

Interactive comment on “Impacts of using reformulated and oxygenated fuel blends on the regional air quality of the upper Rhine valley” by J.-F. Vinuesa et al.

Anonymous Referee #1

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The modeling study performed is a fairly routine assessment of fuel change/new formulation. This still could be a valid scientific contribution if it offered any new insights. Sadly the authors seem to have not thought through their results and what it means. There are claims in the paper that increased NO emissions will reduce the one hour peak ozone values. This needs to be a fairly high NO_x emissions to achieve this and under these conditions the reactions of NO_x with VOC's will reduce the available NO_x and thus could reduce ozone. Not as said in the paper, scavenging of ozone by NO. The NO as a sink for ozone will be active after sunset. Secondly it seems like about 40% of the VOC emissions in this region are related biogenic activity. All the formula-

tions (fuel) modeled increase NO_x emissions. This would indicate that the outside of the immediate urban zone, the ozone will go up and most likely not a particularly good solution on a regional scale. The terrain seems to be elevated and the possibility of ozone being transported down from the middle troposphere to the surface is real and in high altitude regions probably a certainty. There was no discussion in the paper on what was set for the model top boundary conditions. The lateral boundaries are set at about 70 km from the center of the domain. At 2 m/sec wind velocity the boundaries start effecting the models central region and the areas of focus in about 12 hours. These boundaries should either be further away and more carefully selected. There is no discussion on how the vertical profile of trace gases was set at the boundaries and if they change every hour/day.

On page 12 the authors say that "In addition, in these latter areas, the NO emissions are very low and favor the ozone accumulation process". By which I am assuming that they are saying there is less titration of ozone with fresh NO. First, the primary purpose of these types of studies is to evaluate 'peak ozone' value and second understand the '8-hr' in US and it seems like a '24-hr' ozone numbers in the EU. The ozone peak, if generated by local emissions happen during the noon to 2-pm time frame. There is no likelihood of NO titrating ozone at these daylight hours. The authors should check the 'ozone isopleth' plots in the Atmospheric Chemistry and Air pollution book by Seinfeld for example to see what could happen under extreme NO_x emissions and low hydrocarbon loads. There is a decrease of ozone under these conditions resulting from removal of NO_x from the system and not because of NO+O₃ reaction as far as I can tell. The ozone titration reaction is important during the early hours of the morning and nighttime. For example look at your figure 3, the ozone goes to zero during the nighttime and stays there during the early morning hours at the urban sites. Therefore the ozone titration will affect your minimum ozone values, which probably will be zero or near zero even with no reformulated fuel. The entire last paragraph on page 11 is one of the worst explanations of smog chemistry I have seen. The authors also propose a metric for calculating the change in 1-hr ozone and 24-hr ozone, VOC's and NO. The

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metric is calculated for each grid point. There is a problem applying this to secondary pollutant like ozone. When the fuel is reformulated and the amount of longchain alkanes decrease and the short chain alkanes are increased along with oxygenates, the reactivity of the emissions decreases. This could actually make the ozone peak move away from the previous location and generate a new one at some distance away. The way this metric is designed as the authors show generates large negative values for the RIA and RIMs and is very hard to interpret as can be seen from all the troubles the authors have in the manuscript explaining these metrics. I think this is a useless metric for this purpose. Applying this just for NO and all the VOC's also makes no sense, it may be better to apply for NOX and alkanes, alkenes separately. The calculations themselves may be fine, but the ineterpretation of results is terrible and I don't see the point in publishing this paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12067, 2005.

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