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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement report: Nitrogen isotopes (δ^{15}N) and first quantification of oxygen isotope anomalies ($\triangle^{17}$O, δ^{18}O) in atmospheric nitrogen dioxide" *by* Sarah Albertin et al.

Anonymous Referee #2

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This paper provides first results and their interpretation from an atmospheric NO2 sampling technique that includes isotopic analysis of double nitrogen and triple oxygen isotopes. The technique is deployed in an urban location (Grenoble, France) over a short ∼1.5 day period, providing 3-hourly simultaneous isotopic composition measurements for N and O isotopes. Despite the relatively short deployment, the paper serves as an important demonstration of this powerful technique, and the analysis draws conclusions on source influences on NOx in this environment (albeit over this limited period). The paper is well written and structured, with clear figures. My view is that the pa-

per is suitable for publication in ACP, once the following minor comments have been addressed.

Line 80: Could add reference to e.g. Michoud et al., (2014) which is a more recent specific study on HONO and NOy relevant to an urban France location (Paris).

From what I understand, Eq 3 assumes that NO âĂŤ> NO2 conversion is dominated by O3? Do the authors have information available to clarify this here? i.e. is the observed NO/NO2 ratio close to that given by the Leighton ratio approximation based on NO2 photolysis and NO+O3? Is the discussion on lines 370-373 also relevant here?

Line 267: Is there any kinetic / mechanistic theoretical explanation that supports the suggestion that NO+RO2 fractionation is similar to that of NO+O3? Might be worth mentioning here if so.

Also, can the authors comment on the similarity of the chemical environment in the Li et al., (2020) study to their study location?

Line 292-299 - Comparison with COVID lockdown satellite study seems a bit tenuous (i.e. comparing NO2 column change over a large city to limited time measurements here). Are there other estimates of traffic contributions to urban NOx that could be compared?

Line 325: The reaction NO2 + OH to produce HNO3 is termolecular, involving a third body. i.e. NO2 + OH + M âĂŤ> HNO3 + M. See e.g. Atkinson et al., (2004).

Line 369: Derivation of [RO2] and discussion. The diurnal behaviour of the derived RO2 seems surprising. What is assumed for k_NO+RO2 in Equation 10? How sensitive is RO2 to the assumed speciation of RO2? i.e. is the value assumed to simply be that for HO2 or is there some weighting for an assumed VOC mixture, and does this matter much? In general it might be useful to provide a Table (in the Appendix?) of values and sources of rate constant values used.

Typographical errors:

Line 57: ang = and

References

Atkinson, R., Baulch, D. L., Cox, R. A., Crowley, J. N., Hampson, R. F., Hynes, R. G., Jenkin, M. E., Rossi, M. J., and Troe, J.: Evaluated kinetic and photochemical data for atmospheric chemistry: Volume I - gas phase reactions of Ox, HOx, NOx and SOx species, Atmos. Chem. Phys., 4, 1461–1738, https://doi.org/10.5194/acp-4-1461-2004, 2004.

Michoud, V., Colomb, A., Borbon, A., Miet, K., Beekmann, M., Camredon, M., Aumont, B., Perrier, S., Zapf, P., Siour, G., Ait-Helal, W., Afif, C., Kukui, A., Furger, M., Dupont, J. C., Haeffelin, M., and Doussin, J. F.: Study of the unknown HONO daytime source at a European suburban site during the MEGAPOLI summer and winter field campaigns, Atmos. Chem. Phys., 14, 2805–2822, https://doi.org/10.5194/acp-14-2805-2014, 2014.

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