtained only from the model? Is there literature data with relevant information? If so, that information would be good to put in Table 2 for reference and discussion in section 3.2. If these data are not available, that would be worth mentioning and discussing.*

As described in section 2.1, riverine fluxes of nutrients are prescribed from Ludwig et al. 2009. This study groups the nutrient fluxes from 239 rivers around the Mediterranean and Black Sea obtained from measurements and model data. Unfortunately, the estimations of riverine fluxes are not available after 2000. This is why we used the riverine fluxes from the year 2000 in our study. The nutrient fluxes from the Atlantic are computed as the product of the buffer zone concentrations constructed from the World Ocean Atlas (2005) and the water fluxes through the Strait of Gibraltar computed by the model. We added these precisions to the section 2.1.

*My main concern, as mentioned, was that a few key uncertainties were either not made clear enough or fully addressed. These include: 1) Non-Redfieldian marine biogeochemical dynamics. The authors state on P. 4 l. 102 that: "PISCES is a Redfieldian model: the C/N/P ratio used for biology growth is fixed to 122/16/1." Many recent studies have discussed the shortcomings of this assumption in the real ocean, particularly in oligotrophic regions like the Mediterranean. A very large body of work shows that Redfield dynamics may be particularly erroneous with respect to P cycling (e.g., work by M. Lomas, R. Letscher, A. Landolfi, etc. (this is not a comprehensive list)). Given that Redfieldian assumptions are unlikely to represent actual biogeochemical dynamics in this paper's study region, I feel that the authors must spend much more time discussing this uncertainty. It would be good if they could also more clearly state what meaningful information the results provide, given this large uncertainty. Ideally, they would also run additional model tests under non-Redfieldian assumptions.*

We thank the reviewer for this comment and refer to the general comments at the beginning of this section.

We added a paragraph on the implications of the use of a redfieldian model in the discussion section. "The PISCES version used in this study is based on the Redfield hypothesis that C/N/P ratios in organic cells are fixed. This fixed value determines the nutrient ratio for uptake and has the advantage of simplifying calculations in the 3-D high resolution coupled model. However, because the Mediterranean is highly oligotrophic, the biogeochemical cycles may be determined by non-Redfieldian dynamics (see Ribera d'Alcala et al. 2003, JGR). This non-Redfieldian behavior may imply complex nutrient limitations and co-limitations processes that can not be studied with the present PISCES version. To this day, there is no version of PISCES available that



includes the non-Redfieldian biogeochemistry in the Mediterranean. The development and use of such a version of PISCES is a perspective of this work that needs to be undertaken in order to fully understand nutrient dynamics and growth limitation process in the Mediterranean. This study provides first results on the potential impacts of phosphate atmospheric deposition on the Mediterranean nutrient pool and potential implications on biological productivity assuming the Redfield hypothesis."

*2) The influence of soluble organic P in deposition was only touched upon in the manuscript. However, various studies suggest that it could be an important, or even dominant, source of soluble phosphorus to organisms in addition to the PO4 covered in this study (e.g., Chen et al., 1985; Kanakidou et al., 2012 and references therein). Particularly relevant for this paper is the fact that soluble organic P, in the few cases where it has been measured, appears to be much larger in combustion-sourced aerosols than in dust aerosols (e.g., Longo et al., 2014; Zamora et al. 2013). The authors should discuss the implications of/uncertainties related to not including organic P in their analysis. To make the paper more useful to the community, they may also consider running sensitivity tests estimating the potential impact on their results of including this additional P source.*

We agree with the reviewer that including organic phosphorus is an important step for the community. Our hypotheses concerning phosphorus combustion in this study are only based on Mahowald et al (2008). However, Myriokefalitakis et al (2016) consider that organic phosphorus (DOP) can be deposited in the Mediterranean with combustion and biogenic aerosol. DOP is not included in the version of PISCES used in this study. However, if we consider the hypothesis of Kanakidou et al and Myriokefalitakis et al, and given that dissolved organic matter is recycled into inorganic nutrients in the sea, we may be able to consider the inclusion organic phopshorus as a source of atmospheric phosphate. We add some elements in the discussion section: "In the Mediterranean region that is surrounded by many forested areas, biogenic emissions may be an important source of atmospheric phosphorus in the form of organic matter. Moreover, Kanakidou et al. (2012) show that an important fraction of organic phosphorus can be emitted from combustion. In particular, the numerous forest fires occurring every summer in the Mediterranean region may constitute an important source of organic phosphorus. However, the PISCES version used in this study does not include organic phosphorus. In the ocean, organic phosphorus can be recycled by bacterial activity into inorganic phosphate that is bioavailable for plankton growth. Therefore, the inclusion of organic phosphorus in PISCES along with an estimation of organic phosphorus from atmospheric fluxes is a perspective to consider."

