We would like to thank Maurizio Ribera d'Alcala for their comments and suggestions on our manuscript. Please find below our responses (**in bold blue**) as well as the modifications performed in the revised version of the manuscript (*in italics*).

Referee Report, Manuscript Number: BG-2021-94

Phosphorus cycling in the upper waters of the Mediterranean Sea (Peacetime cruise): relative contribution of external and internal sources by Elvira Pulido-Villena, Karine Desboeufs, Kahina Djaoudi, France Van Wambeke, Stéphanie Barrillon, Andrea Doglioli, Anne Petrenko, Vincent Taillandier,, Franck Fu, Tiphanie Gaillard, Sophie Guasco, Sandra Nunige, Sylvain Triquet, Cécile Guieu

The paper presents the results of a study on the phosphorus cycle in the surface layer of 10 stations of Mediterranean sea in the framework of a cruise aimed at assessing the importance of atmospheric deposition in driving the functioning of the Mediterranean food web, with specific focus on the planktonic component. Almost (see below) all relevant processes have been considered and quantitatively characterized so that the study may be considered exhaustive. This is possibly the first time that almost all the relevant data/processes have been assembled at different sites of the Mediterranean sea to compare the contribution of different sources of phosphorus to sustain the primary production at the sites. Some fluxes, e.g., the diapycnal ones, suffers for the lack of the proper measuments but this does not at all affect the overall picture that derives from the study.

The main results are: 1. that atmopsheric inputs of phosphorus, even taking out dust deposition events, are in the same order (in the WMED) or are definitely larger (in the EMED) than the fluxes from the phosphocline; 2. there is a slight mismatch between new primary production inferred by direct measurements of C assimilation and that sustainable by the observed phosphorus fluxes (but see below); 3. there is fine 'compartimentalization' in the supposed upper homogeneous layer of phosphorus dynamics, which could be assessed thanks to the high sensitivity analytical methods that were used. The authors also drew some conclusions about possible, existing problems on the methods currently used to assess the different paths of phosphorus cycling in the ocean.

I definitely support the publication of the paper also because its results may contribute to an in depth discussion on the distinction between new vs. recycled production.

My only suggestion is to think about one component of the P-cycle that is not discussed, though it might be part of another paper of the same special issue, which is the fate of the POP. I made a very rough computation. Considering the observed P fluxes to the PDL which might be assimilated (Table S1), if redistributed in the whole layer they would increase POP by several nM d-1 (~6 nM d-1 for the ION station with C:P=130), which is in the order of magnitude of the measured POP standing stock. If my computation is correct and the system is in quasi steady state, meaning that there is no sharp accumulation of P in the PDL, this flux must be balanced by the export. Part of the POP, as well as DOP, might be exported below the PDL by sinking particles, though their sinking velocity is not very large considering the prevalence of pico-plankton, by the diapycnal export due to the mixing (DOP and

POP gradients should be opposite to that of Phosphate) and by POP removal by consumption. Flow to the adult metazoans would be missed in the particulate but part of consumption might occur via protozoan grazing and mixotrophy by autotrophic organisms, which would add another term to the P turnover, in addition to that of DOP. This might be considered in the discussion.

We are not sure to have understood this suggestion and we apologize for this. If we understood well, external fluxes of P to the PDL (0.913 µmol P m^{-2} d^{-1} in the case of ION station), if redistributed in the whole layer (166 m depth), would result in an increase of POP of 0.006 µmol m^{-3} d^{-1}, that is 0.006 nM d^{-1} which is two orders of magnitude lower than measured POP (10 nM). If this computation is correct (unless we misunderstood your comment), there would not be a significant net accumulation of POP in the surface layer and the system would not be imbalanced. Nevertheless, we will be happy to discuss this again if we did not get your point.

Below some minor suggestions/remarks

l.22 "collocated" better co-located

This will be corrected in the revised manuscript.

l.27-29 and l.177 I think that "..gradient of phosphate concentration over density" would be more clear than "density gradient"

This will be corrected in the revised manuscript.

l.316-317 "was set to zero" ,l.347-348 "has been traditionally seen as a homogeneous layer" and l.372-373 "...the picture of a homogeneous pool of phosphate between the surface and the phosphacline with concentrations theoretically set to zero" I would rephrase them all to convey the concept that the real phosphate concentration and its gradient could not be properly assessed since reported concentrations in the literature were in the range of the detection limit of the classical methodology, which is different from the assumption that were zero. There are plenty of papers and two databases that report measured phosphate concentrations larger then zero in the Mediterranean surface layer.

