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Comment

Interactive comment on “Quantifying methane emissions from rice fields in Tai-Lake region, China by coupling detailed soil database with biogeochemical model” by L. Zhang et al.

L. Zhang et al.

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Dear reviewer,

Thanks for your kind and careful review of our manuscript. Two reviewers presented clear and proper comments, and these comments were very helpful in revising and improving this manuscript.

Our revisions have focused on the reviewers’s comments and improvements to the English throughout the manuscript. Detailed replies or explanations based on reviewer comments are given on the following pages (noted in blue).

We hope the revised manuscript meets the standards of Biogeosciences. Should you

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have any additional questions, kindly contact us at your convenience.

Thanks again and best regards,

Liming Zhang Institute of Soil Science, Chinese Academy of Sciences 71 East Beijing Road P.O.Box 821, Nanjing, 210008 P.R. China - Descriptions on responses to the comments presented by reviewer 1 1. In the introduction, very clear objectives for the paper are set: (1) estimate CH₄ emissions from rice paddy fields in Tai-Lake region; (2) understand the impact of crop system change, and of different agricultural management practices on CH₄ emissions; (3) improving the accuracy of the CH₄ estimates at the regional scale. From these objectives, only the first is fully met: Zhang et al. derived an estimate of arguably good quality for CH₄ fluxes from rice paddies in the study area. It becomes not really clear whether prior regional estimates of CH₄ fluxes from rice paddies in this region (only results from field measurements and simulations are reported) existed, to which the objective (3) refers. However, taking it literally, objectives (1) and (3) are not different. The context however suggests that objective (3) is referring to the comparison of the estimates obtained by using the different soil data bases, thus studying the effect of the scale of the input data. But even in this case the objective is only half-way fulfilled: even though a separate section is dedicated to this question, it is merely a description of the differences by county and does not try to understand the reason for direction and magnitude of the deviations and to bring this into relation with the issues discussed in the preceding sections. Regarding objective (2), assessment is restricted to nitrogen application rates, without discussing potential impact of other agricultural management practices. Answer: Good suggestions! Three objectives were not clear. So, we rewrote this part in order to correspond to the following results. The revised objectives are as follows: The goals of this study were to: (1) estimate the inter-annual variation in CH₄ emissions from rice paddy fields in the Tai-Lake region of China from 1982 to 2000; (2) display the CH₄ emissions patterns in different paddy soil subgroups as well as different soil sub-regions; and (3) compare CH₄ emissions modeled with polygon- and county-based databases.

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2. The overall structure of the paper is very clear and easy to follow; particularly sections 1 and 2 are well made. The language is generally good, even though I think that a check by a native speaker would be very beneficial (e.g. articles!). Title and abstract are appropriate. The artwork is of good quality and appropriate, but in most figure ranges are not indicated. Answer: The revised paper has been carefully revised by Dr. David C. Weindorf of the Louisiana State University AgCenter in Baton Rouge, Louisiana, USA. Thus, the English quality has been substantially improved compared to the previous version. Any final requests for adjustment of the English in the manuscript can be easily incorporated by Dr. Weindorf. All the figures were redrawn according to your comments.

3. In conclusion, the information on which this paper is based is of a very good quality, but I would strongly suggest that some additional efforts are made to transform it into an excellent paper for publication in Biogeosciences. Answer: Some additional information was added to the conclusion section.

4. The number of polygons is impressive. Nevertheless, there are 81 attribute fields, but DNDC recognises; usually only four parameters (texture, bulk density, SOC, pH) and with only; 13 weather stations in the region it becomes questionable whether the number of polygons which are effectively different with respect to DNDC simulations is much lower. This must be discussed in this section. Answer: You are correct. The DNDC model usually only needs four parameters (texture, bulk density, SOC, pH). The sentence has been rewritten in line 183 according to your comment. In terms of the number of weather stations, the Tai-Lake region is generally a plain with no significant altitude differences among counties. Also, this region covers only 36,500 km². Precipitation and temperature differences in different counties are quite small. Thus, we think 13 weather stations basically represent the climatic range in the region.

5. Section 3.2 The historical trend in the use of mineral fertilizer and manure nitrogen for rice production is brought into relation with the inter-annual changes in CH₄ fluxes

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- what I am missing, however, is (an attempt for) a quantification of this effect: which application rates were used in the study period? Are the authors able to separate the effect of mineral fertilizer vs. manure nitrogen? Is the magnitude of the effect (kg CH₄ ha⁻¹ y⁻¹ per additional kg of N application) reasonable? Answer: A quantitative effect of mineral fertilizer and manure nitrogen in CH₄ fluxes was added to the revised paper. Application rates of mineral fertilizer and manure nitrogen for rice production were used in the text. In fact, mineral fertilizer dominated CH₄ emissions in this region.

