Atmos. Chem. Phys. Discuss., 15, C12265–C12267, 2016
www.atmos-chem-phys-discuss.net/15/C12265/2016/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling and measurements of urban aerosol processes on the neighborhood scale in Rotterdam, Oslo and Helsinki" *by* M. Karl et al.

Anonymous Referee #2

Received and published: 9 February 2016

This manuscript addresses the questions of how important coagulation, (evaporation?), condensational growth and dry deposition are for particles emitted from roadways. It attempts to use a dispersion-aerosol dynamics model with some field data for comparison, and then tries to parameterize the model output to produce simplified expressions for dry deposition and coagulation for uses in Gaussian plume models etc. The manuscript is well written, although it expects quite a bit of familiarity with background material, and thus makes it hard to follow for those not closely aligned with the work.

I have several significant concerns with the manuscript. These include:

1. Several fundamental assumptions seem to ignore processes that could make major

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

qualitative differences in the results.

1.a. Given the relatively large contribution of diesels to the vehicle fleets, and in port cities such as Rotterdam, marine vessels their attendant fleet of lorries, the contribution from fractal agglomerates would seem to be much too high to ignore. Ignoring these particles and their very different behavior appears to be capable of introducing a significant error. The distribution data seem to imply significant contributions from diesel particles for some sites (such as Rotterdam) which have few very small particles. At the very least this the magnitude of the potential error from this omission should be estimated. Possibly differences between the sites could be used to better constrain MAFOR.

1.b. Ignoring van der Waals forces leads to underestimation of coagulation rates that is of order a factor of two for the smaller particles (10s of nm). This will make a very substantial difference in the outcome of the calculations, and one that cannot be ignored.

1.c The parameterizations and resulting rate estimates for dry deposition in the literature span more than an order of magnitude, and the calculations in this manuscript appear to be toward the high end. The manuscript would be much more useful and enlightening if it were to address this large source of uncertainty as well. Possibly the data again could be used to constrain dry deposition parameterization schemes. But at least this needs to be addressed directly as a source of uncertainty.

1.d The authors completely ignore additional emissions sources that happen between the roadside site and the “background” site. How does this effect the uncertainties on the estimates?

Both the van der Waals and the diesel agglomerate issue have effects that goes in only one direction, and we know which direction that is. Further, it is possible to estimate (bracket) the size of most of the effects. In this situation, the authors need to address the size of the potential effects, and also include in their analysis the fact that both 1.a and 1.b skew the results in a single direction (unless fractal agglomerates behave like

ACPD
15, C12265–C12267, 2016

spheres and van der Waals forces do not exist, but there is abundant evidence to the contrary in both cases). The dry deposition uncertainty can go in either direction, but also results in large uncertainties. 1.d may be a smaller effect.

2. All studies considering the effects of particle dynamics in emissions to the atmosphere struggle with the issue that dilution is responsible for the majority of the changes observed in particle concentrations and size distributions (see also comments from Prof. Harrison and co-workers). This means researchers are trying to tease out the small effects of coagulation/condensation/deposition (and evaporation?) when the vast majority of the aerosol evolution is caused by dilution, not any of the other processes.

2.a. First, the manuscript would be more clear if the contribution of dilution to the final concentrations was included explicitly.

2.b. The test applied to verify the MAFOR is working – start out with near roadway air, dilute with background air and arrive at the background concentration profiles needs to be tested for sensitivity to processes other than dilution, and to the dilution scheme as well. The bugger with this type of analysis is that the model needs to meet quite high standards to be able to verify parameterizations for dry deposition, condensation and coagulation. And even then, given the uncertainties for the minor processes, it is hard.

3. The estimates of all processes need well founded error bars, or at the very least a hard-headed discussion of uncertainties. This will go a long way to addressing comments 1. And 2. If done well, it will make the manuscript much better.

4. P 35184 Given the uncertainties in coagulation resulting not just from not interacting size bins but also the assumptions in comment 1, the claim of dry deposition rates within 10% seems optimistic.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 35157, 2015.

ACPD

15, C12265–C12267, 2016