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Interactive comment on "Eco-efficient agriculture for producing higher yields with lower greenhouse gas emissions: a case study of intensive irrigation wheat production in China" *by* Z. L. Cui et al.

Anonymous Referee #2

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General Comments

The manuscript presents information on multiple N rate vs. yield on-farm trials conducted between 2007 and 2008 in four Chinese Provinces and an extensive farmers' survey on N management practice in Northern China between 2004 to 2009 in irrigated winter wheat systems. The data from both initiatives is linked to a) N loss algorithms (published elsewhere) to generate N2O loss estimates, and b) emissions factors and other available data to estimate GHG (CO2, CH4 and N2O) emissions associated with fertilizer/pesticide production, transportation and application, to compare "whole life-cycle" estimates of GHG emissions per area and yield for conventional practice (based upon farmer surveys) and improved management practice (based on on-farm trials

informed by local expertise).

The manuscript is quite well written, although further attention to the fluency and precision of the language is needed in a number of places.

I have two main concerns (1 and 2 in specific comments) that are fundamental to the results and interpretation. Given these and the other issues noted below, I recommend that the manuscript be reconsidered after major revisions.

Specific Comments

1. My major concern relates to the use of linear plateau 'response' curves (indeed any 'response' curve) as the 'default' relationship between grain yield and GHG emissions. The term 'response' here is I think misleading. The inference is that GHG emissions are responding to yield directly (or vice versa as noted in the text [P 16890, L8-9]). Total GHG emissions (per area) are of course related to multiple factors, including N rate (that along with other management and environmental factors drives grain yield), as well as practices associated with production, transportation and application of fertilizers and pesticides.

I think best to replace 'response' with 'relationship' or similar.

However, my main concern is not terminology; rather it is the use of these models, and in particular the sole use of the linear-plateau model. Linear plateau and other models including, quadratic-plateau, quadratic, exponential, and square root are typically used to investigate relationships between (N) fertilizer rate and grain yield. Of course they can be used for other relationship parameters, but given that this approach is relatively new, I believe many more model types should have been investigated, beyond the three (quadratic-plateau, quadratic, linear-plateau) used here. Indeed, it is unclear why these 'standardized' rate vs yield models were chosen at all, as opposed to a more rigorous statistical approach to determine the best-fit response, irrespective of model type. The authors cite Cerrato and Blackmer (1990) as a rationale for using these model types,

but this study looked at various model options to investigate the economic optimum N application in corn in relation to yield, and not GHG emissions vs yield.

Linear plateau models, although frequently used to investigate yield responses and economic optimum N rates (EONRs), are biologically unrealistic: the discontinuity is abrupt at the transition to the plateau. For yield 'responses' a quadratic spherical model with plateau may be better as it has a smooth, rather than abrupt, transition to the plateau. Indeed, the quadratic plateau model best described the yield responses observed in the Cerrato and Blackmer (1990) study, and the linear plateau model 'underestimated' EONRs by between 23 and 48% when compared to it.

Due to this discontinuity, the linear plateau model is prone to over-estimation of yields and under-estimation of economic-optimum N rates (read GHG emissions per area on the x axis [Fig 2 and 3] in this study). The results, discussion and conclusions resulting from its use are therefore intrinsically biased in these directions by its sole use.

Please could the authors conduct a more robust statistical analysis (or better describe) using a greater variety of appropriate models, to investigate this fundamental relationship?

From P 16886, L 20-22: "In most cases, the linear with plateau model fit the data best, and was chosen for all of the sites". It is not clear (irrespective of the questionable validity of the model types used) why linear plateau was chosen. Presumably this was based on (adjusted) R2 values, i.e., the higher the R2 the better the supposed 'fit'.

Please could the authors clarify the 'tests' used to determine best fit?

Please note that R2 by itself is not considered a reliable criterion for selection of a model for identification of economic optimum rates of N fertilization (and in this case GHG emissions): it can result in a false sense of confidence concerning the ability of models to describe responses to N when too few treatments (four or less non-zero N rates) are used (Cerrato and Blackmer, 1990). Given the typical lack of significant

difference between fitted models with only R2 used to differentiate response, and concerns with too few treatments (N rates):

I would like to see a greater variety of models tested, or a weighted mean of these models used. For example, the IPNI Crop Nutrient Response Tool - http://nane.ipni.net/article/NANE-3068

2. Another concern is the association between the studies reported here and the N loss algorithms presented elsewhere (Cui et al. 2013, Environ. Sci. Technol. 2013, 47, 6015−6022). I understand that the algorithms from Cui et al. (2013) were derived from analysis of N loss data from a literature survey that focused on field measurements in the major Chinese winter wheat-producing regions, (Supplemental Tables S1 to S3). Some questions arising:

Were the literature data all from irrigated wheat studies? If not, these studies should be disaggregated into irrigated and rain-fed, so that the algorithms pertain only to the irrigated studies investigated here. N losses are known to be substantially different from irrigated and rain-fed studies. The authors should include text to clarify that this is the case.