6. Section 3.2 There is no discussion of the evidence shown in Table 1 that plots with no-fertilizer application yielded higher CH₄ fluxes than those receiving only mineral fertilizer nitrogen? Can this be explained by the fertilizer type? Answer: As for the plots with no-fertilizer application yielding higher CH₄ fluxes than those receiving only mineral fertilizer nitrogen, the discussion of the evidence was added to in the revised paper.

7. Section 3.2 Also, and rather important: what about the water regime in the rice paddies? It is one of the most important factors determining CH₄ fluxes from rice paddies, but it has not been described in the Database development section. Next to nitrogen application rates: were all other management practices constant during 1982-2000? Answer: The water regime in the rice paddies was added to page 9 line 208 in the revised paper. Yes, all the management practices were basically similar in the Tai-Lake region from 1982-2000.

8. Section 3.3 This section describes the different average CH₄ fluxes simulated for the six soil subgroups occurring in the study area. Beyond a description of the results, comparisons are made in pairs; but the selection of these pairs seems arbitrary. For example, the high absolute CH₄ fluxes from the hydro-morphic soils is explained by the high organic matter and total nitrogen content by these soils. However - the main explanation for the high fluxes is the large surface area covered by this soil sub-group, while the average CH₄ flux rate is in the middle of

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all average flux rates simulated. As another example, perco-genic soils were described as of a near neutral pH and low clay content, leading to high CH₄ fluxes (117 kg C ha⁻¹ y⁻¹). According to the reasoning in the text, the submergic soils should have a higher CH₄ flux (near neutral pH and even lower clay content), but they have an average flux of 105 kg C ha⁻¹ y⁻¹ - why is the difference between this pair of soil groups not discussed? Answer: The section 3.3 was rewritten and more discussion was added to this part.

9. Section 3.3 Looking at Figure 5b, two main questions arise: (i) why are the emission rates of the gleyed soils ca. six times as high as the emissions rates of all the other soil sub-groups? And (ii) are the mean emission rates of these other soil-subgroups significantly different? What is the variability within each sub-group? Both questions are not addressed. For example, it would be very informative to show frequency distributions or similar to distil the important differences which then should be discussed in more depth. Answer: We explained the reasons of extremely high CH₄ emission rates for gleyed soils. Also, there was no significant difference between the mean emission rates of these other soil-subgroups. According to your comments, we rewrote Section 3.3.

10. Section 3.3 End of the introductions the 3 objectives is fair enough but I don't think the third objective is dealt with in this paper. I think it should be removed. Answer: We rewrote this part in order to correspond to the following results. The revised objectives are as follows: The goals of this study were to: (1) estimate the inter-annual variation in CH₄ emissions from rice paddy fields in the Tai-Lake region of China from 1982 to 2000; (2) display the CH₄ emissions patterns in different paddy soil subgroups as well as different soil sub-regions; and (3) compare CH₄ emissions modeled with polygon- and county-based databases.

11. Section 3.4 My suggestion for this - interesting - section is to remove the third paragraph (on CH₄ flux rates by county) and to merge it with the next section, which discusses simulation results at county-level anyway. Instead, it would be required to

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build the discussion of the spatial variability on the assessment of the impact of the soil sub-groups. Answer: The section of CH₄ flux rates by county has been merged to the next section, and average annual CH₄ fluxes in different counties was shown in Fig 7a. More discussion of the spatial variability on the assessment of the impact of soil sub-groups was added to section 3.4.

12. Section 3.5 This section is very important as “The study carried out [...] has provided the chance to test the uncertainty as there is detailed soil information available [...]”. However, the discussion scratches on the surface (giving the county-wide deviations obtained between the two simulations) rather than to try to understand reason for the differences in the deviations, for example by abstracting from the set of counties to some pattern leading to high/low and/or negative/positive deviations. It is further not understandable why the authors present only a mid-point for the simulations performed with the detailed soil data base, as a wealth of results should be available for each county. Comparisons are further made with the mean value obtained by the Most Sensitive Factor Method on the basis of the county-wide soil data base. However, this method generates a range of values which likely encompasses the true value not claiming this would be the mean value. The authors could take the opportunity and provide a more in-depth discussion of the effect of spatial heterogeneity/non-linearity of CH₄ fluxes from rice paddies. Even though there is no doubt that “utilizing more precise soil databases will substantially improve the accuracy” (page 4880, line 18), the comparison just showed that there are differences - and does not justify the conclusion. Answer: The section 3.5 was rewritten and more discussion was added in this part, per your suggestion.

13. Page 4869, line 3: “greenhouse effect and global warming are to important aspects” -redundant Answer: ‘greenhouse effect and global warming are to important aspects’ was deleted.

