Atmos. Chem. Phys. Discuss., 10, C12287–C12289, 2011
www.atmos-chem-phys-discuss.net/10/C12287/2011/

Interactive comment on "Temperature thresholds for polar stratospheric ozone loss" *by* K. Drdla and R. Müller

Anonymous Referee #1

Received and published: 12 January 2011

The authors discussed in this manuscript the relevance of polar stratospheric clouds to polar chlorine activation and ozone loss in polar spring. They compare the chlorine activation on liquid binary H2SO4/H2O and STS particles with the chlorine activation on NAT and ice particles.

Moreover they discuss in the manuscript the temperature threshold below which substantial chlorine activation occurs.

In the opinion of Drdla and Müller the binary aerosols are mainly relevant for the chlorine activation and for the ozone loss. Therefore they think that it is not reasonable applying TNAT as a temperature threshold. They recommend instead the new temperature threshold TACL and provide for this threshold a new parameterisation depending only on H2O, aerosol surface area and altitude, but not on HNO3.

General comment

I have some principal problems with this paper. I think that the research done by the authors can be very relevant for atmospheric science, but due to the essential conclusions there should be a more detailed analyse of the results. Because of this I recommend a revision of the manuscript with the goal to support the conclusion that only stratospheric liquid aerosol is responsible for polar chlorine activation in more detail. Therefore and also because of the interactive comment of Susan Solomon I would only recommend this paper after some major revisions.

Individual comments

1) I think that the supplement includes essential information which should be in the paper. So is, for example, the description of the performed simulations and their results first completely understandable after reading the supplement. Especially Table S1 and Table S2 should be in any case in the main paper. Also the description of the development of the TACL formula and the description of the performed simulations should be in the main text. Maybe it would be more reasonable if the authors include the essential information of the supplement into the paper and skip the rest.

2) The manuscript is arguing for major changes in the previous idea of chlorine activation on PSCs. It is claims that chlorine activation mainly occur on liquid particles and that heterogeneous chemistry on solid particles are negligible for chlorine activation. But

a) the presented results are only from one model. In my opinion it would be reasonable to perform simulations with another model. Maybe it is also possible to perform the calculation for the chlorine activation (using the kinetic parameters) with data from observations. That could maybe support the theories of the authors.

b) the simulations are performed only for two time periods, one with focus of the Arctic and one with focus of the Antarctic. The chosen periods are the 1999-2000 Arctic winter and the 2000 Antarctic winter. As in this year the chlorine concentrations are very high and so the ozone loss process is largely saturated there should be simulations of other winters to provide sensitive tests.

c) for the solid PSC particles the manuscript discuss only the contribution of NAT to the chlorine activation. The contribution of ice particles are only mentioned in the supplement. Thereby the discussion concerning the NAT particles is in my opinion understandable and I would support the statement that there is not very much chlorine activation on these particles because the surface area of NAT particles are much smaller as the surface areas of the liquid aerosols. But I think that the heterogeneous chemistry on ice particles is relevant for chlorine activation, because the number densities of ice particles are much higher as the number densities of NAT particles and their surface areas are comparable with these of the liquid aerosols. Unfortunately there is not very much discussion over the ice particles in the manuscript. In fact there is only a short discussion with reference to Table S2 in the supplement. The authors affirm here that ice particles contribute only 0.6% to winter-long chlorine activation in the Antarctic. I can′t believe this rate. The ice particles should be more comprehensible discussed in the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 28687, 2010.