Rather than using a single algorithm for each N loss parameter to calculate N2O emissions (direct and indirect) across all experiment studies and the geographic area covered by the sampling survey, could the authors separate the N loss data and generate more 'site-specific' algorithms to better match the province/county investigated here? The authors should at least provide a reason why this was not possible.

Technical Corrections

Title etc: "irrigated" rather than 'irrigation"?

P16993, L9/19: Fig 1 and Table S1 are detailed, but insufficiently so. Please could the authors add more information to include all N rates investigated at each of the 33 sites, not just median? Also please could the authors include all site coordinates,

precipitation (annual, growing season), MAT, soil texture (sand, silt, clay (%) if possible), and other relevant information so that readers can get a fuller picture of these sites beyond the general description in the Methods and Materials. This could be placed in an expanded Table S1.

P16883, L 14: Please correct “summer maize” to “wheat”, the crop of interest in this study!

This begs the question, if a large portion (70%) of the annual rainfall occurs during the wheat growing season, why is it irrigated, or has this section been copy-pasted incorrectly?

P16883, L 23 etc: With respect to the N fertilizer rates used at the sites, how was the median N fertilizer rate calculated – what does this value represent? Please see above, and include all N rates for each site in appropriate Table.

P16884, L 3: Could the authors expand on what “except for N fertilizer application” entails? What CP N management practices were altered to comply with the HY N practices? Was this just N rate, or was N fertilizer type, timing or placement altered?

P16884, L6: Please revise “the right combinations of planting data..” Here “right” is subjective (can be discussed as “better” or similar in Discussion). Should “data” be “date”?

P16884, L9: “late sowing and overused seeds” is again subjective. Maybe better to say “later sowing and used more seeds”.

P16884, L15: Could the authors please provide more information on irrigation volumes? A range would be appropriate with a relevant reference – this at least should be available. Please could the authors also clarify if three irrigations were used at each site as is stated typical.

P16884, L24-25: Could the authors please provide more information and a breakdown on the split of urea at the individual sites (e.g. 1/3 prior to planting, 2/3 at shooting

stage etc.). Variation in this has important implications for N loss estimates.

P16885, L4: Could the authors please provide a reference for this sampling technique.

P16885, L8: Revise to “Data required included. . .”

P16885, L10-14: Could the authors clarify whether the ‘outliers’ were removed from the dataset, or how they were ‘treated’ if not. Could the authors provide more information on what were the “normally expected” ranges of the entire dataset for yield and N application and therefore justify the exclusion/treatment?

P16885, L23: Table S2 not S1?

P16886, L4 etc: IPCC not ICPP?

P16886, L8-10: Equations 1 and 3 do not match with Cui et al. (2013b, Fig 1a and 1b). I assume because N surplus was used for N2O emissions and N leaching in Cui et al. 2013 and N rate was used here? This is important and needs clarification.

P16886, L24: Fig 3 is GHG emissions vs yield and not N rate vs yield.

P16886, L24: Please clarify whether grain yield is deemed agronomic or economic “maximum” or other. How is the maximum defined?

P16889, L5: Please revise “N application rate of 12Mg ha-1”!

P16889, L12-13: revise “have notable disconnected” to for example “have been notably disconnected” or similar.

P16889, L19: Again IPCC not ICPP. Also IPCC default Tier I values based on scientific literature meta-analyses. Please provide these as refs and not IPCC.

P16889, L22-24: Factors other than N input as determinants of N2O and GHG emissions have been known for decades (not just recently as the refs used suggest). Please revise and include earlier/more relevant references.

P16890, L 16: Please revise “the N cycle depends”. It does not depend upon manage-

ment. Maybe use “more efficient cycling of N” or similar?

P16891, L25: “Considerably” or “substantially” and not “drastically”.

P16892, L5-6: Not so. Yields can be optimized (high) with lower inputs and reduced N2O and GHG emissions. Agronomic maximums are typically only marginally higher than economically optimized yields. Wording here needs greater thought.

Tables: Table 1 and S1 – see P16993, L9/19 comment.

Figure 1: Consider ‘blow-up’ of region to better see individual sites on mainland. Please clarify what inset is showing – a non-mainland site? Needs re-drawing.

Figure 2: The individual graphs are indecipherable – far too small. Make larger or consider more than one site on a graph (eg per province). Irrespective, needs more thought. Needs re-drawing based on comments regarding unsuitability of linear-plateau models.

Figure 3: See Fig 2 and comments regarding unsuitability of linear plateau graphs. Is Fig 3 just a scatter plot of all points from the 33 sites? If so, consider removing Fig 2 completely, and replacing with analogous N rate vs yield comparison.

Figure 4: Again, this is very unclear - too much white space. Better use of axes needed.

Interactive comment on Biogeosciences Discuss., 10, 16879, 2013.