

The paper uses a large data set of soil ammonium and nitrate concentrations and attempts to correlate these values with various soil properties and with land use. Although the paper does not represent novel concepts, it uses a state of the art analysis and a large data set. Given the size of the surface under investigation, the data set is however relatively sparse (as acknowledged by the authors). Nonetheless, the study represents a contribution to scientific progress and an important basis for the investigation of large-scale drivers of soil N, thus in my opinion warranting publication. I have however a couple of main concerns with the paper, which should be addressed before publication. The analyses are clearly outlined and assumptions seem to be valid (with one exception, as noted below). The paper is well structured and generally well written, although there are several cases where the text is unclear; these exceptions are outlined below. Additionally, the conclusion section contains some terms that are not addressed in the paper and I have suggested their removal (see below).

Thank you for the positive comment. We acknowledge that the BASE data is sparse with only 469 sites over all of Australia. But as far as we know, the data is the largest dataset available (possibly worldwide) to study the spatial distribution of soil mineral N and its soil controls.

#### Main comments

I have two main concerns with the manuscript.

**Firstly**, the authors state that they are investigating drivers of soil ammonium and nitrate. This is indicated by the language used throughout the study ('controls' and 'drivers'), including in the aims section. The study is however an observational study, meaning cause and effect cannot be derived from these results; the 'drivers' of soil ammonium and nitrate cannot be identified from such a study, only correlated variables (or 'patterns'). It would be acceptable to state that this study aims to identify candidate soil properties that might be considered, following further study, as 'controls' or 'drivers'; the study cannot however identify these controls and drivers itself. Such wording would agree with the fact that this study represents a basis for further studies –as indeed the authors state several times.

The text throughout the manuscript (including the title) needs to be corrected to reflect this. Words such as 'controls' and 'drivers' need to be avoided.

The BASE data include soil  $\text{NH}_4^+$  and  $\text{NO}_3^-$  as well as soil variables at the continental scale. The reviewer is correct in a sense that we cannot strictly identify direct soil controls on mineral N and imply any mechanistic relationships based on the data. Nevertheless, modelling with Cubist assumes that the independent variables are state-factors that affect soil mineral N, and therefore may be assumed to be 'controls'. We agree that we need to be conservative with our interpretations of the effects of the independent variables on soil mineral N because of the sparseness of the data, and we believe that we have been and that we have clearly stated our assumptions. Further, we suggest that even in small-scale manipulative studies, it is often not straightforward to derive cause and effect relationships in soil system without making assumptions.

**Secondly**, it is unclear to me why the authors have split the soil samples into the two regions ('agricultural' and 'ecological'), given that within the agricultural region there seems to be a large variation in the intensity of land management:

- Would it not make more sense to use the actual land use of the 469 sites as a factor, or to derive a scale of land management intensity from the 28 land use types, and examine the correlation of this with the soil attributes?

Thank you for this suggestion. We split the data into the two regions because the data contain only 469 sites which do not sufficiently represent Australia at the detailed land uses level. In addition, detailed land uses are not well (and equally) balanced in the data (see Table S1). Thus, our regional modeling by accounting broad land uses is the only feasible approach to meet our objectives. Using land use classes as a factor in the modelling will not help to improve our interpretations or help to address our aims. Furthermore, we unfortunately do not have any additional, and more importantly necessary, supplementary information on management practices which could be used to derive a land-use intensity scale across the sites. We can see that this option may be feasible with fully resolved spatial data or a well-established ecosystem model, not an empirical model.

- The authors state that they aim to assess the relative importance of large-scale drivers, which I assume is the purpose of using two large regions (agriculture and ecological). However, I suspect these two regions are inadequate to do this: given that the two regions encompass very large climatic and geological variation, important large-scale potential 'drivers' are not addressed by this method either. The authors could consider incorporating broad climatic information into the analysis. This may reduce some of the noise and thus improve the outcome of the analyses.

