

Interactive comment on “Modeling impacts of farming management practices on greenhouse gas emissions in the oasis region of China” by Y. Wang et al.

Y. Wang et al.

wangy1984@live.cn

Received and published: 19 May 2011

Author(s): Y. Wang et al. MS No.: bg-2011-57 MS Type: Research Article

Dear REVIEWER #1,

We highly appreciate you for reviewing this Discuss paper. We have seriously considered and addressed your valuable comments by point-to-point responses in the following context. We also have made revisions according to the comments in the revised manuscript. Please see the supplement in the submission system. Thank you.

Yours sincerely,

C1144

Ying Wang

Response to each reviewer's comments

To REVIEWER #1: General comments: The author states the objective of this research was to evaluate the influence of various climate scenarios and cultivation methods for an oasis region in China as well as to optimize management measures and to obtain theoretical support of sustainable development of oasis agriculture. The tool chosen for this analysis was the DNDC model which the author references, has been compared against many experimental sites in both China and globally. As with any process-based GHG model there is some uncertainty associated with its estimates and validation at one region should not suggest the model is appropriate for another. This leads into the major concern for this manuscript in that only 1 year of measured data is available for calibration and validation of DNDC at what the author suggests is a unique agricultural region. These management/climate scenario investigations would have more credibility if the author could better demonstrate that the DNDC model was validated against either multiple locations (with similar characteristics) or multiple years of data (at the same location). Additionally some comparison against auxiliary measurements such as soil water content and soil N would be informative to discern if the underlying drivers for GHG estimates were estimated appropriately as these drivers would be directly influenced by climate change. In this respect it becomes somewhat difficult to ascertain the value of such a modeling exercise. With moderate improvements to the manuscript it would be suitable for publication. Response: We fully agree that “these management/climate scenario investigations would have more credibility if the DNDC model was validated against either multiple locations (with similar characteristics) or multiple years of data (at the same location)”. However, our experiment and modeling only began and conducted from 2010, and subsequent observations are continuing right now, we will improve our experiment and modeling in future works. Yes, “some comparison against auxiliary measurements such as soil water content and soil N would be informative to discern if the underlying drivers for GHG estimates were es-

C1145

timated appropriately as these drivers would be directly influenced by climate change”, so we have added soil NO₃⁻ data and soil temperature data for each of the four sites, please see the following figure 1. The relationship between observed and modeled the nitrate (NO₃⁻) for the top 10 cm of the soil profile in summer maize fields, and the relative deviations were about 40%. The results further supported the acceptability of the DNDC model. Please see page 8 lines 14-16 in the revised manuscript. Please see the following figure (Fig. 2). The coefficient of determination (R²) between modeled and observed soil temperature is 0.77, and the relative deviation is about 8.45%.

Fig.1. Comparison of observed and modeled the nitrate (NO₃⁻) for the top 10 cm of the soil profile in summer maize fields

Fig. 2. Comparison of observed and simulated soil temperature

Specific comments Comment 1: Pg 3126 Line 14: What is the impact of removing all live vegetation from the treatment plots? Response: Usually, weeding in experiment plot is removed by hand. Here, we expressed it unclearly. The “live vegetation” in the sentence implied “the newly-germinated weeds”. Therefore, we revised the original sentence into “To avoid short-term fluctuation in the respiratory rate of soil caused by human disturbance, we inserted all of the steel collars into the soil, with a 5 cm wall exposed above the soil surface for installing the monitoring chamber, and cleared the litter and the newly-germinated weeds in the steel collars 24 h before measurement”. Please see page 5 lines 27-30 in the revised manuscript. Comment 2: Pg 3130 Line 17-19: How can you say the model results showed that autotrophic respiration of plant roots is the main source of soil CO₂ emissions? Response: The conclusion came from the modeled results in our study area. Please see the following Table 1. And some papers (Li et al, 2010; Moyes et al, 2010) also support this view. Table 1. Modeled soil CO₂ flux with autotrophic respiration by plant roots and heterotrophic respiration by soil microorganisms

Comment 3: Pg 3132 Line 12: What is a “recession curve”? Response: Recession

C1146

curve is a hydrograph showing the decrease of the runoff rate after rainfall or the melting of snow.

