Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-176-AC1, 2018

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Global Phosphorus Recovery for Agricultural Reuse" *by* Dirk-Jan D. Kok et al.

Dirk-Jan D. Kok et al.

dirkjan.k.1993@gmail.com

Received and published: 30 April 2018

That authors would like to thank the anonymous referee for the helpful feedback. The referee has shared very valid concerns that we hope to address in a revised version of the article. In the below, we would like to share our thoughts with regards to the comments made and present how we would like to resolve the issues identified.

1. The referee identifies a lack in detailed explanation in the text on how we determine the P coming from manure. The referee finds it unclear: (a) how we account for manure that falls on pasture during grazing and the part that falls on roadside and; (b) how we determine manure production.

In the study, we identify livestock density distributions using geospatial data (FAO grid-

ded livestock of the world (GLW)). We then attach a single phosphorus throughput estimate based on literature to each individual in each livestock group (cattle, poultry, swine). With regards to manure accumulating in pastures and on roadsides (a), it is assumed that only half of the global excreted phosphorus from cattle is recoverable due to different pastured-stabled periods (pastured for half a year resulting in loss of phosphorus to pastures, and stabled for half a year during which phosphorus can be recovered in pits). This methodology will be emphasized in the new review. With regards to determining global manure production (b), we acknowledge that we had difficulty attaching a phosphorus production potential to each livestock head for the reasons mentioned by the referee (i.e. varies per volume of feed consumed, spatial data that is not available to our knowledge). In fact, the amount of phosphorus excreted will not only vary with the volume of feed consumed, but also vary per quality of feed, the animal species, race, gender, and the animals age. Slaughterhouse weight appears to be a logical index for these parameters and we regret that we did not think of using this ourselves. We will therefore integrate this in the revised version of the article, and would like to thank the referee for providing this particular insight.

2. The referee finds equation 2 confusing and remarks that no detail on the input is provided. The referee also questions the introduction of the additional parameters (Tg, AH, and C) into the equation.

As the referee remarks, the equation by Doorenbos and Kassam (1979) or Steduto et al. (2012) is specified for a single growing period. The ET used should then coincide with this growing period. However, the growing period starts and ends on different dates depending on geographical location (mostly latitude) of the cropped area, and therefore so will the ET. The crop harvested area maps, meanwhile, are yearly and therefore do not disseminate as to when to growing season starts or ends in different parts of the world. Attaching a relevant ET to each area thus becomes complicated as European corn will grow in European summers, while in Latin America it will likely grow during European winters – meanwhile the crop harvest maps only show that both regions grow corn that year. As such, without manipulating the data, only the growing duration (Tg) can be assumed to be roughly similar for the crops in both areas. To still make an estimate of the potential yield, we disregard for different starts of growing seasons, and calculate ET over the entire year. We then assume that a proportion equal to the growing season duration (Tg) of that year's ET is available to the crop (Tg/365). Thus we ignore ET seasonality, i.e. assume it to be constant over the duration of a year. Then the ET attributed to growing periods are corrected for the area harvested (AH), again assuming that ET is independent of landcover type. Due to these induced assumptions, some parts of the world achieve higher than optimum yields. To correct for this, C was introduced that scales everything above optimum, back down to optimum. Other parameters (e.g. water requirements and crop specific data) were taken from FAO and are assumed globally homogenous. Since estimating the potential yield is only a small (but important) component of this broad scoped study, we agree that integrating Monfreda's (2008) potential yield data offers a better, more credible alternative.

3. The referee finds it unclear from the manuscript how the demand and supply nodes are defined, and more specifically how we determine their location.

In the study, the locations for demand and supply nodes are determined using geospatial data. For unsustainable supply nodes, the mine locations are taken directly from a 2002 USGS vector dataset. For sustainable supply nodes (wastewater accumulation sites), we simply multiply population density rasters with phosphorus throughput statistics (discussed for livestock in point 1) to acquire phosphorus production density rasters. Large, connected areas of high phosphorus production potential are then converted to nodes using GIS tools. To ensure that, for example, the highly populated German Ruhr region is not merged together in a node that represents most of the Netherlands, the raster zones are separated by administrative boundaries. The exact location of the node is in the middle of each separated, high potential density area. This aspect of the methodology will be more explicitly explained in the updated version

of the manuscript.

4. The referee wonders if we account for the legacy P or the residual P that accumulates in the soil.

Although it was considered, we decided not to complicate this study further by including changes in soil stored P. Instead, we treat phosphorus like a hydrological bucket model, where we assume the year to year change in soil stored phosphorus to be 0. The yearly demand is then equal to the yearly phosphorus requirements of crops to meet corresponding water-constrained yield. To emphasize this, we will create an comprehensive overview of all the assumptions in this study

5. The referee remarks the absence of an uncertainty analysis with regards to modeling.

The lack of an uncertainty analysis is concern shared with the authors. We refrained from doing this analysis provided the many assumptions, parameters and variables included in the model which we felt illustrated qualitatively quite well the uncertainty around the results. Recognizing again that doing a quantitative uncertainty analysis is standard procedure for any modeling investigation, we will explore which form of uncertainty analysis is most appropriate for this study.

6. The referee remarks that Table 1 is not clear, and wonders why the values for Livestock Raster vs Livestock node and Human raster vs urban node are different? He also remarks that he is unable to find Figure S2a, S2b, and S2c.

Table 1 is unclear because of improper definitions in the text of the differences between the 'raster' and 'node' values. The difference between 'raster' and 'node' values exists to account for differences in definition of 'recovery potential'. The raster value represents the how much phosphorus could become available if we recover phosphorus everywhere (across the entire raster), while the node value represents only that amount if we recover from high production sites only. This is to say that not all raster values are translated into nodes. Low phosphorus production densities are systematically excluded from further analysis after conversion to nodes (hence the different values between 'raster' and 'node' demand/production). This aspect of the methodology will be elaborated upon in the updated version. Furthermore, Figure S2a, S2b and S2c should indeed reference figure S1(a,b, and c). This will of course be adapted in the next version.

7. The referee believes it will enhance the reliability of result if we compare the results with earlier estimates such as Bouwman et al. (2013) for manure; Van Drecht et al. (2009) and Moree et al. (2013) for domestic.

In the manuscript as is, we compare our results to three other related studies. We intend to further extend this comparison and create a more complete overview by including the works referenced to by the referee, also.

We would like to again thank the referee for his/her review and feedback. We hope to adequately address the issues identified in the updated version of the manuscript, and look forward to any other feedback he/she may have.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-176, 2018.