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Interactive Comment

Interactive comment on "Deep-convective vertical transport: what is mass flux?" *by* J.-I. Yano

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The present contribution did not intend to add anything radically new, but, as clearly stated in the introduction, it merely intended "to make a point clear that mass flux is a more concrete elementary concept.....". In this respect, I am very pleased to find the referee #2 stating that "the paper,...., does not demonstrate any conceptual problem to exist" in the beginning of the comment. That is exactly the main point of the present paper. As stated by the referee #2 two paragraphs later, "there is no fundamental ambiguity regarding the meaning of the various mass fluxes". That is exactly what I intended to say in this contribution.

Though I stated that the current understanding of "mass flux" in the convective parameterization community (specification was by following a suggestion of the editor, Peter Haines, though I had a wider community in mind) has a "peculiar status" in the original manuscript, I could not find any concrete reference to demonstrate that point except for an unspecified workshop personally reported to me by Alan Grant. However, very

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ironically, after receiving three comments on this article (two reviews and a comment by Mark Lawrence and Marc Saltzmann), I find no need to add any extra references about a confusion of understanding of mass flux. In spite of my painstaking effort to clarify the concept of "mass flux" by the present contribution, I am afraid, all commentators still have difficulties in comprehending a basic notion of the mass flux that I tried to explain.

A very basic point, that I even did not mention in the paper, is that a transport rate (vertical in the present case) of any physical quantity (chemical spices in the present case), consists of the two parts: 1) a mass transport rate, represented by mass flux, and 2) a local value of a transported physical quantity. The total transport is obtained by multiplication of these two quantities.

The main point of my argument was that the local value of the transported variable in subgrid scales (as schematically shown in Fig. 1) has a more central role than the mass transport rate, though the previous analyses on convective chemical transport tends to emphasize the latter than the former. Unfortunately, all three comments miss this main point, but they raise their rebuttal strongly focusing on the mass transport rate (or "pathways") issues, neglecting the points concerning the transported variables.

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I definitely agree with the referee #2 that "All aspects of the mass-flux formulation, ... are important". Against the assertion of the referee #2, I did not try, in any place of the paper, "de-emphasizing the fundamental physical decomposition". It is totally contrary. I painstakingly tried to explain the fundamental importance of the physical decomposition in mass flux approaches by totally devoting Section 2 to a historical review of Riehl and Malkus (1958).

The decomposition is definitely a critical starting point of the mass–flux formulation. There is no question about it, but a more important point is that the decomposition is made in a very particular manner. That is the whole point of the historical review of

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Section 2.

I believe, the abstract states these points clear enough. The second sentence says that "The main idea of the convective mass flux formulation does not *purely* reside in dividing the grid box....". This rather compressed sentence clearly makes the two distinguished points. First of all, the decomposition is a central prerequisite for mass-flux formulation, but more importantly, the mass-flux formulation contains something more than a simple decomposition. This second point is made more specific in the sentence that follows: "The main point rather resides on assuming different vertical profiles for transported quantities for different components". Such a particular profile is achieved by assuming a locally isolated "tube like" structure as painstakingly explained in Section 2.

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On the other hand, Referee #1 begins the review by stating "...This large scale resolved velocity provides a robust, although slow transport pathway from the surface to the supper tropical troposphere...", then the Referee concludes the fist paragraph by asking: "So what does it really mean to examine the impact due to deep convection on trace species?"

The present contribution precisely tries to answer this question by alluding to a classical notion of the thermodynamic convective transport. Although the atmospheric movements are upward both in large-scale mean and in convective scale, the characteristics of thermodynamic transport associated with these two upward motions are drastically different, as painstakingly discussed in Section 2 of the paper.

In other words, although the large-scale resolved vertical velocity may be considered to provide a "robust transport pathway", the transport characteristics are drastically different from those of convective transport. The main reason for this difference stems from a drastically different vertical profile of transported quantities as schematically shown in Fig. 1. The vertical profile of transported quantities defines the drastic dif-

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ference between large scale and convective transports. Thus, the importance of the characteristics of transported quantities must be well emphasized.

Once, this very basic point is well understood, as far as my very personal point of view is concerned, it is relatively straightforward to find an effect of convective transport in atmospheric chemistry, as discussed in Section 4 of the paper. Of course, this is a matter of interpretation, and I have argued in no place of the paper that this is an unique way for turning off convection in chemical transport.

Rather I made my best efforts in Sec. 4.a of summarizing the arguments of Lawrence and Salzmann (2008). My own point of view is merely mentioned as an alternative in Sec. 4.b without placing any absolute value on this. My alternative view does not lead to anything new but merely reproduce a standard porocedure of “turning off” parameterized convective chemical transport in a numerical model.

For this reason, I was completely confused by finding myself in position of being criticized that I did not provide a unique answer for turning off convection. Nor I see any obligation to perform any model experiments to defend any methodologies, against the criticism of the Referee #1, because both of the two methodologies under discussions are already proposed.

Especially, the first scheme is already implicitly proposed by Lawrence and Salzmann (2008), but they did not test this new scheme at all. My own contribution in this respect, is merely in stating this possibility in a more explicit manner.

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Disagreements between Mark Lawrence, Marc Salzmann and me may be summarized as follows.

As discussed in Sec.4 of the manuscript, under the conventional approach, convective transport is "turned off" by setting $(\partial Mr/\partial z)_c = 0$ in Eq. (13a). As a consequence, convective *mass* transport is replaced by large-scale *mass* transport. According to

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Mark Lawrence (personal communication), thus, such a procedure "only provides information on the effects of the part of convection which is not directly connected to large scale circulations, and on the importance of the difference between rapid lifting in tubes versus slow mean lifting (at the same equivalent mass flux rate averaged over the size of a grid cell)". Alternatively, by taking a paraphrasing of Marc Salzmann (personal communication), the conventional procedure "merely replaces" convective upward motion by that of large scale. In my own understanding, this is the main argument of Lawrence and Salzmann (2008).

