Reviewer 2

The manuscript by Keck and co-authors describes the deployment of an optical instrument for the analysis of N2O isotopes at an experimental wastewater treatment plant. Despite such analysers being commercially available for over a decade, their operation is technically cumbersome and challenging, potentially limiting the use of these analysers. The fact that the authors have succeeded in operating such an analyser fully automated in the field with high performance is welcome and very newsworthy. The authors performed three experiments: 1) analysis of data from long-term deployment, 2) variation in dissolved oxygen, 3) addition of 15N-labelled compounds. The experiments provide conclusive results that demonstrate the ability of such measurements to deliver actionable data to wastewater treatment plant operators: isotope analysis in N2O, along with knowledge on the isotopic composition of local tab water, indicate the realised/or remaining potential in N2O emissions reduction. The study is comprehensive, very well thought out, and highly relevant. I therefore find it will be a very valuable contribution to AMT.

General comments:

In my view, this manuscript is likely to be of interest to spectroscopists, biogeochemists, but also to GHG emission-focused wastewater treatment plant operators, who may not be the typical AMT audience. With this in mind, I recommend making the rationale behind the presented isotope technique more accessible to a broader audience interested in WWTP operations, which may be minor or major revisions. The authors are affiliated with a world-leading isotope laboratory, and different approaches to interpret isotope signals is common practice for the authors. However, this may not be the case for the wider audience potentially interested in this technique. I'd therefore suggest going through the manuscript and improve the story telling, guide the reader through critical thinking processes you present, take more time to spell out what is happening in the WWTP and how that relates to what you observe in the isotopes. How can you then use isotope data to infer what is happening in the WWTP and how to mitigate emissions? What isotope scenarios may a WWTP operator observe and what does this say about the processes? How are these processes reflected in your isotope data? I'd expect this will maximise the impact and interdisciplinary uptake of this valuable publication.

Reviewer 2 - Reply to the general comment:

We would like to thank the reviewer for this positive assessment of the significance and potential of the measurement method presented.

Further interpretations of our data, focusing on process engineering aspects, have been published in Strubbe et al. (2026) in the journal Water Research, where we expect to reach readers with a corresponding thematic focus. Additional datasets interpreting variability in N_2O reduction and ^{15}N-labelling are in preparation for publication. Although a detailed interpretation of data in conjunction with process parameters and supportive variables (e.g. N-substrate concentrations, carbon and oxygen content) is beyond the scope of this study we agree to add some context on the explanatory framework for the experimental results sections:

Page 21, line 424, section 3.2.1:

"*Controlling the concentration of ammonium, nitrite, and dissolved oxygen nitrifier denitrification was successfully isolated from heterotrophic denitrification and became more active at lower dissolved oxygen concentrations. Lower dissolved oxygen, higher organic carbon availability, and lower pH increased N_2O production by heterotrophic denitrification during aeration. These new insights provide a systematic framework for understanding N_2O dynamics and support the development of mitigation strategies at full-scale.*"

Page 23, line 471, section 3.2.2:

"*Isolating the N_2O-to-N_2 reduction step by on-line isotopic analysis for the first time provides insights into its key drivers, helping identify operational levers to reduce production and enhance consumption of N_2O, supporting the design of net-zero emission treatment systems. The detailed interpretation of positive or negative effects of operational factors on N_2O reduction beyond data shown here, i.e. pH, temperature, total air consumption, total suspended solids, will be published elsewhere.*"

Reviewer 2 - Reply to the specific comments:

Comment #2: Line 39: maybe "…, countries and WWTP facilities still rely on…"?

Reply: Thank you, we changed the text accordingly.

Comment #3: Line 41: However, different emission factors…

Reply: Thank you, we changed the text accordingly.

Comment #4: Line 79: maybe "mass-dependent isotope effect", instead of "normal isotope effect"?

Reply: Thank you, we changed the text accordingly.

Comment #5: Line 88: Here and everywhere else: use mole fraction or amount fraction instead of concentrations for gases. This differentiation is especially important as the manuscript includes concentrations of dissolved compounds. You could clarify that the term mole fraction is used for all gases within a gas matrix, concentrations for everything in a water matrix?

Reply: Thank you for pointing this out. We changed the wording accordingly.

Comment #6: Line 89: maybe "matrix effect" instead of "bulk gas composition"?

