This manuscript describes differences in greenhouse gas fluxes measured continuously or discretely from two onsite wastewater treatment systems that include secondary treatment as part of the treatment train: one with a rotating biological contactor, the other with a coconut husk media filter. The treated water is dispersed to a soil treatment unit and, in both cases, untreated septic tank effluent is also dispersed to the STU. Comparisons of flux values obtained using continuous and discrete measurements are made for the septic tank, the soil above the STU, and the vents at the end of the pipes that deliver effluent to the STU. GHG fluxes from the STU are compared to those from a Control area.

We thank the reviewer for their time and thoughtful comments to improve this manuscript.

There are a number of issues that I think need to be addressed:

1. The difference in $CO_2$ flux between Control and STUs is often negative, that is, the STU is somehow acting as a sink for $CO_2$. The possible mechanism(s) by which this takes place are not really discussed in the paper. Very few microbial processes assimilate $CO_2$ in wastewater (e.g., autotrophic ammonia oxidation), and these would likely be minimized by both secondary treatment processes, which promote ammonia oxidation before it reaches the STU. One large difference between the Control and STU soils is the absence of subsurface horizons in the latter, which would have been removed to install the effluent delivery system. The removed soil would contribute to $CO_2$ flux at the soil surface which, when compared to Control soil, would have a lower $CO_2$ The authors should, then, reconsider comparisons with Control soil, not only for $CO_2$, but for all three gases (assuming they don't have data for an STU that did not receive effluent), since gross consumption and production of $CH_4$ and $N_2O$ can take place in the "missing" soil.

   We acknowledge the comment regarding the lack of a "true control" raised by the reviewer. As the study sites were built before this study commenced, we were unfortunately not able to include the construction of this "true control", i.e. gravel trenches not receiving any effluent. However, we do clearly acknowledge the value of such a control measurement for future studies and will include this into the overall discussion of the results and Conclusions section of the manuscript. The framing of observed fluxes in comparison to control soils will be revised accordingly and a clearer distinction between the soil treatment unit fluxes and natural soil fluxes will be included in the revised manuscript. For the time being, we understand that the control as conceptualised in this this study, might not be a "true control" but rather a "best available control" in the field.

   The reviewer makes a valid point with respect to the potential difference between the undisturbed soil control site and the area above the STU in that soil needed to be removed to install the gravel and trenches. This may make some difference, but it should be noted that the soil removed for the trenches was the subsoil (with very little organic matter in it) and that once the trenches were installed, the more organic rich topsoil (where presumably most of the natural control carbon cycling from the vegetative grass layer is occurring) was replaced. Of course, this soil had been disturbed and so this may account for some of the differences highlighted by the reviewer, but the method of construction does need to be considered in the comparison. Another point is that the width of the trenches is only 0.5 m wide which corresponds to a relatively minor fraction of area of the STU from which soil was

excavated. As can be seen from Figure 1 the gas sampling was randomly spread out across the whole STU know to be receiving effluent and so most of the chambers would be sitting on undisturbed soil profiles (similar to the control). These are all interesting points which will be brought into the discussion of the revised paper.

In the revised manuscript we will also adjust the phrasing of fluxes as net fluxes which should hopefully make the interpretation of the results clearer for a reader.

2. There are several published studies on GHG emissions from secondary treatment units that show that these can be considerable. The treatment units used in this study both rely heavily on microbial processes to remove and transform C and N, which produces $CO_2$ and $N_2O$. In addition, mechanical mixing and/or turbulent flow in these units tends to result in loss of $CH_4$ and $N_2O$ form effluent to the atmosphere. In the absence of values for these emissions, the flux values that were measured lack context. Differences in flux between secondary treated effluent and tank effluent could help provide some context.

   Due to design and access limitations, we were not able to use the current sampling methodology to assess GHG emission directly from the secondary units. We understand, that this limits the overall applicability of the results for system-wide emissions. However, this manuscript presents the first data set of this spatial and temporal scale for on-site wastewater treatment systems, including long-term and discrete measurements from both septic tanks and soil treatment units. As there are tens of different secondary units commercially available on the market, an assessment of this treatment step would be very system-specific and emissions will hugely vary among the available technology options. We thus, limit this study to the parts of the treatment train (septic tank for primary treatment and soil treatment unit for effluent dispersal) that are most likely present in a majority of on-site systems. In Ireland for example, septic tanks with percolation trenches account for an estimated 89% of all on-site systems. In the revised manuscript we will, however, strive to include a selection of results regarding GHG emissions from said studies in our Discussion to help contextualize the result of our study.

3. There is, in general, very little discussion of biogeochemical processes that could explain results in this paper, and limited discussion of results in the context of the current published literature. For the most part flux values are reported and compared within the study, without getting into the biogeochemical and/or abiotic processes that may that drive these in the soil or the effluent. It may be that Biogeosciences is not a good match for this work.

   The reason that we choose Biogeosciences as a journal to publish our work in is that the journal's stated aim is "*dedicated to the publication and discussion of research … on all aspects of the interactions between the biological, chemical, and physical processes in terrestrial or extraterrestrial life with the geosphere, hydrosphere, and atmosphere. The objective of the journal is to cut across the boundaries of established sciences and achieve an interdisciplinary view of these interactions*. We feel that our study does match these aims as it is studying with the effluent percolating through the soil (i.e., the geosphere and hydrosphere) and is quantifying the production of GHGs to the atmosphere. The contaminant attenuation of effluent

as it percolates through soil involves many different microbiological, chemical and physical processes (as has been investigated in research studies by us and others into such on-site treatment processes and discussed with appropriate references in the Introduction section). However, rereading our paper now, the reviewer is correct that we do not elucidate very much on how these processes are acting to produce the fluxes of GHGs that we are presenting in this paper, which we will address in a revision.

The fate of C in the soil is briefly discussed in the current version of the manuscript (L399-406 + Conclusion section) as previous indicated the presence of methanogens in the top layer of soil above treatment trenches. However, we will adjust the discussion of our results to better reflect the potential C & N processes and pathways in the soil in light of these studies. It should be noted that we are currently carrying out studies on the sites to investigate the microbial diversity at different depths within the soil in the soil treatment units which is providing additional insights into the relative abundance of different microorganisms present. We are particularly interested in those organisms which are key with respect to the production of GHGs (e.g. the location of methanotrophs versus methanogens, nitrifiers vs denitrifiers, including annamox bacteria etc.). However, this sort of in-depth study will need to be the subject of a different paper in the future as it would be too much to include in this already very long paper.

4. Most researchers working in this area will not have access to the equipment needed for continuous measurements of GHG fluxes; rather, discreet flux measurements are more likely to be made by most. As such, the results of this study could be made more useful by developing a minimum data set (spatially and temporally) required to approximate the accuracy of flux estimates made using continuous measurements. Although I understand this has clear limitations related to climate, treatment train, etc., it would be a good start, and a meaningful contribution to the field.

   Yes, this is a very good suggestion. We are planning to make the full data set available under Open Access licensing upon publication of the manuscript, thus enabling other researchers to identify such minimum data sets best suitable for their respective site specification based on our data. However, we do wholeheartedly agree that access to this kind of continuous measurement equipment will be a limiting factor for future studies and find the idea of creating a basic protocol for gathering a minimum data set extremely intriguing and useful for the field. Hence, as suggested, we will include suggestions for such a minimum data set required for future studies in the revised version of the manuscript.