Dear Dr. Wrage.

We very welcome your helpful and constructive comments that helped to improve our review paper. Please find below our response to your comments, but note that minor corrections were done without mentioning here. We are confident that we could successfully address all your comments. (Reviewer comments are presented in *italics*)

> *First of all, I am missing a clearer description of the two mechanisms of DNRA that are mentioned (e.g. page 1171, l. 18-22). A small figure showing the pathways and involved enzymes would help to make the subject of this review clearer.*

First of all it should be noted that the pathway does not differ as such between the two mechanisms, but only the involved enzymes. The reasons why we did not show a figure with the mechanisms are that (1) our review did not focus on the enzymatology and biochemistry of the DNRA and (2) as we consider the two mechanisms together due to their similar ecological importance (as stated). If this is agreeable for you, we would prefer to not show the two mechanisms with the responsible enzymes, as from our point of view this is out of the focus of our paper.

> *Secondly, it would be nice for the reader to get a better idea of the important influencing factors. If a meta-analysis is not possible, a table summarizing the results discussed in Part 2 and 5 could grant a better overview of conditions and fluxes. I could imagine this also to be an extension of Table 1, with additional information on conditions and identified main factors. The outcome of this should then also be included in the abstract, which so far mentions redox status and the C/NO3- ratio as important, without saying in which direction they act.*

Please see also our reply to reviewer 2 in respect to your comment. To extend Table 1 by adding all the asked information would make the table rather large and partially “unreadable”. For that reason we will prepare a table with the information as online Supplementary Material, but keep the Table 1 in its current form.

> *p. 1171, l. 23 – p. 1172, l. 1: I was at first a bit confused by this information as it seemed contradictory that the numbers for potential free energy of total denitrification*

are more negative than for DNRA, but you state later that the potential free energy is larger for DNRA than denitrification – per NO3-. Maybe you can add above (1171, l. 23) whether this is per mole of NO3- or N_2

We do not see the contradictory in our argumentation. Usually the free energy of a reaction is calculated on the basis of the electron donor. We first present, hence, the free energy on a mole glucose basis, which is -2669 kJ mol^{-1} glucose for denitrification and -1796 kJ mol^{-1} glucose for DNRA. The more negative value for denitrification means that the energy release is higher. However, when calculated on the basis of the electron acceptor (NO_3^-) the free energies are -560 kJ mol^{-1} nitrate for denitrification and -600 kJ mol^{-1} nitrate for DNRA, indicating the higher (more negative) free energy for DNRA. Your last comment seems irrelevant, as the free energy was calculated per mole glucose (as stated in the paper).

p. 1184, l. 1-3: Where is this shown in Table 1, especially the link with the application of the 15N tracing model?

This is actually not directly shown in Table 1, but is indicated by the findings presented in Table 1. We will, hence, rephrase the sentence to clarify:

"Application of the ^{15}N tracing model developed by Müller at al. (2004; 2007) showed that DNRA is likely to occur in numerous ecosystems and was sometimes the dominant NO_3^- consumption process, as can be seen from the results of the studies summarised in Table 1."

Part 6: Maybe you could in this part also discuss aspects like the influence of global changes on the C/NO3- ratio or drier soil conditions, and potential effects on DNRA

You point to an important further aspect that the global change effects on DNRA will not only be a direct effect on microbial community but also indirectly via changes in regulatory factors. It is, however, up to now very uncertain how the soil factors you mentioned (C content and NO_3^- concentrations) will respond to the global change. For example, a recent meta-analysis, comparing results from four earlier meta-analyses (Hungate et al. 2009), came to the conclusion that the effect on soil C content could not be detected, but most likely due to low statistical power. Given these uncertainties it will be rather speculative how the C/NO_3^- ratio may change in the future. Nevertheless, as there is some evidence that the NO_3^- concentrations may decrease under elevated CO_2 (Barnard et al. 2005), which was discussed to limit denitrification, this may point an increased C/NO_3^- under elevated CO_2 (which in turn should favour DNRA, as already observed in two FACE experiments). However, given the rather speculative nature of this discussion, we would prefer to not include this in the paper.

The effects of drier soil conditions could, on the other hand, limit DNRA. This is supported by the fact that many studies that found DNRA activity in soil were in humid ecosystems, and none in a dry soil. This point will be addressed further by including climatic factors (mean temperature and precipitation; see also comment by reviewer 2) as predictive variables when conducting the multiple regression analysis (if a sufficient number of studies reported these factors).

Cited References:

Barnard R, Leadley P W and Hungate B A 2005 Global change, nitrification, and denitrification: a review. Global Biogeochemical Cycles 19, GB1007, doi:1010.1029/2004GB002282.

Hungate B A, van Groenigen K J, Six J, Jastrow J D, Luo Y, de Graaff M A, van Kessel C and Osenberg C W 2009 Assessing the effect of elevated CO2 on soil C: a comparison of four meta-analyses. Global Change Biology 15, 2020-2034.