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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Linking aerosol fluxes in street canyons to urban city-scale emissions" *by* B. K. Tay et al.

Anonymous Referee #2

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Overall comments:

The paper describes CFD simulations of street canyon flow with a line source with ultrafine aerosol characteristics. Isothermal and heated wall simulations are completed, and a simple parameterisation of net aerosol fluxes from canyon top is derived as a function of flow variables. Whilst the results are generally interesting, there are some key scientific points which need clarifying. The paper is also not clearly written in many places, and contains typos in text and figures.

Key comments:

18069, L18-9: "Steady-state solutions were obtained for all cases" – is that referring to the current work? And "..code was validated..." is that referring to the Ketzel et al work, or the current study? This is unclear, a little more detail required to establish what the authors have done themselves in terms of validation.

18069, L20: good comparisons of which variables? K-epsilon codes may simulate mean flow fields adequately but can struggle in simulating the turbulence fields accurately, particularly in the vicinity of flow separation points. This is important to establish, to understand whether the tool being used for the study has limitations

18072, L7: is it realistic to use exhaust temperature 10K above background air temperature? If buoyancy effects due to the exhaust are "ignored" as stated earlier, why not set it to the ambient temperature? Otherwise it seems an unrealistically small temperature difference.

18075, section 3.2.2: is it possible to compare the aerosol results with Meroney/Pavageau results for tracer gas from a line source? It is not clear whether your aerosol modelling includes deposition, and other inertial effects: comparing with gas concentration distributions is not ideal, but given the lack of data, general windward/leeward wall concentration profile trends could be compared. This also would help to highlight the differences between particulate and gaseous pollutant behaviour.

18081, L10: the authors report that the heat flux is always negative (meaning downward, into canyon?), and the canyon temperature steadily increases – this seems physically implausible, especially given that the temperature gradient is negative (i.e. street is warmer than air above), so how can there be a counter gradient heat flux for heat but not for aerosols, especially in such a turbulent flow and where both have in-canyon sources? I agree with the authors that there may be overall differences in flux magnitudes due to differences in diffusivity, but differences in sign? Is there a problem in the definition of heat flux? How is the turbulent eddy diffusivity for heat defined? The paper records no details about wall boundary conditions for the heat flux (see e.g. Sini et al 1996). These are important points to cover, otherwise the validity of this section, and perhaps the heated wall results, is doubtful.

18091, Table 2: the values of TI are pretty low for an urban roughness sublayer, where TI can reach 1 near buildings. Please justify why this range of values was chosen and why it should span the typical range of scenarios. The use of a uniform height windspeed profile at the inlet should also be carefully justified, given that in reality there is approximately logarithmic increase with height, as this affects shear in the region of cavity top and crucially affects your results.

Specific comments:

18067, L6: I think "urban roughness sublayer" is meant here?

18067, L9: street canyon, not canyon at least at first time of mention

18068, L1: aspect ratio H/W is probably sufficient – suggest removing AR throughout and replacing with H/W

18068, L5: Baik and Kim do not deal with aerosols, they use a passive tracer.

18068, L17: the reference is Barlow and Belcher (2002), not Barlow (2002)

18068, L27: "surface fluxes" not "surfaces fluxes"

18069, L7: is the CFD code commercially available or was it written in-house? Please state its source, and provide reference for the "standard turbulence model equations"

18069, L20: remove "Trapos network" from the brackets

18069, L22, L25: is "vertical cavity dimension" identical to "height of the canyon"? please use consistent words, and better still the symbol H. The geometry of the "inlet scales" and outlet horizontal scales are not clear – are these the dimensions of the computational domain? "outlet horizontal scale was 10x the cavity dimension" is particularly unclear. Consider adding a diagram.

18070, L1: what does this mean? "domain had a total of e.g. 70, 500 grid cells"

18070, L4: please state the range of windspeeds. Is this relevant if you are not simulating vehicular turbulence?

18096, Fig1: why do the streamlines appear paired in 1a) and more evenly (distributed according to the wind field) in 1b – is this actually what the flow does? Also, please state what "medium level turbulence" means? Does it assume a certain value of TI in equation 1?

18097, Fig2: in caption, it states "s=2.5 m/s", is this correct or should it be U?

18071, L6: "Reynolds stresses of aerosol concentration" doesn't make sense: the stresses refer to momentum exchange, not a scalar concentration. Please modify sentence.

18071, L11: this I think should be F_t, not F_a

18071, equation 3: why is the turbulent flux expressed using the 1D K form, rather than the covariance stated above? Is this how it was determined, by calculating Kx and the gradient in aerosol concentration at rooftop?

18091, Table 2 and 18076 L23: you define turbulence intensity as the RMS of fluctuations divided by mean wind, why are units squared here?

18073, L18: which "CFD experiments" – do you mean yours? Please clarify or include references

18098, Fig 3: figure legends, labels too small

18102 Fig 7: this has confusing labels: the caption says it is net flux, the small axes labels on both sides say advective flux (which is what it is...?). It might be clearer to include all aspect ratio results on the same axis, to better highlight the change in sign: it would also more clearly show the lack of sensitivity to TI of some runs. Some appropriate colour/markers would help an already busy plot. BUT: some very interesting results here!

18074, L5-15: please refer to figures where the data is represented so that the reader can verify your statements, even though the figures are discussed in more detail later on.

18074, L20 : viscosity – mu seems to be molecular viscosity on 18071, here it is turbulent viscosity, K? Please use consistent notation

18074, L21: "concentration shear" is confusing – shear relates to a force/stress – do you mean concentration gradient? Please be clear in following discussion to use shear in relation to wind magnitude/direction gradients.

18074, L25: at what height is the vertical concentration height defined? Roof level?

18079, L7: "The implications....would need further investigation" – this could be better phrased, as the next two sections seem to report on the "further investigation"?

18084, L19:"...fit the CFD results to the numerical model (Eq 2.0)". This is not clear – equation 2 is the inlet turbulent dissipation profile. Should it be equation 4, ie the proposed parameterisation? This whole section is not clearly written – have you applied the simple parameterisation from CFD results to the observations of Martin? What is "averaged diurnal observation of emission fluxes", compared to the label on fig 17 "averaged diurnal aerosol flux range" – I think it means you have taken the average flux across a diurnal cycle from Martin's observations to compare with parameterisation prediction? Figure 17 shows a very good agreement, considering the difference in scales!

18100, Figure 5: Please label using consistent labels, i.e. aspect ratio values same as other plots. Maybe also avoid shallow, symmetrical/square, deep labels as these are subjective. The vertical axis seems to be mis-labelled – turbulent fluxes are always positive? Should be concentration gradient.

18103, Fig 8: the label says 2 m/s, the caption says 2.5?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 18065, 2009.