Dear Editor,

We thank the two referees for their detailed and constructive critique. We have completed further 3D runs which make the scaling of parameters at 3D more clear to the reader. We have modified the text of the paper to address the critiques of both reports, and have adjusted the styles of select figures in order to more efficiently call out interesting results.

We include the revised version of our paper. Below, we include a detailed response to the reports; we include the text of the reports in their entirety along with our responses inline.

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 ($\sim$20) and some of their departures from adiabaticity are large ($\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)*

We have reworked the discussion of defining Ra and Pr, and have added a new Eqn (3) and related discussion which demonstrates explicitly how our timescales vary with input parameters. We have also explicitly stated the two types of experiments we perform (varying ϵ while holding Ra constant, and vice versa), and what these experiments do to the timescales of our problem.

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.*

We agree. We have moved the discussion of non-dimensionalization after the definition of the polytrope and before the discussion of diffusivities.

> *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.*

We agree and Eqn. 3 now shows this clearly. We have revised our discussion of Fig. 2c such that it explicitly states that long-lasting eddies are what we expect based on input parameters.

> *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.*

We have removed the three scattered discussions of "windy" states, and consolidated them to a short discussion in the paragraph describing Fig. 1. Figs. 1, 2, and 3 have been updated to make it clear which runs exhibited "windy" states, and we think including them in the discussion at this level may be useful to the field when comparing to the work of Goluskin or others.

> *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?*

The evolution of κ with ρ (see discussion between Eqns 2 & 3) means that the evolved atmosphere has a different conductive state than the initial atmosphere. ρ evolves the most at large ϵ (Fig. 4). Thus, both the numerator and denominator of Eqn. (9) use the same evolved state and value of F_A. In the first version of the letter, F_A was instead defined by the initial atmosphere, and the F_{cond} used in the denominator of Eqn. (9) was based on the initial atmosphere as well, as traditionally in Rayleigh-Benard. This led to

the large deviation in Nu scaling at high and low ϵ, as a result of the large shifts in the density profile. However, as Fig. 4 shows, this shift was merely a result of the density profile changing rather than a difference in the amount of flux being carried.

> *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.*

We also are unsure of what the 2/7 scaling relationship means for these fully compressible states. We are interested in exploring this and potential boundary layer theories in future work, but a full examination of this is beyond the scope of this letter. We have conducted a very small number of high aspect ratio solutions which do not appear to converge to vastly different flow morphology. We mention this in the discussion of Fig. 3a.

> *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?*

The increased scaling of Re with Ra is now explained in the text at the end of the discussion of Fig 3b. 2D solutions build over-constrained spinners like flywheeling convection in Rayleigh-Benard, which don't seem to appear in our 3D solutions at these parameters.

> *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 have expanded our discussion of these two measurements when we discuss Fig. 4 in the text. If density were monotonic, these measurements would be equivalent, but density inversions form within the boundary layers and the

max/min are found away from the boundaries. We have observed inverted boundary layers at the top of the domain which span more than one density scale height at $\epsilon = 1$. These are interesting and it's surprising that they persist 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.

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 have clarified the structure of the introduction paragraph. We included citations to prior work at high Ma and set the context for the present study of fundamental heat transport at both low and high Ma. The most important result of the paper is called out in the last paragraph of the introduction.

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