Reply to comments made by two reviewers and the editor

Reply to comments made by the editor:

Comment 1: The manuscript might benefit from a clear statement that explains your intention and implications of your findings. Perhaps you could slightly rephrase the text, and add it to the end of the discussion or conclusion?

Reply: As suggested, a clear statement that explains our intention for this study was added to the conclusion part of the text: "This study aims to raise the problem of the systematic, 3D heterogeneities around a dripper, present relevant measured and simulated results, and propose relevant methodologies assisting in deciding regarding the size and placement of static chamber bases in drip irrigation". (Lines 434 - 436 in the revised manuscript). The implications of our findings were discussed by adding the following statement: "It will take many users in many conditions (soil types, wetting patterns, variable N carbon (C) and O regimes), rather than a single study, to tell whether these methodologies are constructive". (Lines 456 - 458 in the revised manuscript).

Comment 2: Similarly, the final sentence of the abstract may suggest to some that the estimated optimal size of the chamber is the key finding of your paper. Perhaps it would be better to end with a brief statement that captures (some of) the message in the text above?

Reply: As suggested, a brief statement about the limitations of our findings were added to the end of the abstract: " Further studies under variable conditions (soil types, wetting patterns, nutrient availabilities), rather than a single study, are needed to test the constructiveness of the suggested methodologies". (Lines 26 - 28 in the revised manuscript).

Comment 3: L299-303. This sentence is overly long and unclear.

Reply: As suggested, the text was rewritten and divided into two sentences: Lines 302-303 in the revised manuscript: "What counts is only their product αd_{emit}. It also decreases with increasing dimensionless reference depth below the emitter (αd_{ref}) (Fig. 8A)".

Comment 3: L311-313. This sentence is overly long and unclear.

Reply: As suggested, the text was rewritten. Line 311-13 in the revised manuscript: These relatively high $N_2O_{Adjacent}$ fluxes likely reflect conditions that are more conducive to denitrification (e.g., higher WFPS) or nitrification (e.g., higher NH_4^+concentrations).

Reply to comments made by Reviewer #1:

Comment 1: Please correct minor typos as marked on the attached pdf.

Reply: Thank you for noticing the typos. All were corrected.

Comment 2: Consider expressing fluxes in units more familiar to readers, e.g., g/m^2/d, even as a one-off comparison.

Reply: The units were changed to g m^{-2} d^{-1} in all the graphs and text as suggested.

Comment 3: - L236 check text and caption for Fig. 3 align

Reply: Indeed, the caption had a typo where NO_3 and NH_4 were mixed. It was corrected.

Comment 4: L266-267 Add text to indicate why higher WFPS or higher N concentrations favour higher N_2O. See pdf.

Reply: As requested, an explanation was added to the text "Suggests N_2O_{In} results from conditions more conducive to denitrification (e.g., higher WFPS) or nitrification (e.g., higher NH_4^+ concentrations) (Lines 312-313 in the revised text).

Comment 5: L319-333 is this in the results? Shift to the results section.

Reply: As suggested, some of the text was transferred to the results section (Lines 252 - 253 and lines 295-298, in the revised manuscript). The revised section now reads" Comparison of cumulative N_2O emission measured in 2018, 2019, and 2020 and the simulated cumulative emissions (over 60 days) showed the N_2O_{In} flux to be 40% – 70% higher than the $N_2O_{Adjecent}$ (Fig. 7B)".

Comment 6: P10 a lot of this text reads as results or methods. Suggest some is shifted to appropriate sections and discussion revised.

Reply: As suggested the text on page 10 was modified. A substantial portion of the text was moved to the Methods section under a new subsection titled "2.2.4.

Recommendation on the diameter of the chamber's base" (lines 213 – 235 in the revised manuscript). Another part was moved to the results section (lines 299 – 310 in the revised manuscript).

Comment 7: For the conclusion suggest "...we concluded that static chamber methodology, which requires the insertion of bases into the soil, underestimates N_2O emissions when used in drip irrigation........This effect can be mitigated through optimizing chamber design. A nomogram is proposed..."

