

Interactive
Comment

Interactive comment on “Measurements of OH and RO₂ radicals at Dome C, East Antarctica” by A. Kukui et al.

Anonymous Referee #1

Received and published: 30 June 2014

This is a well structured paper which builds on previous radical measurements made in Polar regions, particularly at the South Pole, and highlights that the elevated oxidative capacity reported at the SP extends to other regions of Antarctica. The authors highlight the dominance of HONO as an OH source, but demonstrate inconsistencies in the HONO measurements and the observed radical concentrations. Using this approach, the authors conclude that the LOPAP technique, used to make the HONO observations, may suffer from an artefact under these conditions; similar conclusions are reached, using an alternative approach, in a companion paper. Inconsistencies in the NO:NO₂ ratio and peroxy radical concentration observed are also highlighted. This paper is well suited for publication in Atmospheric, Chemistry and Physics and I only have some minor comments and clarifications listed below which should be addressed

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prior to publication:

Pg 15005, In 25: Repetition of In 11.

Pg 15006, In 28: this manuscript would benefit from brief description of the role of the 'radical quencher (NO₂)' here.

Pg 15007, In 9: the manuscript would benefit from expanding briefly on the two OH measurements modes here – their purpose, did the two modes agree?

Pg 15008, In 12 – 15: Did the humidity change from point of humidification to end of the calibrator? Were any changes in [H₂O] accounted for?

Pg 15008, In 20: The modelled HO₂:RO₂ ratio is dependent on the [CO] and [CH₄] assumed. As neither CO nor CH₄ were measured, what is the level of uncertainty in the radical ratio and overall [RO₂] determined from estimating these values?

Pg 15009, In 7: Why was the uncertainty greater during the night time?

Pg 15009, In 9: Please provide the LOD for RO₂ also.

Pg 15010, In 26: this interference in the HONO measurements of 10 – 20 pptv needs to be followed by the [HO₂NO₂] assumed by Legrand et al. How does this estimated [HO₂NO₂] compare with the concentration calculated by the 0D model?

Pg 15011, In 14: I believe 'Sect 2.2' should be 'Sect 3.2'

Pg 15012: Following on from the description of the 1D model used to estimate the vertical distribution of HONO, I think it would be pertinent to state at this point the respective measurement heights of the radicals and HONO and, if different, the change in [HONO] estimated by the 1D model between the two heights.

Pg 15016, In 16: '～60%' should be '78%' according to fig 6.

Pg 15016, In 22: remove comma after 'etc.'

Pg 15019, In 9: change to 'reduced by factors of 2, 4.'

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Pg 15019, In 18: change '0.25' to '4'

Section 3.5: Along with the comparison of HONO calculated with the 1D model and estimated from the radical budget, this section would benefit from a discussion on the expected HO₂NO₂ diurnal profile – what profile does the 0D model estimate for example? Does this profile suggest that the correction needing to be applied will vary diurnally?

Pg 15022, In 2: I can't find a reference to the proposed gas-phase source of HONO from HO₂.H₂O + NO₂ in Legrand et al. As highlighted by H Berresheim, the impact of this reaction as a sink for HO₂ needs to be discussed in the manuscript given that the recycling of HO₂ to OH is the second most important OH source.

Pg 15022, In 25: change to 'the peak calculated..'

Pg 15022, In 26: How exactly was P(O₃) calculated? Were losses of NO₂ that did not result in ozone production (e.g. OH + NO₂) considered? I think an equation that highlights the reactions considered needs to be added here.

Pg 15022, In 28: It would be more appropriate to compare the P(O₃) calculated with the diurnal O₃ profile observed during the OPALÉ campaign rather than an earlier campaign at Dome C.

Table 2: Add a line after the reactions listed to separate from the summed radical budget section.

Table 2: How was the 1σ uncertainty estimated?

Figure 7: A comment about the negative intercepts in the correlations between observations and model with zero HONO is needed.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 14, 14999, 2014.

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