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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.

E. Gerasopoulos et al.

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Reply to Referee #2

The referee has made some general but very important remarks concerning the focus of the analysis and the structure of the manuscript. Thus, he suggested that focus should be put on the measurements first of all and then try to relate them with the intensity of the intrusions arriving at the stations each day and the different characteristics that the trajectories may have (e.g which system do the originate from, from what altitude, their RH etc). Moreover, he points out the existence of discrepancies between

the different parts as well as inadequate linkage between them. After the analysis has been repeated with a smaller receptor box and the volume fraction of surface air that has been transported from the stratosphere has been calculated, the whole focus of the analysis has been readjusted so that the connection between different intrusions and their characteristics is highlighted. Statistics on the trajectories that not lead to clear interpretation of the surface observations have been removed and a new section has been devoted on the linkage with the trajectories characteristics. This way, to our opinion, the discussion is easier to follow, discrepancies are removed and the focus is specific satisfying the suggestions of the referee. However, some of his comments are answered separately:

1) A first example of a discrepancy (that is merely pointed out in section 3 with no attempt at explanation) is the near complete lack of response in ozone mixing ratios during the event. How could it be that a steady increase in Be-7 (purported to be derived from the stratosphere) over a period of 8 days would be reflected by 2 brief (few hours long)increases in ozone on the last day of the event (see Figures 1 and 2, and the very short discussion of them in first two paragraphs of section 3)? This raises another point that perhaps should come up later in this review, but it does provide an example about different sections of the manuscript seeming to be isolated from each other. Specifically, it may be very interesting that the only enhancement of ozone was seen on the day with highest Be-7 concentration. Near the end of section 5 the authors point out that the trajectory analysis indicates that the most frequent transport of stratospheric streamers to the site occurred on 26 March and that Be-7 increased sharply between that day and the 27th. How could this have no impact on ozone? Given that the authors present concern about surface level ozone as the primary motivation for their investigation in section 2, they seem obliged to attempt to resolve these apparent discrepancies.

The referee's point is correct. The answer is given in the reply of question 6 of the first referee. With the additional information extracted from the trajectory analysis the

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response of both tracers to the intrusions is now clearly demonstrated in the text. Yet, it is still interesting that even though ozone follows the volume fraction of surface air that has been transported from the stratosphere, 7Be shows a more cumulative behavior possibly related to the change of the main PV system influencing the station. Discussion on this different behavior has been included in the manuscript.

2) Because the assumption of a single, unique, stratospheric end member is shown by the authors to be very unlikely to be true, the quantitative analyses they present (estimating surface Be-7 and ozone, and asserting that stratospheric airmasses increase in RH and decrease in PT at a constant rate after injection into the troposphere) have very weak foundations.

It is true that the characteristics of each air parcel represented by a trajectory and specifically their content to 7Be and ozone may vary considerably between the different systems that influenced the station as well as for different altitudes in the same system. Thus, the referee is in general right, so we have removed the part relating the mean RH and PT with the duration of the trajectory movement into the troposphere. However, we retained the part with the estimation of surface measurements by the simplified mixing formula, accenting the weakness of the assumptions, in order to show that a 5% stratospheric contribution can result to doubling of 7Be but only an increase of 20-30% to ozone. This could partly explain why 7Be kept on ascending while ozone did not, considering also the change between the influencing systems.

3) In summary, the authors do appear to have captured an STT event that extended nearly to the surface over Greece, and they have compiled extensive meteorological evidence showing frequent STT exchange, and subsequent tropospheric transport to the sampling site. The present draft of the manuscript treats these related lines of evidence very much as separate stories, making the whole package much less than the sum of its parts. I would urge the authors to shorten the early part of section 5, and greatly expand a discussion section that focuses on linking specific stratospheric injections to the arrival of these airmasses in Greece in late March, and highlights

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the response of the stratospheric tracers (Be-7, ozone, dry air) that is observed on the ground. Recasting the discussion section in this way, should lead to some clear conclusions that should be highlighted in the final section of the paper.

We have followed the suggestion of the referee and we have now restricted the section 5 to include only information on the model and essential statistics on trajectories, we expanded a discussion section (6) where the linkage between trajectories and the tracers is presented and finally in section (7) “conclusions” we highlighted and categorized the main conclusions derived from the analysis of the event.

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