14. Page 4869, line 5: “Since the 1990” should read “Since 1990” (in the following no more article-errors are listed) Answer: All the

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Since the 1990s; were changed to 'Since 1990; throughout the paper.

15. Page 4869, line 6: CH₄ emission is; should read CH₄ emissions are; (see also line 11) Answer: the GHG emissions on the global scale; was changed to CH₄ emissions are; in the revised paper.

16. Page 4869, line 7: global; add: scale. Answer: the GHG emissions on the global; was changed to the greenhouse gas (GHG) emissions globally; in the revised paper.

17. Page 4869, line 14: to evaluate atmospheric on of agricultural production; please clarify Answer: to evaluate atmospheric on of agricultural production; was changed to to a comprehensive understanding of global GHG dynamics.; in the revised paper.

18. Page 4869, line 16: Recently, using models ... has become popular; what does popular; mean in the text? Are models becoming more reliable? Are they more frequently used? Answer: Recently, using models ... has become popular; was changed to Recently, scientists have applied modeling to estimate CH₄ emissions from cropping systems; in the revised paper.

19. Page 4869, line 18: process model, the latter gives; should read process models, the latter giving; Answer: According to the comments of reviewer 2, the introduction section was rewritten.

20. Page 4869, line 19: remove also; Answer: The word also; was deleted. 21. Page 4870, line 3: environmental impact ... on CH₄ emissions; CH₄ emissions are an environmental impact of rice cultivation. Answer: environmental impact ... on CH₄ emissions; was

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changed to “Using this model, environmental impacts on CH₄ emissions such as climate change, land-use change, and agricultural activities including alternative farming management practices, can be assessed in a comprehensive way” in the revised paper.

22. Page 4870, line 24ff: Please revise sentence “At the paddy field...”
Answer: According to the comments of reviewer 2, the introduction section was rewritten.

23. Page 4871, line 10: “rice-dominating” should read “rice-dominated”;
Answer “rice-dominating” was changed to “rice-dominated” in the revised paper.

24. Page 4871, line 22: “area of extensive rice cultivation” ... is really extensive meant rather than intensive??
Answer: “area of extensive rice cultivation” was changed to “area of intensive rice cultivation” in the revised paper.

25. Page 4872, line 17: full stop between “cycles” and “it”; - references should go after “cycles”;
Answer: The references were revised to the revised paper.

26. Page 4873, line 6: “has been modified”;. Better: “have been implemented”?
Answer: “has been modified” was changed to “have been implemented”;

27. Page 4873, line 10ff: Sentences “The soil Eh” until end of paragraph: remove redundancies.
Answer: “The soil Eh dynamics is one of the key processes controlling CH₄ and N₂O production/consumption in the paddy soils. CH₄ and N₂O are produced under certain Eh conditions (300 to 150mV for CH₄, and 200–500mV for N₂O), so variation in soil Eh determines the dominant greenhouse gas emitted from the paddy soil.” was deleted.

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28. Page 4874, line 3: "In the study"; should read "In this study"; Answer: "In the study"; was changed to "In this study"; in the revised paper.

29. Page 4876, line 16: "application of livestock"; livestock is not applied, but manure or manure nitrogen - please correct wherever it applies. Answer: "the magnitude of applied chemical fertilizers and livestock was greatly increased."; was changed to "After that (from 1986 to 1992), the application rates of fertilizer tended to increase. Chemical fertilizers increased from 260 kg N ha⁻¹ yr⁻¹ to 400 kg N ha⁻¹ yr⁻¹ and manure went up from 230 kg N ha⁻¹ yr⁻¹ to 280 kg N ha⁻¹ yr⁻¹."; on page 11 lines 268-271 in the revised paper.

30. Page 4876, line 19ff: Sentence "In addition, ..."; should be revised, probably splitting into two - it is not clear what the references refer to. Answer: This sentence was rewritten to the revised paper.

31. Page 4876, line 25: "The change could be related to the economic development in this region"; If there is not further explanation to this development (but the change in fertilizer use as a consequence) this sentence does not bring anything new to the text. Answer: "The change could be related to the economic development in this region"; was deleted.

32. Page 4877, line 16: "the average of clay content"; should read "the average clay content"; (occurs several times) Answer: All references to "the average of clay content"; were changed to "the clay content"; in the paper.

33. Page 4877, line 16: "the average of clay content had reached a level of ..."; implies that the average clay content of this soil sub-group is changing over time?? Answer: All references to "the average of clay content"; were changed to "the clay content"; in the paper. 34. Page 4877, line 18: Please revise sentence "The research indicated..."; Answer:

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The research indicated...; was changed to Some studies show that; in the revised paper.

35. Page 4878, line 6: Replace the sub-region; with this sub-region; (and look for similar errors) Answer: All references to the sub-region; were changed to this sub-region; in the text.

