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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.

Anonymous Referee #2

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This paper presents a detailed analysis of an apparent episode of stratospheric air injected into the troposphere and descending to the surface in Greece in late March, 2000. The impact of this event at the surface consists primarily of a significant enhancement in the cosmogenic radionuclide Be-7 (a tracer of stratospheric air masses), with a more ambiguous, short, increase in the mixing ratio of ozone.

The debate about the relative importance of stratosphere-to-troposphere transport (STT) versus tropospheric photochemical production as sources of ozone in the troposphere, and particularly at ground level, is long standing. The authors note that in the study region (northern Greece) ozone mixing ratios at ground stations are generally close to levels that may cause damage to plants, and during the summer the mixing ratios are often above levels felt to cause harm to human health. Further, the region is often just south of the polar jet, hence below the region where tropopause folds may inject significant amounts of stratospheric air into the upper troposphere. If air derived from such STT events is transported down to or near the surface on a regular basis, stratospheric ozone could play a major role in determining the mixing ratios of ozone at the surface. This paper presents a case study of an episode when simultaneous large enhancements of Be-7 at two stations located below 850 m altitude in northern Greece apparently indicate transport of stratospherically derived air down to very low levels in the troposphere. Detailed analysis of meteorological fields shows that STT events were frequent over a large portion of the northern hemisphere upwind of Greece, supporting the interpretation of the excursion in Be-7 as an indication that stratospherically influenced air was descending to the surface in the study region.

The strongest, and most clearly presented, evidence supporting the conclusions drawn by the authors are the time series measurements of Be-7 (Fig. 1), and the descending layer of dry air revealed in radiosonde profiles (Fig. 3). The high concentrations of Be-7 at the surface at the end of March can only have resulted from downward transport of air from higher levels of the troposphere, and the dewpoint depression in the descending air is consistent with a stratospheric influence on the upper tropospheric source regions. However, discussion of these points is quite brief in the manuscript. In my view, the much longer discussions of synoptic conditions and trajectory analysis (sections 4 and 5 of the manuscript) provides nearly circumstantial evidence that STT could have caused the enhancement in Be-7, as these analyses are presented in the present manuscript. In the present draft the separate threads of data analysis are developed in some detail, but are not related to one another very well. By carefully going back and forth between different sections of the manuscript, a reader can find discrepancies between some parts of the separate threads, and also several interesting intersections

ACPD
5, S1–S6, 2005

between them that are not clearly pointed out by the authors.

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.

Section 5 represents the bulk of this manuscript, both in number of words and also in the amount of effort expended during the data analysis. The first 8 paragraphs of the section present the case that STT events were frequent throughout much of March, and that tropospheric air in regions impacted by stratospheric intrusions was often subsequently transported to the sampling site in northern Greece (over periods of 4 to 10 days according to the trajectory model). This discussion clearly demonstrates that STT events that apparently influenced the composition of surface air at the site at the end of March had a wide range of origins (geographic and altitude) and different histories after injection into the troposphere. Unfortunately, throughout this discussion no attempt is made to directly relate intrusions on a given date to their arrival at the station during the last week of the month. It is not until the 11th paragraph of section 5 that an attempt to do this, and to relate STT and transport to Be-7 concentrations, is made. This very short paragraph introduces Figure 6, which probably should be the heart of the entire section. This Figure shows when air that appears to have originated in each different STT event was calculated to arrive at the ground in Greece. The authors describe the figure and relationships to Figure 1 very briefly (e.g. most of the stratospheric trajectories originated more or less overhead, but these dominant “EE” trajectories did not coincide with highest Be-7, and when NA trajectories dominated Be-7 reached maximum concentration). It should be noted again that this part of the discussion does not even mention ozone.

I notice that the 4 days with highest Be-7 had progressively less EE influence, these days were also the only ones felt to have NP influence, and that the last 2 days of March (with the maximum Be-7 and including the only day with observed enhancement in ozone) reflect the arrival of NA trajectories for the first time. Interestingly (perhaps), the EE trajectories generally originated at lower altitude than the other three regions, and also tended to have higher relative humidity. It would seem plausible that airmasses originating higher above the tropopause could have a stronger stratospheric signature (higher Be-7 and ozone) and might retain this imprinting more clearly during their week long transit through the troposphere. It should be noted that these observations are not called to the reader’s attention in the present manuscript. Further speculation suggests that the pulses of enhanced ozone observed on 31 May may reflect either very rapid downward transport from the upper troposphere (with less dilution) or an STT originating well into the stratosphere. Because the authors do not relate individual STT (or even those on a given date) to their arrival at the sampling site it is not possible to check this hypothesis (though it would be possible for the authors to do so).

Section 6 is titled Summary and Discussion, and is the final section of the manuscript. As suggested by the title, conclusions are not highlighted. Several of the paragraphs in this section are nice summaries of the various threads developed in earlier parts of the paper, but again the section suffers from the lack of cross linking, even between consecutive paragraphs. At least 2 of the paragraphs in this section probably should

ACPD
5, S1–S6, 2005

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not be included, if one reads those surrounding them. Paragraphs 4 through 6 summarize the range of differences found for stratospheric streamers originating in separate regions, As noted in my discussion of section 5, many of these differing characteristics should also impact the composition of these stratospheric parcels when they enter the troposphere (greater or lesser amounts of Be-7 and ozone, and likely water vapor). Here in section 6 the connection between streamer origin and tracer concentrations is again not stated, but the concise listing of the differences between trajectory groups (and differences between individual trajectories within each group implied by the standard deviations and distributions shown earlier) indicated that it would be ill advised to consider that all STT events during the study injected stratospheric air with similar characteristics into the upper troposphere. However, the authors implicitly assume that all of the stratospheric air was identical when injected into the troposphere, in the calculations and regressions presented in the third and seventh paragraphs, respectively, of section 6. 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.

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 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.

Technical comments:

Pg 103, line 28, less than 2.5% of the time

Figures 4 and 5, authors have made the panels a little larger in this version than in the previous draft. As expected, the on-line version does allow the reader to zoom in and see what the authors are discussing, but the printable pdf renders these too small to see any relevant details. The regions marked on Figure 5 are much more clear than first draft, but still very hard to pick out on the printed version.

Figure 8 probably should be deleted (see discussion of Section 6 above).

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