We disagree with this comment. For the study, the soil property values from our recent mapping were generated by considering full continental variation in climate and other environmental variables, as stated in P3 L33 and P4 L1 of our manuscript. In this sense, the soil data that we used 'integrates' information from climate and the other environmental variables that determine the distribution of the particular soil properties at the continental scale. In this study, our focus was on proximal soil controls on mineral N, rather than distal controls, such as air temperature or rainfall.

I have a number of additional minor concerns with the paper that need to be addressed:

P3 L15-16: I appreciate that the sampling design of the BASE project is described elsewhere, but it would be useful to have a little more detail on this, including the number of soil samples taken from each 25m x 25m 'site'.

We have added a short description of the sampling design: **"Each sample was collected from a site that represent a unique combination of soil, climate and management. Specifically, between 9 and 30 soil cores were sampled in a 25 m x 25 m quadrat and split into two different depths, 0–0.1 m and 0.2–0.3 m, respectively, and then combined into one composite sample (approximately 1 kg of soil) for each depth."**

P3 L 27-33: The soil maps used were a result of spatial modelling. The outcome of a model cannot be considered as data; please therefore change the word 'data' (L27 and L32) to 'values' or 'information'.

This is a good point. We have changed the word "data" to "values" or "information" throughout the manuscript, as suggested. We think all the continental soil maps currently available are a product of spatial modeling. However, these maps seem to be generally accepted and used as "data" rather than "information". Probably, these values are considered as data because of repeated model evaluation with real measurements. The European soil database and the SSURGO data are good examples.

P5 L8-10: The values given in these lines (sum of  $\text{NH}_4^+$  and  $\text{NO}_3^-$ ,  $\text{NH}_4^+$  and  $\text{NO}_3^-$ ) seem to refer to mean values (for the first of this set of values, it is indeed stated so). Would it not make more sense to give median values here, given that median values are what are shown on the corresponding graphs (figure 2)? The mean average value of a population that is not normally distributed is not particularly informative. Additionally, given that the  $\text{NH}_4^+$  and  $\text{NO}_3^-$  concentrations are not normally distributed, stating the standard deviation of these data is misleading, as the use of a SD value to convey information assumes the population is more or less normally distributed.

We agree. Please note that Figure 2 now shows box-whisker plots. We have reported the median values instead of the mean values, except for the mean comparisons. We also agree that the use of SD is not ideal for non normal data. Thus, we have reported the interquartile ranges, instead of SD values, to better describe skewed data. And we think we can avoid its misuse as the distribution of soil mineral N values is also shown in Figures 2, 4-6.

P5 L 19-13: Why carry out an analysis omitting the samples for which  $\text{NO}_3^-$  concentration was below the detection rate? If a soil sample has a  $\text{NO}_3^-$  concentration below the detection level, this does not equate with 'no data', but rather means that the  $\text{NO}_3^-$  concentration is simply very low (as the authors indeed assume). Unless something specific is being tested, which I do not think is the case here, an analysis with these points removed is uninformative. This subject is re-visited in the discussion (P7 L11-12) but as the text is written, I still do not understand what information this analysis brings. The results of this extra analysis do however indicate that the agricultural soils have a bimodal distribution with respect to  $\text{NO}_3^-$  concentration, i.e. many soils have very low concentration and many soils have a very high concentration (indicative of high addition rates of  $\text{NO}_3^-$ ). If this additional analysis was carried out to illustrate this, the authors need to make this clear in the discussion, and indeed expand this point in the discussion.

We used all the data for our analysis and modelling – values below detection limit were coded as  $0.5 \text{ mg N kg}^{-1}$ . Our extra analysis do not indicate a bimodal distribution of  $\text{NO}_3^-$  contents across the soil in the production region. The  $\text{NO}_3^-$  contents still follow a skewed distribution to the right, with or without these low values under the detection limit. We simply wanted to show a shift in the median (or mean) contents in soil mineral N budget, which is important for a soil N management perspective.

P7 L18-19: What is meant here by this sentence, particularly the term “agricultural soil”? Do the authors mean that not every site in the agricultural region is under agriculture? If so, please change accordingly and change the terminology to ‘soil under agriculture’. If not, please explain the term ‘agricultural soil’.

