WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Investigating the impact of atmospheric stability on thunderstorm outflow winds and turbulence" *by* Patrick Hawbecker et al.

Anonymous Referee #1

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In this work, the role of the evening transition environment upon idealized downburst winds is explored. The evening transition environment is spun up and at different times during this transition, downbursts are initiated utilizing a cooling source forcing. The downburst outflow characteristics are then explored. One of the most significant findings of this work is that the strongest near-surface donwdraft winds (and subsequent maximum near-surface horizontal winds that result following the impingement of the cold downdraft) increase for increasing stability, a somewhat unintuitive result. The authors conclude that rising buoyant thermals in the less stable environment act against the downdraft, weakening it, even though the negatively buoyant air is descending in an absolutely unstable environment. The authors conclude it is the (a) suppression of turbulence in the more stable regimes (b) a stronger ring vortex (c) increased negative

buoyancy "generated from the vertical advection of the surface based stable layer by the ring vortex" that are primarily responsible for the increased winds. They also invoke RKW theory to explain the stronger roll vortex in the stable environments.

I appreciate the efforts the authors have gone through to create the evening transition environments in which the downbursts are initiated. Most, if not all, studies of this nature tend to force the downbursts in horizontally homogeneous environments. I also appreciate that the authors have run ensembles for each environment in order that some statistics can be generated.

Issues, roughly in order of importance:

I have concerns with the experimental design. In the S1 and S2 environments, cooling sources are initiated in environments where buoyant thermals are numerous. Ostensibly these buoyant thermals are forced by a surface that is warmer than the atmosphere immediately above it, from solar insolation. However, I am not convinced that there should still be enough buoyant thermals occurring underneath a real cumulonimbus cloud that is in the process of creating a dry downburst. At this stage in a thunderstorm's life, wouldn't one assume that free convection has been suppressed already by the lack of solar insolation? Or is there enough "stored heat" in the ground to continue to force convection in real storms? These thermals are the main factor the authors invoke to explain their unintuitive results, and I am wondering, is this a physically plausible experimental design? Because to me this is probably the most important result if it's right, and suggests that future simulations of downbursts should incorporate these fluxes rather than just assume a horizontally homogeneous base state.

The model grid is isotropic with a grid spacing of 28 meters. This seems to be pretty coarse vertical resolution near the ground for a study such as this that concerns wind turbines. In Figure 5, the strongest horizontal winds associated with the downburst are always found at the model's lowest grid level. With a semi-slip simulation (in this case, a surface roughness of 10 cm) I would expect there the height at which the maximum

outflow occurs to be above this level. A lot of attention in this paper is paid to the bottom 100 meters of the model domain, which only covers 3 or so grid levels. The difference in downdraft maxima and outflow maxima is pretty small, varying less than 10% from S2 to C1. In many wind engineering studies of downburst outflow, the height of the maximum horizontal winds is one of the important findings. In this paper, it's not discussed, and seems to be assumed to always occur at the lowest model grid level, which seems to indicate insufficient vertical resolution near the ground.

It appears the authors are holding the cooling source function fixed in space even though there are environmental winds at the level where the downburst is initiated. Wouldn't one expect the air-mass thunderstorm creating a downburst to be (roughly) translated to some extent with the environmental winds? Is it physically realistic to hold the cooling source fixed in space when embedded within synoptic scale winds? See for instance Orf and Anderson (1999) where their cooling source was translated in the same direction as the environmental winds in a simple linearly sheared environment.

One would expect that there would come a point where the stable layer would ultimately become deep or strong enough to result in the "intuitive" result of suppressing strong outflow winds. Have the authors run additional experiments in a later nighttime environment to see at what point that point occurs? While downbursts aren't very common late at night, they certainly do happen (and sometimes result in heatbursts!).

The authors describe the stabler environment as having more low-level shear. A hodograph of all five environments (I presume you'd need to take average winds at each level) at the time of downburst forcing would be useful to quantify this; you might be able to combine them in a single figure.

Concerning the important role of turbulence / buoyant thermals, I'd like to see a horizontal cross section of the vertical component of the wind at the time the downbursts are initiated to get a feel for the scale and strength of these thermals, that seem to be playing a crucial role in weakening the downdraft.

p17L13 "It is also speculated that with a shallow surface-based stable layer such as in this current study, it may be possible for the increased negative buoyancy generated from the lifting of the colder air at the surface to further strengthen the outflow and ring vortex in the stable cases." I do not understand this sentence, which seems to be somewhat tautological. With a shallow stable layer, one would expect adiabatic compression of the downdraft to lead to warmer (or at least less negatively buoyant) surface air as it descends through an absolutely stable layer. How does the perturbation potential temperature get *more* negative descending in a shallow stable layer? I do not understand the physics behind this sentence.

Why are the authors running with such a short time step on their finest mesh? 0.1 seconds seems to be unnecessarily small considering the maximum winds found in the simulation (Courant Friedrichs Lewy). Are the authors using a model with acoustic substepping?

In Figure 5, the vertical cross section is held fixed at 6 km. Why not move it so that it's in the same roll-vortex relative location? The top profile cross section seems especially too far west compared to the other two. It's not clear that this is the best way to compare the simulation velocity profiles.

Please be sure to create all of your line plots as vector graphics, not bitmapped, such that they can be zoomed in for the PDF version of the paper.