

## ***Interactive comment on “European surface ozone in the extreme summer 2003” by S. Solberg et al.***

### **Anonymous Referee #1**

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General Comments: This paper investigates an important topic, the relationship between surface ozone amounts and the prevailing meteorological conditions. The authors attempt to link the enhanced surface ozone seen in Europe during the summer of 2003 with what they term as positive feedbacks between the meteorology and several processes that affect ozone formation, specifically increased isoprene emissions, relatively low total column ozone, forest fire emissions, and less efficient dry deposition of ozone. The amount of ozone in 2003 relative to a twelve-year climatology (1991-2002) was also discussed, including an adjustment for any trend that might have occurred during that period. The paper's focus is on the summer of 2003 but it contains some analysis for the spring months of that year as well. Overall I thought it was an interesting albeit qualitative look at a very anomalous summer over the European continent. In the abstract the authors state that they “argue that a number of positive feedback

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effects between the weather conditions and ozone contributed to the elevated surface ozone". I do not disagree that the meteorological set-up over Europe during the summer of 2003 was a dominant factor that aided in an anomalous surface ozone event. However the arguments made by the authors do not appear to be quantitative enough to support their conclusions. The section in the paper that describes the meteorological conditions references several papers that describe the conditions that summer. However given the importance that is placed on the meteorology in this paper, I feel a more extensive and quantitative meteorological section is warranted. I feel that some analysis that looks at some meteorological parameters such as surface and 850mb pressure, surface temperature, and/or horizontal wind field and how they relate to the surface ozone distribution, residence time calculations, isoprene emissions and forest fire emissions would go along way in making a stronger argument. I also feel that a look at some ozonesonde data for the same summer might help to bolster the numbers seen with the EMEP network. Below I've made some specific comments (by section) where I feel that further analysis is required or some clarification is necessary.

Specific Comments: Abstract: 1. In line 10 of the abstract the authors state that they "show that the anticyclonic conditions during ozone episodes were accompanied by an extended residence time of air parcels in the atmospheric boundary layer". I disagree with the statement that the authors "showed" this relationship. They state in the manuscript that work done by Schär et al. (2004), Stott et al. (2004), and others described the meteorological conditions that set-up during this summer. I agree that the authors should not have to redo all the work that was done by these studies; however the crux of the paper is this meteorological set-up and its interactions with all the factors and processes that the authors bring up. I feel that a larger meteorological conditions section is warranted. They should at least perform some analysis of sea level pressure anomalies and/or temperature anomalies and/or precipitation anomalies so the reader can get an idea of the extent of these persistent anticyclonic conditions that had set-up. 2. In line 18 of the abstract, the authors state that the most important contribution to the enhanced ozone formation was less efficient dry deposition of ozone due to stomata

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closure of plants under drought stress. My concern with this statement is that if it's the most important contribution, why is it not addressed more adequately in the body of the text. I saw the Vautard et al. (2005) reference to some model calculations, however that alone does not make this the most important contribution. I think the authors either need to have a section in the text where they discuss this feedback or they need to take it out of the abstract altogether. 3. I do not know why the last statement of the abstract is there. The manuscript is discussing surface ozone not secondary PM. I feel it should be removed.

Meteorological conditions: I believe that I discussed my concern of this section in the discussion of the abstract above, but I'll reiterate it. The specific comment is the lack of any quantitative meteorological analysis given the importance placed on the meteorology within this manuscript.

Surface Ozone: In this section the authors describe the EMEP network. One general comment I have is the lack of comparison of the EMEP data with other data sources. Perhaps the use of ozonesonde data or other surface data available from WOUDC would help to support the EMEP findings. Specific comments: 1. I'm confused by the discussion in the second paragraph. Is the EMEP site actually part of a country's own network or is it entirely separate? Also has there been any validation work performed on this network? I think that if there has it should be included in the discussion. 2. The third paragraph discusses Figure 1. Just a clarification, are the annual maximum ozone concentrations shown in Figure 1 the actual peak one hour ozone concentration that was observed throughout the entire year? If it is, I'm not sure of the importance of this figure. I can see if it was an average value that relative to the 1991-2002 climatology is higher, but an hourly peak throughout the entire year seems to lose its meaning. Can the authors please expand on the value of Figure 1 or clarify for me what the importance of Figure 1 is. 3. Where did the long-range transport event discussed in the last sentence of paragraph 3 come from? I did not see any reference. 4. On page 9010 starting on line 6, there is a discussion regarding the relationship between low atmo-

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spheric humidity and ozone generation. I'm not sure of the purpose of this discussion. Can the authors please clarify by either leading in with or concluding with the purpose of this discussion. 5. The discussion in section 3.1, I thought, was particularly relevant. My only comment about this section is about the figures. It is difficult to differentiate between the "thin" blue lines and the "thick" blue lines (for Figures 4, 5, and 6). Maybe a different type of line would work better.

Residence time in the European boundary layer: I believe that this is one of the more important sections of the manuscript. The idea of residence time of air masses plays an important role when diagnosing the causes of enhanced ozone production, especially during anomalously high surface temperatures and very strong anticyclonic surface conditions. I feel that linking the residence time section with the section on prevailing meteorology would help show the reader the importance of recirculated air masses when it comes to ozone production.

