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Interactive Comment

Interactive comment on "Attenuation of concentration fluctuations of water vapor and other trace gases in turbulent tube flow" *by* W. J. Massman and A. Ibrom

W. J. Massman and A. Ibrom

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We thank the reviewer for his/her comments. I (wjm) will answer these comments in order. If the comments seem sufficient to revise the manuscript I will also note that as well.

(1) Most models of the turbulent tube flow velocity profile are quite similar in shape, so in this regard there is little significance to the change in velocity profile from the 1991 paper. However, what is significant is the decoupling of the model diffusivity profile, $D(\rho)$, from the velocity profile, $U(\rho)$. The first paragraph of section 2.2 explains why this is important. It is also important to note that the models of $U(\rho)$ and $D(\rho)$ used in the 1991 paper had two adjustable parameters that were tuned to fit the data. I did

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Interactive Discussion

Discussion Paper

not want such a model for this paper because I did not want the model to obscure the possibility that the additional attenuation observed by Lenschow and Raupach (1991) resulted from the use of water vapor. So I specifically sought models of $U(\rho)$ and $D(\rho)$ that had fewer free parameters and that were less sensitive to those parameters. I was concerned that the success of the 1991 model to fit the observed data might have been an artifact of the formulation of $D(\rho)$ and its sensitivity to the model parameters. If this were the case it would tend to obscure the physics necessary to describe the interaction with the tube wall. I am willing to revise the manuscript on this point if the reviewer and editor feel to do so will improve the paper.

(2) The effects of wall saturation and temperature on molecular and turbulent viscosity are assumed to be small enough to ignore. These effects are likely to be significant only if the tube is specifically being used to remove (condense out) the moisture from the flow stream. In this case the additional condensation on the tube walls may influence surface roughness and even the cross sectional area of the tube and clearly if large amounts of moisture are being condensed out then the heat exchange between the tube wall and the vapor sample stream should be included in the model. I am willing to revise the manuscript on this point if the reviewer and editor feel to do so will improve the paper.

(3) For a passive scalar the present model is a much better (more physically realistic) model. But for application to any given site either model is probably acceptable because most sites (as I understand) use Eqn (9) with Λ_1 as an adjustable parameter to compensate for effects such as bends in the tube and other issues causing departures from the ideal (long straight) tube. Otherwise Eqn (26) without the G_1 term can be used for a passive scalar in the same way. I am willing to revise the manuscript on this point if the reviewer and editor feel to do so will improve the paper.

(4) For use with any given experimental setup Eqn (26) can be simplified to the following expression: $\Lambda_{1W} = G_0 + G_1 \widetilde{h}\, e^{l_* \widetilde{h}}$, where G_0 and G_1 are adjustable parameters. I am

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willing to revise the manuscript on this point if the reviewer and editor feel to do so will improve the paper.

(5) I think an annotated table of symbols would improve the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 9819, 2008.

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