*3) Uncertainties with the model assumptions themselves require further discussion. For example: The majority of the results focus and rely on modeled ocean surface PO4 concentrations. However, the majority of the model evaluation focuses on subsurface ocean PO4 trends, or surface Chl a trends. There was no in-depth discussion of how well the model compared to surface PO4 data, or what kind of data were available for this comparison. Moreover, the authors do not discuss how surface Chl a is related to surface PO4, either as parameterized in the model, or in actual observations.*

Figure 1 bellow displays the PO4 concentration on the BOUM section in the top 200 m (zoom from the A1 Figure from the manuscript). We have included in appendix a couple of figures to evaluate surface PO4.

BOUM is the most complete dataset available for the Mediterranean because it covers a full, recent west-to-east transect. There are very few estimations of nutrient concentration (and especially phosphate) in the surface layer of the Mediterranean (first 5-100 m) because the concentrations are so low that measures are often below the detection limit of sensors. In this figure, we can see that the model reproduces the increase in concentration below 50 m observed in the western basin but that the increase in concentration modeled in the eastern basin (below 100-150 m) is not observed.

For further comparison, we show in the figures below the average phosphate profiles



in different regions of the Mediterranean compared with data from Manca et al. (2004). These figures show that the model can reproduce the phosphate vertical distribution and that the model values are generally in the range of data standard deviations in surface waters.

*Relatedly, on P11, l.342 the authors state: "Based on our large scale LMDz–INCA model, we estimate that combustion is responsible for 7 % on average of total PO4 supply. In comparison, the average contribution of Pdust to PO4 supply is 4 % (Table 2)." These are very precise numbers that imply high confidence. What is the certainty in the other P sources? Please rephrase, or discuss further.*

The contribution values given in p.11, l342 are the values from Table 2. They are based on our modeling values and take into account only the sources of phosphate that are included in the simulations (namely rivers, Atlantic inputs, desert dust and combustion derived atmospheric phosphate). These are estimates for our present simulation that do not represent the absolute truth on the contribution of atmospheric phosphate deposition, but give light on the relative importance of the 2 atmospheric sources under the specific conditions of the year 2005, according to the LMDz-INCA model outputs. The purpose of this Table (and of this study in general) is to raise questions on the relative importance of the various aerosol sources that border the Mediterranean and their potential impacts on the nutrient supply and biological productivity of the basin. The literature on the Mediterranean aerosols is often centered on Saharan dust deposition which is believed to have the highest impact on the basin's biogeochemistry. The Table aims at shading new light on the other sources and their potential role. Acknowledging that model limitations makes those number highly uncertain, they suggest that Saharan dust might not be as dominant as it was previously believed as a source of bioavailable nutrients. We added these precisions in the discussion section. (lines 371-390).

*4) Potential effects of nutrient co-limitation on the results. Most of the studies*

*that I know of (although I am not an expert), indicate that phosphorus may be co-limiting along with other nutrient sources. This may also be worth discussing further.*
Reviewer is right to point out that nutrient co-limitation is a key question in the study of marine biogeochemical cycles and in particular in oligotrophic areas such as the Mediterranean. PISCES is a Monod type model in which nutrient limitations are calculated in a Michaëlis-Menten formulation. This means that growth rates of phytoplankton increase linearly with nutrient concentrations when these concentrations are below a threshold. In an oligotrophic region such as the Mediterranean the concentrations are low enough for the growth rate to increase linearly with concentration. As a consequence, having no increase in productivity (that is linked to nutrient limitations) after phosphate deposition is a sign that growth rates are limited by at least one other nutrient (most probably N). We added a paragraph on section 3.4. "In general, we can identify 3 different biogeochemical responses in the 3 framed areas of Figure 7. Our hypothesis is that the different responses are linked to nutrient limitations. In the North Adriatic, the influence of coastal nutrient inputs leads to low nutrient limitation and high productivity. In the South Adriatic, the high impact of atmospheric phosphate deposition may be the sign of important phosphate limitation. Finally, the lack of response in South Ionian in spite of the relatively high atmospheric phosphate deposition probably indicates that the region is co-limited in P and N."

