Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-303-RC1, 2016

Hydrology and Earth System Sciences
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Discussions

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Interactive comment

Interactive comment on "Can controlled drainage control agricultural nutrient emissions? Evidence from a BACI experiment combined with a dual isotope approach" *by* M. V. Carstensen et al.

Anonymous Referee #1

Received and published: 19 August 2016

General comments

The authors tested the impact of controlled drainage (maintaining a higher water table) on subsurface N losses from soils. The work was carried out in a multi-year replicated field, where the water table was kept artificially high in two of the four plots for the final two years. Once groundwater tables increased, both NO3- concentrations in the drainage water and the drainage discharge flow decreased. In order to test whether changes were caused by enhanced denitrification, the authors measured dissolved N2O and NO3- isotopic composition in the drainage water. The overall scope of the study is robust, and definitely of interest. However, lack of information in the methods section and inconsistencies in the presentation of the data call into question the validity

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Discussion paper

of this work. I would also recommend careful proof-reading and reorganisation of the manuscript, as the meaning was often difficult to follow.

Specific comments

Material & Methods

âĂć More information is needed on the management of the experimental plots particulary on the activities known to alter N leaching. This includes quantity, timing, and quality of fertilisers, as well as ploughing, fallowing, crop choice, and crop yields. In the first paragraph of the M&M it says that, “Field management practices were similar during the three-year study period involving growth of winter wheat and application of identical amounts of manure and fertilizer in the spring.” Yet the last line of section 2.5 then says that differences between the plots were due to some being planted with winter wheat and some with barley. In section 2.2 it says that field management data was obtained, and in section 2.1 it says that harvest data was also obtained, yet none of this information is presented in the manuscript. This information needs to be established in order to interpret temporal trends in N losses, as well as differences between the control and treatment plots. I suggest adding a schematic timeline of the management scheme to Fig. 1, as well as including indicators of key events such as fertiliser application and implementation of controlled drainage to Fig. 2.

âĂć There are a couple of caveats to the overarching experimental design that are not explained clearly: 1) tweaking of the water table level in the treatments plots (was the procedure identical in both treatment plots? The date also isn’t clear. This should be included both in a field management timeline and indicated on the figures showing changes in the water table over time), and, 2) opening the outflow gate (did this only happen in one of the treatment plots? In the methods section it says that intensive samples were collected during this period, but the data is not shown or discussed). As these events aren’t well explained, they do call into question how representative the overall findings are.

Data quality

âĂć How were the water samples preserved prior to analysis? The lack of mention of any filtration, freezing, etc. make it seem likely that the reported N concentrations and isotopic compositions do not represent the field conditions.

âĂć The first sentence of section 2.5 states that yearly loads were calculated by first dividing the weekly measured nutrient concentrations into daily fluxes via linear interpolation over time. This approach assumes a constant relationship between nutrient export and time. However, this assumption is not consistent with previous findings that, e.g., nitrate concentrations tend to decrease with increased flow. It would therefore be most accurate to calculate total loads based only on the days when stream chemistry data was collected.

âĂć In Table 3 it says that the Y0 column for CP2 is actually filled with values for CP1. What happened to the CP2 data? Why was it excluded? If the data from CP2 was unusable, then this should simply not be included in the table, and a statement about why the data was excluded added to either the results or the M&M. Filling this column with data from the other control plots is misleading, at best.

Data presentation

The data presentation seems overly selective, making it difficult to follow the results or ascertain the accuracy of the conclusions. Most critically:

âĂć Figure 2 only shows data over time for two of the four plots. The other two need to be included if data from them is going to be discussed. The decision to separate each year into a unique (yet unlabelled) sub-plot also makes this figure hard to follow. I'd recommend plotting data from all four plots over a continuous x-axes, using arrows, lines, or shading to indicate the periods that correspond with the 'y0', 'y1', and 'y2' referred to in the text.

âĂć Figure 3: This figure only shows data from Y1 and Y2. Where is the Y0 data?

Additionally, the meaning of the astricts adjacent to the r2 values listed within the plate are not explained in the figure caption, and the slopes reported here do not seem to correspond with those mentioned in the discussion.