Reply: Thank you, we changed the text accordingly.

Comment #7: Line 97: "We demonstrate the first…"

Reply: Thank you, we changed the text accordingly.

Comment #8: Line 102: "…practical recommendation on how to…"

Reply: Thank you, we changed the text accordingly.

Comment #9: Line 116: "sections, optimal"

Reply: Thank you, we changed the text accordingly.

Comment #10: Line 117: “approaches, as”

Reply: Thank you, we changed the text accordingly.

Comment #11: Line 124: Can you comment on a potential N2O blank?

Reply: The N_2O concentration in synthetic air was below 1 ppb; a respective statement was added to page 12, line 219, section 2.3.1 and Table 1

Comment #12: Line 133: “From every 10-minute measurement interval, the first…”?

Reply: Thank you, we changed the text accordingly.

Comment #13: Line 133: Why is the number of discarded minutes variable (3-5), and what does it depend on?

Reply: The analytical setup at the WWTP was periodically used for several projects in parallel, this required us to adjust our measurement interval lengths (8 or 10 minutes). For consistency, we decided to use the last 5 minutes of each measurement interval, which lead to discarding 3 or 5 minutes depending on the interval.

Comment #14: Line 138: Can the SA gas be added to Table 1?

Reply: We added the SA gas to Table 1.

Comment #15: Line 140: “isotopic composition analysed by EMPA”, can you give a reference for the technique?

Reply: The analytical technique used for N_2O isotopic analysis at Empa is presented in Heil et al. (2014). The respective reference was added to the text (page 7, line 145, section 2.2.2).

Comment #16: Line 154: Here and everywhere else: it seems to me that the uncertainty might be within 1 significant digit? Suggest simplifying by rounding, i.e., 0.3, 0.9, 0.5, 0.2 and 0.9 permille?

Reply: Thank you, we adapted the text accordingly.

Comment #17: Line 157: Can you comment on potential offsets between EMPA and Science Tokyo from previous studies/publications?

Reply: Primary calibration gases applied at Empa were analyzed at Science Tokyo, and therefore several inter-laboratory comparisons indicated a good agreement in N2O isotope analyses between Empa and Science Tokyo. We added a respective statement and references to page 7, line 164, section 2.2.2:

"*This discrepancy is higher than differences observed in past inter-laboratory comparisons between Empa and Science Tokyo (Mohn et al., 2014; Ostrom et al., 2018) but was not further addressed as it is within the uncertainty targets of our study.*"

Comment #18: Line 174: You show in Figure 2 that the N2O mole fraction effect is variable with time. The magnitude of the time-dependent variation is massive in some examples. Can you provide a comment on your certainty that this effect is sufficiently captured across your experimental period of 1 year, even though it was only determined on 4 days? Also, I wonder if it's worth focusing on the time-dependent variation within the 11-13 ppm band, which is somewhat acceptable. The variation over the entire N2O range is very interesting but could also be shown as an Appendix figure without the insert, which covers this for 3 out of 5 isotope ratios.

Reply: We agree that the observed temporal variability of the N_2O mole fraction effect on delta values is very high and four measurements within a one-year period are insufficient to account for this effect considering the full measurement range of the presented spectrometer. As correctly observed by reviewer #2 variations within the 11-13 ppm N2O range applied are acceptable, which is demonstrated by the agreement of repeated target gas measurements**.**

We agree to show a better representation of the most relevant lower concentration range (0.5-20 ppm) and compress data in the higher concentration range, where fewer measurements were collected. This is in agreement with our response to comment #9 of reviewer #1.

Comment #19: Line 180: The authors describe data processing details before explaining the analytical setup, described in Figure 4. I wonder if describing the setup first would help the logical flow? Otherwise, when you talk about CO2 removal here, can you give a reference to Figure 4?

Reply: Thank you for the suggestion, we moved the section about the analytical setup before the data processing section and now refer to the relevant section in text: "[...] since any produced CO_2 is removed prior to analysis (see section 2.3.1 for details)."

Comment #20: Line 182: If this is pure N2, wouldn't this experiment vary O2 as well as Ar at the same time, causing this to show the effect of simultaneously varying both gases? You speculate about the Ar effect in line 157, does it matter here?

