Responses to Reviewer 2

Comment 1: Overall this is a well-written paper describing a dataset that may be useful for many purposes. While it is clear that the data were derived from semi-natural sites, the information may be relevant to agricultural issues as well, provided there is clarity on what is represented by the global totals presented. I offer the following comments and suggestions for improvement of the clarity and interpretation of the paper and dataset.

Response 1: We appreciate these positive comments. Below we respond to your comments point-by-point. Note that text in black are the comments, and text in deep blue are our responses.

Comment 2: Line 15 - The term "reserves" has a specific definition applying to the mining of geological phosphate rock resources. I would suggest the word "stocks" be substituted for the word "reserves" as is done in line 26.

Response 2: We appreciate that you point this out. In the revised manuscript, we have replaced "reserves" with "stocks" in lines 15 in the revised manuscript.

Comment 3: Line 15 & 32 - Apparently, P in geological formations and rock below the land surface are not considered. The reference Zhang et al. 2021 in line 32 pertains only to China, and compares soil P only to that in leaves, woody stems, roots, and litter in forest, shrubland and grassland ecosystems. It does not assess geological P. Suggested alternate wording: "In terrestrial ecosystems to a depth of one meter from the land surface, most of the P is found in the soil."

Response 3: Really appreciate pointing this out. In the revision, we have revised "on land" to "in terrestrial ecosystems" (Lines 15). Ecosystem only includes land surface interacting with living organisms, while excluding deep geological formations and rock. As for the first sentence in the introduction, we have re-written it as you suggested, i.e., "In terrestrial ecosystems to a depth of one meter from the land surface, most of the P is found in the soil." (Line 32).

Comment 4: Line 26 (and lines 243-244) - Clarity needs to be provided for these figures on global soil P stocks. Do they include agricultural lands that were not included in the study? If so, do they represent and estimate of the soil total P content prior to land conversion? What percentage of the total would be comprised of such lands? From global fertilizer consumption figures, it can be estimated that ~0.9 Pg of mined P has been added to soils since the early days of fertilizer manufacturing around, and similar figures for the removal of P in crop harvests could be calculated. Such figures would be useful for those concerned about the depletion of soil P relative to the global reserves of phosphate rock (estimated at ~7 Pg P in USGS Mineral Surveys).

Response 4: Many thanks for pointing this out. Yes, we didn't mask out the cropland or other heavy influenced areas from the predicted maps. And our predicted maps and estimates represent "potential natural background values", i.e., background soil P without direct human influence. To avoid misunderstanding and misuse, in the revision, we have marked cropland areas in our predicted maps (Fig. 5 of main text) as a reminder that our predicted values in these grid-cells indicates "potential natural background" values. This has also been described in the introduction (Lines 80-82) and new readme file (Note 1).

According to an empirical model, about 20% fertilizer P is fixed in soil as stable soil P (Sattari et al., 2012). As a simple estimation, the soil P stock estimation gap between our prediction and current state in global cropland areas could be 0.9 Pg * 20% = 0.18 Pg. It is an interesting and great idea to estimate the cropland soil P depletion relative to global phosphate rock reserves, in view of the increasing food demand accelerated flow of phosphate rock from Earth's crust for fertilizer (Cordell and White, 2014). And our predicted maps could be used as an improved natural background map in future studies.

References:

Cordell, D. and White, S.: Life's Bottleneck: Sustaining the World's Phosphorus for a Food Secure Future. Annu. Rev. Env. Resour., 39, 161-188, https://doi.org/10.1146/annurev-environ-010213-113300, 2014.

Sattari, S. Z., Bouwman, A. F., Giller, K. E. & van Ittersum, M. K. (2012) Residual soil phosphorus as the missing piece in the global phosphorus crisis puzzle. Proceedings of the National Academy of Sciences, 109, 6348-6353.

Comment 5: Line 34 - Regarding "soil P form depends on the amount or total concentration of P in soils" — This dependence does not seem clear or intuitive, and is not clearly supported by the three references. In fact Turner and Engelbrecht conclude "We conclude that soil properties exert a strong control on the amounts and forms of soil organic phosphorus in tropical rain forests, but that the proportion of the total phosphorus in organic forms is relatively insensitive to variation in climate and soil properties."

