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

Anonymous Referee #2

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General Comment: The paper provides a reasonable good description of the ozone conditions in 2003, and particularly the summer 2003, across the European EMEP network. However, the presentation of evidence for specific causes of the high ozone is limited, leading me to believe that the specific causes for the high oozone events are indeterminable from the evidence presented. Many of the conclusions are actually conjecture. Unless the authors can provide more and better evidence, then their specific reasons for the high ozone remain unproven hypotheses and not conclusions. I suggest that the authors look carefully at their reasons for the high ozone. If they can not provide better data or more data to support many of those reasons, then I suggest that they change Section 9 's title from "Conclusions" to "Discusstion of Results" and refrain from making conclusive statements that are not supported.

1. Section 1: Residence time is a function of wind speed in the surface and boundary

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layer, the residence time being greater for low wind speed. It has been known for quite some time that low wind speed is one of the environmental conditions that lead to high ozone. Large residence time in of an air mass in the boundary layer is a necessary condition for high ozone, but it is not a sufficient condition. The sufficiency comes only when the air mass has a large residence time in the domain of a high-pressure system [HPS] (i.e., an anticyclone) where environmental conditions (i.e., high temperatures, relatively clear skies, and low wind speed) exist that enhances the ozone chemistry and reduces dispersion. It is also depends on the how fast the HPS is moving, a quasi-stationary HPS being more conducive to the production of high ozone. All of these factors must be considered when looking at residence time as an ally to high ozone. 2. On Page 9010 of the paper that the authors state that "The ozone peak values and maximum in MDM in June and August 2003 presented above should be considered keeping in mind that the emissions of European ozone precursors have been substantially reduced during the last 10-15 years. The annual emissions of NOx and VOC within the whole EMEP region have been reduced by 23% and 32%, respectively during the period 1991-2002." The recent work of Vukovich and Scarborough (2004 -in AE) has suggested that NOx emission reductions of the order experienced in the EMEP region could under the right NOx to VOC ratio and environmental conditions produce higher values of ozone than those experienced in those same environmental conditions before the emission reductions. I have to wonder if we are not seeing this effect here. 3. The fact that there might have been higher values of isoprene in the EMEP during the summer 2003 is another possible reason for the high values of ozone during the period. However, the authors only had one site with isoprene observations, which limits what one can conclude about isoprene in the entire EMEP region. It is, under these circumstances, not realistic to make statements about the isoprene distribution in the entire EMEP region. 4. The sentence on page 9016 that states "Thus we conclude, that both the meteorological situation as well as isoprene measurements indicate that Europe experienced significantly elevated biogenic emissions during the extreme summer 2003, with a potential for increased ozone peak values, while the

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magnitude of this contribution has to be investigated by model sensitivity calculations." This is too strong a statement based on the evidence presented. More conclusive statements could be made with more observations of isoprene and the magnitude of this contribution has to be investigated by model sensitivity calculations. (However, see the point made in #10 below) 5. On page 9017, the authors state "During the August heat wave, low-ozone stratospheric air extended southwards from the Arctic toward northern Europe, as the large-scale circulation in the lower and mid-stratosphere was perturbed by large-scale waves." How do we know this is true? This is difficult to interpret from Figure 11. In any case, the authors should explain in some detail how they think that the low columnar ozone over the region is affecting the ozone in the boundary layer in the EMEP region. Furthermore, without any supporting evidence, anything said about how the low columnar ozone over the region is affecting the ozone in the boundary layer is supposition. 6. On Page 9017 the authors state "A reduced total ozone column will particularly alter the photolysis rate of O3 O'D which in turn may increase the OH concentration and thereby speed up the general oxidation rate of the troposphere." I presume what the authors mean here is because of the relatively low concentration of ozone in the stratospohere, more UV radiation penetrated to the troposphere, which could lead to greater OH production in the troposphere—presumably the boundary as well. However, the authors stated that that there was spring to summer dryness[i.e., drought conditions existed]) in the troposphere in the EMEP region. If this condition included low water vapor concentrations, then this conditions might not necessarily support the notion that higher UV led to higher OH concentrations. What kind of water vapor concentrations were experienced during the high ozone days? 7. The evidence that both the background CO and ozone were enhanced in Europe as a result of the massive forest fires in Siberia and Canada is an interesting observation. Since ozone begets ozone (Dodge, 1989–in JGR; Jeffries and Tonnesen, 1994–in AE), the higher background ozone could be one of the more important precursors for the high ozone experienced during the summer 2003. This should be considered in-depth by the authors. 8. It seems to me that the forest fires on the Iberian Peninsula might be

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very important as well since 1.) the authors previously stated that the forest fires in Siberia and Canada enhanced the background CO and ozone in Europe and 2.) their trajectory analysis showed that the air mass from that Peninsula. This goes back to my previous statement that a higher background ozone may be one of the precursors to the high ozone experienced during the summer 2003. However, do the authors have any CO and/or ozone observations that show the CO and the background ozone was enhanced when the air mass from the Iberian Peninsula reached the EMEP region? 9. I think the authors should change the title of Section 9 from "Conclusions" to "Discussion of Results." I do not think the authors presented sufficient evidence in the paper to state conclusions. 10. In Section 9, the authors stated that "the [ozone] removal mechanism due to dry deposition was reduced due to stomata closure of the plants." The plant stomata is presumed closed because of drought stress. They also state in the same section that 'the emissions of natural biogenic VOC increased significantly." I have always thought that if the plant stomata were closed, biogenic emissions should decrease, not increase. The authors base the increased biogenic emissions on one observation (See Section 6). Therefore, either 1) the authors notion that the ozone removal mechanism due to dry deposition was reduced to stomata closure of the plants is not correct because the stomata are open and emission of bioVOCs is increased, 2) the measurements of biogenic emissions shown in Section 6 are wrong, or 3) the measurements of biogenic emissions shown in Section 6 are correct, but they only represent the local area about the station and not the EMEP region. Generally, biogenic emissions increase with increased temperature. However, when moisture is not available as in a drought, the plants shut down under high temperatures and biogenic emissions decrease. Most models of biogenic emissions have the emissions decrease in a drought situation. The impression given in the paper was that the EMEP region was under the influence of a drought. The authors need to explain the basis of their findings in more detail.

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

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