The authors would like to thank the two reviewers for their time and effort on the constructive criticisms and comments to the submitted manuscript. Both referees raised several general comments to the submitted manuscript regarding the site selection and experimental design. We have addressed the comments in the manuscript with the following points and changes to the text (changes to text within the manuscript are italicized).

Methods:

A major limitation of the study is the fact that only two sites were measured. It was the authors' belief that by comparing the two endpoints on the connectivity spectrum, we would be able to address changes of connectivity. The appearance of macrophytes and more organic sediments is not inconsequent, but rather further demonstrates the changes which occur with restoration. However, it is not possible to separate these successional events from the changing microbial rates. As the ecosystem changes with respect to connectivity, differences will occur at multiple trophic levels. Following the conclusions of Welti et al 2012a, the connectivity changes the properties of the water bodies, which has further implications for the sediment biogeochemistry.

We have updated the text of the manuscript to better explain our reasoning:

Based on the difference in hydrological exchange condition, both floodplains developed differently, which has led to differences in sediment structure (Welti et al. 2012a).

In these previous studies, the mean sediment respiration and potential denitrification measured at the selected sites over two years were very similar to the mean of the entire floodplain for the same time period (Disconnected site DEA mean 55 ± 53.23 mg N m^{-2} h^{-1} (N=55), Lobau mean of 48.70 mg N–N_2O m^{-2} h^{-1} (N=204); Restored site mean 2.08 ± 0.85 (N=72), Orth mean of 6.23 ± 7.79 mg N–N_2O m^{-2} h^{-1} (N=120)) (Welti et al 2012a). Therefore, despite both systems being heterogeneous, the chosen study sites can be considered representative for the majority of the landscape in their respective floodplain.

All cores were sampled in non-vegetated areas.

While measuring only two sites within these dynamic and variable floodplains is a limitation of this study; we have nevertheless demonstrated that floodplain reconnection can have a significant impact on sediment biogeochemical processes. Restoration, however, does not just solely impact the biogeochemical processes, but rather changes several properties of floodplain waterbodies (Welti et al 2012a).

Experimental design:

The sediments in the mesocosms were under constant anoxia after 3cm as determined by a micro-oxygen probe. However, the probe broke during the experiment and the data was not used in the manuscript. Great care was taken during sampling and transport so that the sediments were disturbed as little as possible. It is the authors' assumption that by using a larger core, we can mimic *in situ* conditions by minimizing the overall impact of disturbance.

The water in the column was not bubbling in the cores, only upon leaving the mesocosm and entering the reservoir. This did not increase the dissolved oxygen in the water column over time.

By connecting the three cores to one reservoir, the goal was to limit any changes in water column chemistry and expose the sediments to the same exact treatment over time. Since the cores were taken separately, we argue that they are true replicates of each site.

This issue was addressed in the revised manuscript with the following changes:

Triplicate cores were connected to a 40L reservoir providing the experimental treatments. Water was pumped via a peristaltic pump from the reservoir to the individual cores at a rate of 5 L hr^{-1}*, creating a residence time of approximately 2 hours in each mesocosm. Water was recycled through the reservoirs for the entirety of experiment. The residence time was selected to maintain slow, but well-mixed conditions so that dissolved oxygen concentrations did not decrease over the five days. Mixing tests prior to the start of the experiments showed complete and non-turbulent mixing within the mesocosms.*

N_2O/CO_2 ratio and include more detail.

Due to the interference with mz 44, N2O and CO2 were quantified for each sediment core with a GC (specified in the manuscript) prior to the tracer experiments. This ratio was used to correct for m/z 44 in the described manuscript. This has been revised in the manuscript as follows:

Prior to the start of the mesocosm incubations, the individual N_2O concentrations were estimated from each core from the total N_2O and CO_2 concentrations measured at m/z 44, 45, 46 then corrected after determination of CO_2 concentration by GC analyses (CO_2 as CH_4 concentrations using AGILENT 6890N, Santa Clara, U.S.A., connected to an automatic sample-injection system DANI HSS 86.50, Headspace-sampler, Cologno Monzese, Italy).

Nitrification:

Due to the experimental design, it was not possible to calculate nitrification in any of the experimental treatments, but was calculated based on 15N-NO3 dilution for the control treatments. It was very low (~0.01umol N/h/m2) and therefore not considered as a significant part of the N_2O flux. Due to the fact that the experimental design did not allow comparison with the other treatments, it was not included in the paper, but was addressed in the discussion.

Due to the addition of nitrate in the mesocosm treatments, it was not possible to calculate nitrification rates for these sediments. However, as the concentration of ammonia present in the water columns was considerably less than that of nitrate (*Disconnected site approx. 40:1, Restored site approx. 100:1), water column nitrification may not be an important source of nitrate or N_2O in these ecosystems. In future studies, the coupled use of $^{15}N\text{-}NH_4$ and $^{15}N\text{-}NO_3$ would provide useful insight on this pathway.*

Mass balance

Referee 2 commented on the validity of the mass balance in the conclusion of the manuscript.

This is a valid point and a discussion was added to the text:

Our mass balance approach may over-estimate assimilation into biomass by attributing lost N to this pool, but it nevertheless indicates that a large portion of N can be used up by autotrophs.

