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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Snow optical properties at Dome C, Antarctica – implications for snow emissions and snow chemistry of reactive nitrogen" *by* J. L. France et al.

**Anonymous Referee #1**

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The manuscript of France et al. describes field and modeling measurements aimed at better understanding the optical properties of snowpack near Dome C, Antarctica and use updated information regarding light attenuation and e-folding depths to model the flux of NOx from snowpack and compare to other measurements and modeling. In the broader sense this relates not only to atmospheric concentrations of nitrogen oxides (and related ozone chemistry) but also to the preservation of nitrate in snow/firn and eventually ice cores. The fate of nitrate (via photolysis) in snowpack is key to understanding nitrate preservation, but accurate descriptions of this chemistry necessarily requires quantitative understanding of the optical properties of the snow. This paper does a nice job of combining measurements and modeling to this end.



The manuscript is well written, and the experiments nicely address the overarching science questions posed. The topic is well suited to the readership of ACP and of great current interest from atmospheric, radiative modeling, as well as glaciology standpoints. Although not a main focus of the manuscript, the finding that humic-like substances play an important role in light absorption in relatively clean Antarctic snow is also an interesting finding and I hope the authors also pursue this in future studies, i.e. the relative roles of black carbon v HULIS, etc.

Just a very few minor things regarding the manuscript: 1) Should parenthetical references be listed chronologically? e.g. pg 11962, line 4, Davis et al 2001 should come in the list before Davis et al 2004. 2) This is really a matter of style, but I would remove the preambles at beginning of sections 3 and 4 (where you essentially say "this is how we split up these sections"). To me, as I'm reading, it is apparent how you are splitting up the sections, so I think this is superfluous. 3) I'd rephrase page 11974 line 4 to: "The measured e-folding depths are $\sim$3 times longer than the 3.7 cm at 320 nm previously calculated by Wolff et al. (2002)". 4) Page 11978 line 6: Is "smoothen" a word? I think smooth would fit there just as well.

And two minor questions that are more curiosities, and you may or may not want to add a line to the manuscript addressing them: 1) How long does each set of measurements take? Just wondering if you get a change in SZA during the timeframe of the measurements, or any other conditions that would affect your values? 2) Do you have any feeling for how the BC vs HULIS component of absorption may change if you get farther away from the Dome C base? I'd think there would be a fairly substantial local BC source there. Perhaps farther out, where things might be more representative of "background", the HULIS component is even greater compared to BC?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 11959, 2011.