Reply: We agree that in the described experiment both oxygen and argon concentrations are varied, which is not discussed. In fact, diluting Cal1 with N_2 and synthetic air to 12 ppm N_2O and a target O_2 concentration of 5% / 10%, e.g. a reduction in O2 by 15.9% / 10.9%, reduces argon by 0.69% / 0.55%. According to our experiments published in Harris et al. (2020), gas matrix effects of argon are expected to increase oxygen effects by around 10%. We added this information to page 10, line 197, section 2.2.4:

"*As N_2 did not contain Ar, in parallel with a decrease in O_2 by e.g. 16 % Ar dropped by 0.7 %. Earlier studies (Harris et al., 2020) indicate that the gas matrix effect attributed to O_2 for an OA-ICOS analyzer as tested here might therefore be overestimated by around 10 %, and our uncertainty estimates (section 2.5.2) are rather conservative.*"

In addition, we added a respective statement to the legend of figure 3 (page 11, Line 213, section 2.2.4): "*Presented O_2 gas matrix effects are conservative, i.e. overestimated, due to a parallel decrease in Ar mole fractions (0.5 % Ar per 10 % O_2).*"

Comment #21: Line 186: Maybe "…was linear over the entire…"?

Reply: Thank you, we adapted the text accordingly.

Comment #22: Line 189: Maybe "was measured using a PG…"?

Reply: Thank you, we adapted the text accordingly.

Comment #23: Line 206: The typesetting of the letters in the bracket seems a bit off.

Reply: Thank you for pointing this out. We fixed the typesetting in the new manuscript.

Comment #24: Line 220: It is not clear to me if all isotope measurements of the entire year are corrected for the same O2 difference, or each 5 minute average isotope value for the O2 difference corresponding to those 5 minutes?

Reply: Thank you, we reformulated the corresponding sentence to (page 16, line 311, section 2.5): "*With respect to the O_2 mole fraction correction of δ-values an offset correction to average O_2 mole fractions in the analyte gas (20.6 % O_2) was applied over the entire measurement period, instead of a correcting for actual O_2 mole fraction values*"

Comment #25: Line 229: I do not understand this section. Please clarify.

Reply: We reformulated the section as mentioned above in response to comment #13 of reviewer #1.

Comment #26: Line 235: What is the lower-left region? Does this correspond to a figure?

Reply: Yes, the statement refers to Fig. 5 (new manuscript). For clarity we highlighted the 5 lowest-ranked points and plotted the figure as Fig. 5 in the main text.

Comment #27: Line 236: What are the 5 lowest-ranked points? Are they shown in Figure A1, if so, can this be visualised/described with more clarity?

Reply: Please see reply above.

Comment #28: Line 239: I'd suggest moving Figure A1 into the main text.

Reply: Please see reply above.

Comment #29: Line 248: How is the repeatability of the instrument determined?

Reply: By the standard deviation of repeated drift-corrected measurements of the target gas. See our response to comment #15 of reviewer #1.

Comment #30: Line 269: How many measurements fell outside the 11-13 ppm band, what percentage was discarded?

Reply: During standard WWTP operations from September to December 2024 the proportion of data outside of the 11-13 ppm band was 53 %. We included this in the current manuscript as follows (page 12, line 219, section 2.3.1):

"*The target mole fraction of the dilution system was set to 12 ppm N_2O and data with N_2O mole fractions outside the range of 11 to 13 ppm were discarded (53 % of data).*"

Comment #31: Line 282: Could "retained" be a better word to describe what this filter does?

Reply: Thank you, yes indeed, we adapted the text accordingly.

Comment #32: Line 286: Does the CO2 sensor not have a fixed flow rate?

Reply: The flow to the CO_2 sensor was throttled to approximately 80 ml / min, to ensure a constant overflow passing the sensor. We updated the Figure 5 (new manuscript) to include the overflow after the CO_2 sensor for clarification.

Comment #33: Line 294: "The reactors received subsamples of the local municipal wastewater"?

Reply: Thank you for indicating an unclear formulation. We changed the sentence to: "*The reactors were fed with local municipal wastewater taken directly from the sewer*".

Comment #34: Line 303: Maybe "The following representative datasets were obtained under three different experimental conditions and are used to demonstrate the usability and applicability of our analytical; setup."?

Reply: Thank you for proposing a better formulation that is now included in the text for better understanding.

Comment #35: Line 305: I suggest be more clear: "Experiment 1: To determine…" and then refer to this in the results/discussion section.

