

## ***Interactive comment on “Daytime formation of nitrous acid at a coastal remote site in Cyprus indicating a common ground source of atmospheric HONO and NO” by Hannah Meusel et al.***

**Anonymous Referee #2**

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This paper uses measurements of HONO with a wide range of supporting data to assess sources of HONO in the remote coastal site in Cyprus. The findings are that there is a common source of HONO and NO and it is speculated that this is emission from microbial communities on soil surfaces. The work is important as HONO provides a route to OH radicals that is often not considered and sources of HONO in both urban and remote regions are uncertain. The authors have done a good job presenting their data and the conclusions they draw are reasonable. It undoubtedly adds to the sphere of knowledge surrounding atmospheric HONO. The paper is well presented with good clear figures and should be published in ACP subject to the authors addressing the

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following comments.

General comments:

The main conclusion of the paper is that there is a soil source of HONO and NO, which is arrived upon by looking at correlations between the ‘missing’ HONO source (i.e. the difference between HONO calculated using a steady state approximation including a series of known sources and the measured HONO) and a missing source of NO (based on NO deviations from the Leighton ratio). A strong correlation is given as evidence of a common source. Is this source thought to be photolytically driven? If not why are observations of NO at night seemingly zero (although it is quite difficult to see the exact levels on the plots), whereas HONO is shown to increase during the night. Maybe this is just a result of NO reacting with O<sub>3</sub> before the measurement location but the authors should clarify this. How far from the potential soil emission source in the measurement site? The authors should also comment on how this effects the validity of the steady state approximation, with reference to the Lee et al. 2003 study that gives caveats for the use of a steady state approximation to interpret HONO measurements.

I find the analysis of OH production showing the importance of HONO confusing because it details production of OH from HCHO, which is indirect and requires conversion of the HO<sub>2</sub> produced with NO to form OH. I believe it would be better to just include HO<sub>2</sub> + NO as an OH source, regardless of where the HO<sub>2</sub> is coming from. Another option would be to have a total HO<sub>x</sub> radical budget analysis. The authors should also comment on the fact that the HONO source here is only important near to the surface (an estimate could be made of the vertical structure of HONO) and thus it is not relevant for the entire troposphere. This is important when considering HONO as an atmospheric ‘oxidant’.

The authors mention in the experimental description that OH was measured during the field campaign but there is then no further mention of it in the manuscript. Have the authors (or anyone else) examined the OH data to assess if the measured HONO is

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required to close the HOx budget? I realise this may be the subject of further publications but if it is stated that HOx was measured it seems odd that no mention is made of the results.

The manuscript is generally well referenced however a recent study by Lee et al. 2016 in London contains a lot of detail about potential HONO sources in an urban area and should be referenced. There is also a recent study by Mamtimin et al (2016) which discusses biogenic NO and HONO emissions that seems to be extremely relevant to this work. The authors should comment on how their results compare to this.

Minor comments:

The authors should make sure they clarify what the error bars on plots and in the text actually refer to (e.g. figures 4 and 7) P. 12 line 11: Use O<sub>3</sub> rather than ozone as has been done in the rest of the manuscript P. 7 line 7: there is a discrepancy between the detection limit stated here (2pptv) and that in the experimental section (5pptv) – please confirm.

References:

Mamtimin, B., Meixner, F. X., Behrendt, T., Badawy, M., and Wagner, T.: The contribution of soil biogenic NO and HONO emissions from a managed hyperarid ecosystem to the regional NO<sub>x</sub> emissions during growing season, *Atmos. Chem. Phys.*, 16, 10175-10194, doi:10.5194/acp-16-10175-2016, 2016.

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Lee, B. H., Wood, E. C., Herndon, S. C., Lefer, B. L., Luke, W. T., Brune, W. H., Nelson, D. D., Zahniser, M. S., and Munger, J. W.: Urban measurements of atmospheric nitrous acid: A caveat on the interpretation of the HONO photostationary state, *J. Geophys.*

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