We agree and we will rephrase this all throughout the revised version of the manuscript.

l.387-388 "..our estimates are in the lower range" why in the lower range? even with a lower epsilon they are larger.

Here we compare diapycnal fluxes of P to the base of the ML (that is, above the phosphacline) between our study (0-0.19 µmol m^{-2} d^{-1}) and Du et al (2017) (0.21-0.44 µmol m^{-2} d^{-1}). These fluxes are certainly lower than those associated to the phosphacline which should not reach the upper euphotic layer.

We realize that the sensitivity test to epsilon performed in the paragraph just above (on the P fluxes across the phosphacline) can lead to confusion and does not add that much to the discussion. We will thus remove it to avoid misunderstanding.

Obviously, the sensitivity of computed diapycnal fluxes to the ε term in the equation may question these estimates. Now that analytical locks have been "opened" allowing to detect consistent gradients of phosphate concentration over density above the phosphacline, future concomitant measurements of microstructure measurements of turbulent dissipation rate or modeled Kz-profiles (Costa et al. 2017) will allow quantifying with better accuracy the vertical flux of new P reaching the upper waters of the euphotic zone.

l.408-409 "This result contrasts with the longtime idea that, under stratification conditions, the upper waters of the Mediterranean Sea receive new P mainly exclusively from the atmosphere" indeed this is what the study shows for the EMED.

You are right. We have rephrased this sentence to clarify our point which is that atmospheric deposition is not the only external source of P to the mixed layer, particularly in the WMED.

"In the western part of the transect, from ST10 to ST4, both fluxes were of the same order of magnitude, except for FAST where atmospheric deposition was dominant. This contrasts with the longtime idea that, under stratification conditions, the upper waters of the Mediterranean Sea receive new P exclusively from the atmosphere. Contrarily, from ST5 to the easternmost part of the transect, diapycnal fluxes of phosphate were negligible compared to atmospheric deposition (Fig. 4, Table S1). Although these findings must be taken cautiously given the uncertainties in the estimation of diapycnal fluxes, it opens exciting questions on the biogeochemical response of the Mediterranean Sea, and more generally of marine oligotrophic regions, to expected changes in atmospheric inputs and stratification regimes (Powley et al., 2017)".

l.449-453 "..lateral transport of phosphate notably contributed phosphate external supply to upper waters, explaining the low contribution of both atmospheric deposition and diapycnal fluxes to new production" I am a little perplex about this statement because the spatio-temporal scales of lateral transport are different from those assessed with the observations discussed in the paper.

We agree that this part of the discussion was very confusing since it mixed up mechanisms and processes occurring at different spatio-temporal scales. We have rephrased this section considering two different scales of lateral fluxes: large scale (basin/year) and mesoscale. We will make the distinction in the revised paper between the basin scale (where homogeneity in surface phosphate precludes horizontal gradients) and the particular case of ST10 where lateral phosphate supply may be more important due to local transport specific to the presence of a mesoscale gyre (Guieu et al. 2020 for details).

The imbalance between estimated external sources of P and new production was particularly marked in ST10, the westernmost station of the transect, where external P supply may explain up to 10% of new *production in the best case. This contrasts with the rest of stations in the Algerian basin where external P supply explained between 30 and 90 %. As said before, it was hypothesized that, at the basin scale, lateral transport was not a dominant P source mainly based on the homogeneity of surface phosphate concentration excluding the presence of important horizontal gradients. Nevertheless, ST10 might be an exception to this assumption. Using satellite derived currents, we observed the presence of a mesoscale eddy and ST10 was located on the edge of this eddy (Guieu et al. 2020). This can imply a mesoscale transport of nutrients from Atlantic water containing significant levels of phosphate (Huertas et al.. 2012). It may not be excluded that in ST10 lateral transport contributed to phosphate external supply to upper waters, explaining the low contribution of both atmospheric deposition and diapycnal fluxes to new production.*

l.521-523 "...the obtained numbers are distinct enough to confirm the domination of internal sources in the total phosphate supply to the surface waters of the Mediterranean Sea under stratified conditions, with little variation of this dominance across the longitudinal transect." This might deserve some more comments (see also above). On one hand this confirms that in the sampled region in late spring recycled production dominates, which was more or less known, but also that phosphorus turnover time is very fast, and that, I suspect, most of the DOP that sustains the recycled production in the EMED dreives from the atmopsheric deposition more than from Atlantic water. What comes from diapycnal fluxes is just recycling on a longer time scale.

