Response to the Editor's Comments: MS. HESS-2014-272

The authors wish to thank the Editor for the very constructive comments and valuable suggestions made on the revised manuscript. These have helped improve the manuscript to convey the manuscript's message in the most clear, concise and coherent way:

Editors Comments

I wish to thank the authors for their replies and making an updated manuscript. I have taken some time to go over the discussions and revisions and find myself torn on the suitability of this paper. I welcome the general focus and analysis in the context of GHG emissions in particular, and the sections on retention time and surface-groundwater connectivity are relevant and topical. Therefore, I think the topic and content could ultimately be suitable for publication in HESS, however currently the writing lacks focus, is repetitive, and in general the paper currently lacks in-depth critical review and synthesis of the published literature in this area – the latter is an essential element for a HESS review article. For these reasons, the paper is unsuitable in its present form and needs a major overhaul. Below are some high level points that need to be addressed:

Lack of focus – Both the abstract and introduction (and elsewhere) contain significant "text book" information without a clear narrative or focus. Some points to consider: The abstract doesn't state the aim of the paper until 2/3 of the way down (the first 2/3 is all background info), and the aim is to provide a "review and discussion of C and N dynamics". No clear findings are presented except that at the end of the abstract it is stated that further examination of N transformations are necessary. This needs to be sharpened. The introduction highlights the motivation (that CW performance is unclear and the emission of GHG is unclear) which is good, but no clear aim is listed and no comment about the rationale for the approach adopted during the review is made and it is left to the reader to guess what is the focus is. An aim (or multiple aims) is still essential for a critical review, eg. to understand the range of variability in X, or the dominant processes controlling Y, or to identify knowledge gaps. Following from this, it is not clear if the authors are focusing entirely on wastewater wetlands or other types (eg. stormwater treatment systems?). Currently the abstract hints the paper is considering wastewater systems, but the introduction is more ambiguous mentioning wastewater and stormwater systems and others (page 3 - line 77) with no further clarification. A comment in the replies seems to indicate the focus is broadly covering all wetlands. If so, do CW systems perform equally well at removing N under these different contexts? Are the some generalizable findings about how different types work? Or if the review is focusing on one type then the title should reflect that. How does this review build on or differ from other review works on CW's

that have been made? There is a partial treatment of C in the paper, relevant to GHG emissions. In response to a reviewer comment, C was removed from the title, but I am not sure the solution of removing C from the title addresses the underlying problem – the paper superficially treats C cycling. If GHG's are a core focus of the paper, then C needs to be treated equally with N. A symptom of the above issues is seen in the repetition of ideas throughout the paper. This is as bad as including two exactly identical statements in one paragraph (mid-way through page 3), but also more generally concepts of isotopes and pollutant swapping are repeated several times in different sections. The section in the introduction from lines 104-150 is essentially going into the detail that is later reviewed in the dedicated sections.

