Atmos. Chem. Phys. Discuss., 12, C2562–C2564, 2012
www.atmos-chem-phys-discuss.net/12/C2562/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A stratospheric intrusion at the subtropical jet over the Mediterranean Sea: air-borne remote sensing observations and model results" *by* K. Weigel et al.

Anonymous Referee #1

Received and published: 15 May 2012

In their paper Weigel et al. look a a stratospheric intrusion at the subtropical jet, based on CRISTA-NF measurements and CLaMS model simulations. After presentation of measurements of the AMMA flight on 29 July 2006 (section 3), the authors discuss the case in greater detail (section 4) based on three basic questions (section 4.1-3). The paper is well structured and all points are adequately discussed and set into context.

Major points which should be considered are:

1) The whole introduction is quite short and in parts rather technical. For instance,

on P2,L54-68 it is discussed that the CLaMS model is capable for the kind of study presented. However, I think such a 'validation' should be given in the methodology section, i.e. where the CLaMS model is explictely introduced. There are other such points in the introduction: earlier (P2,L31-35) CRISTA-NF's capability is discussed. In short, the introduction focus to strongly on technical aspects. I would like to see more strongly discussed: i) What is the meteorological relevance of the study?; ii) What can be learned from the study which was not yet known, i.e. what's new? To this aim, some extra paragraphs dealing with 'meteorology' and including literature reviews of the phenomenon of interest are needed! Note also that Gettelmann et al. 2011) is cited very often, even if there are more original papers available. Gettelmann et al. 2011 is a nice review, but at places the original studies should be mentioned. As an example, on P5,L148-149 it is stated, citing Gettelmann et al, that the best choice of PV threshold for definining the dynamical tropopause depends on location and season tropopause. However, there are recent original studies exactly discussing this pöoint, e.g. "Kunz, A., P. Konopka, R. Müller, and L. L. Pan (2011), Dynamical tropopause based on isentropic potential vorticity gradients, J. Geophys. Res., 116, D01110, doi:10.1029/2010JD014343".

2) As already stated in point 1), I would like to see explicitely what is new. All results are consistent and well described; the discussion is scientifically solid. And still, sometimes I had the feeling that I 'only' read the confirmation of things which I already knew, or that the authors have a nice measurement system which

I would appreciate a short discussion in which the authors explicitly show what new insight can be gained from the case study. At the moment it looks more like a presentation of the measurements and modelling capabilities of CRISTA-NF and CLaMS.

Minor points:

1) At several places minor language problems can be discerned. A native speaker should carefully read the manuscript to correct them.

2) In Fig.1 The upper and lower panel contain redundant information: both show PV and wind speed. I think the figure would be more readable if the upper panel only includes PV aned the lower one only wind speed.

3) Fig. 2 is not particularly easily read! For instance, it is written that the vertical extent of the symobls denote the vertical resolution of the retrieval results. However, it is difficult to get this from the figure. I wonder whether it would not be more informative, albeit less fancy, to split the figure into two purely horizontal views, where only part of the information is shown.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7793, 2012.