We apologize for the confusion. Now we have termed the two regions as “agricultural” and “non-agricultural” as we think this is more clear-cut. We hope that this change in the terminology makes regional comparisons by broad land use more comprehensible. We clearly define these two regions in the first sentence of the section 2.2: **“(2) the samples from the sites that mainly originate from dryland and irrigated cropping, and from improved and native pastures used for animal grazing (hereafter referred to as the “agricultural” region), and (3) the samples from the protected sites and those in natural environments in natural environments outside of the agricultural production zones (referred as the “non-agricultural” region)”**.

P8 L2-3: This sentence is either incorrect (as I have understood it) or imprecisely written: I understand from this sentence that the  $\text{NH}_4^+$  concentrations between the soils from the agricultural and ecological regions are different. According to figure 2 and text in the results section (P5 L8-9) however,  $\text{NH}_4^+$  concentrations are similar. I suspect a more complex pattern is meant by the authors; this needs to be more clearly explained.

Thank you for picking that up, this was our mistake. The sentence has been corrected to **“As shown above, the amount of  $\text{NO}_3^-$  or the sum of  $\text{NH}_4^+$  and  $\text{NO}_3^-$  ...”**.

P9 L 18-25: The text here is difficult to understand. I have a few suggestions that might help:

We sincerely appreciate the suggestions.

L 19: Replace “by each specific region” by “within each region” (if I have understood correctly).

We have followed the suggestion.

L20: Replace “on each region specific basis” with “for each region” (if I have understood correctly). The sentence L20-22 is very unclear. Is it referring to the higher prediction error for the high concentrations of  $\text{NO}_3^-$  (in the ecological region in particular)? Rewrite.

We have followed the suggestion. The following sentence has been rewritten as: **“In contrast, the model performance of soil mineral N was substantially limited by high prediction error, particularly over a high range of contents at all sites and in the non-agricultural region.”**

L23: “presence of small values” is too vague. Do the authors mean that the high frequency of samples with very small  $\text{NO}_3^-$  concentrations is the cause of the limited overall model performance? Needs to be explained.

We have modified “presence of small values” to **“presence of samples with  $\text{NO}_3^-$  contents under the detection limit”**.

L24: Replace “most of model errors” with “much of the model error”

We have followed the suggestion.

L24-26: This sentence is too vague. This sentence relates to the presence of small values of what is meant be “the limited data set” exactly?

We agree - it is too vague. We have re-written the sentences as: **“As a result, much of the model error resulted from the lack of accuracy. In addition, the models may not capture all the processes and resulting variation as they were based on the limited data sets.”**

P10 L7-9: The authors here imply that they have identified a process in the results, the “potential to maintain or increase  $\text{NH}_4^+$  concentrations”. This process is a possible explanation of the results they have found, but is not in itself a result. Please change text accordingly.

Thank you. As suggested, we have changed the sentence as: **“Despite this limitation, our results suggest that soil organic matter and its C:N:P stoichiometry may contribute to the potential to maintain or increase  $\text{NH}_4^+$  contents ...”**.

P10 L33-34: The term “management” should be avoided here, unless the authors specify what they mean by management, as a term separate from and in addition to ‘land use’.

We have deleted “management” as we have not addressed any management effect on soil mineral N.

P 11 L 2: The term “human modification” should be removed; this term implies some sort of scale or land use intensity (e.g. nutrient input levels), but this has not explicitly been investigated in this study.

We have removed “as subjected to different levels of human modification”.

Figure 1: Three main regions are shown here, whereas two are considered in the text. I recommend that the number of regions considered should be consistent. Alternatively, if the three ecological regions were considered distinct enough to warrant their separation on the map, why not use three regions in the analysis?

We agree that the shading of three regions in Figure 1 is initially confusing because we analyse only two regions. However, the reason that we shaded three regions in Figure 1 rather than only two is that we want it to be clear to readers that we acknowledge the fact that within the ‘agricultural’ region there are two regions that differ in the intensity of management. As stated previously, we would have definitely preferred to separate the data into three regions, however, the paucity and sparseness of the dataset did not allow that. Thus, our broad land use regions: “agricultural” and “non-agricultural”. We made this definition and our assumptions clear in the text and in the figure caption. We hope this clarifies the concerns of the reviewer.

