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Interactive comment on "Aerosol and NO_X emission factors and submicron particle number size distributions in two road tunnels with different traffic regimes" *by* D. Imhof et al.

Anonymous Referee #1

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This is a well written and presented paper which is technically sound and in a topical area. Although other papers have been published on similar topics, the results make a valuable addition to the technical literature. The research has been conducted in a careful and thorough manner and the results are presented and discussed in a technically sound way. The inclusion of work from two road tunnels which shows their different behaviour is a significant asset.

There are a number of minor aspects in which the paper could be improved.

a) Page 5135 Lines 11-14 Accurate measurement of the fluxes of inlet and exhaust

air is essential to the calculation of particle fluxes. More detail of how the flows were measured (presumably by profiling in at least two directions) and how the measured data were then processed would be valuable. b) Page 5141 Line 6 The term “maximum diameter of the nucleation mode” is confusing. At first reading it appears to imply the largest diameter within the nucleation mode, whereas in fact it means the most frequent diameter of the nucleation mode. c) Page 5142 Lines 15-24 The general interpretation of a competition between condensation onto prexisting surfaces and nucleation is accepted. However it’s difficult to comment on whether tunnel conditions are representative of the outside atmosphere, since the former is influenced by ventilation rates and the latter by atmospheric conditions. Actually specifying a surface area threshold where the changeover in behaviour occurs is probably beyond that which is warranted. d) Page 5146 Lines 5-11 Whilst the reviewer can accept that the regression technique can be used to estimate PM1 values from measurements of total volume, the fact that the volume measurements cease at a diameter of 700nm, means that part of the volume is not included and therefore the estimate of effective particle density is erroneous. Some comment on this by the authors is required. e) Page 5147 Lines 18-23 No mention is made of the fact that the PM2.5 measurements were made with a TEOM instrument with a heated inlet, whilst the PM1 measurements were made with a gravimetric sampler at atmospheric temperature. Consequently PM1/PM2.5 ratios derived from the measured data will differ from those measured if both instruments had the same sampling characteristics. f) Page 5148 Lines 3-4 The negative PM10 emission factor calculated for LDV may not be statistically significant but, even allowing for the confidence intervals, the emission for PM10 from LDV is smaller than that for PM1. This casts considerable doubt on the error limits attached to the data and the authors need to be more cautious in quoting their emission factors.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 5127, 2005.