Reply: Thank you. We changed the text accordingly

Comment #36: Line 306: Here and elsewhere, the text talks about "standard operation", but it is not always clear if this refers to the measurement setup of the WWTP. Could this say "standard WWTP operation"?

Reply: Thank you. We changed the text accordingly

Comment #37: Line 319: "different operational conditions of the WWTP."?

Reply: Thank you. We changed the text accordingly

Comment #38: Line 335: "11 to 13 ppm N2O target range."

Reply: Thank you. We changed the text accordingly

Comment #39: Line 348: "…the isotopic measurement presented here will be capable…"

Reply: Thank you. We changed the text accordingly

Comment #40: Line 349: It might be worth mentioning that these data are in fact "actionable", therefore high impact and potential to influence decisions by WWTP operators.

Reply: Thank you, we added the following text to 3.1 Performance of the measurement setup (page 19, line 390, section 3.1):

"*Thus, the isotopic data are actionable, enabling the identification of the root source of emissions and their underlying drivers in near real time. This information can support evidence-based operational decisions and targeted mitigation strategies, thereby enhancing the ability of WWTP operators to reduce N_2O emissions effectively.*"

And to the conclusions (page 26, line 523, section 4):

"*Thus, the isotopic data are actionable and directly relevant for improved process understanding and optimization to support evidence-based operational decisions targeting the reduction of N_2O emissions from WWTPs*."

Comment #41: Line 351: I wonder why Figure 5 does not include SP?

Reply: We included SP in Figure 6 (new manuscript).

Comment #42: Line 357: “Experiment 1: Constraining…”? For a reader who is not familiar with the experiments, relating this section directly to the experiment description would make it a lot easier to follow. In my view, this is the text location from where onwards it could be very beneficial to get more guidance on the assumptions and what has been done. Why do you do what you do, why are several data important.

Reply: Thank you for this indication leading to better readability of our manuscript. We suggest the addition of the following text:

"*To determine the predominant microbial N_2O biological production pathway, we analysed the N_2O isotopic composition over several months (September to December 2024) under standard WWTP operation, i.e. at a dissolved O_2 (DO) concentration setpoint of 2 mg L^{-1} (±0.5 mg L^{-1}) and without N-substrate additions.*"

To rationalize discrepancies between measurement results and indicated source signature areas the following text was added to page 21, line 415, section 3.2.1:

“*This interpretation may strike the reader as odd, as the data points so obviously lay outside of the shaded source signature areas that are traditionally visualised as boxes in dual isotope plots. These source signature areas (see Fig. 7 and Fig. 8A) are based on a limited number of pure culture studies and laboratory incubations (Yu et al., 2020) and therefore cannot be perceived as hard limits but require refinement to fully capture the dynamics of complex mixed microbial communities as are found in wastewater treatment reactors. Thus, the isotopic source signatures and their overlaps will be subject to change with increased understanding of the complexities of managed and natural N_2O source systems. To determine system-specific source signature areas and to robustly differentiate between the contributions of nD and hD to N_2O formation, it requires further dedicated experiments that aim at stimulating both processes independent from each other […]*”**

Comment #43: Line 361: I am not sure if “corrected” is the right concept here. The goal is to observe the difference between H2O and N2O. Why is that important, what does Dd18O tell us? Could this be described with readers in mind who are not isotope specialists?

Reply: We now included a section under "data processing" addressing the dependence of $\delta^{18}O$ (N_2O) on $\delta^{18}O$ of ambient water (page 14, line 256, section 2.4):

"*As oxygen exchange between nitrogenous oxides and H_2O can occur in abiotic systems but also mediated by microbes (Kool et al 2007), the $\delta^{18}O$ of N_2O partly reflects sthe $\delta^{18}O$ of the dissolution water. This dependency was considered by subtracting the $\delta^{18}O$ signature of the local tap water ($\delta^{18}O$ (H_2O) = −11.2 ‰) from the $\delta^{18}O$ of N_2O before further interpretation.*"

Comment #44: Line 363: It took me a while to understand that the slope of 0.37 is the slope you show in Fig 6 from your data of Experiment 1, and that it is not the slope reported by Yu 2020. Can this be clarified in the text, and maybe also included in the figure itself? At a later point, you mention the range in slopes reported by Yu 2020. Maybe the range reported by Yu 2020 can be presented here, when Yu 2020 is mentioned first to discuss your slope in the context of their findings?

