

## ***Interactive comment on* “Chemical footprints of anthropogenic nitrogen deposition on recent soil C : N ratios in Europe” by C. Mulder et al.**

### **Anonymous Referee #4**

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This manuscript reports a study of C:N ratios in the top soil at European scale with the main aim to answer the question whether soil C:N ratios have responded to spatially different long-term N deposition regimes across land uses in Europe. The topic of the study has been debated for long and is probably considered intuitively known, but the evidence is not impressive. The question of the study is therefore very relevant, and the comprehensive new soil dataset combined with modelled N deposition is an interesting platform to explore the links between historical N deposition and current topsoil C:N ratio. However, as outlined below I believe the paper needs a substantial revision to explore the full potential of the study.

My main concern with this manuscript is the brief superficial nature of the text which also leads to a serious lack of transparency in terms of materials and methods used.

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We need to know the details about the experimental platform, N deposition assessments as well as soil analyses so the paper can be “stand alone”. It is not sufficient to refer to the Toth et al. paper for all details regarding soil C:N data.

The manuscript structure is not consistent with the usual guidelines for a scientific paper. It lacks more specific Abstract and Introduction sections (the latter without Materials and method info). There are no clear research questions or hypotheses suggested for the study in the Introduction, the Methods section is very brief in terms of deposition and soils data, and there is little information provided on statistical analyses done (apart from the clustering). The information on statistics comes here and there in the Results and Discussion section and in Table and Figure legends.

The Discussion ignores the biogeochemistry behind the findings. Why is there no response of cropland soils (here called “managed”)? I assume they are already loaded with N from fertilization so there is no expected signal from N deposition, but it needs to be discussed. Why do “unmanaged” soils in high deposition areas not respond to short-term deposition estimates? The paper tends to mainly present these results which are interesting, but a scientific study needs to also discuss the mechanisms behind the findings. Are results plausible in light of current knowledge (other studies) of N deposition influence on soil C/N? Does it have any importance that forest floors/organic layers (known to be good indicators of N deposition and N saturation in certain land uses) were not sampled and analysed?

I also miss a discussion of other factors contributing to C/N ratios, i.e. based on the Cools et al. (2013) study which in fact focused on some of the responsive land uses, i.e. forests. Cools et al. found no large effect of N dep compared to other factors, e.g. tree species – could this be because their study did not omit high N dep regions? There seems also to be a lot of information on specific land uses and other site descriptors – I wonder why this information was not used to a greater extent since the “cropland” and “unmanaged” categories each included very different land uses in terms of N budgets and N cycling rates.

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Another issue for consideration is the terminology used. The authors use terms such as “unmanaged”, “nature”, “natural ecosystem” which are at best very badly defined but also not applicable to the land use included in the study. As one example European forests are not “unmanaged” neither are they “natural ecosystems”, and the same can probably be said for grasslands

Specific comments p. 4316 l. 1. The Abstract is not informative. There is too much background without focusing on the key results of the study. p. 4316 l. 5 change “as” to “and”. p. 4316 l. 16. What is mean by “The global cycle of nitrogen (N) is unique...”? Which cycles are not unique? p. 4316 l. 19. Need a reference here regarding soil respiration, perhaps Janssens et al., Nature Geoscience 3, 315 - 322 (2010). p. 4316 l. 23. “Ecological stoichiometry” – please use a more specific term, e.g. just “stoichiometry of vascular plant tissues”? p. 4316 l. The Introduction is very short and lists a lot of issues and buzzwords with little reference to ecosystem implications. The justification for this study including the background needs to be framed better. p. 4317 l. 3. Suddenly the Introduction turns into a Materials and methods section from here. It is OK to briefly review what the study is about but I miss aims and hypotheses of the study presented in a clear and concise manner. Please move this info to Methods. p. 4318 l. 1+. Sections 2.1 and 2.2 must be amended with more detail on the deposition data and the soil data (including a brief explanation of the experimental design and chemical analysis). The paper needs to be stand alone, so only refer to Toth et al. (2013). p. 4318 l. 6. It is not clear why only climate data from the short period 1985-1996 was used? p. 4318 l. 21. Why not use all this detailed information on land uses that differ a lot in N deposition/fertilization and N budgets in the statistical models? p. 4319. l 2+ Please explain more clearly that the cluster analysis is a spatial analysis and not a temporal analysis. What does “preclustering of cases” mean. Is case the same as a site? The statistical section should use the experimental units of the study. p. 4319. Please change the heading of section 2.3 to statistical analyses and include descriptions of p. correlation analyses between deposition and recent soil C:N ratios here. p. 4320 l. 20. Please specify what is meant by “C:N ratios

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in mass units”. p. 4321 l. 2+. IT would be very interesting to see a graph of the “cropland/managed” relationship and the “unmanaged/natural” relationship p. 4321 l. 22. It is not clear how it is possible to say that “. . .soil C:N ratios were most sensitive to 5-year pulses of atmospheric nitrogen supply. . .” when all five-year estimates were closely correlated. p. 4321 l. 25+. This sentence is very generalizing and it is not clear what is meant (e.g. about carbon-nitrogen feedbacks and harvesting in forests). p. 4322 l. 1-4. This sentence is not justified by the current study. Please delete. p. 4322 l. 6. The Conclusions should be rewritten without Discussion and inclusion of references. These parts need to go to the Results and Discussion section. p. 4322. l. 18. I do not think that this study was able to explain why N deposition affects natural ecosystems. The mechanisms are not addressed. p. 4323 l. 1. Please find a more relevant conceptual study than Schmidt et al. (2011) to support your statement (when moved to the Discussion section). Fig. 3. There are some strangely low C/N ratios  $\ll 10$  in the low N dep group, whereas such outliers are almost not present in the high N dep group. On the other hand, some of the high values ( $C/N > 25-30$ ) indicates that the organic layer was perhaps not always separated from mineral soil.

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