Atmos. Chem. Phys. Discuss., 5, S7–S12, 2005
www.atmos-chem-phys.org/acpd/5/S7/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Deep stratosphere-to-troposphere transport (STT) over SE Europe: a complex case study captured by enhanced ^{7}Be concentrations at the surface of a low topography region" *by* E. Gerasopoulos et al.

Anonymous Referee #1

Received and published: 26 January 2005

General Comments: While previous studies have shown that STE processes can influence ozone mixing ratios in the free troposphere of the eastern Mediterranean, this paper provides the first proof (through 7BE measurements) that stratospheric air can also impact the surface of this particular region. This finding is significant and the results deserve publication in ACP once important revisions have been made. A major conclusion of the paper is that the stratospheric influence originates from several tropopause folds that impact the region over several days. I am not convinced that this

is the case and would like to see the authors refine their trajectory analysis so that they can provide a more quantitative contribution from each intrusion.

My major concern with this paper is the reliability of the trajectory analysis.

1) The methodology needs more description. How many trajectories are released from the receptor box at each 6-hour time step? And what is the horizontal and vertical spacing of the back-trajectory release points within the receptor box? What is the temporal and spatial resolution of the ECMWF wind fields?

2) The wording of the methodology needs to be modified on lines 11-13 on page 109. The way this is worded, it sounds like you were able to identify which trajectories needed to be calculated before the calculations took place. A better sentence would be: Clusters of back trajectories were released from the receptor box every 6-hours and allowed to advect according to the model wind fields for 10 days. Trajectories were tagged as having a stratospheric source region if they attained a PV value greater than 2 pvu.

3) The receptor box is very large, roughly 500x555 km and at any given time I could imagine a box this size could experience 2 or even 3 distinct types of air masses. This may be the reason that so many intrusions appear to have influenced the measurement sites. I think the receptor box is far too large for the analysis, especially since the 2 measurement sites are so close together. If the authors have a reason for choosing such a large box they need to state why. I recommend that they re-run the analysis with a smaller receptor box, and either replace the results from the large receptor box or compare the two sets of results. I think a 1x1 or 2x 2 degree box should be sufficiently large, especially since trajectories will be released every 6 hours. I realize that there is no way to choose a receptor box that has the “correct size” and choosing a box that is too small can be as misleading as choosing a box that is too large, but in the current draft of the manuscript the large receptor box is likely introducing more air mass variability than the sites actually experienced.

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4) The authors report the percentage of trajectories that originated in the stratosphere. But what do these percentages really mean? I'm not sure because I wasn't told the density of release points, or even if the trajectories can be considered representative of the mass of the air in the receptor box. Following the methodology of Cooper et al, On the life cycle of a stratospheric intrusion and its dispersion into polluted warm conveyor belts, JGR (2004), the trajectories within the receptor box should be released from an evenly spaced grid, for example, every 10 km in the horizontal and every 20 hPa in the vertical. This would work out to be roughly 1300 trajectories in a 1x1 degree box between 1000 and 800 hPa. From what I have read about Lagranto it can run large clusters of trajectories so this number shouldn't be a problem. Because the trajectories are spaced evenly in the vertical according to pressure, each trajectory represents an equal mass of air. So now, if 15% of the trajectories originate in the stratosphere, you can estimate that 15% of the mass of air influencing the site at the time of the trajectory release originated in the stratosphere. You can then take this model-derived value and compare it to the 5% of the air that was believed to have originated in the stratosphere, as discussed in Section 6. With this trajectory technique you are running a simplified version of a retro-plume as described by Stohl et al., A backward modeling study of intercontinental pollution transport using aircraft measurements, JGR, 2003.

In summary the revised trajectory approach recommended above would allow the authors to be more quantitative in their estimate of the air that originated in the stratosphere and they would be more certain as to which intrusions affected the measurement sites. My suspicion is that once the receptor box is reduced in size just one or two intrusions will have any real impact on the sites.