Comment 4: Table 1: It is not necessary to have the information presented in Table 1 included in a table. You can just include the relevant text in the manuscript. Response: We agree with you. We have revised it into the text: “The experiment included four treatments: (1) M (manures at 2000 kg C ha⁻¹); (2) N (nitrogen applied as urea at 300 kg N ha⁻¹); (3) MN (manures at 2000 kg C ha⁻¹ and nitrogen applied as urea at 300 kg N ha⁻¹); and (4) B (neither fertilizer nor manure).”

Comment 5: Table 2: Please put a footnote to describe the headers for the four different treatments. Response: We accepted. Please see page 22 lines 3-5 in revised manuscript.

Comment 6: Pg 3122, Line 3: An important method of investigating. . . Response: We accepted. Please see page 1 line 13 in revised manuscript.

Comment 7: Pg 3122, Line 8: Then sensitivity tests on the validated DNDC model were carried out on three variables: Response: We accepted. Please see page 1 line 18 in revised manuscript.

Comment 8: Pg 3122, Line 14: decreases with manure amendment... Response: We accepted. Please see page 1 line 24 in revised manuscript.

Comment 9: Pg 3123 Line 25-26: awkward sentence please reword. Response: We accepted. The sentence is revised to clarify that “An undesired consequence of the intensified application of fertilizer is of course the increase of the agricultural emission of the Greenhouse Gas (FAOSTAT, 2003).” Please see page 2 lines 21-24 in revised manuscript.

Comment 10: Pg 3123 Line 29: Studies have shown. . . Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

Comment 11: Pg 3124 Line 2: . . . N fertilizer applied are larger than they would be with-

C1147

out fertilizer. . . Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

Comment 12: Pg 3124 Line 7: However there are fewer studies on the. . . Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

Comment 13: Pg 3124 Line 20: Oases are dispersed like “islands”. Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

Comment 14: Pg 3125, Line 9: During the experiment, soil. . . Response: We accepted. Please see page 4 line 8 in revised manuscript.

Comment 15: Pg 3125, Line 24: Can you express in seed per hectare instead of g/m² Response: Yes, we accepted. It is revised to “50 kg ha⁻¹”. Please see page 4 line 21 in revised manuscript.

Comment 16: Pg 3126 Line 7: . . .fluxes were measured using the closed-chamber method. Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

Comment 17: Pg 3126 line 10: What is meant by the test? Response: In this sentence, “the test” means “the gas sampling”. The sentence “. . .in each plot 24 h before the test.” is revised to clarify that “. . .in each plot 24 h before the sampling.” Please see page 5 line 4 in revised manuscript..

Comment 18: Pg 3126 line 10: . . .was measured using a GC-ECD as detailed by. . . Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

Comment 19: Pg 3127 Line 17: . . .the seasonal change of determining factors for. . . Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

C1148

Comment 20: Pg 3127 Line 23 : . . .found that the model underestimated N₂O emissions. . . Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

Comment 21: Pg 3129 Line 2: . . .base fertilizer at planting Response: We accepted. However, the sentence has been deleted due to the revision of the related passage.

Comment 22: Pg 3130 Line 17: Reduce the number of significant numbers. Response: We accepted. It is revised to “0.47, 0.47, 0.47 and 0.51”. Please see page 8 line 29 in revised manuscript.

Comment 23: Pg 3131 Line 10-11: Avoid one sentence paragraphs. Response: We accepted. Please see page 9 lines 18-19 in revised manuscript.

Comment 24: Pg 3131 Line 26: There are too many significant digits (ex. 411.89 kg C /ha/yr) Response: We accepted. It is revised to “380 to 412 kg C ha⁻¹ yr⁻¹.” Please see page 10 lines 2.

Comment 25: Pg 3132 Line 7: Should read “wilting point” not “wilting coefficient” Response: We accepted. We changed “wilting coefficient” to “wilting point”. Please see page 10 line 10 in revised manuscript.

Comment 26: Pg 3132 Line 15: Avoid one sentence paragraphs. Response: We accepted. Please see page 10 lines 17-18 in revised manuscript.

