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Interactive comment on "Observations and model calculations of trace gas scavenging in a dense Saharan dust plume during MINATROC" *by* M. de Reus et al.

Anonymous Referee #1

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General comments

The authors are to be congratulated to the analysis of the peroxy constituents during a dust event at Izana. The measurements are very interesting and original. This is excellent and requires fast publication!

I have though questions to the modeling part, which for now prevent me to find this paper throughout excellent. To the degree that I wonder if the paper without the model study wouldnt be interesting enough. My main concern is the way the box model is applied: Why is the model run for 10 days for short-lived species? Why do the authors

think that the history of the airmass and the composition change as a consequence of the contact with mineral dust can be neglected? What are the results if the model is run for only 3 or 6 hours or having a diurnal cycle on radiation and temperature at least? I was wondering if a diurnal cycle is incorporated but not mentioned in eg the photoloysis rates? But if not - which is what I understood so far - why not? If the peroxy radical concentrations are a consequence of instantaneous chemical and meteorological conditions - why running the model for 10 days? If the actual concentrations are a consequence of the evolving species composition - why running the model for 10 days and not taking into account the gradual impact dust has on the photooxidant composition? Has the actual ambient air reached a steady state at the time when it was measured in Izana?

My second major concern is that the H2O2 reduction during the dust event is not enough discussed, nor explored. While it is convincingly shown that the data are not sufficiently good to demonstrate dust impact on ROX and HCHO mixing levels, some doubt can be raised for dust effects on H2O2 levels as well. Since it seems - from the data that could be discussed already convincingly - to be more promising to go after the effect on H2O2, the authors spend much more pages on demonstrating that ROX measurements are probably not precise enough, that HCHO results are sensible to uncertainties in the actual composition of the air. I wonder if no other model parameter could explain the low H2O2 levels during the dust event? How is the temporal evolution of H2O2 during the 10 days box model simulation? I would be interested to see how the initial conditions influence the H2O2 simulation. What happens to the NOy pool, the ozone?? Is ozone kept constant in the simulation? Isnt that an infinite pool of oxidant for a simulation of ten days? I think the paper would benefit from extending this section and eventually shortening the HCHO and ROX parts.

Specific comments:

pages/lines as in ACPD online version:

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p658/line12: "for global models comparison appears to be a main limitation": Comparison to observations is a major challenge and method for ANY model. Global models can have pretty convincing capabilities. A fair consideration of other models then box models seems to me possible.

p658/17: typo: to the extent! possible

p660/15: destroyed by "catalase" ??

p660/26: AL detectors: spell out

p661/6: "losses of of HCHO and H202": How large are they, how variable, how uncertain?

p663/17-19: time specification could be omitted here, is detailed later

p664/5-6: "the median mixing factor": Is the median from three/four values? Maybe better give these numbers directly.

p664/18-20 and table 2: HOw are missing data from beginning of the experiment have been dealt with? Does the choice of the averaging period influence the ratio among the components? SOme more descriptions in table 2 for which period averages were obtained, would be nice.

Table 2: "dust-poor" and "low dust" are both used in text, tables etc. Are they equivalent? Eventually use just one term.

p664/27-..: How as optical thickness measured? above Izana? Down to sea level? Was optical depth really 0.5 in the dust-poor time? Seems to me too high.

p665/20: "high humidity during dust": Any idea of why the "desert air" is humid?

p665/22: "inhibition of VOC diurnal cycle": Too general description of data. Look at Isoprene!! My understanding of the isoprene data is that local emissions from the forests in the upper part of the mountains can well have influenced the Izana measurements

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also during the dust event. Despite reduced H20 mixing conditions then as infered from the radiosonde data. In contrast to pollution from the coastal marine level of the island the forests on the upslopes could influence air arriving at the site more often. Such an impact of isoprene which entered into the air mass on the last "meters" could also explain that there was no steady state achieved in the chemical state of the gas mixture. The authors mention this very late and just shortly p676/7-12. I think the observations strongly support such a hypothesis and it would deserve to be mentioned earlier.

p665/27: "NOy converter and aerosol nitrate artefact": Does the converter really convert coarse aerosol nitrate which is sticking on a mineral dust particle as calcium nitrate? Are there losses in the inlet for this coarse nitrate fraction? Would be the consequence of an error on this HNO3 mixing ratio in the model simulations?

p667/20: Some more explanations on the box model would be useful: Which "long-lived trace gases" are constrained and how? How are temperature, RH, dust variations taken into account?

p668/10: Setting the HNO3 to zero is probably a valid assumption. But - is the evolution of the NOy pool during transport from the dust sources in Algeria to the measurement site well represented this way? What are the consequences of different initial conditions for the ensemble of the boxmodel simulation with respect to the NOy pool?

p669/10: How much time is needed to reach steady state for H202 runs? And - shouldnt there be some more discussion as said above on the consequences of running a model for 10 days?

p669/13: Just for clarification, add: When is the solar zenith angle of 70degrees at Izana exceeded?

p674/24: "For the marine boundary layer calculations..." Which marine boundary layer? polluted? in contrast to remote?

p675/7: "..we included a sinusiodal dry deposition rate...": If Izana is meant to be a free

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troposphere station - why should dry deposition be important? Especially during a dust event, with reduced vertical mixing as indicated by the radiosondes, where should this dry deposition happen? Just on the upslope part of the mountain?

p676/20: "this is mainly due to the fact that dry deposition of H2O2 is not included": I do not agree. There are other uncertainties in this simulation. Peroxy radicals not included etc.

p680/4: "heterogeneous removal reactions were only activated the last three days of the simulation": ??? All of sudden the model becomes a Lagrangian box model with a history... Why only at this point introducing a time varying boundary condition? See my general comments.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 655, 2005.