Cooling Layer Experiment

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1. Why do I care?

I would like to study the conversion of KE to ME in rotating stars (i.e., dynamos) especially as the rotation rate is varied, in a high-Rayleigh-number regime where diffusion is not dominating the value of the KE (that such a state exists was shown nicely in Featherestone & Hindman 2016: FH2016). To get this right, I need to be sure (or at least believe) that the total KE in the convection available to convert, the fluid Rossby number, and the global differential rotation/circulation, are behaving reasonably with variation of the rotation rate (which I will vary by varying Ro\_c). A priori, I do not think the global sims are behaving reasonably, because of the way they are driven (a lot more on this below).

So at order 0 (before rotating and magnetizing) I propose to change the nature driving! Now for the gory details.

1. Introduction: mixing-length theory (MLT).

MLT offers a very simple and tantalizing view of stellar convection, which results in typical flow speed U ~ (F/rho\_0)^(1/3), where rho\_0 is the background density and F = L/(4 pi r^2) – F\_rad is the flux deficit convection must carry in a steady state. **Under MLT, flow-speed, flow length-scale, the KE, the thermal fluctuations, and maybe the local Rossby number (if rotation doesn’t change things dramatically) are all known functions of depth.** I would argue that to a large extent, the reality of the MLT picture of convection remains untested numerically. This is because stratified convection simulations are run with spurious preferred length-scales: the box-scale (in many local models, if the box isn’t big enough) and the depth of the conductive thermal boundary layer at either end (almost all models---except the ones with top radiative MHD---both local and global, that I am aware of have such a conductive boundary layer).

The goal of this document is to outline an experiment that removes the conductive boundary-layer depth (at both ends) as a relevant quantity. As explained below, this is the situation more in-line with how stellar convection seems to be driven, at least for the Sun; possibly all stars, if my understanding of low-mass structure was more complete.

But first, here is how MLT arrives at its nice result (it is non-trivial, and the assumptions below should be tested systematically in a stellar-like system). **For now, ignore both rotation and magnetism.**

1. In a strongly stratified star, convection is driven locally. It thus occurs with a length-scale l ~ H\_rho, where H\_rho = -(dln rho\_0/dr)^(-1) is the local density scale height.
   1. This is a mass-conservation argument. And implicit is an anelastic-like assumption: the density of moving fluid parcels is never too far from the background stratification rho\_0 (r).
   2. If (a) holds, an upflow quickly (after it traverses a distance H\_rho) becomes much less dense that it was before and must diverge horizontally to conserve mass (the other case of diverging vertically seems to never be considered; perhaps it would lead to something unphysical?).
   3. The diverging fluid cools adiabatically from decompression, eventually becoming cool enough to fall back down (i.e., the fluid overturns over a scale height, since it went up, sideways, then down).
   4. The same argument applies to downflows, which converge, heat up adiabatically from compression, and become upflows.
   5. All in all, l ~ H\_rho. U ~ l/t, where U and t are the typical velocities and overturn times, which are still unknown.
2. Convection must transport flux F. This means
   1. F ~ F\_conv =rho\_0 **u** (c\_P\*T’ + u^2), where **u** is the vector fluid velocity. **This assumes an ideal gas** (otherwise, the internal energy term e, which here = c\_V\*T’, is complicated).
   2. The locality assumption allows us to set each term to be roughly equal. If convection is driven locally, we expect U^2/l ~ (T’/T\_0) g, and then we use g/T\_0 = c\_P/H\_rho (the hydrostatic background assumption)
   3. This assumes molecular viscosity and thermal conduction do not affect the transport (probably a good assumption because nu and kappa are so small; but also non-trivial since the fluid could be extremely turbulent, in which case there are very small scales transporting energy that are not taken into account by the above arguments).
   4. Really, we have made a number of other implicit assumptions, none of which are clear:
      1. The correlation coefficient between u\_r and T’ is reasonably close to +1. This seems reasonable at first (from buoyancy considerations), but is complicated especially considering entrainment / non-locality.
      2. The KE flux is negative. This is actually non-trivial. We know |u\_up|< |u\_down|, especially where H\_rho is small. We also know that [integral over upflows] rho u\_up = - [integral over downflows] rho u\_down, after a temporal average, to conserve mass. However, if you do the Reynolds decomposition of < rho u\_r u^2> you find one term that is always negative for |u\_up| < |u\_down| (and this is the term that makes us believe the KE flux is negative) but there is a correlation term that has unknown sign, depending on the filling factors of u\_up and u\_down and the asymmetry in the two distributions of u\_up and u\_down.
      3. The KE flux is not very nearly equal and opposite to the enthalpy flux. Assuming the second correlation term mentioned is irrelevant, this requires the asymmetry |u\_up|< |u\_down|, to be small enough.
   5. We assume no great asymmetry between vertical and horizontal flow speeds, so u\_r ~ u\_H ~ U.
   6. Once we have b (F\_conv ~ rho\_0 U^3), we simply equate F ~ rho\_0 U^3 and arrive at the MLT result. Yay!
3. How convection is driven in stars (at least in the Sun).