Comment 27: Pg 3132 Line 21: What is “denitrogenation process”? Response: I made a mistake: Nit is the “denitrification process” rather than “denitrogenation process”. The denitrification process is the process by which a nitrate becomes molecular nitrogen, especially by the action of bacteria. Please see page 10 line 22 in revised manuscript.

Comment 28: Pg 3132 Line 22: . . .which in turns drives denitrification until. . . Response: We accepted. Please see page 10 line 23 in revised manuscript.

C1149

Comment 29: Pg 3132 Line 27: ...will be produced in fine-textured soil. Response accepted. Please see page 10 line 27 in revised manuscript.

Comment 30: Pg 3133 Line 11: ..was increased by 229%. Response accepted. Please see page 11 line 7 in revised manuscript.

Comment 31: Pg 3133 Line 15: Reduce the number of significant digits. Response accepted. It is revised to "215, 607 and 506". Please see page 11 lines 10-11 in revised manuscript.

Comment 32: Pg 3133 Line 21: ...and from 1113 to 1843 kg C ha⁻¹ yr⁻¹. Response accepted. Please see page 11 line 17 in revised manuscript.

Comment 33: Pg 3134 Line 2: Excess of N fertilizer could significantly. . . Response accepted. Please see page 11 lines 24-25 in revised manuscript.

Comment 34: Page 3134 Line 8: ...the rate of increase of the CO₂ flux decreased. Response accepted. Please see page 11 lines 29-30 in revised manuscript.

Comment 35: Page 3134 Line 26: ...were kept constant with the observed. . . Response accepted. Please see 12 lines 14-15 in revised manuscript.

Comment 36: Pg 3135 Line 6: Reduce the number of significant digits "42% or 31%" Response accepted. Please see page 12 line 21 in revised manuscript.

Comment 37: Pg 3135 Line 25: ...net GWP for the four scenarios. Response accepted. Please see page 13 line 10 in revised manuscript.

Comment 38: Pg 3137 Line 1: The authors acknowledge the financial support provided by. . . Response accepted. Please see page 14 line 5 in revised manuscript.

Comment 39: An additional suggestion to improve the overall writing style is try not to always write in the style "The results show that" or "Table 2 shows. . .". The manuscript will read better if the results are just referred to instead of being used as the lead in for the discussion. Response accepted. The text is revised as directed.

C1150

Reference Li, J. M., Ding, W. X., and Cai, Z. C.: Effects of nitrogen fertilization on soil respiration during maize growth season, *Chinese Journal of Applied Ecology*, 21 (8), 2025-2030, 2010. Moyes, A. B., Gaines, S. J., Siegwolf, R. T. W., and Bowling, D. R.: Diffusive fractionation complicates isotopic partitioning of autotrophic and heterotrophic sources of soil respiration, *Plant, Cell and Environment*, 33, 1804-1819, 2010.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/8/C1144/2011/bgd-8-C1144-2011-supplement.pdf>

Interactive comment on *Biogeosciences Discuss.*, 8, 3121, 2011.

