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Interactive comment on "Rapid meridional transport of tropical airmasses to the Arctic during the major stratospheric warming in January 2003" *by* A. Kleinböhl et al.

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We thank the reviewer for the comments. In agreement with reviewer 1 the main point of this review is also the lack of trajectory calculations to study the origin of the airmasses that were observed in the Arctic, as well as to better estimate the transport times. We followed the reviewer's suggestions and included trajectory calculations at different potential temperature levels to study these issues in greater detail.

We found that the calculations of back trajectories generally support the origin of the airmasses that was estimated from the measurements in the considered altitude range. The trajectory calculations clearly show origins around a latitude of 20 deg N for air

parcels initialized at 800 and 1000 K. Furthermore the timescales for this transport could be estimated more precisely to be about 4-5 days at 800 K, and about 3 days at 1000 K. As the trajectory calculations were requested by both reviewers, we would like to reference to the reply to reviewer 1 for a more detailed discussion of the trajectory calculations.

Based on the results of the trajectory calculations we change our argumentation in section 4 such that it is now mainly based on the trajectory calculations, rather than the use of a simple model of linearized ozone chemistry, and uses the chemical studies only to support the plausibility of the airmass origins and the estimated transport times. The reviewer stated considerable concerns about the use of the linearized ozone scheme and suggested the use of a box model with full chemistry. We respect these concerns, however, we would like to explain in the following why we did not follow this suggestion.

The reviewer states that the use of a linearized ozone chemistry is not state of the art. It is true that running a full interactive chemistry model along trajectories can easily been done today. As we have a state-of-the-art stratospheric chemistry model available, we considered including calculations from such a model as well. However, we doubt though that the use of such a model would really strengthen the point we want to make. In particular we do not agree with the reviewer's statement that potential issues with the initialization of full chemistry calculations would be less severe than potential problems with the linearized chemistry. This is not obvious to us and has to be proven. Apart from N2O and ozone there are basically no measurements of other trace gases available to initialize a full chemistry model. For initialization we would have to rely, e.g., on the output of a 2D model. But in that case there would be very little improvement over the use of a parameterized chemistry. So in conclusion we prefer to use here a model that includes the minimum number of processes needed, not the maximum number of processes that are possible.

Linearized ozone chemistry schemes have been successfully used in studies of atmospheric dynamics, (e. g. Sinnhuber et al., 2003). Also recent simulations by Kuttippu-

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rath (PhD thesis and manuscript in preparation) using a 3D chemistry transport model with the LINOZ scheme generally show reasonable agreement to measurements as well as to climatologies. The recent work by McCormack et al. (2004) suggests that linearized ozone models can be reasonably used to calculate ozone distributions, although the paper formulates doubts about the appropriateness of the LINOZ scheme for upper stratospheric studies. Considering the content of this work this seems to be the case for mid-stratospheric airmasses largely confined at high latitudes. However, this is not obvious for the case in which mid-latitude airmasses were transported towards the pole, in particular at timescales of 4-5 days where LINOZ gives very reasonable results (comp. Fig. 11 in McCormack el al. (2004)). As the latter case is the most comparable to the situation studied in our work it seems unlikely that uncertainties or inaccuracies in the LINOZ coefficients should have a larger impact than the other error sources discussed in our paper.

We would like to emphasize that the change in the line of argumentation for the estimate of the transport times puts considerably less weight on the arguments obtained from the chemical model calculation. Furthermore we would like to point out that the accuracy of the ASUR ozone measurements is estimated to be about 15% and hence could be a significant source of uncertainty in a model initialization. Taking these arguments into account we hope that the reviewer agrees with our assessment that the use of a linearized ozone chemistry will be sufficient to support the plausibility statements we make in the revised version of the paper.

Sinnhuber et al., Geophys. Res. Lett., 30, 10.1029/2002GL016798, 2003.

McCormack et al., Atmos. Chem. Phys., 4, 2401-2423, 2004.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7121, 2004.