In my view, after a few days’ obsession, MLT has one very nice piece of observational evidence, which is solar granulation.

1. **In any star, energy comes from the thermonuclear core in the interior (Stein + Nordlund seem to have forgotten this) and it is fundamentally the heating from below that drives convection, supplying a luminosity L that convection must ultimately carry. But the structure of the (non-conductive) thermal boundary layers at top and bottom are unique: the bottom one (heating from below) is very thick and the top one (cooling from above) is extremely thin.**
2. For stars with an outer convection zone (in the Sun at least; maybe this is also true generally?), F\_rad gradually tapers off, being significant in the lower ~1/3 of the CZ, so that F only becomes appreciable in the upper ~2/3. This is like a very thick thermal boundary layer below, most well represented in simulations by an internal heating function Q(r). Simulations can include realistic Q(r) easily.
3. In the Sun (maybe all stars?), cooling happens in a corrugated surface that is essentially infinitely thin (between optical depth tau = 1 and 100; but tau ~ T^10 owing to the dependence of the H^- opacity on temperature, so the volume over which cooling happens, even though it’s variable tau, is ~1/10 the local temperature scale height, say 30 km for the Sun). Because of the granulation pattern and the huge temperature difference across granules, the “cooling surface” is not a perfect sphere, but “corrugated” to follow the instantaneous distribution of granules, as shown nicely in Stein + Nordlund 1998, Fig. 24.
4. This cooling layer has extremely complicated physics, owing to 3D “tau-near-1” (neither thick nor thin) radiative transfer and a nasty equation of state (if there even IS an equation of state; there is not LTE): line absorption/emission, partial ionization zones, molecular dissociation/recombination, sunspots/faculae, blah blah blah.
5. If we largely ignore these physical details, the overall result of the photosphere is a corrugated, effectively infinitely thin cooling layer. Let’s ignore the corrugation too (until sims can easily include free surfaces) and approximate the photosphere by a cooling function C(r), which carries out the solar luminosity over a width 1/10 the surface value H\_rho. This thickness of C(r) is waaaay too thin to simulate realistically.
6. What is remarkable (to me) is the following: the solar cooling layer drives convection on the scale of granulation (~1 Mm horizontal scale is observed; also about ~1 Mm vertical scale from Stein + Nordlund simulations).
7. The top density scale height at the photosphere is ~300 km = 0.3 Mm.
8. It thus seems that the top density scale height, *not* the super thin C(r) (and presumably not all the complicated physics) seems to describe granulation pretty well.
9. Mixing-length arguments also work well for granulation: i.e., we can use l ~ H\_rho ~ 1 Mm, take rho\_0 ~ rho\_0 at 1 Mm below the surface (3 x 10^-6 g/cm^3), and find U ~ (F/rho\_0)^(1/3) = 2.6 km/s which is pretty damn close to the actual granular speed. Note that 1 Mm spans 3 density scale heights at the top; there are 14 density scale heights across the convection zone. The appearance of the granular scale suggests that the top three scale heights are irrelevant (there is some discrepancy: I’m not sure it’s obvious the granules should be ~1 Mm and not ~300 km).

**For the Sun, the ultimate picture we have is thus the following, in terms of the basic length-scales:**

**The CZ (200 Mm thick) is extremely stratified, with H\_rho = 100 Mm at the bottom and 0.3 Mm at the top. Convection is driven simultaneously by a very thick (~70 Mm) bottom heating layer Q(r) and a very thin (0.03 Mm) top cooling layer C(r). The scale we see at the surface (1 Mm) is commensurate with the top density scale height and does not seem affected by the thinness of C(r).**

