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Discussions

Interactive comment on "Current state and future scenarios of the global agricultural nitrogen cycle" *by* B. L. Bodirsky et al.

Anonymous Referee #1

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Review to Bodirsky et al., BGD 9, 2755

Recommendation: Major revisions

General:

The paper "current state and future scenarios of the global agricultural nitrogen cycle" provides the results of a model chain covering land use, economic and emission aspects. Especially the link of demand-based modeling with production and production types has, to my knowledge, not been implemented in a similar way before and is worthwhile to be published. The paper is very well written and nicely explains the approach taken as well as the results obtained. It also discussed in some detail the reasons and even the implications of these results.

As with any modeling approach, validation of the results is key to providing confidence in the model results. This paper falls short in validation – it arrives at different conclusions to existing publications (Bouwman et al., 2009; 2011) and provides very limited explanations. Already using and re-evaluating the data presented (Table below) allows to further understand these differences. The authors will have access and the experience to go beyond that.

So, in a revised version, the authors should (*) be more explicit in what their scenario assumptions entail, (*) validate their model results against other existing scientific material and (*) be more wary regarding the uncertainties and correct some errors. Such an improved paper would be very helpful to understand (and intervene into) possible future developments, and I would like to see it published.

Detailed recommendations and comments:

The following table may help illustrate the modeling work (this is what is easily available from the material presented – obviously the authors are able to access much more of their own results)

NOTE: TABLE IS REPEATED IN CORRECT FORMATTING AT THE END OF THIS FILE

Table: Calculated ratios from Bodirsky et al., Tab. 1 and 2, help understand model assumptions

1995 2045 2095

A1 A2 B1 B2 A1 A2 B1 B2

Food demand p.P. (GJ/hd) 4.11 5.35 4.72 4.65 4.62 6.49 5.47 5.14 4.85

Animal product demand (GJ/hd) 0.657 1.284 0.803 1.023 0.969 1.427 0.930 0.668 0.825

manure per animal product (g N / MJ) 29.6 26.4 30.9 27.6 29.6 28.3 29.8 30.0 30.7

harvested N per food demand (g N / MJ) 2.74 4.91 3.61 4.70 3.90 5.56 4.30 4.03 4.30

fertilizer N per food demand (g N / MJ) 3.39 3.00 3.80 3.03 3.17 3.35 3.56 2.24 2.62

N2O per (residues & fertilizer & manure & BNF) 1.32% 1.42% 1.54% 1.39% 1.45% 1.25% 1.55% 1.33% 1.45%

manure/ mineral fertilizer 1.4 2.1 1.4 2.0 2.0 1.9 1.4 1.7 2.0

This table may indicate some key model characteristics

1) The strong increase in food demand satisfied even in scenarios with extreme population increase (A2) while at the same time a shift towards animal products is evident in all scenarios (slightly even in B1 for 2095). E.g. in A2, 2095, animal products available per person increase by almost 50%, food demand by 30%, while at the same time population increases by a factor of 3 compared to 1995

2) Manure per animal product is relatively stable – this seems to indicate that the N efficiencies do not change (note that there is huge differences in animal N efficiencies between e.g. beef and chicken, e.g. according to Smil 2001 – it is not clear which factors the authors assume but their losses in animal production seem to be quite stable anyway)

3) Harvested N per food demand increases in all scenarios – one would expect this as a consequence of increased lifestock production, but why does it affect also B1 in 2095 when the share of animal product in feed is down?

4) Fertilizer N per food demand slightly decreases – indicating increased NUE over time (consistent with Table 1)

5) Except for A2 (extreme population increase) the increased meat demand is met by efficient N recycling – manure application increases clearly more than mineral fertilizer application.

The above should not imply any valuation of the assumptions taken by the authors, but instead would allow to lay open these assumptions which so far are just hidden in the models. E.g., from the above it appears as, in A2 scenario, global crop production (at quite similar N efficiencies as in the 1990s) increases by a factor of 6 at least (see (1) above) – but it is not clear if this is from demand side economic modeling only, or whether any restrictions in land availability are considered when arriving at such a high value. After all, how is a sixfold increase in crop production feasible under conditions of regionalization and limited exchange of agro-scientific progress?

In addition to the papers mentioned (Bouwman et al.), the authors may wish to support their case by consulting other N scenarios like those presented by Erisman et al. (2008), Nat.Geosci 1:636-639 and van Vuuren et al (2011), Current Opinion in Environmental Sustainability 3:359–369. The former publication, also focusing on SRES scenarios, provides a much simpler estimate of future developments of N pollution – discrepancies or agreement with a more complex model as the one presented by Bodirsky et al. will however strongly enhance the explanatory value of the complex model. The latter (note also the references within) refers to quite detailed results – even with respect to N2O emissions, though limitations as discussed below apply here too – from the RCP exercise. While I understand the motivation of the authors to use in their work SRES instead of RCP, I wonder if it is a good idea to ignore RCP developments.

