Dear Dr. Mosier.

We are very grateful for your many 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 as suggested without mentioning here. We are confident that we could successfully address all your comments. (Reviewer comments are presented in *italics*)

> *I think that the authors could use their extensive knowledge of soils and soil microbial processes to better interpret the information presented, and provide more definitive conclusions as to where and when DNRA is important in terrestrial ecosystems.*

To address this point we will include, before the conclusion section, an additional section on "Importance and regulation of DNRA activity". In this section we provide some more statistical relations between DNRA and soil properties (as requested by Reviewer 2 and 3). Based on present knowledge the potential for DNRA seems to be present in almost all terrestrial ecosystems, but the process itself is of importance mostly in relatively undisturbed forest and grassland ecosystems (see Table 1 in the paper). As these are known to often exhibit also a high heterotrophic nitrification activity, we can hypothesise that DNRA rate is high in soil showing generally high heterotrophic N cycle activity, which would also fit with the proposed link between heterotrophic nitrification and DNRA (see Fig. 1a). Moreover, based on multiple regression analysis, we show that DNRA is best predicted by a combination of high soil N and C content.

In addition, we add a new paragraph where we discuss some methodological aspects since no generally agreed methodology is available to quantify DNRA. Moreover, some studies have used inappropriate experimental set-ups for quantifying DNRA, which will be highlighted.

> *Could the Abstract include examples of where DNRA is known to be important?*

Based on the discussion in the newly added section on "Importance and regulation of DNRA activity" (see above) we provide information (1) in which ecosystems DNRA is important and (2) which soil factors best predict DNRA in terrestrial ecosystems.

> *P. 1183, last 4 lines: Why was the van Dam (1966) study introduced here and not discussed further?*

The methodology presented by Tietema and van Dam (1996) for quantifying gross N transformations, including DNRA, was to our knowledge only applied in one subsequent study (Verburg et al. 1999). In this study, however, DNRA rates were not quantified but only simulated, as no ^{15}N tracing of $^{15}N\text{-}NO_3^-$ into $^{15}N\text{-}NH_4^+$ was conducted. Due to the lack of more studies applying the methodology, no further discussion could be made. We will add a statement on the limited number of studies applying this methodology. It should be further noted that we discussed the "scientific" findings of the Tietema and van Dam (1996) later on in our paper in the section "Effect of global change".

P. 1187, line 12. The Schimel and Bennett (2004) reference is not noted in the Fig. 1 caption. How does the Schimel and Bennett N mineralization paradigm fit into Fig. 1?

We have revised the Fig. 1 in order to explicitly incorporate the paradigm by Schimel and Bennett (2004), i.e. to divide the mineralisation pathway into the depolymerisation and ammonification process. The reference is given in the caption.

Cited references:

Schimel J P and Bennett J 2004 Nitrogen mineralization: challenges of a changing paradigm. Ecology 85, 591-602.

Tietema A and Van Dam D 1996 Calculating microbial carbon and nitrogen transformations in acid forest litter with ^{15}N enrichment and dynamic simulation modelling. Soil Biology & Biochemistry 28, 953-965.

Verburg P S J, Van Dam D, Hefting M M and Tietema A 1999 Microbial transformations of C and N in a boreal forest floor as affected by temperature. Plant and Soil 208, 187-197.