Response 5: Many thanks for pointing this out. In the revision, we have re-written this sentence as: "Moreover, the amount or total concentration of P in soils determines P concentration in all major forms in soils (Hou et al., 2018a; Turner and Engelbrecht, 2011)." (Lines 35-36).

Comment 6: Lines 45-48 - the importance of bioavailable soil P relative to total soil P to ecosystem functioning should be discussed.

Response 6: Many thanks for this advice. This manuscript didn't include data or results on soil bioavailable P, so we didn't discuss the importance of bioavailable soil P relative to total soil P either in introduction or in discussion. But yes, this is a very important issue. We have complied a database of Hedley P fractions concentration in global natural soil, which including more than 1800 site-level measurements. Now we are drafting a manuscript, in which we will discuss the importance and drivers of soil bioavailable P relative to total P across terrestrial ecosystems.

Comment 7: Line 63 - the two references, Carpenter and Bennett 2011 and Steffen et al 2015 do not support the reliance on accurate soil P maps.

Response 7: Many thanks. We have replaced these two references with four other references to support the reliance on accurate soil P maps, i.e., (Alewell et al., 2020; Ringeval et al., 2017; Beusen et al., 2015; Wang et al., 2010) (Lines 64-65).

Reference:

Alewell, C., Ringeval, B., Ballabio, C., Robinson, D.A., Panagos, P. and Borrelli, P.: Global phosphorus shortage will be aggravated by soil erosion. Nat. Commun., 11, https://doi.or/10.1038/s41467-020-18326-7, 2020.

Beusen, A.H.W., Van Beek, L.P.H., Bouwman, A.F., Mogollón, J.M. and Middelburg, J.J.: Coupling global models for hydrology and nutrient loading to simulate nitrogen and phosphorus retention in surface water – description of IMAGE – GNM and analysis of performance. Geosci. Model Dev., 8, 4045-4067, https://doi.or/10.5194/gmd-8-4045-2015,

2015.
Ringeval, B., Augusto, L., Monod, H., van Apeldoorn, D., Bouwman, L., Yang, X., Achat, D.L., Chini, L.P., Van Oost, K., Guenet, B., Wang, R., Decharme, B., Nesme, T. and Pellerin, S.: Phosphorus in agricultural soils: drivers of its distribution at the global scale. Global Change Biol., 23, 3418-3432, https://doi.or/10.1111/gcb.13618, 2017.
Wang, Y.P., Law, R.M. and Pak, B.: A global model of carbon, nitrogen and phosphorus cycles for the terrestrial biosphere. Biogeosciences, 7, 2261-2282, https://doi.or/10.5194/bg-7-2261-2010, 2010.

Comment 8: Line 92 - “heavily” should be “heavy”
Response 8: Done (Line 93).

Comment 9: Line 99 - “potentially useful” should be better defined. What factors decided the exclusion of 77% of the papers?
Response 9: In the revision, we have described in more details how we screened the literature in this step (Lines 102-103). As we want to search existing global or regional databases that may include soil total P concentration measurements in (semi-)natural ecosystems, by looking at title and abstract, we excluded studies at site level (or local scale) or with artificial treatment (e.g., fertilizer treatment, elevated temperature, or elevated CO_2 etc.). And 163 papers were removed at this step.

Comment 10: Line 121-124 - For the under-represented regions, no exclusion of agricultural land was described. Are these regions then more likely to include land that is not semi-natural?
Response 10: For the under-represented regions, we have also excluded agricultural soils or other human influenced soils at this step. We have stated (Lines 89-93) we collected soil total P concentration measurements in (semi-)natural terrestrial ecosystems. To avoid any misunderstanding, in revision, we have clarified this again here (Lines 126-127).

Comment 29: Line 127-128 - What were the data sources for climate, vegetation, etc?
Response 29: We collected climate variables (i.e., MAT and MAP), vegetation type, soil physiochemical properties (e.g., SOC, soil clay and sand contents, soil pH) of corresponding soil total P measures from source paper when they are reported. In revision, we have clarified this (Line 133). And “In cases where information on predictors were not reported, we extracted the missing data from gridded datasets (Table S3) based on the geographical coordinates of the measurement sites.” was stated later (Lines 158-159).

