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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Ozone decomposition on Saharan dust: an experimental investigation" *by* F. Hanisch and J. N. Crowley

Anonymous Referee #2

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General Comments

The current manuscript is much improved over the first version that I reviewed. The axes on graphs now make sense and plots have been lined up properly with respect to the axes. Many of the references provided in my first review indicating that this is not the first study of ozone uptake and destruction on powders and authentic dusts have been added. One section that was poorly referenced has now been removed. However, there are still some additional problems with the referencing as well as a few scientific problems that are noted below. Most importantly, the authors are using a model to take into account surface area properly and because of this there is now good agreement found between studies in different laboratories.

Specific Comments

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Discussion Paper

1. The discussion on the use of the pore diffusion model states "Keyser and co-workers have developed a model for ice surfaces (Keyser and Leu, 1991, 1993) which in principle can also be applied to granular surfaces (e.g. Timonen et al. 1994; Leu et al., 1995). This model predicts a linear dependence of the uptake coefficient on mass..." The term "in principle" suggests that this paper represents a first example where this model has been applied to granular surfaces. It is clear that there are several references missing here most notably the Underwood et al. (2000) reference. In the Underwood et al. paper (JPCA, Vol.104, pages 819-829, 2000), the use of the KML model in Knudsen cell experiments was described for the very first time. Since Underwood et. al. (2000), many if not all studies using the Knudsen cell technique for measuring heterogeneous reaction kinetics on powders are now reporting a more careful investigation of the powder thickness dependence. This is important so that the surface area can be more accurately taken into account in these studies. The discussion the authors give in this section about the model, the use of the model and characteristics of the model, e.g. a linear and plateau regime, is very close to that given in Underwood et al.(2000) and therefore that paper should be properly referenced.

2. What are the error bars in the O2:O3 ratio? What are the assumptions in determining the O2:O3 ratio? This is important given that it is suggested that there is "missing" O2.

3. The discussion of the deactivation/reactivation mechanism has me somewhat confused and puzzled. It is not clear what the authors are trying to say and how it relates to ozone destruction on dust in the atmosphere. Are the authors suggesting that ozone destruction is not catalytic, in contrast to what has been shown in many other studies? Is the deactivation of Saharan sand toward ozone destruction only important at an ozone pressure of 1 Torr? Does the mechanism for ozone uptake change at a pressure of 1 Torr compared to pressures that are six orders of magnitude lower? If so, this deactivation mechanism is a "non-issue" for the atmosphere and perhaps should be noted in a footnote instead of in the main portion of the text.

4. In the text it states that in Hanning-Lee et al. (1996) and Michel et al. (2002), no

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pressure dependence on the ozone destruction rate was reported. Why do the authors think they obtained a different result in their study?

Technical Comments

I think most of the typing errors pointed out in the original manuscript have now been corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 1809, 2002.

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