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Discussions

Interactive comment on "Long-term monitoring of nitrate-N transport to drainage from three agricultural clayey till fields" *by* V. Ernstsen et al.

Anonymous Referee #2

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This paper reports fluxes and concentrations of nitrate in tile drains from three fields with contrasting hydrological regimes in Denmark over an 11-year period. Hydrological and climatic variables were also measured, as were details on cropping, fertilisers, slurry inputs and other aspects of agricultural practice. The authors took care to ensure that each sampling point drained a single field with a single crop at any one time. This and the long sampling period makes this a very valuable dataset, since drained clay soils like these are an extremely common land use in the wetter areas of Europe and elsewhere, and contribute significantly to the nitrate load of rivers. The paper thus addresses relevant scientific questions and presents new data. The methods used are clearly described, and the English is generally good (see below for a few suggestions for improvement).

I think, however, that the paper needs some more work on the results before it is a suitable quality for HESS. There is a lot of information in these results which could be used better, but the authors give no indication that they are proposing to take the analysis further. At present, the paper contains some measurements of nitrate concentration for 3 sites which differ in various ways, but the main driver of the difference in responses seems to be rainfall and the resulting hydrological relationships. The main difference in response is that the high rainfall site has the greatest mass flux of nitrate, and the greatest flux as a percentage of inputs, but lower concentrations. The low rainfall site has high concentrations but a lower mass flux. There may be differences in nitrate leaching due to cropping regime, but these are not systematically explored, just presented in a single figure. There is no discussion of how or whether the 3 sites could be generalised to tile-drained fields in general, which leaves the wider significance of the paper in doubt. Faced with these results, my instinct would be to try and fit a simple model to get a feel for the extent to which the results could be generalised rather than just being characteristic of these 3 fields.

It would be helpful if the authors would define some hypotheses which they could use their data to test. For instance, that nitrate loss from the lower rainfall site is dependent on a few large rainfall events whereas that from the high rainfall site occurs over the whole spectrum of rainfall intensities. This appears to be true from Fig. 6, but it needs to be quantified. Other hypotheses might be that that N loss is due to an interaction between rainfall and stage of crop growth, or N application date or rate, or the crop being grown. Are there differences between crops in N retention or release? The information is shown in Fig. 4, but needs to be quantified and preferably tested for differences statistically. The authors are right is saying that field-scale information is necessary for differentiation of N regulation, but they are missing an opportunity here to show how this information can be quantified and used for regulation. So I agree with Referee 1 that the authors need to show how to use their data for this purpose. The Abstract concludes that ". . .local hydrogeological conditions need to be taken into account in a differentiated N-regulation of agricultural fields. . .". Would they say that it

would be beneficial to restrict N applications on high (hydrologically-effective) rainfall sites in order to reduce N loads on rivers? Or to restrict N application on low rainfall sites to reduce nitrate concentrations? Or to restrict applications at certain times of year or under certain weather conditions? Should certain crops be avoided in some situations? Merely saying it needs to be done lacks credibility if not supported by data from the paper, and is not helpful to regulators.

As well as taking the data analysis further, the authors need to consider how well the data support some of their conclusions. There is some discussion (e.g. p. 655 l.15) of how denitrification would be expected to be more effective at the wettest site (Estrup), yet this is the site with the lowest percentage nitrate retention. Why is this? Is there any evidence that denitrification is occurring at all (e.g. from the seasonal pattern of nitrate concentration, or in relation to temperature)? The authors need to take a more critical look at their data in general.

The Abstract is rather unclear and does not do the paper justice. In particular it is not obvious that the descriptions (A), (B), (C) in line 16 onwards represent the three fields referred to in line 11. Transport fluxes should have a time dimension (kgN/ha.yr?) here and throughout the paper. The main results need to be stated more clearly, as well as the main differences between the sites (i.e. hydrologically-active rainfall).

Technical Comments and Queries p. 644 l.12 Define Ap for those not familiar with this terminology. p. 646 l.18 on. The nature of temperature variation at the sites is clear from Fig. 2, but this description of temperature ranges gives an impression that the temperature regime is more severe than it actually is. I would recommend using standard metrological statistics e.g. mean temperature, mean seasonal maximum and minimum temperatures etc. to characterise the temperature regime. A meteorologist would advise. p.647 l.10, Fig. 3 etc. Water fluxes in m3/ ha.time would be better expressed in mm/time (1 m3/ha = 0.1 mm). This will be a more familiar unit for hydrologists, and they can then be compared directly with the precipitation, evaporation and runoff fluxes in Table 1, Fig.3 etc. p. 645 l.9 and p.648 l.8 Does “commercial N-fertiliser”

mean “inorganic N fertiliser”? Organic N fertilisers and even slurry are available commercially, so this distinction needs to be made. p.667 Fig. 4 What is the crop in the white areas of the graph?

Specific Comments and Corrections p. 642 l.11 Awkward phrase - suggest “. . .enhances crop yields on highly productive soils with poor natural drainage.” p. 646 l.3 “filtered” for “filtrated” p. 648 l.4 and elsewhere. No need for a dash between “N” and “fertiliser”. “N fertiliser” is correct. p. 656 l.7 “primary” should be “primarily” p.657 l.14 This reference (“Commission. . .”) is out of alphabetical order. p. 659 l.8 “Kladivko” should be “Kladikov” both here and where referred to in the text. p. 667 Fig. 4 legend “BBCH” should be defined both here and in the text.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 639, 2015.