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

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In their paper Weigel et al. look at 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 explicitly 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 defining the dynamical tropopause depends on location 
and season tropopause. However, there are recent original studies exactly discussing 
this point, 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 explicitly 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 presenta-
tion 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 and 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 symbols 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.