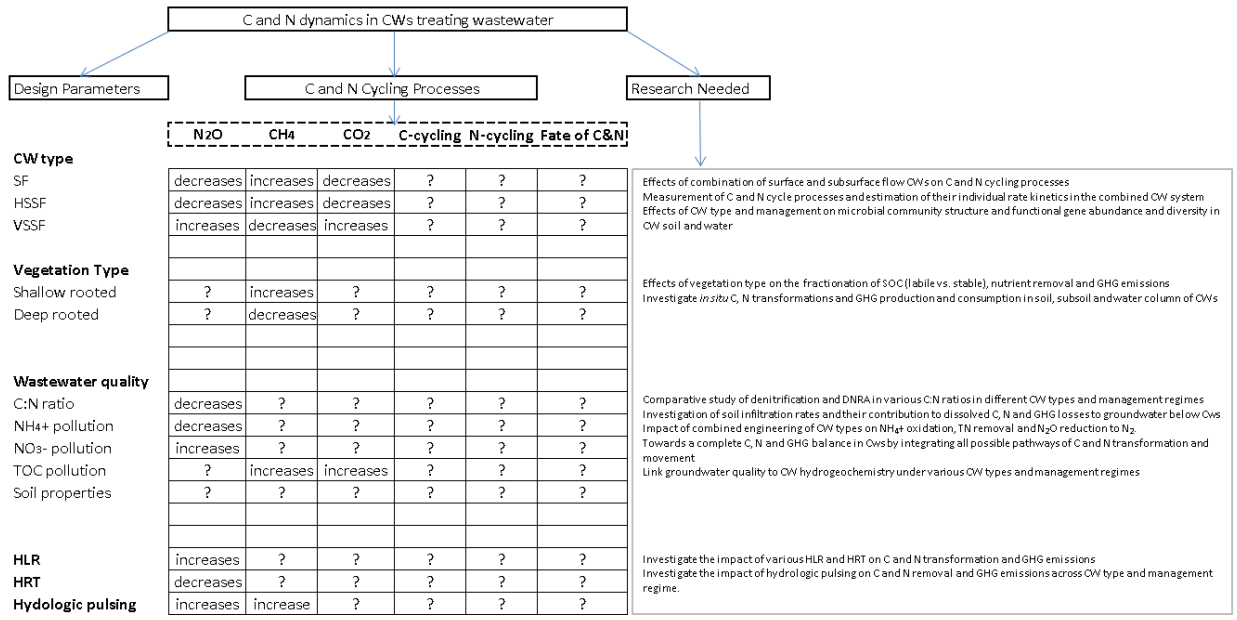
Need for more depth and detail – There are quite a few superficial statements that could benefit from being more precise or more detailed, and also several areas come to mind where I was looking for the review to go into … some examples: "the efficiency of OM removal is often satisfactory, but N removal is generally limited" – OM is also consisting of N as DON/PON so this sort of statement is ambiguous. Presumably you are referring the fact that TN goes down but DIN does not. If so, then try to be specific and say that the wetlands are removing PON and not removing NO3/NH4, and provide an explanation or interpretation, eg sedimentation of particles is occurring but unsuitable conditions and/or lack of retention time for denitrification to occur etc (see comment on synthesis below). Nr is introduced but not explained or defined – is it DIN? DON is reactive too. Little treatment is given with respect to plant types; Limited discussion on measuring surface-atmosphere fluxes of GHG directly (eg. via eddy correlation or chamber work) despite GHG being a focus; Significance of different types of soils in the soil section; Description of modelling needs to undertake nutrient budgets; Significance of management actions on GHG; C:N ratios and organic matter quality. How do we go about scaling GHG emissions up to regional / global scales, Role of temperature, etc. Insufficient synthesis – As mentioned by reviewer 2, the paper needs to be more than "he found this, she found that…" (ie 'PhD introduction') style of review. In a response to reviewer 2 some changes have been made but this is insufficient in my view - there should be some clear "take home messages" emerging from the different sections of the analysis that readers can easily walk away with. The authors could improve the paper in this regard by critically synthesizing the results from the literature in the form of tables and graphs and then undertaking analysis of this information that has been collated to compute means and standard deviations (e.g., some suggestions: mean TN and Nr reduction efficiency categorised for different wetland types/sizes, range of N2O efflux rates reported, denitrification rates as a function of retention time etc) of what has been reported in different contexts (surface/subsurface;

wastewater/stormwater). Ideally the product of the analysis should be a refined conceptual model, with clear statements of specific experimental work (or modelling work) that needs to be undertaken (beyond simple statements like "further studies are required on XXXX general area"). Finally, the discussion should talk about how we could engineer the systems to improve efficiency, based on the learnings from the present analysis. I do think this is possible but I do acknowledge it will require some more substantial effort. Given the significance of these recommended changes, I would expect a major revision of the paper would be required that would then be subject to a further round of review.

Authors Responses

The authors have given careful consideration to the comments and suggestions of the reviewers and editor and have spent a lot of time preparing the manuscript for re-submission. Taking the comments of the editor on board the authors have made large changes to the overall manuscript. The abstract and introduction sections of the manuscript were almost re-written to clearly highlight the aim of this review paper. In this review, we provide an overview of the current knowledge and discussion concerning the biogeochemical processes that control nutrient removal in CWs used for treating wastewater and the likely impacts of these processes on aquatic and atmospheric environments.

We have illustrated present knowledge and knowledge gaps using Figure 1 (see below).



Question marks (?) in the Figure above indicate knowledge gaps. For a holistic assessment of CWs we have proposed the research needed to close such knowledge gaps. For the research

community elucidation of such gaps will allow for a better understanding of C and N dynamics in CWs treating wastewater. In fact our point here is that many of these knowledge gaps (? In Figure 1) are currently ignored completely in CW, N and C removal calculations i.e. they are not even considered using default values e.g. contribution of denitrification, DNRA, anammox etc…. An exception here is IPCC default values for GHG – 0.25% of input N for N_2O and so on. Even these default values (i.e. IPCC) need further refinement across CW types, climate, vegetation etc.

At present our understanding of such systems is based on traditional influent-effluent balance scenarios that infer nutrient removal efficacy. This paper highlights that such an approach does not go far enough and therefore our understanding cannot be sufficient. Figure 1 highlights all of the areas which need further attention and we offer research solutions to gather this much needed data. The various pathways that deliver pollutants and greenhouse gases from CWs to the aquatic and atmospheric environment were evaluated towards a complete balance of C and N in the system from source to receptors. The previous reviews made on this area mainly had focused nutrient removal efficiency with a conventional input – output balance which does not include the mechanisms of nutrient removal and the fate of the removed nutrients. We have now focused on C and N equally in the revised manuscript. Overall, all the sections of the paper were almost re-written with in-depth analysis and synthesis of the published literature. Nutrient removal efficiencies and greenhouse gas emissions data were summarised in tabular forms and mean (±SE) values were calculated for various CW types and managements. We cannot yet suggest any best CW type or plant species with the design optima in terms of nutrient removal and GHG mitigation because the research based information are not yet abundant. For this, transformation kinetics of C and N and net GHG emissions through all possible pathways are required to provide a holistic assessment.