Report on revised version of 'The importance of vertical resolution in the free troposphere for modeling intercontinental plumes' by Zhuang et al.

The revisions to the paper are welcome. The addition of the reference to D'Isidoro et al (2010) and associated discussion (p 3), as well as the discussion of the scope of the work (specifically the exclusion of convection and boundary layer turbulence), helps to set a better context and perspective for the work.

I have a remaining general comment on perspective, a number of minor comments, and one important comment concerning possible errors in the figures. While it would be nice to address all these issues, I think the 'important comment' is the only comment that's essential to address in the event that the authors consider, but disagree with, the other comments.

General comments:

I think there remain a few places where the perspective could be further improved. For example the authors say (p 13, line 31-32) 'the best possible simulation would preserve the entropy'. If, in the true solution, there are a lot of small filamentary structures on small scales which are much smaller than the grid scale (as seems likely at large times), then one has a choice between an entropy conserving solution with the filamentary structures being resolved and so on the wrong scale, or a solution which smoothes these small scales out and increases the entropy. It seems clear that the second of these, which does not preserve entropy, is to be preferred. One could argue that the authors' statement is correct if they mean the best possible simulation at any cost, but even this is doubtful because in reality there is molecular diffusion which will act on the smallest scales (as acknowledged in the discussion associated with the D'Isidoro et al reference). Note I'm not saying that reducing the increase in entropy a bit isn't a good thing (the increase may be too big due to numerical errors), but I am saying that 'the best possible simulation would preserve the entropy' is going too far.

I note that the Methven and Hoskins situation (mentioned in the authors' response), where small scales in the tracer field are generated by a smooth large scale wind (with chaotic trajectories), is in effect the situation I discussed previously in relation to Batchelor's k^{-1} viscous-convective regime. If the smaller scales in the velocity field are present in reality (i.e. are not absent due to a very large viscosity and large Prandtl number), then not including them in the simulation is likely to lead to too much, not too little, small scale structure in the scalar field. This doesn't require a response but I mention it as it provides more context for some of my previous comments.

Important specific comment:

It looks from figure 6 as though none of the C48 cases have entropy below 19 on day 8. However figure 7 shows a case (C48,L160) with entropy of about 18 and another between 18 and 19. Also it looks from figure 6 like some of the L20 cases have entropy below 19 on day 8, but figure 17 has them all above 19. Have I got this right? It's easy to get it wrong with the C and L numbers increasing as Δx and Δz decrease. If I'm right, have the authors got the figure 7 entropy the wrong way round, or have they got the figure 6 plots the wrong way round, or is it a consequence of the contour plotting program doing some smoothing? It's harder to see, but the right hand plots in figure 6 also look inconsistent with figure 7. I think this needs to be resolved before publication.

Minor specific comments:

Page 11, lines 28-30: This discussion of convection and boundary layer turbulence is welcome. However one can have plumes which remain low in the atmosphere (even in the boundary layer) and plumes can be affected by convection at any point in their transport. Hence it is not the case that convection and boundary layer turbulence are only relevant as part of 'plume initialization and termination processes'. I think it would be fine to simply say that convection and boundary layer turbulence are out of scope.

Page 12, line 5: It might be useful to say something about why the plume subsidence is typical of observations, although this is a side issue to the main topic of the paper and so not essential. It's not clear what the authors have in mind. On average vertical velocities must be zero, but this might not be so in particular parts of the world. The vertical velocities may be highly skewed, with an extreme case being deep convection and compensating subsidence, leading to mostly slow subsidence but occasional rapid uplift. Finally a plume released high up is likely to end up lower. The Crawford et al 2004 reference only appears (based on a very quick look) to argue for subsidence as a result of the particular flow regime being considered. Possibly the authors mean that the subsidence seen is a 'typical rate of subsidence for subsiding plumes', not that subsidence is typical?

A comment on Reviewer 3's last comment and the authors' response: I agree with reviewer 3 that it would be interesting to do this test with the initial plume having smooth edges, although this could be regarded as a separate project. I'm not sure how to interpret the authors' response. Perhaps they have done the test and the entropy increases by a factor of about 20 at the highest resolution – if so it might be of interest to report this in the paper.