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Interactive comment on "Modelling photochemistry in alpine valleys" *by* G. Brulfert et al.

Anonymous Referee #3

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General comments:

Road traffic can cause serious air pollution problems in alpine valleys. In order to reduce the air pollution it is necessary to understand the complex processes leading to this problem. The Chamonix Valley investigated by the authors is polluted due to the intensive traffic through the Mont Blanc tunnel. The big fire in this tunnel allows to study the air pollution in the valley before and after the reopening of the tunnel. This is a unique opportunity to do an experimental investigation with different emission scenarios. In addition, it can be proved whether model simulations can reproduce the modifications of the air pollution being observed. All this aspects are considered in the POVA project. This project is a substantial contribution to the understanding of the

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relevant processes causing air pollution in alpine valleys. This aim of the POVA project is well elaborated in the abstract and also in the introduction of the paper.

However, the paper does not contain the whole aspects of the POVA project. Only one episode in July 2003 after the reopening of the Mont Blanc tunnel is considered in the paper. Already the title of the paper is misleading. Only the Chamonix Valley is treated. Therefore, this valley should be mentioned in the title. In addition, the authors should elaborate more the aims of their contribution to the project. The abstract should be modified, because the reader gets the wrong impression that the whole POVA project is subject of this paper.

Despite this remarks, the paper is a valuable contribution to the problem of modelling air pollution in alpine valleys. This complex problem is not yet sufficiently investigated. The paper is well structured and easy to read. In the introduction the authors include their investigation into the frame of studies that dealt with a similar subject, and they explain the specific aspects of their contribution.

The authors apply a quite complex model system for their simulations, which is described in chapter 3. The basic approaches used in the models are explained. Of course it is not possible to go into details, but some more information would be helpful for the reader (see specific comments).

The simulations are performed for a week in July 2003, which can be regarded as typical summer episode with mainly nice weather conditions. In order to achieve realistic boundary conditions for the domain with the finest grid, several nesting steps are carried out. This is a prerequisite to get realistic results in the inner domain.

Comparisons with measurements prove the quality of the results of the simulations. The interpretation of these comparisons is too positive. Weak points in the simulations are discussed insufficiently (see specific comments).

An additional interesting aspect of the investigation is the determination of the emission

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sensitivity regimes characterizing whether NOx or VOC emission reductions lead to a decrease of the ozone concentration. The authors find out that only VOC emission reductions decrease the ozone concentrations. The results are based on arbitrary 50% reductions of the VOC and NOx emissions. Can any relation be found with the real conditions during the period between the closing and the reopening of the tunnel? Are the result in agreement with measurements performed during the period when the tunnel was closed?

In the conclusions the authors emphasize that the model results are in good agreement with the observations. The comparisons show that there are some obvious deviations between the measurements and the model results. These discrepancies should also be commented in the conclusions.

The paper can be recommended for publication provided the following specific comments are taken into account and the questions raised are answered satisfactorily.

Specific Comments:

The TRANSALP experiment is mentioned in the introduction together with the citation of the paper from Loeffler-Mang et al.. This paper contains results from the TRACT experiment and not from the TRANSALP experiment. It did not take place in an alpine region. TRANSALP was a sequence of experiments studying only the transport of a tracer across the Alps.

Figure 2 shows the percentages of the land use types in the Chamonix Valley. A figure representing the horizontal distribution of the land use types in the valley would be more informative for the reader.

In Figure 4 a schematic representation of the model system is plotted. Four different models are applied for the simulations. The meteorological model MM5 contains a sophisticated nesting procedure. Why is it necessary to apply the ARPS model for the finer grids?

If possible, a nesting of results from two models using different physical approaches should be avoided. The same problem arises with the chemistry transport models CHIMERE and TAPOM. Do they use the same chemical mechanism? If this is not the case, there are problems in defining the boundary conditions of those species concentrations which are not included in both models. The authors should discuss this problem in the paper. Are the nesting steps and grid sizes in the chemistry models the same as in the meteorological models? These data should be included in table 1.

The emissions are available in a 100 m*100 m resolution. Usually the emissions become less accurate the finer the grid is. What about the quality of the emissions. The time resolution of the emissions should also be mentioned.

