

Interactive comment on “Partitioning of reactive nitrogen (NO_y) and dependence on meteorological conditions in the lower free troposphere” by C. Zellweger et al.

Anonymous Referee #2

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General:

This paper summarizes "results of continuous total reactive nitrogen (NO_y) measurements along with seasonal campaigns of speciated NO_y (NO, NO₂, PAN, HNO₃, and particulate nitrate) ... for a two-year period at the high-alpine research station JFJ." It presents what is sometimes called a "climatology" of reactive nitrogen for this station, including an analysis of how the NO_y species vary according to meteorological conditions. It is a nice summary, and there is really no more specific, substantial scientific focus in this paper. The results are really not very surprising. (E.g., the claim that "The NO_y mixing ratio and partitioning is shown to strongly depend on meteorological conditions," is what most would expect. Meteorology drives surface (and other) observations

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to a large extent.) The measurements nonetheless comprise a valuable contribution to the literature and merit publication, as they appear to be of high quality, and are nicely presented, and are unique in their relatively long term and in their degree of NO_y speciation for air over Europe.

Specific:

P. 3: "there is significant uncertainty in the levels and speciation" To what degree is it really uncertainty and not just tremendous variability?

p.6: Why is the PLC conversion efficiency so variable? Does this indicate a problem? Were calibrations done frequently enough to capture the variability?

p.14: "whereas values of ~0.005 are observed in the upper troposphere" Is this literally the value of NO_y/CO, as stated in the manuscript, or is it really Δ -NO_y/ Δ -CO, that is, deviations above the background? This is an important difference (presuming the background is not negligible, as would be expected for CO, though for NO_y it is not always the case).

p.18: "Alternatively, the NO_y/CO ratio proved to be an interesting new approach in assessing the age of an air mass." I question how new this really is. I believe there is prior work by Parrish et al., Stohl et al., and likely others, that already captures the essence of the point being made here. Are the authors claiming novelty not already found in the works by these authors? If so, it would help to be more specific.

Technical:

p.10: Is "CBL" previously defined?

p.13: "area surface" → "surface area"

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 2259, 2002.

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