We agree. However, given the uncertainties associated to our results and our ‘snapshot’ approach, we prefer not to throw any conclusion concerning larger spatio-temporal scales which would not be supported by our data. The fate of atmospheric deposition in the euphotic zone was assessed through a high frequency study of a rain event at the FAST station in Van Wambeke et al. (2021). This work showed a very rapid response of the microbial food web and a return to initial biogeochemical conditions within two days.

Van Wambeke, F., Taillandier, V., Deboeufs, K., Pulido-Villena, E., Dinasquet, J., Engel, A., Marañón, E., Ridame, C., and Guieu, C.: Influence of atmospheric deposition on biogeochemical cycles in an oligotrophic ocean system, Biogeosciences Discuss. [preprint], https://doi.org/10.5194/bg-2020-411, in review, 2020.

References

l.159 2020, Desboeufs et al. also in l.219, l.295, l.418 and Fu et al., also on l.211, l.439 in preparation if it is for Atm. Chem. Phys. is not in this special issue

We apologize for this. Desboeufs et al. is submitted and in progress of edition for discussion in ACP in this special issue. For Fu et al., it is in prep. and should be submitted before the end of the summer in ACP out of the Special Issue.

l.214 (Izquierdo et al., 2012) missing in the reference list

The reference will be added to the list.

l.318 Van Wambeke et al. (1999) missing

The reference will be added to the list.

in the reference list but never cited:

Hashihama, F., Kinouchi, S., Suwa, S., Suzumura, M., and Kanda, J.: Sensitive determination of enzymatically labile dissolved organic phosphorus and its vertical profiles in the oligotrophic western North Pacific and East China Sea, J Oceanogr, 69, 357–367, DOI 10.1007/s10872-013-0178-4, 2013.

This reference is now cited in the revised manuscript following the suggestion of referee 1.

Markaki, Z., Loÿe-Pilot, M., Violaki, K., Benyahya, L. and Mihalopoulos, N.: Variability of atmospheric deposition of dissolved nitrogen and phosphorus in the Mediterranean and possible link to the anomalous seawater N/P ratio, Marine Chemistry, 120(1-4), 187–194, 2010.

Removed

Table 4 what does it mean Long in a correlation table? some correlations are not particularly useful

We agree and we will remove non relevant correlations in the revised manuscript.

	PD	DIP	DOP	POP	V_{max}	K_m	V_{max}/POP	T_{MUF}
PD	1.0000							
DIP	-0.259 p= 0.470	1.0000						
DOP	-0.396 p= 0.257	0.0354 p= 0.923	1.0000					
POP	**-0.757** **p= 0.011**	0.4070 p= 0.243	**0.721** **p= 0.019**	1.0000				
V_{max}	0.780 **p= 0.008**	-0.5710 p= 0.085	-0.470 p= 0.170	**-0.819** **p= 0.004**	1.0000			
K_m	-0.108 p= 0.767	0.3773 p= 0.282	-0.003 p= 0.994	0.046 p= 0.899	0.164 p= 0.652	1.0000		
V_{max}/POP	0.801 **p= 0.005**	-0.5438 p= 0.104	-0.450 p= 0.192	**-0.809** **p= 0.005**	**0.983** **p= 0.000**	0.051 p= 0.890	1.0000	
T_{MUF}	-0.510 p= 0.132	**0.7101** **p= 0.021**	0.619 p= 0.056	**0.751** **p= 0.012**	**-0.740** **p= 0.014**	0.130 p= 0.721	**-0.685** **p= 0.029**	1.0000

Table 4. Pearson correlation coefficients among biogeochemical variables related to the phosphorus pool inside the phosphate depleted layer (n= 10). PD = phosphacline depth. DIP: dissolved inorganic phosphate concentration. DOP/ dissolved organic phosphorus. POP: particulate organic phosphorus. Vmax: AP maximum hydrolysis velocity. Km: AP Michaelis Menten half saturation constant. Vmax/POP: AP maximum hydrolysis velocity normalized to POP. TMUF: turnover time of spiked DOP substrate, MUF-P. Significant correlations at p< 0.05 are highlighted in bold.

Table S2 better to replace Tmoy(enne) with Tavg or T mean

We apologize for the mistake. This will be corrected.