Specific changes:

Referee 1:

Page 4135 Line 19: Concentrations of what?
Changed: nitrate concentrations

P 4137 L14 and 21: The hypothesis in these lines almost reads the same. Please remove duplication.
Sentences combined in L110

P4141 L9: delete 'through the tube'
Deleted

P4142 L5: It not true that 98% of the N2 and N2O would be in the headspace. Given the headspace and water volumes in the vials a substantial amount of gas would remain in the water. I guess that what is meant here is that 98% of the equilibrium concentration was reached. Please rephrase. Also, were data corrected for water-gas partitioning?
Text changed. Data were corrected for water-gas partitioning and is now included in text.

P4144 L7: Do these masses present production rates or just concentrations of N2O and N2? Please clarify.
Concentrations, changed in text

P4148 L10 and further: How can an increase in NO3 concentration from 3.84 to 34.7 microM due to 15N-NO3 label addition lead to only an 22at% labeling in the NO3 pool. Should be something like 90at%. Please explain (exchange with sediment?).
This is an embarrassing transcription error. All values have been checked and updated. The at% of each treatment was calculated as such:
$AT\% = (Vol_{Bkg} * 0.366 * Conc_{Bkg} + Vol_{Trc} * 98 * Conc_{Trc}) / (Vol_{Bkg} * Conc_{Bkg} + Vol_{Trc} * Conc_{Trc})$
Where Vol_{bkg} *and* Vol_{trc} *are the volumes of the in situ water (81L) and the tracer added (9L)* $Conc_{Bkg}$ *and* $Conc_{Trc}$ *are the nitrate concentration of the background and tracer*

All further calculations were double checked and found to be correct.

The low AT% calculated for the RIVER treatments is due to the high background concentration of nitrate in the Danube River.

P4154 L3: Please specify what is meant by decoupling between the water column and the anoxic sediment
This was written as link to the following paragraph and is now re-written in the text (P 20 LN536). As presented in the Dodds et al 2000 paper, water column and phytobenthic uptake may prevent N from reaching the sediment. We propose that this could be the case in our study site as well.

P4154 L3: How can NO3 assimilation by algae lead to the release of NH4. There is probably some leakage from the algal cell from NH4 produced during assimilatory nitrate reduction, but this is not assimilation and other processes like DNRA seem more likely explanations?
We measured an increase of $^{15}N\text{-}NH_4$ in the water column following the initial nitrate additions along with lower NH_4 concentrations relative to NO_3. We believe that nitrate assimilation is indeed a pathway used by biomass in the water column when NO_3 is in excess relative to NH_4, which could result in a release of N-containing organic molecules and NH_4 over time. Due to the filter culling cells at 10μm, we believe this may be enhanced by the experimental design. DNRA rates in the water column were calculated, but were negligible. We have changed the text in the manuscript to reflect this comment.

P19 LN 535 We found a delayed increase of $^{15}N\text{-}NH_4^+$ in the water column, following the $^{15}N\text{-}NO_3$ addition, which suggests biomass assimilation of NO_3^- and subsequent release of organic molecules and ammonia.

P4155 L11: mention that assimilation is here by algae
Changed in text.

P4155 L11:
C1541: This explanation for the higher denitrification rates in the restored system seems very vague to me. How about differences in organic matter content and carbon mineralization rates between the two sediments that were selected for this study. The NO3 addition experiment didn't detect any difference in denitrification rates even though both amount and frequency of the NO3 additions were varied.
As pointed out by Rev. 1 no significant change was detected between the treatments for each site. While the potential enzyme is much higher in the Lobau site (Welti et al. 2012a), the actually realized ones are lower compared to the restored site and not controlled by NO_3 inputs. One possible factor has been tested in Exp.2 and showed that the DOM in the water has some importance. A second factor could be the microbial community structure as mentioned in the manuscript. Other relevant factors could be resource competition with other organisms as shown in the mass balance (Fig. 5). Other

processes such as the ones suggested by the reviewer have not been measured in our study, but we have found some evidences for high microbial rates in the sediments. The higher BSP rates in sediment and water column measured at the Lobau site suggests higher mineralization rates in the Lobau sediments and possible competition for labile carbon. Furthermore, as shown in Exp 2, under high NO_3 concentrations a change was observed due to changes in the DOM compositions, not the DOC concentration.

P4157 L26: DEA?
Denitrification enzyme activity; changed in text

Table 1: Please add the %Corg and LOI data - Seems important sediment characteristics that were measured (see methods).
Added to table.

Referee 2

page 4135, Line 19: rising nitrate concentrations
Changed

page 4137,Line 27 more heterogenous carbon pool present due to addition of the Danube water. This hypothesis is not clear, please explain. What do you mean by more heterogenous?
We meant that since the carbon is collected from a larger catchment, it should be of more heterogenous origins, age and degradational state. This was added to the manuscript:

P 6 LN 105: We also hypothesized that adding Danube River water would increase the denitrification rate in the disconnected site due to a more heterogeneous carbon pool (derived from a larger catchment area) present in the Danube River.

page 4154 line 7 autotrophs are generally assumed to prefer ammonium than nitrate.
Give a reference to this statement. In fact in many ecosystems nitrate is the preferred nitrogen source as ammonium uptake might result in internal acidification.
Removed from text

Add more general information on site characteristics to Table 1, pH and soil texture.
Grain size distribution and C-LOI% was added

Figure 5 is not very clear in black and white.

We have updated and recolored the figure.