Carbon monoxide: I think the discussion surrounding the Siberian boreal forest fires is also important. However, I do not see the benefit of Figure 9. The increase in CO seen in the figure does not appear to be that dramatic, or as nearly dramatic as the summer event is characterized to be, plus the location of these two European sites is pretty far north (the most southern one is at 66 degrees north). So I'm really not sure if the analysis performed here is needed. One thought is to remove the CO canister analysis and combine the Siberian boreal forest fire discussion and the Iberian Peninsula forest fire discussion into one section that discusses the potential impact of forest fire emissions on both background ozone and enhanced ozone production in general. Please give me your thoughts on this suggestion. I do have two specific comments about statements made in this section. The first is regarding line 12 on page 9015. The sentence starts off "It is, however, likely". I understand that those fires contributed to elevated ozone at some North American locations; however I'm not sure that you can say that these large scale burning episodes increased the background level of tropospheric ozone at northern latitudes in general. I would consider rewording

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the statement or taking a look at some other tropospheric ozone sources such as ozonesondes or satellite data at other northern latitude locations in order to see if they're also showing elevated amounts. The second comment is regarding line 19 on page 9015. The authors state that, based on their analysis, a positive feedback between global warming and tropospheric ozone is shown I don't see how you can make that leap and would recommend taking that statement out or expanding on it.

Isoprene: I think this section is important as evidenced by Figure 10 showing a drastic spike in isoprene during 2003. I agree that model sensitivity calculations would be a logical next step.

Total ozone: I'm not convinced by the argument that lower total column ozone led to an increase in surface ozone during the summer of 2003. Looking at the image of total ozone for August 10, 2003 (Figure 11), it appears that there is actually a large area over Europe that has total column ozone that is 300 DU or greater (which is an approximate average amount for the midlatitudes). Again, I think if you're going to attempt to show a relationship between total column ozone and surface ozone I think you need to be a little quantitative. I believe the TOMS website allows you to access the Level 3 data, which might better show what the actual total column amounts were during that day, allowing you to better determine the relationship between lower total column ozone and surface ozone. I would suggest either quantitatively analyzing it, expanding the Jonson et al. (2000) section to better explain what the model results were, or take out this section altogether.

Forest fires on the Iberian Peninsula: I think this section is important and very relevant. I agree that the forest fire emissions can possibly be linked to the elevated surface ozone, and the FLEXPART simulation helps to validate that. However I would possibly also look at what the flow regime was like from August 4th on. Looking at Figure 12, you can see the smoke plumes moving north. Looking at the flow for the next week or so not only at the surface but at different levels in the atmosphere might help to determine whether these emissions could be contributing to the production of tropospheric ozone

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or if they were getting caught in the westerlies and transported directly over most of Europe. Also, I would remove the word “clearly” (line 28 page 9018) since I do not believe that the authors have done that.

Conclusions: There are a couple of wording issues in the conclusions that I would consider changing. One line 10 on page 9019, the authors state that they clearly linked the surface ozone values to the meteorological conditions. I do not agree that that has been accomplished yet. As discussed earlier, I think the authors need to include some actual meteorological analysis in order to make this claim. On line 17 on page 9019, the authors state that the hemispherical background levels of ozone and CO were “presumably” elevated due to the Siberian boreal forest fires. I do not disagree that those fires had a large impact on ozone and CO at select areas in the northern hemisphere. However without some sort of quantitative analysis I think it’s hard to make that leap. And lastly on line 20 of page 9019 the authors state that the work they accomplished in linking the meteorology with enhanced surface ozone is also relevant for particulate matter. Like I stated earlier, the focus of the paper is surface ozone and meteorology, and I believe that you cannot make the same conclusion for particulate matter and meteorology.

Technical Corrections: 1. Abstract: Page 9004, Line 18, the words “sat on” do not appear to make sense with the rest of the sentence. I would suggest either removing them or finding a better word. 2. Introduction: Page 9005, line 13, the term AOT40 is used. Please spell out what AOT is the first time. 3. Surface Ozone: Page 9007, line 22, you have “for the regional concentration field”, while I believe it should be “of the regional concentration field”. 4. Surface Ozone: Page 9008, line 9, you have “does not refer”. I believe it should be “do not refer”. 5. Surface Ozone: Page 9008, line 20, you have “for Harwell were the 2003”. I believe it should be “for Harwell where the 2003”. 6. Surface Ozone: Page 9010, line 4, after Vautard et al. (2005) there is an “e.g.”. It looks like it should not be in there. Please clarify. 7. Surface Ozone: Page 9011, line 22, it has “Eq. (5) where then used”, while I believe it should be “Eq. (5)

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were then used". 8. Surface Ozone: Page 9011, line 25, the sentence on this line sounds confusing, specifically the way the words "statistically significant different" all seem to run together. Please clarify. 9. Carbon Monoxide: Page 9015, line 4, Should September 2002 actually be September 2003?

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9003, 2005.

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