Reply: We updated Fig. 7 (new manuscript) to include the regression line formula and changed the text to (page 21, line 408, section 3.2.1):

"*Concurrent changes in SP and $\Delta\delta^{18}O$ (N_2O, H_2O) along a straight line with a slope of 0.37 indicate successive reduction of N_2O to N2 as the slope falls within the range defining the reduction line, spanning from 0.23 to 0.98 (Yu et al., 2020).*"

Comment #45: "From Fig 6, we can infer nitrifier denitrification (nD) or heterotrophic denitrification (hD)…"

Reply: Thank you, we changed the text accordingly.

Comment #46: Line 369: Can you explain more broadly why you think your data show nD and hD as the most dominant sources, even though your data are outside the corresponding shaded areas in Fig 6?

Reply: Thank you, this question arises easily when looking at the dual plots. We changed the text accordingly to (page 21, line 411, section 3.2.1):

"*From Fig. 7 we can infer nD or hD as the dominant N_2O production process, as measurements fall within the range of isotopic signatures expected for denitrification processes with a minimal effect of N_2O reduction (Yu et al., 2020) and are in accordance with other studies on wastewater treatment (Gruber et al., 2022; Wunderlin et al.,*

2013). Furthermore, the absence of elevated SP values (> 30‰) suggests that Hy was not a source term during our measurements (Frame and Casciotti, 2010; Sutka et al., 2006; Wunderlin et al., 2013; Yu et al., 2020). This interpretation may strike the reader as odd, as the data points so obviously lay outside of the shaded source signature areas that we traditionally visualized as boxes in dual isotope plots. These source signature areas (see Fig. 7 and Fig. 8A) are based on a limited number of pure culture studies and laboratory incubations (Yu et al., 2020) and therefore cannot be perceived as decisive limits and require refinement to fully capture the dynamics of complex mixed microbial communities as are found in wastewater treatment reactors during regular operation. Thus, the isotopic source signatures and their overlaps are subject to change with increased understanding of the complexities of managed and natural N_2O source systems. To determine system-specific source signature areas and to robustly differentiate between the contributions of nD and hD to N_2O formation, it requires further dedicated experiments that aim at stimulating both processes independent from each other. A series of such experiments applying the analytical toolkit developed in this study is described in Strubbe et al. (2026)."

Comment #47: Line 377: Can you add the slope value or the equation for the linear fit to the figure?

Reply: We included the equation in Fig. 7 (new manuscript).

Comment #48: Line 381: "…with progressive heterotrophic N2O reduction."?

Reply: We changed the text accordingly.

Comment #49: Line 383: "Experiment 2: Assessing…"?

Reply: Adapted accordingly.

Comment #50: Line 387: "The slope of our reduction line in Experiment 2 (0.8…)", or can it be clarified further that this slope is based on different data, excluding Experiment 1? Also, can you comment on why the slopes are different between Experiment 1 and 2? What does this tell someone who is not an expert in isotope techniques?

Reply: We reformulated the text to (page 22, line 441, section 3.2.2):

"*The enrichment of SP, and $\delta^{18}O$ caused by N_2O reduction in experiment 2 is characterised by a slope of 0.80 (Fig. 8A), which falls within the data range defined by Yu et al. (2020) for such a process, supporting the validity of the here reported N_2O reduction fractions. As the two experiments (1 and 2) are separated in time, the difference in the reduction line slops is likely related to a shift in the active microbial community over time. This variability is within the range seen in scientific literature under similar settings (Gruber et al., 2022; Yu et al., 2020, Strubbe et al., 2026), but still represents an open research question worthwhile exploring in more detail*."

Comment #51: Line 389: Here and everywhere else, consistent use of "DO concentrations" would help clarity.

Reply: Thank you, we changed the text accordingly.