Reply: We adopted the suggested edits to the text and modified it to "… we concluded that static chamber methodology, which requires the insertion of bases into the soil, underestimates N_2O emissions when used in drip irrigation (lines 438-439 in the revised text). "These effects can be mitigated through optimizing the chamber design. A nomogram is proposed…" (lines 442-443 in the revised text).

Reply to comments made by Reviewer #2

Comment 1: Calculating cumulative emissions – why don't you use linear interpolation? I do not think that your results will change, but I think that use of arbitrary Q10 is not any better than to estimate cumulative/daily flux using linear interpolation. Calculation of daily emissions can be done by dividing annual accumulative emissions by number of included in cumulative flux estimation days.

Reply: We thank the reviewer for raising that point. All calculations were modified as suggested using linear interpolation and numerical integration between sampling times. The text was modified accordingly: "…by linear interpolation and numerical integration between sampling times. Cumulative N_2O flux estimates for N_2O_{In} and $N_2O_{Adjacent}$ were taken as the average of the cumulative fluxes of the 12 individual chambers, each". (Lines 115-117 in the revised manuscript)

Comment 2: Why do you think that your modeled emissions are better than observed? I think that for real answer if static chamber methods under- or overestimate real fluxes a comparison between static chambers and eddy-covariance based measurements is needed. And, **Comment 3**: You are using 3D flow model and some simple assumptions, you may model the water flow well, but I doubt that you can model N_2O emissions. At least DayCent model, after ~30 years of development cannot.You are using 3D flow modeld and some simple assumptions, you may model the water flow well, but I doubt that you can model N_2O emissions. At least DayCent model, after ~30 years of development cannot.

Reply: Our field measurements show that the measured N_2O flux is highly affected by the location of the chamber's base relative to the drip line. We agree that eddy-covariance based measurements may provide a good tool to validate our field measurement on a larger scale. However, this method is limited to large areas and cannot distinguish between different zones in the orchard (wetted, windrow, etc.). Further, it might be biased by emissions that come from the trees. Hence, in the discussed setup, eddy-covariance could not be used.

Please note that we do not think that our modeled emissions are better than the measured/observed emissions. Further, we do not think that the simple assumptions used by us to model N_2O fluxes accurately capture the "true" N_2O flux, as rightfully commented by the reviewer. Yet, our results raise a question as to what is the "correct" or the most representative way to capture the true ambient N_2O emissions around a single dripper when using static chambers.

Since N_2O fluxes are mainly driven by N availability and form (i.e., nitrate vs. ammonium), oxygen availability, and soil water content [readily expressed as the ratio between the volumetric water content and the porosity (WFPS)], we decided to use a robust 3D flow model to study the effect that a chamber base may have on water and N distribution in the topsoil. The primary assumption for both the 3D flow model and the DIDAS model simulations/computations was that the ambient N_2O emissions (i.e., not affected by the base) would be those emitted from locations with minimal perturbation by the base. That is, places where the N-form concentrations and distributions and the water content are not affected by the base.

With this assumption in mind, the model was used to:

1. Evaluate the impact of the dripper location relative to a base (i.e., inside, adjacent, a few cm away, etc.);
2. Find the optimal base diameter;

Comment 4: I am wondering if your proposed method to calculate optimal chamber (base) size for field measurements of soil N_2O emissions (monogram) will provide better flux estimation. I think that few weeks of additional measurements comparing the proposed "optimal base size" with bases used to acquire data for the manuscript under discussion will improve the manuscript. If you are claiming that using modeled flow of water and nutrients, you can determine the optimal chamber – prove it.

Reply**:** In continuation to the reply to comments 2 and 3, to date, the standard method for static chambers calls for the use of basses. Hence, there is no way to get the "true" ambient N_2O emissions from a single dripper. With that in mind, and the model's limitation in predicting N_2O emissions (simplified assumptions), we do not see how few weeks of additional measurements comparing the proposed "optimal base size" with bases used to acquire data will benefit the manuscript.

Our intention was to merely raise the problem of the systematic, 3D heterogeneities around a dripper, to present relevant, simulated results and to propose relevant methodologies assisting in deciding regarding the size and placement of the base. It will take many users in many conditions (soil types, wetting patterns, carbon, nitrogen and oxygen regimes), rather than a single study, to tell whether these methodologies are constructive or not.