Davies and Schuepbach (1994) describe the descent of stratospheric air to the surface of Europe through mid-latitude cyclones and relate the transport pathway to the location of the surface low and the associated cold front. This paper would also benefit from such an analysis because it would allow the reader to better understand how the stratospheric air reached the surface. The authors would then be able to compare their

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case study to those described by Davies and Schuepbach who gave the first thorough description of how an intrusion impacts the surface of Europe. However, the conclusions of Davies and Schuepbach were only supported by circumstantial evidence in the case for the intrusion reaching the surface of Holland because they did not have 7Be data to unambiguously show that the ozone had a major stratospheric origin. A figure showing the mean sea level pressure, lows and fronts for a series of days surrounding the March event would be very useful.

The standard of English is fairly good but there are many instances when non-standard expressions are used or when sentences do not flow well. In the Specific Comments section below I have pointed out some of these cases. However, I strongly recommend that the authors ask a colleague whose first language is English to proof read the paper.

Specific comments:

Abstract The abstract states that the trajectories were used for the “exact identification of the different intrusions and the attribution of each intruding parcel of air to a certain filament of high PV” Given the uncertainty in trajectories that are at least 6 days old and given my concerns listed above about the size of the receptor box, the word “exact’ is much too strong. A better statement would be something like “trajectories were used to identify the stratospheric intrusions most likely to impact the surface sites”

INTRODUCTION A paper that is highly relevant to the Introduction is: Cooper et al., On the life cycle of a stratospheric intrusion and its dispersion into polluted warm conveyor belts, JGR, 109, D23SO9, 2004. This paper follows the life-cycle of a deep intrusion over the Pacific, illustrating its filamentation, decay, dispersion, and eventual mixing with polluted air transported through warm conveyor belts. The paper also discusses the amount of ozone in the fold that remains in the stratosphere and the amount that is mixed into the troposphere.

Page 103 line 19 The authors state that deep STT transport is more important than

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shallow STT transport because it leads to composition changes. But shallow STT events still inject stratospheric air into the troposphere which eventually mixes with tropospheric air, causing a change in the composition of the troposphere. Some reworking of the sentence on lines 16-20 is required.

Page 104 line 19 What do you mean by non-physical? That small scale processes are not simulated by the models?

Page 106 lines 22-23 I'm not sure what you mean by "ozone concentrations at LVD showed no significant scatter." In addition to Figure 2 it would be very helpful if you showed the ozone time series for the study period. You don't need to make a new figure, you can just superimpose the time series onto Figure 1 and Figure 6. Also it would help if you superimposed the 7BE time series on Figure 6. Once the 7BE and ozone data are plotted together the authors need to give more explanation as to why the 7BE values fluctuate more than the ozone values.

Page 108 line 4 What do you mean by "attenuates? That the filament elongated and dispersed?

Page 109 line 6 You state that stratospheric air made it ALMOST to the surface. But it did in fact make it to the surface as demonstrated by the measurements at the surface stations, so the word "almost" should be removed.

Page 110 line 20-21 Are the stated RH and PT values the same as those that the trajectories had when they crossed the tropopause?

Page 112 lines 8-15 This paragraph isn't very clear: "respective intrusion duration." With respect to which intrusion?

Figure 2 the caption mentions the 95% confidence interval. Confidence interval usually applies to a statistical test. As far as I can tell a statistical test was not applied to these data, so I assume that the authors are referring to the central 95 percent of ozone values?

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Figures 4 and 5 in my pdf version are still too small to be effective and need to be enlarged.

Page 13 line 10 Change to: “as the stratospheric air is mixed with the surrounding air”

Page 104 line 23 Replace “second fortnight of March” with “second half of March”

Page 105 lines 18-21 Change to: “It is situated 50 km from Thessaloniki in a flat region surrounded by small hills on all sides. To the north the slope drops gradually toward Lagada and Volvi lakes, and to the west toward the Gulf of Thermaikos.

Page 107 line 10 Change to: “The event was related to”

Page 107 line 15 Change to: “Red and yellow areas on the charts”

Page 107 line 17 Change to: “As early as 15 March 2000 a PV streamer had formed over Scandinavia and Poland (not shown here), which on 16 March was well established over Eastern Europe, north of Greece.” The rest of the description of the PV features on pages 107 and 108 needs to be changed to the past tense, as I have done above.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 101, 2005.