36. Page 4879, line 9: heterogeneity is soil properties; the; should read heterogeneity in soil properties, the; Answer: heterogeneity is soil properties; the; was changed to heterogeneity in soil properties, the; in the revised paper.

37. Page 4879, line 13-15: Too long as polygons are already introduced. Answer: As above described, a polygon-based database was built up based on a 1:50 000 soil map for the Tai-Lake region. There are 52,034 polygons in the polygon based database.; was changed to The polygon-based soil database contains 52,034 polygons..; in the revised paper.

38. Page 4879, line 22: In the cart; - figure? Answer: In the cart; was changed to In the figure; in the revised paper.

39. Page 4880, line 3-9: How a relative deviation is calculated must not be explained Answer: The relative deviation(y) of two methods was calculated by the following equation: $y = \frac{(x_s - x_0)}{x_0} \times 100$; where x_0 is the county level average of CH₄ emissions with the county-based database, and x_s is the CH₄ emissions produced with the polygon-based database. ; was deleted.

Descriptions on responses to the comments presented by reviewer 2 1. The paper titled ;Quantifying methane emissions from rice fields in Tai-Lake region, China by coupling detailed soil database with biogeochemical model; identifies the current need to accurately quantify the greenhouse gas emissions that contribute toward global warming. Methane (CH₄) is of particular importance when quantifying

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GHG emissions from rice paddies but it is impossible to use measurements alone to estimate the regional emissions. As suggested by the authors the use of verifiable process based models is one approach that can be used given that the model is calibrated and tested sufficiently against local measurement data and that the database used for input accurately represent the region. The paper in its present form has many serious issues before it can be accepted for publication. The scientific concept of comparing two databases for quantifying CH₄ emissions using the same process-based model is interesting however the sentence structure and formatting of the paper is so poor that it becomes exceedingly difficult to give a proper review. This paper requires a thorough editing by someone who has a good grasp of the English language. A comprehensive review at this time is not possible until this is done. Answer: The revised paper has been carefully revised by Dr. David C. Weindorf of the Louisiana State University AgCenter in Baton Rouge, Louisiana, USA. Thus, the English quality has been substantially improved compared to the previous version. Any final requests for adjustment of the English in the manuscript can be easily incorporated by Dr. Weindorf.

2. There is however a few general scientific comments that need to be addressed before resubmission of the paper. The authors state the county-based database is built from the polygonal database using the constraints in the DNDC model that require the maximum and minimum values for specific soil characteristics be inputted. Unfortunately, as evidenced by the results that show a -42.10% deviation between the total emissions between the two datasets, this technique might not be the most appropriate when scaling up a database to a larger land unit. Soil properties need to be scaled up by weighting their overall contribution to the larger soil unit, in this case the county level. Otherwise all soil types are given an equal contribution to the larger soil unit and inevitably skew the result for the larger soil unit. The authors demonstrate this issue when discussing the impacts that paddy soil subgroups have on CH₄ emissions. For example the submergenic soil group had a high emission rate of 105.41 kg C ha⁻¹ y⁻¹ and according to the methodology described it would have contributed equally to the country based emission rate regardless of the soil area it occupied. Considering

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the sample size used in the modeling exercise the result of the two databases should be on average very similar if the dominant soil is used to describe the county based soil unit. A better justification for using the smaller soil unit would be for implementing government policy that suggests management changes to reduce CH₄ emissions. A smaller soil unit would ensure that this policy could be implemented at the farm level successfully. This issue will need serious attention before the paper can be considered for publication. Answer: According to your suggestion, further explanation has been added to part 3.5.

3. There are also a number of other less serious scientific comments that need to be addressed. It would be beneficial if the authors could provide some measurement studies that are from the initial 1982-1986 period that support the output from the model simulations (Fig 4). Undoubtedly the emission rates would be much lower if fertilizer inputs were reduced, however, comparisons to measurements would still indicate if the magnitude is correct. Answer: First, thanks for your good advice. If we can find the measurement studies to compare the output from the model simulations, it would be beneficial for our paper. Unfortunately, field measurement of CH₄ emissions from the rice fields in the Tai-Lake region was not available before 1987 as far as we know.

4. Also, it is important that the number of significant digits for reporting results is uniform throughout the results. One loses some credibility by stating results to several decimal places. Answer: We revised the number of significant digits to keep the results uniform.

5. The equivalent FAO soil classification should be used when describing any soil group in the paper. Answer: The equivalent FAO soil classifications of soil group in this paper were added to the revised paper. 6. Parts of the introduction need to be written in a more concise manner. The authors should add discussion on how CH₄ emissions could be reduced from these soils. Answer: Per your suggestion, part of the introduction was rewritten and made more concise. Also, more discussion on how CH₄ emissions could be reduced from these soils was added.

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Interactive comment on Biogeosciences Discuss., 5, 4867, 2008.

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