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Interactive comment on “Measurements of OH and RO_2 radicals at Dome C, East Antarctica” *by* A. Kukui et al.

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Overall, this is a very well written paper with strong enhancements in current knowledge of HOx/ROx/NOx chemistry in the Antarctic troposphere and thorough comparison with previous measurements by Mauldin et al. at South Pole. The crucial role of HONO (and/or HNO4) has been nicely confirmed.

However,the only major issue I have is that the paper is strongly entangled with the paper by Legrand et al. (same issue), and that in my opinion it needs to be somewhat "disentangled" to stand on its own. This pertains specifically to sub-chapter 3.5, the "Comparison with 1D model" where it seems that the reader has to flip-flop back and forth between the results of both papers in order to follow (and accept) the arguments

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made in the discussion (e.g., how much interference exactly due to HNO4? how crucial is snow temperature? how justified is a turbulent vs. molecular flux approach for HONO vertical transport?). I suggest that everytime Kukui et al. use a result from Legrand et al. it should be clearly outlined, and indeed explained, in their discussion even if it means repeating some of the context from Legrand et al.

Also, I cannot quite agree with the authors' conclusions (p. 15022, line 20) that the model agrees well with nighttime HONO, as this seems to be not convincing for the 0-6 hours period shown in Fig. 10.

A third point I would like to make is the authors' brief acknowledgement of the recent work by Li et al. (2014) of which they mention only in passing that the HO2(H2O) reaction with NO2 would be negligible at Antarctic temperatures as a source for HONO. However, they neglect to recognize that this reaction could - on the other hand - be a significant sink for HO2, and this should have implications for the model results.

Other than these three comments I have only some minor comments:

pp. 15007-8: With respect to instrument calibration please discuss a) whether low ambient H2O concentrations presented any difficulties, and b) where and how far away the NO, NO2 exhaust flows were disposed and whether there could have been interferences with the measurements (also with respect to CO, CH4 for RO2) ?

p. 15011, line 13: Where and how have these been "estimated in section 2.2" ? Please explain.

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