Atmos. Chem. Phys. Discuss., 14, C4935–C4937, 2014
www.atmos-chem-phys-discuss.net/14/C4935/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol-CFD modelling of ultrafine and black carbon particle emission, dilution, and growth near roadways" *by* L. Huang et al.

Anonymous Referee #2

Received and published: 16 July 2014

The paper entitled "Aerosol-CFD modelling of ultrafine and black carbon particle emission, dilution, and growth near roadways" by Huang et al. deals with CFD simulations using the ANSY FLUENT of ultrafine and black carbon emissions. The topic of the paper is very interesting and overall the attempt of including the chemistry in this type of CFD simulations is of great relevance. Nevertheless I found that the manuscript itself is not properly constructed because I found little matching between what is written and what is shown. The manuscript also suffers from a poor use of Tables and Figures, too limited in my view which make the reading rather heavy. I suggest the authors to reduce to minimum the number of in-test numbers and to summarize them whenever possible

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

in tables. The authors may have considered to use Appendices to provide all required details without distracting the reading from the main message. Given the complexity of the various elements composing the paper each requiring specific attention and depth, they are instead all mixed together failing in conveying robust conclusions. As a reader of a scientific publication I would search for details on novel ideas or models or maybe I could be interested simply in the treatment of boundary conditions and then how field data are fed into the CFD simulation. All I was hoping to find is missing. Despite in length the introduction focused on the justification of k-epsilon in versus LES, little is new in the CFD modelling. The treatment of ABL in CFD is rather known as well as the role of turbulence in the mixing process. Instead I was also surprise no mentioning of the diffusion process and no discussion on the Schmidt number. This may have a significant role especially considering the different phases of the aerosols dynamics. Why the authors believe that ABL parameterisation is more important than the diffusion part? There is evidence for ignoring this aspect? Nevertheless information on the Schmidt number needs to be provided and choice properly justified. In general, the validation of the CFD is poor. No discussion has been included in the verification of the boundary dimensions and grid size. There is no statistics reported on the number of runs made and whether the assumption of stationary conditions hold Some comments should be added here. In general the paper requires some major restructuring before it can be considered suitable for publication. In my view is poor on both the experimental description and in the CFD.

Specific issues 1) I found specifically that the description of the atmospheric conditions is limited or hardly documented e.g. the authors claim that the simulations refer to neutral conditions but the period of measurements is between 5-6am or 6-8am... now the time of the year is missing and generally at the sites' latitude 6-7 can be dark and cold and therefore typically stable conditions occur... unless we are in the summer, in this case it is more likely that a convective boundary layer is growing. Surely neutral conditions would require rather high wind speed – again wind speed information is missing! Another missing information is concerned the wind direction. Simulations

ACPD
14, C4935–C4937, 2014

are run for conditions perpendicular to the road. . . how many cases have been used? 2) Figures need substantial improvement. Fig.1 needs to include more details about dimensions and type of boundary. Perhaps a Figure showing the measurement site and physical distances would be helpful with an indication and a summary of the meteorological conditions including stability (perhaps through a Richardson number or the Obukhov length scales). 3) I suggest the authors clearly describe the different cases in the CFD simulation. 4) I suggest to summarize somewhere the CFD validation (type of runs, how many etc) and indicate whether in simulating various types of cars you also changed the dimensions of the cars. If yes obviously this would require a further assessment of the grid influence on the solution. 5) An indication of the effect of the Schmidt number is mandatory.

Minor issue 1) Please revise the English whenever possible, I found not too technical in several occasions. Often the authors refer to pollution gradients which is incorrect. Also they refer to “decay of turbulence mixing decay”? This is not clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 12235, 2014.