Comment #52: Line 391: I am not sure I understand this section properly and suggest this to be clarified further. How do those redN2O percentages relate to DO and isotopes? Can you guide the reader by simply explaining something general along the lines of "if this goes up, that goes down because of that and that." And then "what we see in our data is… suggesting that… Therefore, isotopes can highlight times when DO can be optimised to mitigate N2O emissions. "

Reply: Thank you, we changed the wording to make it more accessible also for readers that are not familiar with isotopic analysis (page 23, line 447, section 3.2.2):

"*In general, higher N_2O reduction was seen in the reactor that was set to a lower DO concentration compared to the reactor with a higher DO concentration setpoint. This can be seen in Fig. 8A, as the data from the reactor with a lower DO setpoint are higher up on the SP and $\delta^{18}O$ scales compared to those from the other reactor. Intuitively, this can be explained by the fact that enzymes responsible for N_2O reduction preferentially cleaving the bonds between lighter isotopes, i.e. between ^{14}N-^{16}O rather than between ^{15}N-^{16}O or ^{14}N-^{18}O, which leads to an accumulation of molecules with ^{18}O or ^{15}N in the central position in the residual, unreduced N_2O fraction. When applying Eq. 8 to calculate the N_R for both scenarios, we can quantify the difference in f_{N2} between the treatments. The low DO concentration setpoint of 0.5 mg L^{-1} led to an increase in the fraction of reduced N_2O by 40 % (±2 %) compared to the DO setpoint of 2 mg L^{-1}. There-*

fore, on-line isotopic analysis can highlight periods during which the DO setpoints can be optimised for N_2O emission reduction."

Comment #53: Line 397: Can "decreasing O2 concentration" be changed to "decreasing DO concentration", or is this referring to O2 mole fractions in the gas phase?

Reply: Thank you, we changed it accordingly.

Comment #54: Line 398: Maybe "In contrast, Rees…."?

Reply: Thank you, we changed it accordingly.

Comment #55: Line 401: What species are you thinking of, "related to microbial species"?

Reply: Thank you, indeed we refer to microbial species here.

Comment #56: Line 405: I can't see the colour/time gradient in the plotted symbols in A and B. Can the slope be written in the figure? The caption talks about % uncertainty, the axis is in fractions of 1, which seems inconsistent. Can all be in %?

Reply: We changed the colour gradient and converted the y-axis to percent.

Comment #57: Line 413: "Experiment 3: 15N…"?

Reply: Adopted.

Comment #58: Line 421: Can you expand the explanation of your thinking about the stagnation, i.e., 15N bulk initially increases because of that, then stagnates when NH4 is depleted, while Dd18O still increases. What is happening when Dd18O still changes, but not 15N bulk? Why is NO2 still changing for a bit, and why is NO3 changing throughout the entire experiment? I can't see much difference in the colours of the symbols in panel A. Is there a better way to show the length of time that has passed for each data point?

Reply: We thank the reviewer for this insightful comment. We agree that the observed behavior of the isotopic signatures during the labelled-N experiment raises several interesting process-related questions. However, we would like to emphasize

that the primary objective of the present manuscript is the development and validation of the analytical approach and the demonstration that the system is capable of resolving low-level ^{15}N labelling of the nitrogen pool. A detailed mechanistic interpretation of the isotope dynamics observed during the incubation experiments is beyond the scope of this methodology-focused paper. We are currently preparing a follow-up manuscript that will specifically focus on the interpretation of the labelled-N experiments, including the temporal evolution of the ^{15}N and oxygen isotope signatures and the associated nitrogen transformation processes. Rather than providing a necessarily speculative and possibly incomplete discussion here, we believe that a dedicated treatment of these results is more appropriate in that context. To avoid overstating our interpretation, we have therefore kept the discussion in the present manuscript focused on the analytical performance and proof-of-capability aspects of the experiment.

Regarding the Figure 9 (new manuscript), we appreciate the reviewer's observation. We have revised the color gradient in Panel A to improve the distinction between symbols and to provide a clearer visualization of the temporal progression of the experiment.

Comment: Line 423: Isotope conventions are -27 should be shown as –27, delta symbols are italicised.

Reply: Thank you, we adjusted "–" signs and δ accordingly.

Comment: Line 430: "standard WWTP operation"?

Reply: Thank you, we adapted the text accordingly.

Comment: Line 436: This is an unconventional way to refer to site preference. Why not use SP?

Reply: Thank you, we changed the text accordingly.

Comment: Line 452: "…greenhouse gas emission reduction."?

Reply: Thank you, we adapted the text accordingly.

