**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Explicit cloud-top entrainment parameterization in the global climate model ECHAM5-HAM" *by* C. Siegenthaler-Le Drian et al.

Anonymous Referee #2

Received and published: 17 March 2011

This paper presents an attempt to improve the climate simulation of boundary layer clouds in the ECHAM5 model. This model uses a turbulence parametrization that, as the authors note, has been demonstrated to perform poorly so here they attempt several changes (but mainly focus on adding an explicit representation of cloud-top entrainment) in the hope of improving that performance.

I certainly admire the authors' honesty in presenting results from tests that really look remarkably poor, in terms of their model's inability to reproduce even very basic features of stratocumulus clouds, such as a well-mixed boundary layer at night. I found it extrememly frustrating reading this paper that their hands are apparently tied regarding

a vertical resolution which is so clearly woefully inadequate for the complexity of the processes they are parametrizing (especially the vertical distribution of terms in the TKE budget). That said, I would have found their arguments much more compelling if they had shown results from a high(er) resolution SCM simulation and used that to motivate their changes at coarser resolution. In particular, the reason for introducing the new radiative term in the buoyancy production, in (18), is not explained well at all. My interpretation is that they are trying to reproduce the cell-averaged buoyancy production near cloud-top which would in reality be driven by cloud-top radiative cooling destabilising the θ_v profile, given they can't resolve this process. Deardorff's α term was to represent the preconditioning of entrained air through radiative cooling within the inversion and has nothing at all to do with buoyancy production in the cloud layer. Its role here looks much more like an arbitrary tuning coefficient, without any data shown against which to tune it! Some basis on physical reality could be gained even from comparison with the buoyancy flux profile from a high resolution "truth" SCM simulation.

To summarise, too much of what has been done has not been sufficiently justified, seemingly arbitrary choices have been made using crude switches, insufficient detail is given in the results from the SCM (such as the flux profiles and split between clear sky and cloudy entrainment) and their explanation of the results, particularly in terms of the "correct" location of TKE and its sources and sinks in the vertical make little sense. I therefore recommend it be rejected for publication.

Further detailed comments in the order they appear are:

1. p1982: "the PBL top is not readily found in a GCM due to the low resolution". There are significant issues simply in how you define the PBL top, regardless of resolution.

2. p1983: the requirement that there is subsidence is actually a strong and, I would have thought, unnecessary constraint. GCMs typically have a pdf of w even

above stratocumulus that easily spans zero. So, for example, as gravity waves pass over your stratocumulus you will shutting off entrainment for reasons completely unrelated to the physics of the stratocumulus. Why is this constraint necessary, given you already require a cloud layer capped by a strong inversion?

3. LTS is a very crude requirement that stops the parametrization being used for a significant fraction of the world's stratocumulus when it clearly ought to be and brings with it an imposed climate change sensitivity (since LTS is likely to increase in a warmer climate). Even just insisting on an inversion strength of a few K would seem preferable to me, and just as robust.

4. p1983: "observations of entrainment rates show values one order of magnitude higher". You must mean entrainment efficiency, or entrainment coefficient, as typically entrainment rates in cloud free convective PBLs over land, for example, are significantly higher than over stratocu due to their much weaker inversions.

5. p1985 and Fig 6: I find this schematic confusing rather than helpful. "χ in the gridbox above the cloudy layer [k-1?] is supposed to be completely homogeneous". So why do χ^c and χ^e differ in Fig 6 in the cloud-free grid-level above the cloud-top and why should they have different gradients above that?

6. section 3.5: at the opening of this section you state that if the Sc criterion is fulfilled the buoyancy flux includes a contribution from the LW flux divergence. Yet a few lines later you say "the radiative contribution...is applied above all clouds" and this is justified because "radiative cooling occurs at the top of all clouds". This distinction between when to apply the extra radiative cooling term and the entrainment rate parametrization seems completely arbitrary and only suggests seriously crude "tuning". Ie, something must go badly wrong if these are made consistent. But if that is the case then it makes me highly concerned whether the whole approach has any validity.

