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Biogeosciences Discussions

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.

Anonymous Referee #1

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General comments

The paper by Zhang et al. touches a very important topic: what is required to get a good estimate of regional greenhouse gas fluxes from agricultural soils? First, a well-validated biogeochemistry model (DNDC), knowledge about regional soil cultivation and farming practices, and third, critically, datasets with environmental model drivers of a high quality. Zhang et al. show that all conditions were fulfilled in their study. The Tai-Lake region appears to be an optimal choice for a case study for regional upscaling: the region is dominated by rice growing activities, statistical information required to parameterize the model are available from official sources, and a soil map

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of a very high resolution is at hand, derived from over 1000 soil profiles and yielding polygons of less than one square kilometre to cover the study region. The authors take the opportunity to perform a second set of simulations - using county wide soil data ("min" and "max" according to the "most sensitive factor" methodology developed for DNDC) to assess the gain in accuracy that rewards those who move from the default DNDC-set-up and invest in setting up detailed soil data bases. The study has been carried out carefully and bears the potential of a very interesting and highly relevant paper. However, in my opinion, the authors do not yet fully exploit the potential of the data generated with regard to the presentation and discussion of the results.

In the introduction, very clear objectives for the paper are set:

(1) estimate CH_4 emissions from rice paddy fields in Tai-Lake region;

(2) understand the impact of crop system change, and of different agricultural management practices on CH_4 emissions;

(3) improving the accuracy of the CH_4 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 CH_4 fluxes from rice paddies in the study area. It becomes not really clear whether prior regional estimates of CH_4 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

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practices.

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.

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.

Specific comments

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.

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 CH_4 fluxes - 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 CH_4 ha^{-1} y^{-1} per additional kg of N application) reasonable? There is no discussion of the evidence shown in Table 1 that plots with no-fertilizer application yielded higher CH_4 fluxes than those receiving only mineral fertilizer nitrogen? Can this be explained by the fertilizer type? Also, and rather important: what about the water regime in the rice paddies? It is one of the most important factors determining CH_4 fluxes from rice paddies, but

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it has not been described in the “Database development” section. Next to nitrogen application rates: were all other management practices constant during 1982-2000?

Section 3.3

This section describes the different average CH_4 fluxes simulated for the six soil sub-groups 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 CH_4 fluxes from the “hydromorphic” 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 CH_4 flux rate is in the middle of all average flux rates simulated. As another example, “percogenic soils” were described as of a near neutral pH and low clay content, leading to high CH_4 fluxes (117 kg C ha^{-1} y^{-1}). According to the reasoning in the text, the “submergic” soils should have a higher CH_4 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? 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.

Section 3.4

My suggestion for this - interesting - section is to remove the third paragraph (on CH_4 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 build the discussion of the spatial variability on the assessment of the impact of the soil sub-groups.

Section 3.5

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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 CH_4 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.

Technical corrections

Page 4869, line 3: “greenhouse effect and global warming are to important aspects” - redundant

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

Page 4869, line 6: “CH_4 emission is“ should read “CH_4 emissions are” (see also line 11)

Page 4869, line 7: “global” add: scale.

Page 4869, line 14: “to evaluate atmospheric on of agricultural production” please clarify

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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?

Page 4869, line 18: “process model, the latter gives” should read “process models, the latter giving”

Page 4869, line 19: remove “also”

Page 4870, line 3: “environmental impact ... on CH_4 emissions” CH_4 emissions are an environmental impact of rice cultivation.

Page 4870, line 24ff: Please revise sentence “At the paddy field....”

Page 4871, line 10: “rice-dominating” should read “rice-dominated”

Page 4871, line 22: “area of extensive rice cultivation” ... is really extensive meant rather than intensive??

Page 4872, line 17: full stop between “cycles” and “it” - references should go after “cycles”

Page 4873, line 6: “has been modified”. Better: ”have been implemented”?

Page 4873, line 10ff: Sentences “The soil Eh” until end of paragraph: remove redundancies

Page 4874, line 3: “In the study” should read “In this study”

Page 4876, line 16: “application of livestock” livestock is not applied, but manure or manure nitrogen - please correct wherever it applies.

Page 4876, line 19ff: Sentence “In addition, ...” should be revised, probably splitting into two - it is not clear what the references refer to.

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

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fertilizer use as a consequence) this sentence does not bring anything new to the text.

Page 4877, line 16: “the average of clay content” should read “the average clay content” (occurs several times)

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??

Page 4877, line 18: Please revise sentence “The research indicated...”

Page 4878, line 6: Replace “the sub-region” with “this sub-region” (and look for similar errors)

Page 4879, line 9: “heterogeneity is soil properties; the” should read” heterogeneity in soil properties, the”

Page 4879, line 13-15: Too long as polygons are already introduced.

Page 4879, line 22: “In the cart” - figure?

Page 4880, line 3-9: How a relative deviation is calculated must not be explained

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