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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Daytime HONO, $NO_2$ and aerosol distributions from MAX-DOAS observations in Melbourne" *by* Robert G. Ryan et al.

**Anonymous Referee #1**

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This paper reports measurements of HONO, NO2 and aerosol using a MAX-DOAS instrument in the city of Melbourne, Australia. It shows enhanced levels of HONO, often peaking in the middle of the day, which would not typically be expected. The authors postulate a ground based photoactivated source of HONO, using evidence based on the dependence of high HONO levels since rainfall, combined with the observed diurnal profiles.

It is an interesting paper with potentially significant results in terms of the effect of HONO as an OH source and hence on atmospheric oxidizing capacity. It is well written with good, easy to see figures. However, it suffers from the fact that no concurrent



other measurements were made, making a full analysis of the effect of HONO on the chemical processes happening difficult. Hence many of the conclusions drawn are based on a bit of speculation which is not ideal. However the data is of such interest (especially as it contains vertical profiles of HONO) that I do believe it should be published subject to some extra analysis. I realise there is no way to go back and back the extra measurements required but I think there are things that could be done to improve the analysis and conclusions.

One of the great advantages of the MAX-DOAS measurement is that it gives a vertical profile of HONO. Often, measurements are only made at the ground and as HONO is so short lived and postulated sources are often surface based, it is possible that the effect of HONO as an OH source in the entire boundary layer is overestimated. Here, the authors calculate P(OH) from HONO and ozone photolysis and show that in the daytime, OH from HONO is an order of magnitude more than from ozone. However could they do this for the entire vertical profile measurements and hence provide a comparison between the two sources of OH for the entire boundary layer? This would provide an interesting contrast to just looking at the surface data.

The authors should also make some comment about other radical sources and how these may compare to the primary OH production from HONO and ozone photolysis (even if they have to estimate what concentration of other species may be).

I think showing correlation between HONO and NO2 at different altitudes as well as just at the ground (as in figure 12), would provide some information as to a potential HONO source. Presumably the correlation should get less with increasing height if the HONO source is some form of ground based NO2 conversion.

It is a shame there is no NO measurement to allow a steady state and thus a 'missing' HONO concentration to be calculated. However, the authors could make some broad estimate of NO based on their NO2 measurement and at least a rough estimate of OH concentration and calculate steady state HONO. I think this is important to show how

the daytime HONO observed cannot be explained by standard chemistry.

Some attempt should also be made to calculate the source of HONO from other postulated mechanisms (e.g. surface NO2 conversion, soil based emission) to give some idea as to whether these mechanisms can produce the daytime HONO observed. Again it is difficult to do this without some of the supporting data, however estimates could be made based on measurements in other cities in the literature.

It would be useful to have a table of HONO and NO2 levels from the literature from other cities round the world. Whilst there is some mention of comparisons in the text it would be clearer if this was brought together in tabular form to allow for easy comparison.

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