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7. p1990, "STD...destabilizes the profile at cloud base (instead of at cloud top)". This statement exemplifies everything that is wrong with this paper! To me there is nothing wrong with STD in this regard, given its resolution. The top grid-level containing cloud is cooling radiatively, becomes destabilized compared to the level below and thereby generates TKE in the PBL. The fact that there is only one grid level with cloud, so that this TKE is generated at the base of the cloud layer is purely a function of the resolution and **only increasing the resolution can improve it**. ENTR, by contrast, generates TKE in the strong stratification above the cloud, which is completely unphysical. You then use the entrainment parametrization to specify the fluxes at this level for the cloudy part of the gridbox and use these large TKE values to generate the fluxes in the clear sky part of the gridbox. Given you have significant TKE in strongly stable stratification, isn't (2) going to give large clear sky fluxes? Or do your "newly computed mixing length and stability function for the clear sky part" stop this happening? It is crucially important that you show both the resulting flux profiles from the SCM and how they are partitioned between cloudy and clear skies in order to demonstrate the method has any basis in reality. Potentially excessive clear sky entrainment might explain how the inversion remains static in the SCM simulations when there is huge imbalance between subsidence and entrainment, see below.

8. p1991, LWP is "smaller in MCV than in STD because of higher precip during the first hours of simulation": there is absolutely no way this initial extra precip could affect LWP in the subsequent days! Please work out the real reason.

9. Section 4: no comment is made on the lack of a well-mixed PBL at night which would also in reality generate positive surface sensible heat fluxes. This is a fundamental failing in the performance that needs to be examined in great detail. Why is there a stable (and foggy) layer at the surface? A possible explanation comes a few lines later: "the θ_l profile of ENTR is more stable because of the warming of the cloud top due to entrainment". In reality, cloud-top radiative

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cooling would outweigh entrainment warming, thereby destabilising the PBL. If entrainment warming dominated there would be no cloud-top destabilisation, no PBL turbulence and so no entrainment. Why doesn't that happen in the SCM here? I'd strongly recommend checking those clear sky fluxes!

10. p1992, "In STD and MCV,the cloud layer is colder than the sub-cloud layer": according to Fig 11 this isn't true!

11. p1993: "the mean entrainment velocity ...is 0.5mms^{-1}. The subsidence velocity at this height is roughly 5mms^{-1} (large-scale divergence for ASTEX was $5x10^{-6}s^{-1}$, I believe). So, in theory the inversion should be dropping at $\sim$ 400m per day. It clearly isn't so something other than the explicit entrainment must be maintaining the inversion height. Are the turbulent fluxes at cloud top consistent with the explicit entrainment rate (and the inversion jumps)? As above, what are the clear sky fluxes (cloud fraction is never more than 0.7 so these will be important)? Alternatively, or additionally, following Lenderink and Holtslag (2000), what happens if you increase the applied subsidence? I suspect the cloud-top will still not fall (untill all the cloud is evaporated), suggesting there is also spurious entrainment from a mismatch between the turbulence and subsidence.

12. p1995 "we obtain a reduction of the dependence of LWP on CDNC in ENTR": but all you've done is to reduce the LWP for all CDNC (and in fractional terms you've done this more at low CDNC, reduced to 82%, than high, 85%). There is clearly no fundamental change in behaviour at all.

13. p1995 "the free atmosphere above the inversion [in ENTR] contains more vapor" and "if we look at the inversion on top of the PBL, the profiles ...simulated by ENTR are worse than ...MVC". In what way are these results worse as there is no truth to compare against? The increased moisture between 900 and 950 hPa must simply be because, during this averaging period, the PBL in ENTR was as deep as 900 hPa more often (which I would have thought was an improvement).

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14. p1996 "The observational data": I assume you mean ERA, which is a blend of observations and model data - "reanalysis data" perhaps?

15. p1997 "one can see some improvements...ENTR produces fewer clouds in the stratocumulus region": this is surely much worse, not an improvement!

16. p1999 "it may be due to the modification of the triggering of the convection": is there some change made to the code here, to alter how convection is triggered or do you mean the change in convective activity in the model?