1. How convection is driven in stratified simulations (at least the ones I am familiar with).
2. Some local codes (MuRAM, BiFROST, Stein/Nordlund), etc. drive the convection fairly realistically at the top, solving radiative transport (binning the frequency) and using a tabulated EOS (like OPAL, Mihalas-Dappen, etc.). They reproduce spectral line profiles and observed intensity patterns very similar to solar observations of granulation. However, they are ill-suited to assessing the MLT assumptions above because they do not include enough of a spatial scale range (they typically only extend 10-20, maybe 50 Mm deep) so they get granulation right but have a box mode at 20-50 Mm. They also employ some sketchy things like hyper-diffusion and very strange lower boundary conditions. They thus get the heating from below very, very wrong. Their approach is to claim that the heating from below is irrelevant and that downflows never come back up! Additionally, not many systematic comparisons between MLT and the local sims have been done to begin with (there are some, of course, which I should read more closely).
3. Most global codes get the bottom boundary layer right (using an internal heating function or setting realistic kappa\_rad) but they employ a *conductive* thermal boundary at the top, thus getting the top boundary very, very wrong. It’s still a cooling layer, but there is no systematic way to set its width. Some simulations are thus stellar-like (in the sense of having a thermal boundary layer width substantially less than the top scale height), but others are not (boundary layer width is commensurate or greater than the top scale height). It’s currently untested weather this effect matters, but after obsessing, I’d argue it probably does.
4. Many of us thermally diffuse on the entropy, we thus set an upper value of dS/dr, then the cooling layer width determines the entropy deficit imparted to cooling fluid parcels at the surface. Contrast this to the observation of granulation at l~1 Mm scale, which shows that the entropy deficit of granules is determined by the requirement that g S’/c\_P accelerates granules to speed U ~ (F/rho\_0)^(1/3) in time l/U. The top scale height thus seems to determine the entropy deficit for granular downflows as well!
   1. *By using a top conductive boundary layer, we are thus driving the system with entropy perturbations that have nothing to do with what we would expect for the top fluid parcels at the driving scale.*
5. Hotta (RSST, now R2D2) does employ a cooling layer at the top but incorrectly assumes that its width is commensurate with H\_rho, not much thinner. Further, Hotta does no systematic studies of any kind ☺
6. FH2016 nicely show that to scale into a diffusion-free limit (wherein the total volume-integrated KE is independent of the Rayleigh number), one needs to increase Ra\_F, which simultaneously decreases the cooling layer width. It is interesting that for N\_rho = 2, 3, 4, H\_rho at the top = 50,20,10 Mm. At Ra ~ 10^5, where the cases enter the asymptotic regime, the conductive boundary layer width is ~20, 12, 8 Mm. Thus, the asymptotic regime seems defined by the place the conductive boundary layer thickness is less than the top-most density scale height.
7. It’s speculation at this point, but I wonder if FH2016 if the value of the KE that FH2016’s simulations asymptote to is not determined by the “potential energy of the thermal boundary layer” (their argument), but simply by the top value of H\_rho---and also I wonder if U ~ (F/rho\_0)^(1/3) in their sims.
8. What to Do.
9. **The relevant parameter space the simulations should be exploring are strongly stratified spherical shells with controlled cooling layers much thinner that the outermost density scale height.**
10. We clearly can’t do the solar case but we can do:
    1. Experiment 1:
    2. Take a 5-scale height polytropic model with aspect ratio 0.72 (like bottom 5 scale heights of model S).
    3. Do non-rotating and non-magnetic.
    4. Don’t do this in a box because I don’t know how to use Dedalus, and because we’re investigating partly global-scale motions, which should be sensitive to the box size. Do a spherical shell in Rayleigh.
    5. Put an internal heating proportional to rho\*T as we’ve done.
    6. Use stress-free, impenetrable, zero flux (ds/dr = 0) boundaries at both ends; all flux driven through will be accomplished by heating/cooling
    7. Put a thin cooling layer at the top say of width 30% the top density scale height.
    8. Pick Pr = 0.1.
    9. Define the flux Rayleigh number as we have been, but integrate Q(r) – C(r) now.
    10. Crank up the Rayleigh number, investigate scales of motion, KE\_tot, turbulent free fall, entropy drop, mixing length assumptions.
11. The results of Stein/Nordlund + solar granulation suggest that the actual width of the cooling layer doesn’t matter, since it’s way thinner 1 Mm. But it *might* matter that it’s at least less than the granular scale (the top scale height).
    1. Take the highest-Ra case from before.
    2. Change the cooling layer depth: both much << H\_rho,top (if the sim doesn’t crash) and very large (say symmetric heating/cooling about the middle of the CZ).
12. This may be prohibitively expensive for a 5-scale height case, but I’m not sure. For the 5 scale height case, I estimate we’d need n\_r = 512, n\_theta = 3,072, to cover 5 grid points radially across the cooling layer and 3 points horizontally. That’s right at the max of what we can run (and its expensive). However, if I’m right and only scales of motion of order H\_rho are ultimately driven, we may not need multiple grid points across the cooling layer, and subsequent sims could be run at lower resolution or with multiple chebyshevs at the top.
13. If we *did* use Dedalus, we probably want a box say 400 Mm x 400 Mm x 190 Mm to accomplish something similar to 21 (we don’t want an artificial box mode at 100 Mm, the bottom scale height). The top scale height is 6.3 Mm and 30% of that is 1.9 Mm. For (5,3) grid points coverage that’s 1,024 x 1,024 x 512. I’m not sure if it is really much less expensive. Unless can Dedalus do more resolution (both horizontally and vertically) it different places? I imagine not because it’s a fully spectral code.
14. If all of the above proves prohibitive, we could go down to 4 scale heights across the CZ instead of 5. That would take the top H\_rho from 6.3 Mm to 10.4 Mm and thus make the 3D sim (10.4/6.3)^3 ~ 5x less expensive.
15. If N\_rho = 4, Hrho\_bot/Hrho\_top = 8.3. For N\_rho = 5, the ratio was 14.8. So we sacrifice some scale separation.