Environmental sensitivities of shallow-cumulus dilution
Part 2: Vertical wind profile

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This study explores how cloud dilution is affected by environmental shear using simulation ensembles of shallow convection in maritime and continental conditions. Idealized shear profiles are designed to test the effects of shear within the cloud and subcloud layers separately. These experiments produce contrasting results in that cloud layer shear leads to enhanced dilution, while subcloud layer shear leads to reduced dilution. The authors then use theory and simple calculations to discuss the various factors driving these behaviors.

I really like this paper. The experiments and analysis are thoughtfully designed and clearly presented. The writing is also very clear. I think it will make a great contribution to the existing literature.

On a personal note, I've tried to investigate this same question of how shear influences dilution in several different ways, but I never found any notable relationship. After reading this manuscript I realize that all my experiments were similar to the ARM SGP simulations presented here in that the convection was mainly forced by a large buoyancy source, and my shear profiles created a cancellation between competing effects. So I really appreciate that the experiments here demonstrate how the effects of shear are conditional on how the convection is being forced and whether the shear is in or below the cloud layer.

The language tends to focus on how the experiments are looking to make a direct connection between shear and dilution, but the results seem to confirm that the changes to dilution are secondary to how the shear impacts the cloud momentum and initial turbulence statistics. This doesn't make the authors' wording right or wrong, but I do wonder if the summary language (maybe the abstract as well) could be altered to reflect this. This is a pretty loose observation, so take what you want from it.

I have outlined a few other minor concerns below, but otherwise I think the paper is in great shape.

Are the WIND experiments really needed?

I'm not convinced that the "uniform wind" experiments add much to the analysis given that the surface friction leads to subcloud layer shear that is so similar to the SCL-SHR experiments. If the wind was nudged to maintain a profile that was closer to the initial profile then I would feel differently, but as it stands the data from the WIND and SCL-SHR results are very similar, so the WIND experiments don't seem to add much insight.

It also seems sensible to show the CL-SHR and SCL-SHR results next to each other to better highlight the contrast in the dilution results. For instance, combining Figure 5a-b with

Figure 7c-d would highlight the main conclusion more clearly in my opinion. I realize this might involve a substantial reorganization of the paper, but it might be worth it. I'll leave it to the author to decide whether to keep the current form or not since the main conclusion is not affected either way.

<u>Connection between shear and updraft width</u>

Since this paper talks about cloud width in sheared environments it might be good to mention the results of this recent paper by John Peters:

> Peters, J. M., Nowotarski, C. J., & Morrison, H. (2019). The Role of Vertical Wind Shear in Modulating Maximum Supercell Updraft Velocities, *J. of the Atmos. Sci.*, **76**(10), 3169-3189.

That paper is not directly applicable since it focuses on supercell storms, but the connection they draw between cloud width and storm relative inlow seems like it could play a role in other cloud types. The relationship between subcloud layer TKE and cloud width in the simulations presented here is probably the more accurate and relevant explanation for shallow convection, but the consistency between these results that more shear always leads to wider clouds is intriguing.

<u>Dilution estimation terminology and method</u>

On line 189 the authors introduce the dilution calculation method by saying:

> "*Diagnosis of the simulated bulk fractional <u>entrainment</u> rate, or simply the 'dilution rate'...*"

I think it's important to highlight that the "bulk dilution rate" calculated here includes the effects of both entrainment and detrainment (at least that's what I get from the description of the calculation). I know that often "bulk entrainment" implies that the detrainment is assumed to be zero (except at "cloud top"), but there's been so much inconsistent use of this terminology that it seems important to be as explicit as possible when discussing this stuff. I would suggest doing a little rewording to clarify what the bulk dilution estimate is actually measuring, and note that it's not just the effects of entrainment that would be estimated by a direct measurement scheme, like the ones by David Romps and others.

It also might be worth showing the budget equation used for the bulk dilution calculation. It might seem unnecessary since budget equations like this have been shown in so many papers, but I think it helps the reader understand the nuances of what is actually being calculated when the equations are shown in their full detail. This wouldn't need to include any sort of derivation, just the final equation that the dilution estimate is based on.

<u>Use of the sigma symbol</u>

I got thrown off a little bit due to the sigma symbol being used for 2 different things: the convective growth rate on line 295 and the cloud layer TKE on line 395. I think it's worth changing one of them for clarity's sake.

Aerosols

I’m not normally someone to bring this up, but since you’re comparing continental and maritime environments it would be good to mention if there’s a difference in CCN concentrations. I couldn’t find a mention of this in the current manuscript, so let me know if I missed it. I doubt the droplet size distribution would matter for this study, but it’s been such a hot topic in the field that it’s always good to mention how the model is configured in this respect.

- Walter Hannah