*I also had a variety of other, more minor suggestions/concerns: P2l.27: "The most important aerosol deposition fluxes to the global ocean are induced by sea salt and natural desert dust (Goudie, 2006; Albani et al., 2015) respectively corresponding to material recycling and external inputs." Did the authors mean "most important" here (which is dependent on the process of interest) or something like, "largest by mass"? Please rephrase.* Largest by mass.

*p.2 l. "It is especially important to constrain external sources of phosphorus because it limits productivity in many regions of the oceans." Reference?*



We added a reference to Moore et al (2013).

*p. 2 "The main sources of atmospheric phosphorus for the surface waters of the global ocean are desert dust, sea spray and combustion from anthropogenic activities (Graham and Duce, 1979; Mahowald et al., 2008). I don't think sea spray should be considered a source, because as the authors stated, it is recycled material.*
We agree with the reviewer and removed sea spray.

*P2l54: "The Mediterranean Sea is also a hot–spot for climate change impacts (Lejeusne et al., 2010), in part because it is the recipient of aerosols from a variety of different geographical sources." I don't see how being the recipient of aerosols from a variety of geographical sources makes the Mediterranean Sea a hotspot for climate change impacts (was that referenced in the Lejeusne article somewhere)? Suggest rewording.*
We rephrased this part : "The Mediterranean Sea is also a hot-spot for climate change impacts. Moreover, it is the recipient of aerosols from a variety of different geographical sources. The impacts of aerosol deposition on the Mediterranean region are not fully understood and they may change in the future as a result of climate change impacts on land and sea."

*P.4 l. 95: "These evaluations showed satisfying results." Please be more specific?*
Changed to "These evaluations showed that the NEMO model is able to produce satisfying results when studying characteristics such as age-tracer of water masses of passive tracer transport."

*p. 4, l. 111: "Biogeochemical characteristics of the latest version of the NEMOMED12/PISCES model are evaluated in Richon et al. (2017)." Am I correct in understanding that the Richon et al., 2017 model setup is very similar and*

*relevant to this work? If so, I recommend that the authors just cite this paper and summarize the relevant information on how well the model performs from Appendix A in the text, instead of including Appendix A which just repeats the information in Richon et al., 2017 as far as I can tell. Figures A1 and A2 are already in Richon et al., 2017 almost exactly, so those can also be removed.*
The model setup in this paper is the same as the one in Richon et al (2017). In the present study, we compare the model outputs with data for the year 2005. We decided to follow the advice of the reviewer and removed the appendix. We added in section 2.1 "The model NEMOMED12/PISCES is run in the same configuration than in Richon et al. (2017) who provide an evaluation of the model. In particular, the authors show that NEMOMED12/PISCES reproduces a correct west-to-east gradient of productivity when compared to satellite chlorophyll estimates in spite of some underestimation in the areas of high productivity such as the Gulf of Lions that they trace back to the circulation anomalies of the western basin. The vertical distribution of nutrients is satisfyingly reproduced by the model in spite of underestimations in the Levantine Intermediate Waters (LIW) because of the too smooth nutricline."

*P5, l.151: "Another important source of P aerosols in this region is sea spray" I recommend removing the word "source" and with something like "input" since recycled aerosols are not really a new source of P.* Changed.

*P7, l213: "The underestimation of total P deposition is also likely due in part to our omission of P from other potential sources such as PBAP and sea salt." Estimating deposition velocities from aerosols accurately is a major challenge (e.g., Jickells et al., 2017; Baker et al., 2017; Duce et al., 1991) and it is associated with high uncertainties in deposition fluxed to the ocean surface. I think this would be worth mentioning and keeping in mind as another major uncertainty for this comparison.*
We agree with the reviewer that calculating deposition fluxes from aerosol concentration can lead to high uncertainties, in particular when extrapolating the fluxes to an



entire region based on average concentrations. This is the reason we tried to evaluate directly the modeled deposition fluxes, taking into account dry and wet deposition processes and daily variability.
Deposition time series are only available at a few stations, and observations are not available in 2005 (our model year). This leads to a high uncertainty in our comparison data. But we believe that these measurements are more reliable to compare deposition fluxes than basin scale estimations that do not account for large deposition gradients. Our approach is more process-based and should lead to less uncertainty than basin scale extrapolation from velocity fluxes. However, the exclusion of soluble organic phosphorus, PBAB and sea salt inevitably leads to some additional uncertainties.

