



Interactive comment on “Mass-flux subgrid-scale parameterization in analogy with multi-component flows: a formulation towards scale independence” by J.-I. Yano

Anonymous Referee #2

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I have severe misgivings about this paper which are detailed below. However, the main criticisms are :

- (1) the paper makes no comments or suggestions on the key parameters of entrainment or detrainment that are introduced. As such the proposed framework doesn't appear to represent a significant improvement over current parametrizations.
- (2) It is not clear how the large scale flow enters into this framework, or the method for applying the results of the parametrized processes to the large-scale flow which, after all, is the purpose of a parametrization scheme in a large scale model.
- (3) The paper does not really justify the use of the mass flux approach for a wide

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range of processes. The suggestion that a 'unified' approach is the road for future parametrization down plays the importance of a better understanding of the physical processes which is needed to decide how a parametrization should be constructed.

The paper concludes that implementation of this proposal would be a major exercise but I think that without clarifying the above issues it appears to me to be impossible. In addition the uncertainties discussed in section 4 would be sufficient to ensure that no NWP/climate centre would be tempted to try and implement the proposal.

General comments

Introduction

The introduction refers to fractional areas of subcomponents relative to the model grid size. However, the grid scale really determines the minimum scale of the large-scale flow that could be represented by the model. The fractional area of convection should be relative to the physical scale that the model is to simulate.

The paper is clearly focused on tropical convective flows. In this case the precipitating stratiform subcomponent is part of a larger convective system that includes the convective plumes (the smallest features in Fig. 1). This connection is important, but is not necessarily relevant to all stratiform cloud, e.g. those associated with mid-latitude baroclinic systems. Can the clouds in mid-latitude systems be included in the the proposed framework. The answers to questions such as this are critical for considering the proposed framework for implementation in a climate/NWP model.

Section 2.

(2.1) The inequality given by Eq. 2.1 really only defines a requirement on the grid scale. Clearly $L \ll \Delta X$ is needed for the numerics to represent the large-scale flow accurately. The condition $\Delta X \gg \Delta x$ is the standard assumption that allows a unique separation between mean and fluctuations by filtering and allows the use of the Reynolds averaged Navier-Stokes equations. The model formulation (both dynamics

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and physics) should ensure that variations in the output from a parametrization varies smoothly on the scale ΔX . Equation 2.1 mixes up what are numerical requirements with those that are physical approximations.

The small fractional area assumption used by Arakawa and Schubert (1974) is an additional assumption that allows the Reynolds fluxes to be parametrized in terms of the properties of an updraught ensemble and the large scale flow. If this assumption is relaxed then it is not clear that the usual mass flux approximation applies (for example, a different approximation is appropriate in the dry convective boundary layer). The introduction implies that the proposed parametrization can be applied to boundary layer problems, but it is not clear how this would be done.

I didn't find the digression into multi-component flows to be of any real benefit to the paper.

(2.2) The paper suggests that a non-hydrostatic framework is reasonable for developing a parametrization because the interest is in the effects that the subgrid variability has on the large-scale flow. However, to the extent that the parametrization contains information on the properties of the sub-grid variability these properties will reflect the non-hydrostatic nature of the small scale flow. In particular the present approach requires entrainment and detrainment rates to be specified and however this is to be done it must reflect the non-hydrostatic characteristics of the subgrid flow.

(3.1.1) In this section the paper considers interactions between the subcomponents, leading to the need to define entrainment rates between components. However, within the proposed framework positional information of the subcomponents is lost since all of the integrals are carried out over all of the elements that belong to a particular subcomponent. How does the proposed scheme determine which subcomponents are in contact.

(3.1.3) The upstream approximation is a numerical approximation and is not a fundamental aspect of the physical approximations.

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(3.1.4) What is ϕ_j in Eq. 3.14

(3.4) I don't understand why Eq. 3.20 is not Galilean invariant, or why Eqs 3.8a,b change under Galilean transformation. What are the implications of this lack of invariance.

(4.2) The paper argues that the proposed scheme does not need a closure in the same way the classic approach of Arakawa and Schubert (1974) does. However, simply stating that this is a problem for data assimilation is not helpful since it is unclear how the subgrid divergences would be estimated in practice. In addition how would such a scheme interact with the surface where appropriate (for example in representing cold pools). Is it assumed that a separate boundary layer scheme will be run? How would this be incorporated into the proposal?

(4.3) Determining the entrainment and detrainment rate is as crucial in this proposal as they are in current mass flux convection schemes. However, the present paper suggests no new ideas about this problem. Since these are the key physical parameters in the proposed scheme it is not clear that without a better understanding this approach to parametrization offers any advantages over conventional approaches.

(4.4) The author states that this scheme does not need a triggering condition but does require that the fractional area of the subcomponents remains finite, if small. However, in reality particular subcomponents, such as those shown in Fig. 1, are not present everywhere, all of the time. So the reality is that fractional areas do go to zero, and the requirement for finite fractional areas is unphysical.

(4.5) This section is odd in suggesting that deep convection be excluded, given that the parametrization of deep convection motivates the proposed framework.

(4.9) I think the problem of resolution dependence in current parametrizations arises because there is no clear definition of what the scale of the large scale flow represented by the model, although shortcomings in the parametrization schemes are also

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important. The key change in the nature of the parametrizations occurs when the horizontal scale of the large-scale flow to be represented is comparable to the depth of the troposphere (for moist precipitating convection). This is when horizontal flux divergences are likely to become important and the nature of the parametrization problem changes to become properly three dimensional. For larger scale flows, as considered in this paper, the scale of the large scale flow is much larger than the depth of the troposphere, the horizontal flux divergences are small and hence a one dimensional approach to parametrization is appropriate.

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