This paper generalises the MLM formalism of Nakamura and collaborators that describes tracer evolution with respect to a tracer-based coordinate system. This formalism has been profitably applied over the last 20 years or so to interpretation of model data and observations on stratospheric chemical tracers and indeed to study stirring, mixing and tracer transport in other atmospheric and oceanic contexts. The new element of the formalism presented in this paper is that in principle it allows consideration of local processes, whereas the standard formalism considers averages around the tracer coordinates. Additionally it claims to provide terms in the equations under the new formalism that are individually associated with stirring and with mixing.

After setting out the new formalism, the paper applies it, and claims to validate it, against model simulations of stratospheric methane.

I do not regard this paper as suitable for publication in ACP. I have no reason to dispute the details of the mathematical derivation set out in Section 2, but it is complicated and poorly motivated. There are many 'correct' rearrangements of the equations for tracer transport, but the key question is whether a particular rearrangement is genuinely helpful in understanding the processes operating and their effect on evolution observed in models or observations. Convincing the reader of this requires clear motivation for the various decisions made on how to rearrange and group terms. I don't see that here. Furthermore I don't see from the model results presented in Section 3 any evidence that this new formalism is genuinely helpful in understanding stratospheric tracer transport. If there is any 'validation' here for the new formalism then it is in the very narrow sense of 'validation' = 'not wrong'. Comments in the text are essentially saying 'the diagnostics we obtain from this new formalism are consistent with what we know already about stratospheric tracer transport'. That doesn't seem very strong validation to me.

There are further serious questions that can be raised about individual components of the paper -- does the idea of separating 'stirring' and 'mixing' make any sense when to a large
extent stirring is a one-way route to mixing? Can any useful information really be extracted from the data presented in Figure 2 to obtain estimates of the value of diffusivities? What useful has been learned from applying the new formalism to analysis of the evolution of methane in the polar vortex presented in Figure 10?

The standard of presentation in the paper is generally poor, both in terms of clarity of explanation of details and in justification of the key principles of the approach. I am very doubtful that readers of ACP will gain anything useful from this paper.

Detailed comments (brief and not intended to be complete)


l89: ‘D’Ovidio et al’-- but more generally you have given no information on how your formalism relates to these previous papers (D’Ovidio et al, Nakamura 2001) which went beyond tracer-contour-averaged diagnostics.

l128: ‘Generally the relationship ... is one-to-one ... ’ -- the only way it wouldn’t be is if sigma < 0 somewhere -- which would have other implications for the system that you are considering.

l210: The equation above this line is essentially unreadable. If you are serious about presenting this equation then you need to choose a notation that gives the reader a better chance of understanding the structure.

Equation (12): You claim that these quantities do not require isentropic wind information. But partial m/partial t|^stir does, according to (10b), require isentropic wind information. How can this be resolved?


l412: I don’t understand this point re ‘frequency distributions’. But more seriously, how exactly has ‘tendency due to vortex variation’ been calculated? Perhaps ‘tendency due to stir, mix and vortex variation’ is simply obtained by time-differencing the depicted time variation? A general comment here is that the general reduction in CH4 concentrations through the winter is surely due to diabatic descent bringing down low values of concentration from the upper stratosphere (e.g. Randel et al 1998 JAS, Juckes 2007 ACP). You haven’t mentioned that at all.