*Figure 2 caption: please note somewhere that this is model output.*
Done

*Table 2: Please mention in the Table or the caption that these estimates are modelderived. Also, as mentioned, the caption "Total P" is confusing –please clarify what you mean here – I think this value include riverine P? If so, please title this with something else distinguishable from total P in aerosols, and total sources of soluble PO4. Does the Krom et al estimate include rivers? Please specify*
Precisions added in the figure caption

*Fig. 4: Please define in the caption what the red and black bars indicate (which is where my eye goes first to find this information). Also, it would be useful to have the same numbers in the different regions that correspond to their label in Figure 2. Also, please clarify the units of the bar plots.* Caption changed.

*P.8, l. 244: "Our previous study showed that June is the period of most significant impacts from aerosol deposition in spite of the low fluxes, due to thermal stratification (Richon et al., 2017). " Please be more specific here - most significant*

*impacts on what?*
Changed to "more important impacts on surface marine productivity"

*p.8, l. 248: "The North Adriatic is under strong influence of riverine inputs and atmospheric deposition of P from combustion (Figure 3)". Did you mean Fig.4?* We changed the reference

*Section 3.2: it might be useful (although not strictly necessary for me to recommend for publication) to know how your model dust observations compare with AOD trends in the region, which are available during your study period.*
We did not follow this reviewer suggestion. However, as previously stated, we do not believe that AOD is a good proxy for deposition. High deposition is generally related to rain, which means very cloudy conditions unfavorable to AOD measurements.

*P9, l. 273: "Atmospheric phosphorus deposition has different impacts on PO4 concentration depending on the source, the location, and the period of the year." Suggest changing to, "Atmospheric phosphorus deposition has different impacts in the model on PO4 concentration depending on the source, the location, and the period of the year."* Done

*Section 3.3 and figure 5: Please define "maximal relative effects" and "relative impacts" and what a percent of average maximal relative effect means and how it is calculated. Where do you get the surface PO4 data? From the model or from observations? If in the model, how well does the model reproduce observations?*
We rephrased the caption.

*P9 l. 278: "Figure 5 shows the relative impacts of phosphorus deposition from the two sources (combustion and dust) on surface PO4 concentration for the month of June. The relative impacts of atmospheric deposition from different sources are*



*varying over time. . ." Please specify why you focus on June. You do not show or discuss how the relative impacts vary over time – please do so if you wish to keep this sentence.*
As stated in section 3.2, we focus on June because it is the month of the year when maximal effects of deposition on surface productivity are observed. In Richon et al. 2017, we link this result to the vertical stratification and high surface nutrient limitations associated with sufficient deposition fluxes. We removed the term "varying over time" from the sentence because it was confusing.

*Fig. 6: again, what does Total P represent in this instance? Pdust + Pcomb? Also, in the discussion of this figure, I think it is important to be much more focused on the uncertainties in your findings – e.g., regarding the relationship between modeled PO4 and Chl a, Redfieldian assumptions, etc.*
We added the following information: "In this Redfieldian version of PISCES, chlorophyll production is linked with nutrient uptake that is constrained by the Redfield ratio. Therefore, the addition of excess nutrient will enhance chlorophyll production as long as other nutrients are bioavailable in the Redfield proportions. These results may change in a non Redfieldian model."

*P10 l.330: "We performed a Student's t–test on the grid matrix of relative impacts of Pdust and Pcomb over the three regions . . . and found that the mean values are statistically different (p–value < 0.01). This shows that even though the impacts of Pdust are close to the effects of Pcomb in the South Ionian, they are significantly dominant. " What do you mean by "dominant" specifically? Larger? Just because differences are significant, does not mean that the differences are meaningful. Please clarify (or remove the sentence, since it does not appear to be central to the paper).*
Reviewer is right. Also, given the high uncertainty on deposition fluxes, we chose to remove this sentence.

*P13, l.: "In the coastal Adriatic and Aegean Seas that are under strong influence of anthropogenic emissions, we showed that combustion-derived phosphorus deposition has effects on the biological productivity." Suggest rephrasing to: "In the coastal Adriatic and Aegean Seas that are under strong influence of anthropogenic emissions, we showed that combustion-derived phosphorus deposition may have effects on the biological productivity" or something similar. I also suggest emphasizing that your idealized experiment results indicate that these effects are likely to be fairly small, although other experiments with more realistic biogeochemistry are necessary to further constrain this problem.*

We thank the reviewer for this suggestion. We modified the sentence and added to this paragraph "In general, results from this idealized study suggest that the impacts of atmospheric deposition of phosphate are likely to be fairly small, even though atmospheric sources of phosphate seem to be important contributors to the total nutrient pool in some regions of the basin."