âĂć Units are needed for all parameters in Table 1 and Table 3, as well as quantitative information on uncertainty for each number shown

âĂć In the final sentence of paragraph three in section 4.1 it says that, "...controlled drainage also resulted in an approximately one-month delay in drain flow compared with control plots.". As drain flow shown in Fig. 1 does not seem to support this, more evidence on where this statement comes from is needed.

âĂć Nutrient data is presented as concentrations (when units are shown), but the focus of the paper is 'loss' (i.e., concentration x discharge x time), it would therefore be useful to see the data in flux units (g s-1).

âĂć N2O data is only shown in terms of dissolved concentrations. As water in the drainage system will be influenced by both atmospheric N2O and biogenic N2O, it would be more useful to discuss these findings in terms of % saturation. Emissions of N2O from the system also depend on saturation dynamics (see classic description of N2O solubility in Weiss & Price (1980) Marine Chemistry).

âĂć Section 2.2 says that groundwater (∼7 piezometers per plot shown in Fig. 1) was sampled monthly for nutrient concentrations. However, the only groundwater data shown is the (unitless) annual nitrate value in Table 3. How variable were the concentrations over time? Did they differ between the control and treatment plots? How was groundwater data used to calculate N and P losses? What was the P concentration in groundwater?

âĂć More information is need on the spatial and temporal variability in other nutrient parameters discussed (N2O, P, NH4+, SO4, DON, and PON). While some of this data is included in supplemental figures, the critical parameters should be included in the main manuscript in order to create a coherent and convincing story. This could be as simple as adding information on variability and sample numbers to Table 3.

Data interpretation

âĂć Given the experimental design, this paper needs to be organised to more logically explain how variables are, 1) different in treatment plots before and after induced conditions, and, 2) how treatment plots differed from control plots (i.e., where they the same prior to changed drainage conditions, as in, were the controls actually good controls?). The results and discussion are very disorganised, and the selective data displayed, make it hard to tease out the answer to either of these questions

âĂć The discussion around the NO3- isotope data is a bit hard to follow. First, it would be useful to include a 95% CI for each slope described in Fig. 3 in order to more accurately judge if they overlap with the range expected for denitrification (1:1 – 2:1). As it seems that all of the data does plot roughly along a denitrification line, section 4.4 needs to be revised to discuss the data in terms of NO3- 'more impacted' v 'less impacted' by denitrification as values move up and down the denitrification line. It would then be useful to discuss what factors influenced these moves. As the authors note in the second paragraph of 4.4, denitrification is probably always occurring somewhere in an arable soil. It's therefore useful to keep in mind that the leached NO3- isotopes are a reflection of the degree to which denitrification is controlling the NO3- flux, and not direct measures of denitrification activity. This also means that it's a bit of an overstatement to say that higher NO3- isotopes show enhanced denitrification on a specific day. Instead, this higher value may indicate that reducing conditions dominated in the period prior to sampling (though, as this was only observed in one of the three plots, it also seems possible that this sample wasn't particularly representative of reality?). Overall the ∼1:1 ratio of d18Ovd15N suggests that NO3- leached from the plots has undergone variable degrees of denitrification. So what controls these variations? Did isotope values increase in response to rainfall, season, temperature? And are these variations different between the control and treatment plots? I suggest checking out the recent advances in the interpretation of NO3- isotope data from, e.g., Hall et al. (2016) Oecologia and Wells et al. (2016) Water Resources Research when re-evaluating this data.

âĂć The abstract and conclusion both mention ‘pollution swapping’, whereby decreases in NO3- leaching are countered by increases in N2O emissions. Here the drain N2O data is interested from the point of view of obtaining a more complete picture of N leaching losses, but not conclusive evidence for/against pollution swapping. This is because soil surfaces are the primary source of N2O emissions (and thus the focus of concern in ‘pollution swapping’ follow drainage manipulation). Additionally, it is unclear if / how dissolved N2O was affected by controlled drainage, as in the first paragraph of section 4.4 it says that N2O-N was higher in the impacted plots, but then in the next paragraph it says that differences in N2O-N concentrations were not significant.

âĂć The conclusions seem to say that the manuscript makes no contribution towards understanding controlled drainage systems. A clearer case for why this manuscript should be published / read is needed.

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