C1151

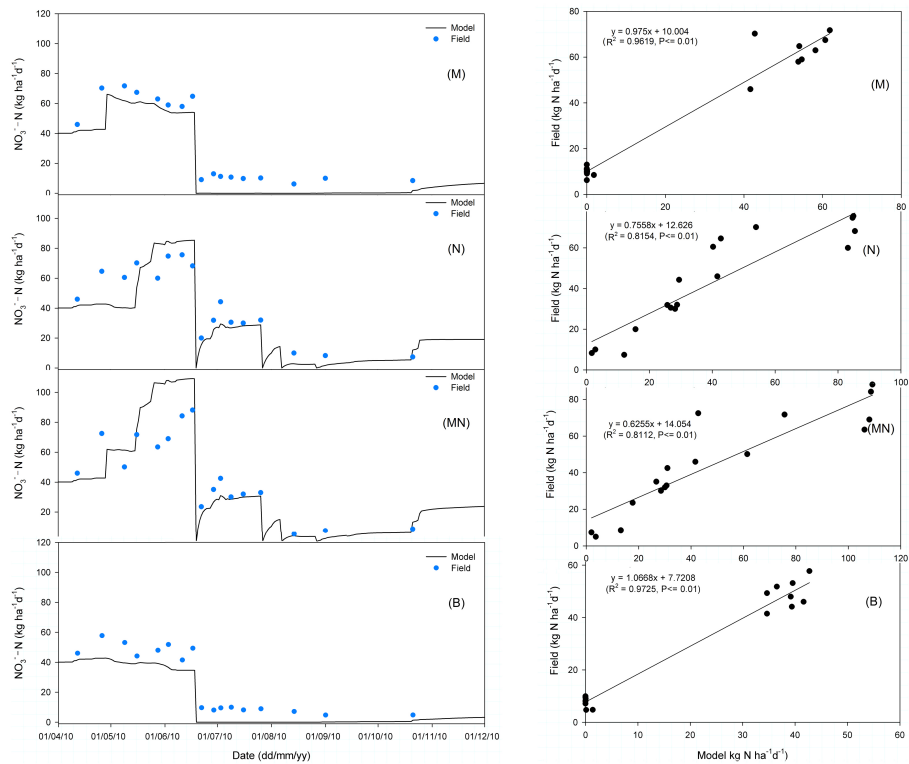


Fig. 1. Fig.1. Comparison of observed and modeled the nitrate (NO₃⁻) for the top 10 cm of the soil profile in summer maize fields

C1152

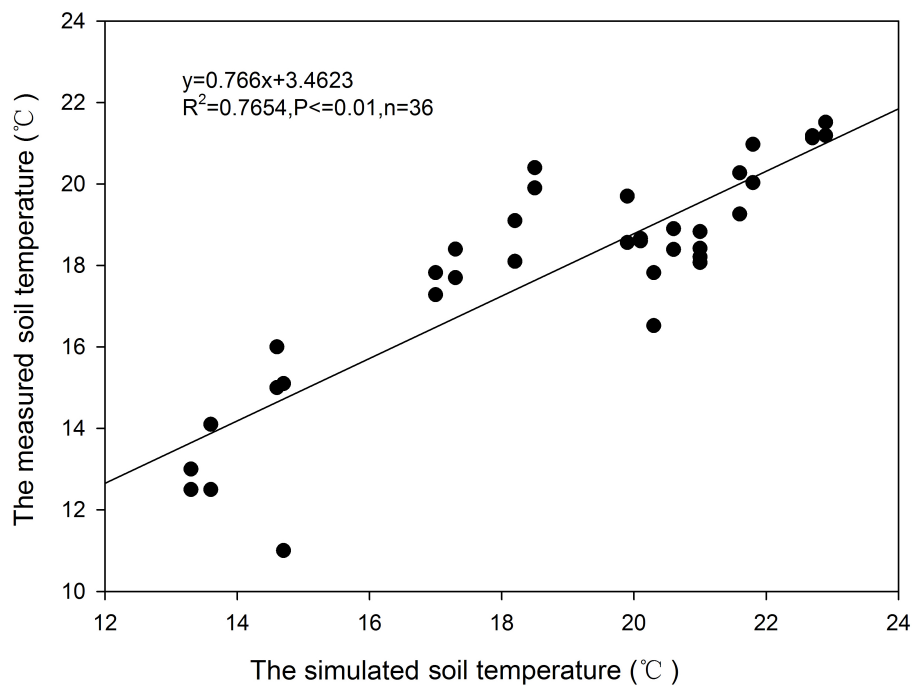


Fig. 2. Fig. 2. Comparison of observed and simulated soil temperature

C1153

Table 1. Modeled soil CO₂ flux with autotrophic respiration by plant roots and heterotrophic respiration by soil microorganisms

Treatment	Root-respiration kg C ha ⁻¹ y ⁻¹	Soil-heterotrophic-respiration kg C ha ⁻¹ y ⁻¹
M	3416	1524
N	4654	939
MN	4635	1751
B	595	359

M was a traditional agricultural fertilization mode when there was no chemical fertilizer provided; N was a fertilization mode with high input and intensive agriculture; MN was a fertilization mode recommended by local experts; B was a controlled trial.

Fig. 3. Table 1. Modeled soil CO₂ flux with autotrophic respiration by plant roots and heterotrophic respiration by soil microorganisms