---

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-242, 2017.



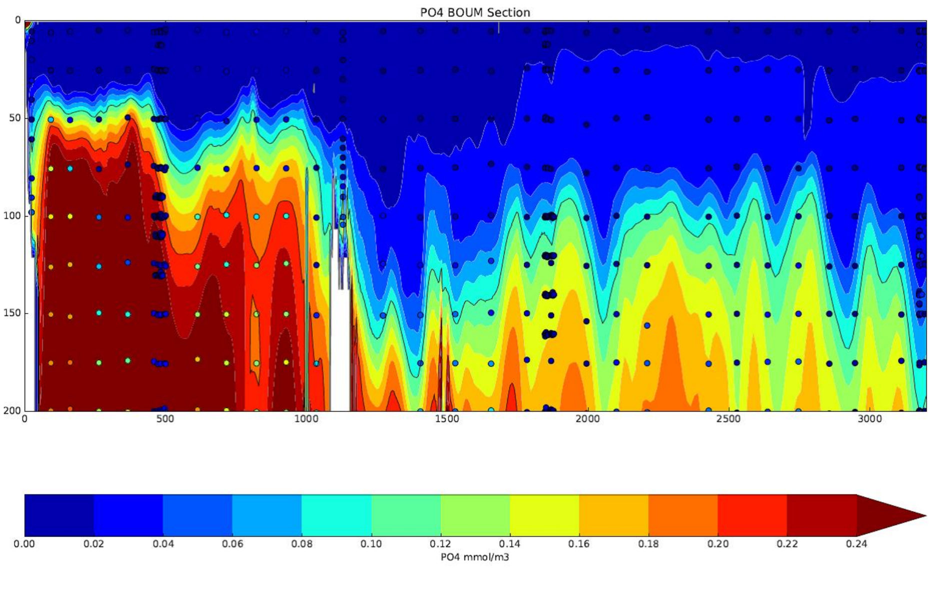


**Fig. 1.** PO4 concentration along the BOUM section (Moutin et al 2012). Zoom from the top 200m.

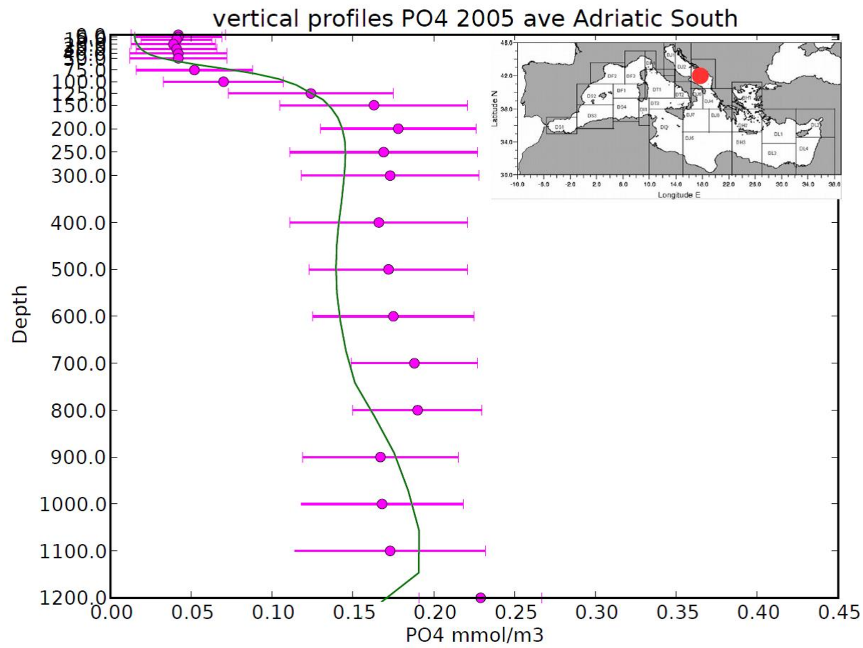


**Fig. 2.** Average concentration in 2005 in the South Adriaticregion (see map). Measurements and standard deviations are in pink, modeled values are represented by the green line.