I can agree, at least, "as a kind of rhetoric" that in that sense, convective transport is not completely turned off. This would still be a valid alternative point of view.

In turn, my main point is: because a change of transport characteristics by replacing the convective transport by large-scale counterpart is so drastic that, nevertheless, it is best to call this procedure "turning off" convection. As carefully discussed in Section 2, if convective transport is turned off in this manner, the tropical heat budget becomes totally different. Consequently, even though the conventional method may look like not totally turning off the effects of convective transport, the "main" function of convective transport is effectively turned off by this method. The mass-flux convective parameterization is specifically designed for describing such a unique convective transport process.

It should be emphasized again that the characteristics of transport by "real" deep convection due to "tube" mechanism is drastically different from that of a mean large-scale ascent. This is definitely true for heat transport, and it expected to be true for many chemical spices.

The next challenging question for me is how we can improve the understanding of convective chemical transport processes by following Riehl and Malkus' "tube" mechanism argument. If we could find a well conserved (passive) chemical tracer constantly emitted from tropical oceans and effectively destroyed only at tropopause, such a tracer

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would generate a vertical profile qualitatively similar to that of moist entropy but different in details due to a different generation rate from the surface and a distraction rate at tropopause. Thus, such tracers would provide independent measures for a spectrum of convective "tubes" or convective mass fluxes.

The problem becomes more challenging in real chemical transport problems, because all the chemical spices decay at finite time scales. A sink term must be added to a budget analysis for these realistic chemical spices. This generalization under mass-flux formulation would be straightforward, once a fractional area occupied by deep convection could be specified. However, specifying the latter is not straightforward. The problem becomes more challenging when rapid, complex chemical reactions are going on inside deep convection.

Clearly, the convective chemical transports are far more complex processes than convective heat transport. A direct analogy of the latter with former would be misleading. Despite criticism of the Referee #1, I did not attempt any direct analogy between them in the paper. Nevertheless, I believe that the framework of Riehl and Malkus is extremely rich and it will serve greatly in promoting our understanding of all these complex convective chemical transport processes.

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With all of these respects, I sincerely request the two referees to read the current manuscript carefully again and try to find out how transport by large-scale mean ascent and by convective upward motion makes a difference. Issues are not of "pathway" (or trajectory) itself, but what we *see* along a given "pathway" (or trajectory). A different pathway provides a different vertical profile for transported quantities. In other words, an important issue to count on is local characteristics of transported quantities as well summarized by Figure 1 of the manuscript.

Unfortunately, I see all the commentators having difficulties in fully comprehending this basic point. Exactly for this reason, I believe, the present contribution is worthwhile for

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a publication in ACP.

In preparing a version for ACP, I would like to synthesis all the points of debates so that the article could be presented more in a self-contained manner, though I would like to wait an ultimate decision of the editor in charge on this matter.

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Finally, I have to admit a rather trivial mistake in the submitted manuscript to ACPD: I forgot to add a density factor in definition of the mass flux. This mistake will also corrected in the final version to ACPD. Nevertheless, the point should be made clear that none of the commentators picked up this rather trivial mistake,

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Reply on specific issues (in order of publications):

Referee #2:

- 1st paragraph: please note that an "alternative" solution to this problem presented in Sec. 4.b yields an identical result to a procedure *criticized* by Lawrence and Salzmann (2008).

- Residual environmental descent M_e': here, I merely tried to reproduce discussions by Lawrence and Salzmann (2008). I personally do not see any ambiguity associated with this concept, either.

- Yes, it is true that moist entropy is not strictly conserved under precipitation, although the effects would be minor. The point would be remarked as a footnote in the final version for ACPD.

- Eq. (11) can be re-written as

$$M_c = M_{c,0}\mathcal{H}(z - z_s)\mathcal{H}(z_t - z) \qquad (R.1)$$

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in terms of the Heaviside's stepfunction $\mathcal{H}$ defined by

$$\mathcal{H}(x) = \begin{cases} 0 & x < 0 \\ 1 & x \geq 0 \end{cases}$$

Note that a derivative of the Heaviside's stepfunction leads to Dirac's delta:

$$\delta(x) = \frac{d}{dx}\mathcal{H}(x) \qquad (R.2)$$

By taking a vertical derivative of Eq. (R.1) and a substitution of the formula (R.2) leads to Eq. (11) of the text.

In turn, Fig. 2 was confusing, so will be revised in the final version for ACPD.

Referee #1:

1) What is the point of quoting St. Augustine? The goal of the paper is not propose any new methodology for assessing the role of convective chemical transport, but merely in clarifying the concept of "mass flux" in convective transport little.

2) Figure 6 of Mapes (2001) clearly shows that observed vertical profiles of moist static energy (which can well approximate moist entropy) are in agreement with a schematic profile shown by a solid curve in Figure 1 of the manuscript.

3) The argument here is simple as follows: whenever a large quantity is multiplied by a small quantity, the product is always smaller than the former. We cannot say about the degree, but that is the only point of the discussions in lines 11–14 in page 3540.

More specifically, in traditional approximation, the convective vertical velocity is assumed to be the order of $w_c \sim O(\sigma_c^{-1})$, thus $M_c = \sigma_c w_c \sim O(1)$. This point would be added as a footnote in the final manuscript for ACPD.

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Regardless of the details of the logic, the convective mass flux turns out to be the same order of magnitude as the environmental descent as clearly demonstrated by Figure 4 of Lawrence and Salzmann (2008).

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