

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

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### **General comments**

We thank the anonymous reviewer for his suggestions that helped a lot to improve the paper. One point now more clearly emphasised in the paper is that the focus is on the onset of chlorine activation. As suggested by the reviewer the paper has been substantially restructured so that a supplement is no longer needed. There are also suggestions of extending the work which are partly covered by a recent paper (Wegner et al., 2011). Please find a detailed response to the comments below.

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### **Response to individual comments**

1. We agree with this point – thanks for the suggestion. We have included and integrated all relevant information from the supplement into the main text, so that a supplement is no longer needed.
2. The point the paper is making is not quite as strong as stated here. We are not saying that there is no heterogeneous chemistry on particles other than cold binary particles (see also reply to the comments by Susan Solomon). The focus here is on the *onset* of chlorine activation.
  - (a) In principle it is certainly a good point to test the mechanism in another model. However, the onset of chlorine activation is rather similar in models with similar assumptions about heterogeneous chemistry; it is important whether PSC formation in the model is assumed at TNAT, whether supercooling is required and whether a temperature dependence of the reaction probabilities  $\gamma$  is implemented in the model (Krämer et al., 2003, see also discussion). So it is more important (as done in the paper) to test various assumptions about the onset of heterogeneous chemistry. This point is discussed also at the top of the discussion section. However, in a recent paper, the mechanism is tested in the CLaMS model along trajectories in a case study of winter 2004–2005 (Wegner et al., 2011).
  - (b) Again, we agree that simulations of polar ozone loss over longer time periods would be an important way to further scrutinise the conclusions put forward in the paper. But this is beyond the scope of the paper. However, note that the saturation of ozone loss is not as problematic here as thought, because chlorine activation is *not saturated*, which is the focus of Table S1 and the accompanying discussion (which now were moved to the main paper as suggested). If the model predicted the right ozone loss for the wrong

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reason it would be obvious from the simulated chlorine activation (see also reply to Susan Solomon).

- (c) We agree with the reviewer that ice particles provide the strongest chlorine reactivities (Dameris et al., 2007). The reason why they are not discussed in the paper is that they can never be responsible for the *onset* of chlorine activation as some kind of reactive particle surface will always be present before ice formation. We agree with the reviewer that Table S2 is confusing – we have therefore dropped Table S2.

## References

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