Reviewer #1

Some minor revisions are needed:

1. In the section of Results and discussion, some explanations should be added. For example, in Table 3, in the areas that cultivate onion, the third parameter (μ* =0.45) is CanalNO3 not $^{\perp}$ λhet , which is different with other crop type. What's the reason intrinsically?
The reviewer makes and excellent point. In the irrigation application scheme, onions receive irrigation water more frequently than do other crops. Hence, the concentration of nitrate in the canal water (which is used as irrigation water) will have a larger influence on groundwater nitrate concentration underlying fields that cultivate onions. All other crop types have a value of μ* for canal nitrate concentration less than 0.15.

This is now discussed in the text (**Lines 428-432**):

"The exception is areas that cultivate onion, in which $Canal_{NO_3}$ *(μ* =0.45) ranks in the top three behind* F_{NH_4} *(1.21) and* N_{up} *(0.99).This is due to the fact that onions receive irrigation water (from the canals) more frequently than do the other crop types. Hence, the* C_{NO_3} *of the canal water has a stronger influence on groundwater* C_{NO_3} *underlying onion-cultivated fields than for the other crop types."*

2. Line 445, importance-->important
This has been changed.

3. In the Figures, the letters are lowercase (i.e. Figure 4a,b,c,d), while in the text, you used uppercase (i.e. Figure 4A,4B, etc.)
Thank you – this inconsistency has been corrected throughout the manuscript. All letters for figures are now in lowercase in the text.

4. Line 199, why organic nitrogen is not included in the total N?
The interim standards are set for Total Inorganic Nitrogen (TIN). This is now clarified in the text.

We thank Reviewer #1 for the helpful comments.

Reviewer #2
In the revised manuscript Bailey et al present an improved version of their previously submitted manuscript. My concerns, and the concerns of reviewer #1, were answered thoroughly and respective modifications were performed. Concerning my first concern (comment 1), the selection of sampling distribution for the Morris sensitivity analysis, the Bailey et al. provide an acceptable justification for the way they applied Morris.

To make this helpful information also accessible for future readers I suggest to include few summarizing sentences of their response into the respective part of the methods section.
This has been added to the Methods section (Section 2.3) (**Lines 297-300**):

"Similar to any standard SA practice, parameters are drawn from their predefined distributions, with each model input parameter ω_i varied across p discrete values [Saltelli et al., 2008]. Generally, results of SA are not sensitive to the choice of distribution from which values are sampled."

Except for that the manuscript is ready to be published.

We thank Reviewer #2 for the helpful comments and suggestions.