

## ***Interactive comment on* “Sources and fate of nitrate in groundwater at agricultural operations overlying glacial sediments” by Sarah A. Bourke et al.**

**Sarah A. Bourke et al.**

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Hi Sebastian, Thank you for your thoughtful comments on the manuscript, and my apologies that it has taken me so long to reply.

Thanks for letting me know that a summary of DON, NH<sub>4</sub> etc in the body of the text is helpful to the reader. I can do that, no problem.

Your second point is about redox conditions. I totally agree with you that it would be preferable to have measurements of redox conditions at these sites. The vadose zone and fractured upper part of the clay are likely to have been oxic, and below that there is

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often a colour change that suggests reducing conditions, but we did not measure redox directly. I would imagine that the redox conditions could vary within a manure pile, with oxic conditions at the margin and reducing conditions deeper in the pile, but we also did not measure this. I will look for published studies from similar sites that can provide further guidance on likely redox conditions at these sites.

We do have measurements of DO in the monitoring wells (see Table attached), but on considering the sampling method I didn't feel confident that they reflected in-situ groundwater concentrations. Because of the low-hydraulic conductivities the wells are bailed dry and then allowed to recover, then DO was measured potentially days after the well was bailed dry. I have previously conducted an experiment on hydrochemical equilibration of groundwater in an evaporation pan, and the DO was fully equilibrated with the atmosphere in 1 day. As such, I couldn't rule out that the DO concentrations measured in the recovered wells had partially equilibrated with atmosphere in some samples. Do you think it's preferable to report these data anyway, and discuss the potential for equilibration within the well prior to measurement?

And finally, on denitrification, to me this is strongly supported by the denitrification trend in the nitrate isotope data. While other mechanisms may be at play, I don't think we have measured evidence for them and discussion in the paper would be purely speculative. Happy to discuss any of the above further.

I realise the discussion period closes next week, so feel free to email me directly: sarah.bourke@uwa.edu.au

Cheers, Sarah

Please also note the supplement to this comment:

<https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-31/hess-2018-31-AC1-supplement.pdf>

31, 2018.

**HESSD**

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