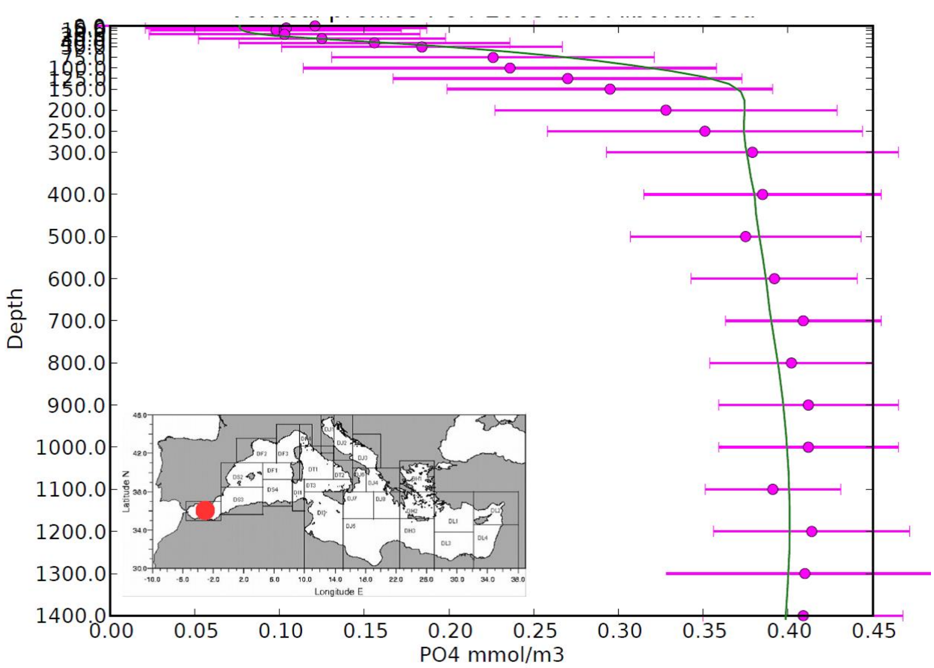


**Fig. 3.** Average concentration in 2005 in the Alboran Sea region (see map). Measurements and standard deviations are in pink, modeled values are represented by the green line.

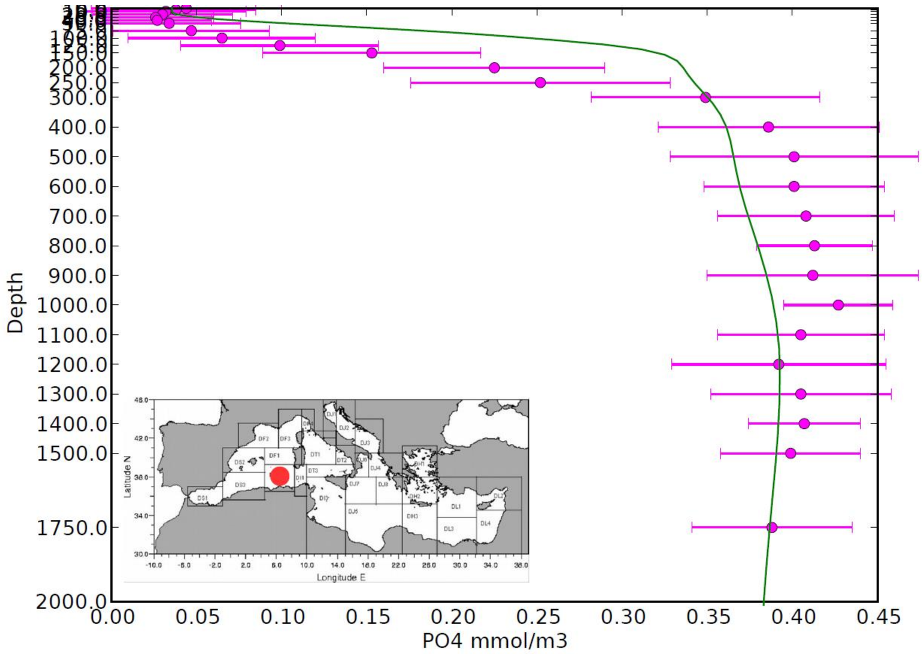


**Fig. 4.** Average concentration in 2005 in the Algerian current region (see map). Measurements and standard deviations are in pink, modeled values are represented by the green line.