Comment 11: Line 179-180 - need to state the assumption that cropland in its native state had the same set of relationships as for semi-natural land. The possibility that land with different total P levels, or different total P relationships, should be discussed explicitly.
Response 11: We really appreciate this suggestion. We have stated this assumption in the revised manuscript “Here we assume that cropland and other heavy influenced areas in its native state had the same set of relationships as for (semi-)natural land.” (Lines 188-189). And yes, there is a possibility that in these underrepresented areas different soil P levels and different total P

relationships could exist. So, we have pointed this out as a data limitation in our revised manuscript (Lines 363-365), which is "First, there are some regions were still underrepresented, e.g., northern Canada, Russia, middle Asia, and inner Australia, which may result in a low accuracy of the predicted values in these regions (Ploton et al., 2020). Further, our assumption that soils which are or have been in agricultural use can be characterized in their native state by the same relationships as semi(natural) soils might not hold true. For example, as fertile soils are preferred in agriculture and forestry.".

Comment 12: Line 201 - the reported mean and median values then represent a mix of topsoil and subsoil values. Since subsoil comprised only 15% of the samples, it has little effect, but the numbers for a single specified depth (topsoil) would be more useful and relevant.
Response 12: Many thanks for this suggestion. In revision, we have re-organized table 2 and 3 in the main text. Now these two tables show soil total P in different biomes and soil orders at 0-30 cm and 0-100 cm, respectively.

Comment 13: line 239 - in Figure 4 it is clear that many of the predictors have non-linear relationships to soil total P. Does the random forest method account for non-linearity?
Response 13: Yes, the random forest method can deal with non-linear relationships (Breiman, 2001). Here we used partial dependence plots to illustrate the relationships between one predictor and the predictions of the trained model. This is a common diagnostic to illustrate the dependence of 'black box' machine learning predictions to potential drivers (Heffelfinger et al., 2020; von Fromm et al., 2021; Berkström et al., 2020). Partial dependence plots look at the variable of interest across its corresponding range in training data. At each value of the variable, the model is evaluated for all observations of the other model inputs, and the output is then averaged. Then, a partial dependence plot can show different types of relationship between predictor and prediction, such as a step function, curvilinear, linear, and so on.

Reference:

Breiman L: Random Forests. Machine Learning 2001, 45:5-32.

Berkström, C., Eggertsen, L., Goodell, W., Cordeiro, C.A.M.M., Lucena, M.B., Gustafsson, R., Bandeira, S., Jiddawi, N. and Ferreira, C.E.L.: Thresholds in seascape connectivity: the spatial arrangement of nursery habitats structure fish communities on nearby reefs. Ecography, 43, 882-896, https://doi.org/10.1111/ecog.04868, 2020.

Heffelfinger, L.J., Stewart, K.M., Shoemaker, K.T., Darby, N.W. and Bleich, V.C.: Balancing Current and Future Reproductive Investment: Variation in Resource Selection During Stages of Reproduction in a Long-Lived Herbivore. Frontiers in Ecology and Evolution, 8, https://doi.org/10.3389/fevo.2020.00163, 2020.

von Fromm, S.F., Hoyt, A.M., Lange, M., Acquah, G.E., Aynekulu, E., Berhe, A.A., Haefele, S.M., McGrath, S.P., Shepherd, K.D., Sila, A.M., Six, J., Towett, E.K., Trumbore, S.E., Vågen, T., Weullow, E., Winowiecki, L.A. and Doetterl, S.: Continental-scale controls on soil organic carbon across sub-Saharan Africa. SOIL, 7, 305-332, https://doi.org/10.5194/soil-7-305-2021, 2021.

Comment 14: Line 295 - "soil P is largely composed of organic P" is contradicted by Turner and Englebrecht 2011 who reported organic was 26% of total P for lowland tropical rain forests.

Exceptions include the tundra and boreal sites included in this study. These sites were likely the drivers of the SOC-TP relationship.

Response 14: We appreciate this comment. In the revision we have removed this description. Now the first explanation why significantly positive correlation was found between soil total P and SOC across terrestrial ecosystems only emphasizing the coupling between P and C in soils. “Phosphorus couples with organic C in soil because soil P has a relatively fixed ratio to organic C (Spohn, 2020; Cleveland and Liptzin, 2007).” (Lines 310-312).

Comment 15: Line 312 - Amberger reference missing.

Response 15: It has been added in the revised manuscript (Lines 413-414).