Final author response to the reviewers and editor

We thank the reviewer and the editor for the positive and encouraging assessment of our manuscript and for recognizing its relevance to the trace gas community. For clarity, our response to the reviewer comment is provided in blue text. The line number reference refers to the revised manuscript with tracked changes enabled.

During the final review of the manuscript, we identified an error in the results and discussion sections. We had previously stated that flux selection was not possible due to missing HMR and kappa values for most of the GC fluxes. Upon re-evaluation, we found that some of these values were incorrect or/and that the distinction between fluxes for which HMR models were available and those fluxes for which the flux selection procedure actually selected HMR models had not been clearly made. This distinction has now been clarified and we now mention both values for all flux-selected GC fluxes (CO_2, CH_4, N_2O). This revision is intended to improve the overall clarity of the results. It does not affect the interpretation of the results or the discussion of the manuscript in a significant way.

We apologize for that minor error and hope that the revised formulation meets the expectations of the journal. The corresponding changes were made at four locations in the manuscript (l. 225–230, l. 262–266, l. 294–298 and l. 409–413) and are indicated using track changes. Furthermore, we have double-checked the values in table 1 and confirm that they are correct and have added a footnote, as suggested by the reviewer.

Review comment #1

I had already reviewed this manuscript (egusphere-2025-2862) after its first submission. I consider it an important contribution to the trace gas community, because it compares two measurement techniques, one of which is currently the most commonly used method (GC) for measuring and calculating trace gas fluxes, and the second is a currently very promising alternative (laser). Although laser techniques have been in use for a long time, a relatively new laser was used here, which, due to its light weight, can be carried on the back without much effort and therefore requires less complex peripherals for the measurements! The authors have either (mostly) taken my suggestions into account or, from my perspective, sufficiently explained why they do not want to implement my suggestions. Therefore, in my opinion, the manuscript is ready for publication. My only comment would be to recommend adding a footnote to Table 1 explaining why the figures for CO2 and N2O are identical! This is not clear to the reader! Therefore, just a very minor revision!

Regarding the reviewer's remaining comment, we agree that the identical values for CO_2 and N_2O in Table 1 may not be immediately clear to the reader. We have double-checked these values and confirm that they are correct. We have therefore added the following footnote to Table 1 (line 242–243) of the revised manuscript: "*Identical values for CO_2 and N_2O result*

from the analytical precision of the instruments (200 ppb for CO_2, 0.2 ppb for N_2O) and the very similar molar masses of both gases. Therefore, the differences are only reflected in the units, not in the actual values."