Please note that we have made a new Table 1.

Technical corrections

P1 L11: It is unclear what the ‘other’ ecological region refers to.

This has been corrected to the “non-agricultural” region.

P3 L17-22: The sentences from L17 to 22 need to be moved out of this section; I suggest to the discussion.

We understand that this reads like discussion. However, we wanted to justify our assumptions.

P5 L5-6: In the first sentence of the results, the mean  $\text{NH}_4^+$  and  $\text{NO}_3^-$  concentrations are stated, referring to figure 2. However, in figure 2, the median concentrations are given. Please correct text (or change figure w2) accordingly.

We have now reported the median values with our new Figure 2. In the previous figure, both mean and median values were reported.

P5 L25: Change “large” to “high”

We prefer “large” to ‘high’ as this refers to ‘contents’.

P5 L26: "in that" needs to be inserted between the words ".....environments or" and "used mainly.....".

This comment makes no sense to us. Thus, we have not followed the suggestion. We will address the comment if the reviewer is willing to provide some clarification.

P6 L16-17: This sentence belongs in the discussion.

Not necessarily. We described that the model estimation was not accurate for high mineral N contents.

P7 L8: Remove "In our case".

We have followed the suggestion.

P7 L7: Change "which may suggest" to "which suggests"

We have followed the suggestion.

P7 L22-23: Change "...which was considerably low in the soil from the agricultural region compared to the ecological region" to "... which was considerably lower in the soil from the agricultural region compared to that in the ecological region".

We have followed the suggestion.

P10 L6: Change "difficulties to directly measure" to "difficulties in directly measuring"

We have followed the suggestion.

P10 L10: Change "also indication that" to "also an indication that"

We have followed the suggestion.

P10 L11: Unclear. What depends mostly on soil mineralogy? Soil input stoichiometric ratios or final soil elemental ratios?

We have re-written the sentence to make it clear. **"There is also an indication that final soil elemental ratios are less affected by soil input stoichiometric ratios than previously expected and depends mostly on soil mineralogy ..."**

P10 L28-29: Unclear. I think this sentence needs to be re-written as: "Therefore, the importance of soil elemental interactions in determining the variation of mineral N at different spatial scales across and within various cropping and ecological conditions needs to be estimated." I have possibly interpreted this sentence incorrectly. If this is the case, what is "it" (L28)?

Thank you. We have followed the suggestion. **"Therefore, the importance of soil elemental interactions in determining the variation of mineral N at different spatial scales across and within various production and ecological conditions needs to be estimated."**

Figure 2: Please clarify, do the error bars shown represent the standard deviation of the data values i.e. population, or the standard error of the mean?

In the previous figure, the error bars represent the standard deviation of the data values. We made new box and whisker plots in Figure 2. Its caption has been changed to **"Figure 2. Mineral N contents (mg N kg<sup>-1</sup>) and fractions of total N in soil. The bottom, middle and top of each box represents the 25th, 50th (median) and 75th percentiles, respectively. The points above the whiskers are extreme values. Means between main agricultural and non-agricultural regions of Australia are significantly different at P-value < 0.001 (\*\*\*) , if indicated based on ANOVA on the log of the values."**

Figure 4: Is 'standard error of the (estimated) mean' meant here?

In the caption, "the estimated mean" has been corrected to "**the estimated mineral N content**".

Additional technical corrections:

We have added the units of measured soil properties of the BASE data and values from the soil maps in the section 2.1.  $\text{NH}_4^+$  and  $\text{NO}_3^-$  were reported in mg N/kg. Organic C, total N, total P, and texture were reported in %. Bulk density and exchangeable CEC were reported in  $\text{g/cm}^3$  and meq/100 g, respectively. Thus, we have corrected the term "concentration" to "content" for soil mineral N as determined by the mass of sample throughout the manuscript.