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Interactive comment on "A global model of carbon, nitrogen and phosphorus cycles for the terrestrial biosphere" *by* Y. P. Wang et al.

Anonymous Referee #3

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This paper by Wang et al sets out to assess the global terrestrial carbon, nitrogen and phosphorus budgets using a coupled biogeochemical model. The authors examined the spatial distribution of steady state pools and fluxes of C, N and P using the monthly net primary productivity estimates of Randerson et al (1997) for 1990. In general, the results from this modelling study are similar to previous estimates of the global distribution of these elements. The authors claim that P limits NPP by about 20% in tropical savannas and broadleaf moist forests, but elsewhere NPP is primarily limited by nitrogen by up to 40%. This paper does not, however, provide a convincing case for these claims for the following reasons:

1. While the carbon and nitrogen sub-models are reasonably well 'constrained' by observations (e.g. where supplementary information is used to justify model parameters)

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the phosphorus sub-model is virtually un-constrained by a lack of data on the key processes and pool sizes. The output of the phosphorus sub-model is therefore largely driven by the dynamics of the carbon cycle and the C:P ratios as governed by the P mineralisation rate. The rate parameters were determined by estimation against Cross and Schlesinger (1995) which is a small study of 88 published papers from sites in Canada, the USA, Spain and South America. This is not a comprehensive body of observations against which definitive estimates of P pools and fluxes can be made. Similarly the evaluation datasets are comprehensive for carbon (latitudinal summations of a range of global studies), limited for nitrogen (latitudinal averages for two studies by Post et al) and virtually non-existent for phosphorus (one study by Cross and Schlesinger 1995). As a result, the claims regarding the global phosphorus budget (the most important new piece of work in the paper) provide virtually no new information than what has been generated using simpler methods 20 years ago.

2. The steady state condition of the model is not met in 1990. With global CO_2 concentrations increasing by about 25% over pre-industrial values in the late 20th Century, the global biosphere should be responding to increased carbon inputs by entraining more nitrogen and phosphorus. The idea that increasing atmospheric CO_2 concentration leads to increasing root exudation, mycorrhizal infection and nitrogen fixation thereby increasing nutrient availability in ecosystems was first promoted in the late 1970's and early 1980's (see Luxmore 1981, BioScience, 31, 626) and there have been numerous later publications examining the processes and potential magnitude of this effect (e.g. see Lloyd et al 2001 in 'Global Biogeochemical Cycles in the Climate System' Academic Press). The correct approach would be to model the steady state in pre-industrial atmospheric CO_2 concentrations and then examine the dynamics of the pools and fluxes in the disequilibrium state in the 1990's to ascertain the magnitude of the entrainment of nitrogen and phosphorus into the global terrestrial biosphere.

3. The phosphorus model is not robust. Figure 1 depicts the transfer of inorganic P to the 'sorbed P' pool but there is no equivalent for sorption of organic P. It shows a flux out

of the 'strongly sorbed P' pool as a loss from the system (why is occluded P lost from the system?). The plant uptake of P appears to occur from every pool. The appendix shows that phosphorus mineralisation is represented by a single asymptotic function modified by the sorption/desorption of P by soil minerals but there is no consideration at all in this paper of the role of mycorrhizae, root architecture and organic acid root exudates; all of which are important determinants of plant P acquisition. The main text states that " the rate of sorbed P to strongly sorbed P is assumed to be proportional to the amount of sorbed P in the soil". There is no basis for this assumption as this is a function of mineralogy and soil age. The parameters Kplab and Spmax are both determined from the Cross and Schlesinger (1995) study and then the results of the Cross and Schlesinger paper are used to evaluate the equilibrium pool sizes and fluxes in section 5.1. This is in spite of the fact that the model considers the whole root zone (to an unspecified depth) whereas Cross and Schlesinger considered phosphorus fractions only in the top 15 cm of soil. The assumption that 15% of soil P is occluded was used to estimate the total amount of occluded P (4.6Gt). This assumption was that purported to come from Johnson et al (2003). Johnson et al do not consider occluded P at all in their paper, rather they examine the size of the labile inorganic and organic P pools in tropical and temperate forest soils. I can only assume that this 15% assumption is based on their Figure 6 which is a conceptual diagram of the proportions of P along an axis of increasingly weathered soils. This is not a basis for assuming global occluded P is 15%.

Detailed comments:

Abstract P9892 L13-14: That biochemical P mineralisation has not been included in other global models previously is not true. Check the literature.

P9893 L24: Phosphatase production by soil microbes is one means by which organic P can be mineralise. The literature contains a large number of papers on the role of organic acids in solubilising inorganic unavailable phosphorus, mycorrhizae, plant root phosphatase production, and the impact of elevated CO_2 concentrations on these

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processes. This paper ignores all of this literature.

P9894 L8-10: The wide variation here is due in part to differences in which pools are included in the global totals.

P9895 L18: Why is CASA followed by an apostrophe?

P9896 L21: What is the physiological basis of the 16 gN/gP threshold?

P9900 L20-25: The assertion that occluded P is not available is not true for disturbed ecosystems such as cropland where P is released from the occluded pool and is available for mineralisation. Also on long timescales (decades - centuries) P in the occluded pool is potentially available for plant uptake.

P9901 L9: CO_2 concentrations should have square brackets

P9903 L22 - P9904 L3: The authors state that "the above information is not sufficient to constrain most of the model parameters" and then justify not calibrating the model. This justification is not sufficient for this model to yield accurate estimates of pools and fluxes as stated above. As a result the model yields results that are not different to earlier studies based on simpler methods.

P9905 L2-4: The sentence on median values is not required.

P9906 L21-22: "quite sensitive": How sensitive? IN what way? What about disturbance by fire?

P9907 L1-4: to what depth are these soil C density estimates made?

P9908 L12-20: This comparison between Figure 6 and Cross and Schlesinger (1995) does not match at all. Why not put the soil orders in increasing stage of weathering as was done by Cross and Schlesinger? The P fractions for different soils do not match the results presented by Cross and Schlesinger.

P9908 L25-30: The authors state that the "fraction of P in soil organic matter is usually

less than one third of total P for most soils" when in fact the results of Cross and Schlesinger show that the fraction of organic P varies systematically from 5% to 35% depending on soil age.

P9911 L13-14: What soil depth?

P9911 L24-25: "Their soil N estimate is much lower..." How much?

P 9913 L9: weathering, fertiliser, and dust account for 56%, 42% and 2% of total P input: Where do these figures come from?

P9920 L 15-23: This lead phenology sub-model is based on deciduous forests. How does it apply to evergreen vegetation or rain fed seasonally green savannas?

P9921 L1-6: What determines these parameters?

Table 1: "80% of their maximal values" on what basis?

Figure 3 "medium" you mean median.

Figure 6: "with phosphatase production" means the same thing as "biochemical P mineralisation". Use one term.

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