

Interactive comment on “Assessing biogeochemical effects and best management practice for a wheat–maize cropping system using the DNDC model” by F. Cui et al.

F. Cui et al.

xunhua.zheng@post.iap.ac.cn

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Dear Reviewer: Thank you for the truly helpful comments on the manuscript. We have further revised the manuscript following your suggestions and comments. Our detailed responses are attached below.

Anonymous Referee #1 This manuscript is an important contribution to the validation of the DNDC model. I however still find some issues not very clear (and some I raised before).

Materials and methods 1- I am not clear what the fertiliser applied was, I assume it was inorganic fertiliser. I presume this is in the supplement but I can not access it. This

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should be stated in the methods section.

The details of fertilizer type and application rate were displayed in Table 1S in the supplement. Since there are many treatments and each treatment may have different fertilizer type and application rate, it will make the text too long if I put all these information in the text.

2- I raised this point in my previous revision: the model version used is quoted as DNDC95. Did you use the web based version or a modified version of that one? This is very important as there are several versions of the model and there is extensive discussion at the moment on how to deal with these multiple versions. If the web version has been modified this needs to be stated in the paper. If modified, it needs to be said what were the modifications and is this accessible to others?

The model (version 95) was downloaded from web (<http://www.globaldndc.net/>) in April 2012. The modifications were mainly on the user-defined input parameters (e.g., the crop parameters, see P8569, L11–P8570, L6) and the output of $i\dot{A}\dot{D}SOC$ and soil moisture (see P8586, L4–L13). The user-defined input parameters are usually site-specific, but the modification of the output of $i\dot{A}\dot{D}SOC$ and soil moisture are accessible to others (this was added in the revised manuscript; see page 32, line 17–18 in the revised manuscript).

3- Also raised previously: The authors state: “To simulate the effects of nitrification inhibitors, the efficiency and the effective duration was set as 0.6 and 30 days for dicyandiamide, and 0.9 and 30 days for 3, 4-dimethyl pyrazole phosphate” It is not explained how these assumptions were made, is there a publication to support these values? Also, how was this taken account of in the model?

To simulate the effect of nitrification inhibitors on nitrification rate and thus N_2O and NO emissions, the DNDC model assumes nitrification inhibitors reduces the nitrification rate by a constant factor (efficiency) for a fixed period of time (effective duration). In actual, however, situations may be more complicated. The effects of a nitrification

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inhibitor may vary among different soils and influencing factors (e.g., temperature, soil pH buffering capacity and nitrification rates). This makes it impossible to get a universal value of the efficiency or the effective duration for a nitrification inhibitor applicable to all soils. So in this paper, the values of the efficiency and effective duration for di-cyandiamide and 3, 4-dimethyl pyrazole phosphate were finally set according to model validation based on measured data of N₂O and NO emissions (see page 12, line 8-9 in the revised manuscript).

Discussion: Observed data are unpublished in several of the figures, also quoted in the text in several occasions. I disagree with the use of unpublished data.

Regarding unpublished observational data, in fact, only a small portion was involved in each of the several figures. The observed, systematic dataset we used in this paper were very large. Manuscripts using the unpublished data are still in preparation, and will most likely be published in the near future. To assure the reliability, we introduced the method citation for those unpublished data (See P8569, L25-27, P8570, L14-17) in the text, and we also added the method citation to the footnotes of the figures in the revised manuscript (see page 54, line 8-10; page 55, line 4-5 and page 58, line 8-9 in the revised manuscript).

Figure 7 shows data modelled using 20 yr means. Did the authors average the rainfall too? This is questionable as rainfall is a very important parameter for N₂O emissions, a rainfall mean of a long period does not mean anything. Authors should use different rainfall datasets perhaps generated randomly or using extreme weather years.

Figure 7 shows average results of the 20 simulated years, and the meteorological data for these 20 years were actual daily meteorological data at Yuncheng station from 1991-2010 (<ftp://ftp.ncdc.noaa.gov/pub/data/gsod/>), please see P8573, L2-4 in the text. As this was supposed to represent the actual rainfall situation, we did not consider using rainfall dataset generated randomly or using extreme weather years (I added this sentence at P8573, L1 before the sentence “the simulations were driven by actual

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daily meteorological data. . . .” in the revised manuscript (see page 16, line 2-4 in the revised manuscript).

Other corrections: P8569, Para 15: “and they are curial for simulating crop growth” replace curial for crucial.

Revised.

P8571, Para 20: “with the former be dominant in the past”, replace be for being.

Revised.

P8581, Para 5: “simulated NH₃ volatilization” I presume it meant “stimulated”?

Revised.

P8581, Para 5: “According the results given” According to the results: : :

Revised.

P8586, Para 5: “both the unfroze water”, unfrozen?

Revised.

P8587, Para 5: “to be popularized right away”, to implement.. “Use of nitrification inhibitor”, inhibitors.

Revised.

Figure 7 caption: “for the sceanrios with”, scenarios.

Revised.

Some other changes by the authors for further improving the manuscript, please see page18, line 4-16; page 28, line 13; page 28, line 17-21 and page 52, Table 4 in the revised manuscript.