

This paper summarises an interesting dataset from the ABMI, investigating patterns pertaining to human development, species richness, and community structure across wetland community types. I think the approaches taken are generally plausible, and the results are not too surprising. However, the Discussion section seems to be missing a part where the authors critically reflect on whether the combination of their dataset with their chosen modelling strategy has any weaknesses with respect to the ecological inferences drawn. At the moment the Discussion just takes the results and assumes that they are a completely correct representation of reality, with no uncertainty attached to them. This is a missed opportunity to reflect on the modelling approach taken relative to the data, and to therefore make the paper more interesting and useful to other ecologists. Some points that might be considered in such a section of the Discussion are given below in the more detailed comments against manuscript line numbers.

## ## Specific comments

L94 Confusing, as introducing NNS can also increase local diversity, as you imply in the first paragraph of the introduction. It's obviously debatable that NNS always reduce local diversity in terms of always being the proximal cause (as opposed to passengers of change responding to other factors).

L123 This sentence should end with "in our study area" to avoid over-reaching.

L154 I think referring to unique visits in time and space as "sites", which normally implies a location in space only, is the opposite of clear. Why not refer to them as "samples"?

L165 What's the lower bound on the area sample for the wetland plots?

L216 The intermediate level here seems rather arbitrary. Looking at Fig 1 there seems to be a good spread of samples between 25 and 75%, and one can well imagine that at the scale of the remote sensing data, a site with 25% human-influenced land-cover could be quite similar to one with 75% at a landscape scale – e.g. perhaps a wetland site is in a similar location to another with respect to human development, but it just happens that the location of your SPOT6 polygon includes a corner of some development in one instance, and most of it in another. At the very least it seems to me that this choice of 45-55% could do with a sensitivity analysis, particularly as your choice results in a small intermediate sample, and PERMANOVA is known to be liable to confounding changes in variances with changes in locations (i.e. means). (See David Warton's papers on mvabund as a more robust alternative to PERMANOVA in some situations).

L228 Including sampling methodology as a fixed effect: does this really deal adequately with the different totals and types of areas sampled between the two methods? Terrestrial is 1 ha, wetland is from some unknown lower bound up to 14 plots x 20m<sup>2</sup> = 280 m<sup>2</sup>. Normally the comparison of differently sized areas in terms of richness requires species-area curve adjustments (e.g. see Palmer et al. 2008); now, I can see that a fixed effect allows for some constant difference in richness between method types alongside adjusting for other variables in the model, and maybe this is enough, but on the other hand your wetland sites apparently have transects that vary in length, so is this fixed effect approach sufficient? Something for the Discussion.

L229 Leading on from the comment against L154, it's worth noting here that you do not even follow your own convention of calling samples "sites".

L230-L235 You seem to approach the question of non-linearity from the point of view of an a priori decision to compare a model with a polynomial to one without, but nowhere do you explicitly say that this was a priori based on some literature evidence or hypothesis. This always makes me

suspicious that the decision was actually based on a lot of data dredging and model experimentation, and subsequent lying by omission by not stating that this was actually what happened. If it this modelling strategy was actually decided upon before looking at the data, please state this clearly, otherwise be honest and state that it was a result of experimenting with the data, and discuss what implications this has for your inferences (e.g. invalidity of claimed P-values).

L232 Leading on from this, if the approach wasn't a priori, then why not also look at whether a cubic term improves fit in terms of AIC. Or use AIC to choose the number of knots in a spline? Even if the quadratic approach was determined a priori, looking at more flexible forms might reveal other patterns worthy of discussion in an exploratory mode. I think the AIC-based approach to spline knots is available in Frank Harrell's "Hmisc" R package (which accompanies his Regression Modelling Strategies book). Also, is AIC appropriate, or should AICc have been used?

L283 The first sentence of the Discussion illustrates my point that you are lacking some critical discussion of your own methods and the potential influences of these on your ecological inferences. For example, you say here "we found maximum richness in wetlands surrounded by intermediate extents of human development... etc.", and then proceed as if this has been proven beyond reasonable doubt. But even your own fitted model in Fig 1a shows that this is barely true overall, or for wetland sites in isolation, and this before even considering uncertainty in these estimates, or in model selection (in terms of whether other plausible models could have been used – note that this is not just about AIC-based model selection, which is itself dependent on certain assumptions about the data and of course on the precise dataset in hand). To be honest, I find the whole discussion rather dull, as it is just trotting out literature-based arguments, without any critical discussion about what was actually done with this dataset in this paper.

#### #Section B

Given that there are only two figures in the paper, I don't really see why so much has to be in supplementary material. I would, for example, have thought that the NMDS plots were central enough to actually be in the paper.

#### #Ref

Palmer, M.W., McGlenn, D.J. and Fridley, J.D., 2008. Artifacts and artificions in biodiversity research. *Folia Geobotanica*, 43(3), pp.245-257.