It seems perfectly acceptable to focus on one "problem area" associated with Nr, with N2O emissions, to provide insight into the model's abilities. In an uncertainty analysis (this is missing: the authors focus instead on the spread of the SRES scenarios as the overall uncertainty margin), also the uncertainty of applying the 2006 IPCC guidelines (the authors refer to it as Eggleston 2006) needs consideration. IPCC 2006 (in their volume 4, table 11.1) suggest an uncertainty range of one order of magnitude – clearly indicating the difficulties involved in quantifying N2O release. Thus the authors may wish to be a little bit more cautious in their wording (p. 2758, line 6 "our study . . . pioneers integrating . . . recent . . ."), and instead acknowledge that IPCC (2006) is a good technical guidance for operative country level assessments, but does not attempt to mimic the soil processes and is also quite uncertain. Also note that IPCC very generally assigns emissions to a certain source – subsequent emissions (even if "indirect" is included here) not necessarily cover the full life cycle.

Some details to be considered:

P. 2774 line 8: One striking methodological difference between the 1996 IPCC inventory guidelines (IPCC, 1997) and the 2006 guidelines is often overlooked. The 1996 guidelines operate in stages, such that NH3 release (default 10-20%) from soil N input is subtracted before N2O emissions are calculated (1.25% of N remaining in soil after subtracting gaseous emissions) while the 2006 guidelines base their estimate of 1% emissions on the total N-input, part of which evaporates as NH3. In consequence, direct emissions of both approaches are very similar – a difference is only to be seen on indirect emissions from leaching, much smaller than the authors claim.

Also note that SRES, to my knowledge, would not offer N2O emission data. If the authors know some subsequent work doing so, it would be worthwhile to compare their results.

P. 2773 line 10: direct air pollution from NO2 (not N2O) – but also note that soils typically emit NO which first needs to be oxidized in the atmosphere.

P. 2757 and elsewhere: For the time being, the classical ODS (namely, CFC's) covered in the Montreal protocol are the prime ozone depleters – see also Ravishankara et al. (2009)

P. 2760 and elsewhere: Smil (1999) provides excellent overview data – as a model input such information however needs some critical assessment (uncertainty analysis, at least quantitatively, to understand how much the model results are influenced).

P. 2778, appendix A states that "all BG crop residues remain on the field", but P. 2757, line 17 mentions a feature of the approach of this paper, that "most [other?] studies do

not consider . . . belowground residues as major Nr withdrawals . . ." – so what?

P. 2774, "Conclusions" actually are no conclusions (new thoughts and discussion, mainly) – may be considered "Outlook" instead. Also note that a statement such as "current scientific examination of Nr mitigation options is concentrated mainly on the farm level" (P. 2775) is ignoring work like the "European Nitrogen Assessment", as well as many global studies which the authors are very well aware of.

P. 2774, first paragraph: I would like to see results as indicated here shorthand in more details, by scenario! It does make a difference if N efficiencies are assumed to increase or decrease, how animal husbandry and crop production releases N (on the global level) in order to devise interventions.

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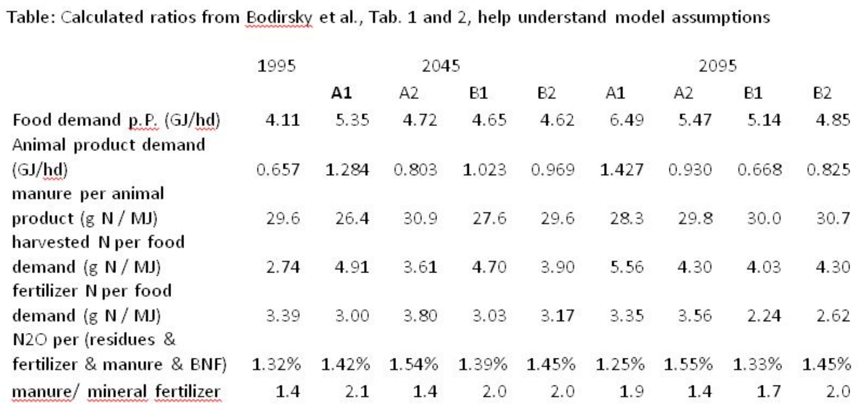

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Fig. 1.