The ARPS model is coupled off-line with TAPOM. If the grid size of the models is small and the model domain contains mountains, like in this case, an off-line coupling may lead to errors in the results of the chemistry transport model. The magnitude of the errors depends on the coupling procedure. Therefore, this procedure should be shortly described in the paper. A coupling between the MM5 model and the CHIMERE model is not mentioned in the paper. From Figure 4 it seems that there is no coupling. Does this mean that the boundary conditions of the chemical species concentrations don't fit to the boundary conditions of the meteorological variables? The authors should discuss this problem.

In the chapter 'Validation' the simulation of a simplified case is mentioned. No results are presented and no comparisons with other data are carried out. Therefore, this part of the paper can be removed.

In the discussion of the comparison between measured and simulated meteorological data the authors state a good agreement. But there are some systematic deviations in the comparisons. At the station Les Houches the model overestimates the peak temperatures by about 3 degrees at five days and by about 10 degrees at the first day of the episode. At the fifth day the inverse effect is found. Corresponding deviations at the

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first and the fifth day of the episode are also found at the station Chamonix. It seems that the ARPS model can only simulate clear sky conditions. At the station Argentiere the minimum temperature is underestimated by about 3 degrees. The authors concede that there may be some problems with the lower boundary condition. Are there any measurements of the soil temperature and soil humidity available? Are these data used from the results of the model run with the next coarser grid? It is also obvious that at most days the simulated maximum temperature occurs earlier than the measured one. The authors state that the shifts of the wind direction at the monitoring stations simulated by the model occur at the right time. The comparison for the station Argentiere (Fig. 5) shows that the measured wind direction changes quite sharp, whereas the simulated wind direction turn lasts a longer time. In the afternoon the observed and simulated winds seem to turn in inverse directions. The authors should not only emphasize the good agreement. They should also discuss the deviations.

The comparison of the measured and simulated wind profiler data (Fig. 6) shows that the agreement for the lower altitudes is better than for the higher ones. It is very difficult to locate the height of the boundary layer. The authors should explain the criteria how they define this height. It seems that the mixing height modelled is lower than the measured one. Again, the authors emphasize only the agreement without going into more details.

The diurnal cycles of the ozone concentrations at the stations Chamonix, Clos de L'Ours and Bois du Bouchet are well simulated. This result is supported by the scatter diagrams in Fig. 9. In order to avoid misunderstandings the formula for the correlation coefficient R2 should be given in the text. At the stations Bossons (its location is not indicated in Fig. 1) and Argentiere the measured and simulated ozone concentrations agree less well than at the other stations. Especially, the night time ozone concentration at the station Argentiere is not well simulated by the model. Are there any reasons for this behaviour? At all stations the morning increase of the ozone concentration calculated by the model occurs earlier than in the measured data. Are there any local effects which are not resolved by the model?

Despite the comparisons between measured and simulated concentrations of the relevant species at the monitoring stations presented in the paper, the reader has no impression about the horizontal distribution of these concentrations in the valley. Therefore, it is necessary to show such distributions at least for ozone on one day and for the two wind regimes in the valley together with the corresponding wind fields.

The authors state that the peak ozone concentrations in the valley are mainly influenced by the background ozone concentrations as can be seen from the ozone concentrations measured at the stations Plan de l'Aiguille and Col des Montets and the correlations plotted in Fig. 10. Figure 11 shows that a VOC emission reduction causes a decrease of the ozone concentration. Because the peak ozone concentrations are mainly determined by the background ozone concentration, the greater reductions cannot occur in the afternoon. For which period of the day these reductions are calculated?

Technical comments:

In Figure 7 the scale of the vertical coordinate for the station Bois du Bouchet should be the same as for the other stations.

The simulations by the model CHIMERE are carried out with grid sizes of 27 km and 6 km. Is this correct?

The captions of some figures should be more precise (stations are not compared to models).

In Figure 3 (lower picture) the grid resolution of D3 should be 3 km and not 2 km (see Table 1)

Which time is TU (see caption of Figure 7)?

The date of the reopening of the Mont Blanc tunnel should be mentioned in the paper.

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Interactive Comment

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1797, 2005.

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