References:

Frame, C.H., Casciotti, K.L., 2010. Biogeochemical controls and isotopic signatures of nitrous oxide production by a marine ammonia-oxidizing bacterium. Biogeosciences 7, 2695–2709. https://doi.org/10.5194/bg-7-2695-2010

Gruber, W., Magyar, P.M., Mitrovic, I., Zeyer, K., Vogel, M., Von Känel, L., Biolley, L., Werner, R.A., Morgenroth, E., Lehmann, M.F., Braun, D., Joss, A., Mohn, J., 2022. Tracing N2O formation in full-scale wastewater treatment with natural abundance isotopes indicates control by organic substrate and process settings. Water Res. X 15, 100130. https://doi.org/10.1016/j.wroa.2022.100130

Heil, J., Wolf, B., Brüggemann, N., Emmenegger, L., Tuzson, B., Vereecken, H., and Mohn, J. 2014. Site-specific 15N isotopic signatures of aiotically produced N2O. Geochim. Cosmochim. Ac., 139, 72–82, https://doi.org/10.1016/j.gca.2014.04.037

Harris, S.J., Liisberg, J., Xia, L., Wei, J., Zeyer, K., Yu, L., Barthel, M., Wolf, B., Kelly, B.F.J., Cendón, D.I., Blunier, T., Six, J., Mohn, J., 2020. N2O isotopocule measurements using laser spectroscopy: analyzer characterization and intercomparison. Atmospheric Meas. Tech. 13, 2797–2831. https://doi.org/10.5194/amt-13-2797-2020

Kool, D.M., Wrage N., Oenema1, O., Dolfing, J., Van Groenigen, J. W., 2007. Oxygen exchange between (de)nitrification intermediates and H2O and its implications for source determination of NO3 and N2O: a review. Rapid Commun. Mass Spectrom, 21, 3569–3578, doi: 10.1002/rcm.3249

Mohn, J., Wolf, B., Toyoda, S., Lin, C.-T., Liang, M.-C., Brüggemann, N., Wissel, H., Steiker, A.E., Dyckmans, J., Szwec, L., Ostrom, N.E., Casciotti, K.L., Forbes, M., Giesemann, A., Well, R., Doucett, R.R., Yarnes, C.T., Ridley, A.R., Kaiser, J. and Yoshida, N., 2014. Interlaboratory assessment of nitrous oxide isotopomer analysis by isotope ratio mass spectrometry and laser spectroscopy: current status and perspectives. Rapid Commun. Mass Spectrom., 28: 1995-2007. https://doi.org/10.1002/rcm.6982

Ostrom, N.E., Gandhi, H., Coplen, T.B., et al. 2018 Preliminary assessment of stable nitrogen and oxygen isotopic composition of USGS51 and USGS52 nitrous oxide reference gases and perspectives on calibration needs. Rapid Commun Mass Spectrom.; 32: 1207–1214. https://doi.org/10.1002/rcm.8157

Strubbe, L., Keck, H., Magyar, P.M., Mohn, J., Joss, A., Froemelt, A., 2026. Activating specific N2O production pathways to understand emission dynamics in wastewater treatment. Water Res. 296, 125580. https://doi.org/10.1016/j.watres.2026.125580.

Sutka, R.L., Ostrom, N.E., Ostrom, P.H., Breznak, J.A., Gandhi, H., Pitt, A.J., Li, F., 2006. Distinguishing Nitrous Oxide Production from Nitrification and Denitrification on the Basis of Isotopomer Abundances. Appl. Environ. Microbiol. 72, 638–644. https://doi.org/10.1128/AEM.72.1.638-644.2006

Wunderlin, P., Lehmann, M.F., Siegrist, H., Tuzson, B., Joss, A., Emmenegger, L., Mohn, J., 2013. Isotope Signatures of N2 O in a Mixed Microbial Population System: Constraints on N2 O Producing Pathways in Wastewater Treatment. Environ. Sci. Technol. 130118101927005. https://doi.org/10.1021/es303174x

Yu, L., Harris, E., Lewicka-Szczebak, D., Barthel, M., Blomberg, M.R.A., Harris, S.J., Johnson, M.S., Lehmann, M.F., Liisberg, J., Müller, C., Ostrom, N.E., Six, J., Toyoda, S., Yoshida, N., Mohn, J., 2020. What can we learn from N2 O isotope data? – Analytics, processes and modelling. Rapid Commun. Mass Spectrom. 34, e8858. https://doi.org/10.1002/rcm.8858