Dear Editor,

Editor letter goes here

Sincerely,
Evan H. Anders & Benjamin P. Brown

Response to report B:

The second submission of this paper, after the first round of review, is SUBSTANTIALLY different from the first time around, and essentially needs refereeing from scratch as a new paper. The paper does still essentially examine the heat transport in compressible convection as as function of superadiabaticity and Rayleigh number (and therefore varying thermal diffusivity) for fixed density stratification and Prandtl number via 2D simulations The new paper adjusted the formulation of the Nusselt number, changing the results substantially, and added some 3D simulations to the previously all-2D simulations. The major result is the scalings of the heat transport (measured by their Nusselt number) with the Rayleigh number bears a remarkable similarity to standard Boussinesq Rayleigh-Benard convection, at all the wide range of superadiabaticities simulated. This in itself is an interesting result, as it somewhat surprising considering that their fixed background stratification has quite a strong density contrast (∼20) and some of their departures from adiabaticity are large ($\epsilon \sim 1 \rightarrow$ adiabatic index m=0.5).

I think this paper is getting closer to publishable, although I still find the explanation of the setup of the model in particular somewhat opaque though, and the interesting results not really addressed in any detail, as I'll explain below.

I am glad that the authors took my comments on the setup from before at least partially to heart! I still think things could be clearer. Most specifically again, is the question of what are the salient parameters of compressible convection. There are 4, as mentioned

> *previously, and the authors here have chosen to keep one fixed – a measure of the stratification, for which they use n_ρ. For the others, it is nice to cast things in terms of a Rayleigh number and a Prandtl number since this gels nicely with standard Boussinesq Rayleigh-Benard convection (RBC). However, then the third parameter, the superadiabaticity, which the authors call ϵ, is also part of the definition of the Rayleigh number, and therefore, as it should be, the Rayleigh parameter is really a definition of the thermal diffusivity. To the unaware reader more familiar with RBC, this can be a bit confusing, since there are no independent measures of the driving (superadiabaticity) and the thermal diffusivity there. It would really help the reader here to point out the following in relation to the simulation sets that are performed:*
>
> - *At fixed ϵ, varying Ra means that the thermal diffusivity χ_t scales like $1/\sqrt{Ra}$.*
> - *At fixed Ra, varying ϵ means that the thermal diffusivity χ_t scales like ϵ.*
> - *Since Pr=fixed, viscous diffusivity scales like thermal diffusivity.*
> - *(and of course, all vary with depth individually)*

I need to add this to the experiment section.

> *Beyond that, I think everything is right. The expression for the polytopes in terms of the number of density scale heights makes the notation a little over-complex. There are some disconnects in where the non-dimensionalisation is performed (they say that "take R=1" at one point and then non-dimensionalise R out again later, for example). The description of which of the diffusivities or the conductivity/dynamic viscosity are constant could certainly be tightened up.*

I need to tighten up the description of polytropes, and only talk about non-dimensionalizing R once.

> *With regard to the results:*

In light of what I said above, the results in Fig 2. are not surprising. Higher Ra here at fixed epsilon means lower thermal diffusivity, and therefore eddies might be expected to retain their identity longer. The above explanation would help.

Agreed, we hope that the added explanation makes this clearer to the reader.

There is quite a bit of mention of windy states without any real technical description of what they are. Please either give more information (at least something visual to distinguish from non-windy states) or remove the distraction.

I think it might be a good idea to remove them in order to keep it letter-length, but I'll see if I can keep it short & explain relevant pieces.

Nusselt number: Usually this is a ratio of heat transport in the turbulent state to that in the conduction state. So therefore, arent the two F_As different on the top and the bottom? The top has a modified kappa but the bottom has the original kappa profile?

Nope! That's our new change. Maybe make this clearer.

I think the paragraphs on the Nu vs Ra and the Re/Pe vs Ra are the meat of the paper! It would really be nice here to know what causes the difference between 2D and 3D in the Nu plots. The 2/7 law is often associated with more windy states. That seems LESS likely in 3D, so what is going on? Note also that the sensitive dependence on the exact roll or other structure seems to imply that the simulation box is too small. This dependence should not be the case.

Yeah, AR=4, 8, and 16 all return the windy states, so there's some strange physics here. I'll try to explain the discrepancy between 2D and 3D, though.

The Re vs Ra are a bit mysterious at low Ra. I would expect the $Re \sim Ra^{1/2}$. Can you explain these alternative scalings or at least why the expected scaling emerges at large Ra? Are the 2D ones just over-constrained?

Yes, can explain: we're combining the 1/2 scaling law that we expect from the Ra changing diffusivity with the 1/4 scaling law of rising Ma in 2D, which produces the 3/4 law.

> *Regarding Fig 4 – are the authors calculating the integrated evolved density profile to get this number? If the effect is only in the boundary layer as they mention, I am surprised that the deviation is so large. This figure needs much more explanation!*

We're calculating the horizontally averaged, time-integrated, evolved density profile and then taking two measures of it. One is max/min, one is top of domain / bottom of domain. We should probably explain this more and mention that it persists even at high ϵ in 3D.
This concludes our response to report B. We thank referee B for this second report.

Response to report C:

> *This Letter reports results from a numerical study of thermal convection in a compressible fluid at unity Prandtl number and a range of initial stratification. The main result is the scaling of the Nusselt number with the Rayleigh number, supplemented by some observations such as characteristic changes in the transition between subsonic and supersonic regimes.*
>
> *This problem is highly challenging. Ideally, a numerical exploration should be guided by some theoretical insights or practical observations. The present paper appears to be "purely" numerical without such guidance, albeit some comparison with the well-known problem of incompressible Rayleigh–Bernard convection. Nevertheless, the results are significant and should be published. While I do not wholeheartedly recommend this paper to PRL, I would not object to its acceptance.*

Is there any way we can bring in some more "we're guided by X and thus we expected to get our results"?

> *Below is a minor comment that the authors may find useful.*
>
> *Most parts of the paper is relatively well written, except in the introduction. Here, there is room for improvement. The introductory paragraph is virtually empty. I would cite a couple of papers for "These prior studies" and be specific about both "important insight" and known "fundamental properties". The final paragraph of the introduction has failed to fulfill its mission. I would explicitly state (at least) the most significant result of the paper in this paragraph.*

We need to beef up the intro, as specified here.

This concludes our response to report C. We thank referee C for this report.