Dear Editor,

We have returned the revised version of our paper, "Convective heat transport in stratified atmospheres at low and high Mach number," in which we have addressed the concerns of the referees. We hope this version of the document is acceptable for publication. The text has been adjusted to address the criticism of the referee reports, with a detailed description of our changes below.

We are grateful for the careful reading and responses of the referees. We thank referee A, whose comments pushed us to undertake our first 3D runs in which we found the transport results were nearly identical to our 2D results. Upon reflecting on the comments of referee B, we realized that the old definition of the Nusselt number (Nu) that we were using was flawed, and have since defined a slightly modified, more appropriate Nu. This new definition better unifies the results at low and high Mach number (Ma) and makes it easier to understand the changes between the low and high Ma regimes. Furthermore, the comments of referee B drove us to better analyze all properties of our evolved atmospheres, and we have come to better understand how the Reynolds number scales with the Rayleigh number, and also how the structure of the evolved atmospheric density compares to the initial atmosphere.

We apologize for the three month delay between receiving the referee reports and the return of the revised draft. In these months we have recomputed all solutions, designed and tested our 3D implementation, and thought carefully about the proper definition of Nu. Through this process we have gained a greater understanding of the fundamental fluid properties in our simulations and how they change coherently across parameter space. We have removed the previous figure 3 from our draft, as we feel it did not share any important information (the sum of the fluxes adds up to a flat line in an equilibrated solution, and this is well known). The previous figure 4 has now become figure 3, and in addition to showing the scaling of Nu with Ra, we now also show the scaling of the Reynolds number, Re, with Ra. We have added a new figure 4, in which we find that the density contrast across the domain at high ϵ appreciably shrinks in the evolved state compared to the initial state. Furthermore, we find that density inversions form near the boundary layers of these atmospheres once the mean atmospheric Mach number approaches one, and show that this density inversion becomes significant at high Ra and high ϵ.

Detailed response to referee report A:

The refereed paper is a reasonable step forward in an extremely difficult and very important for understanding of fundamental physics in planetary and star atmospheres problem of heat transfer in convection of compressible, stratified media at moderately high Mach numbers.

Authors expended previous studies to wider region of parameters, which control an efficiency of the convection, and reported observation of three regimes of convection with different scaling of the convection efficiency (Nusselt number) vs. temperature gradient (Rayleigh number) with a complicated time-space behavior of velocity, density and temperature fluctuations.

Authors attacked this problem using high-end computer facilities of NASA and efficient well justified numerical schemes. Nevertheless, to be able to perform the direct numerical simulations of fully compressible Navier-Stokes equations coupled with an equation for the temperature transport, they restricted themselves by considering two-

> *dimensional version of the problem. Unfortunately, they did not clarified, in which respect the two-dimensional simulations reflect the real features of the three-dimensional physics. There is a well known example (incompressible hydrodynamic turbulence, governed by the Euler equation), in which an additional integral of motion in two-dimensional case completely changes the basic physics of the problem, including the direction of the energy flux over scales.*

We have run and analyzed select 3D cases at both low and high ϵ at select values of the Rayleigh number. These simulations have shown us that the density profile, Mach number, and Reynolds number of the evolved 3D state are nearly identical to the 2D simulations. The Nusselt number appears to change slightly between the 2D and 3D cases at low ϵ, but the high ϵ results agree almost perfectly. These results are shown in Figs. 1, 3, and 4, and the agreement suggests that 2D cases represent the bulk properties of the 3D dynamics well.

> *The Letter is clearly written with a well balanced general introduction to the field, a formulation of particular simplifications of basic equations of motion and a presentation of the results of numerical simulations. According to my understanding, the paper should be interesting for non-experts in the field. I tend to recommend this manuscript for publication in PRL, after detailed clarification of the relations between two- and three-dimensional description on the basics of Rayleigh-Bernard convection and on their generalization to the compressible case.*

Boussinesq theory and DNS results have shown that the scaling of Nu with Ra is nearly the same in 2D and 3D for fluids with $\text{Pr} \geq 1$ [1]. We have shown that this is true in our results, and have mentioned this connection to Boussinesq convection in the first paragraph of our discussion section.

Detailed response to referee report A:

> *This paper essentially examines the heat transport in compressible convection as as function of Rayleigh number and Mach number for fixed density stratification and Prandtl number via 2D simulations The major results cited are the scalings of the heat transport (measured by a Nusselt number) with the Rayleigh number.*
>
> *This paper has one substantial result and hints at another with limited investigation. The major interesting result is that as flows become supersonic, then the heat transport characteristics change dramatically due to shock heating transported into the downflows. The other aspect that is hinted at but not really elucidated, is that compressible convection at high and low superadiabaticity is substantially different.*
>
> *The result regarding the heat transport characteristics and scaling are sufficiently difficult to obtain and sufficiently interesting to warrant publication. However, this paper is presented in a very mysterious way that significantly confuses these interesting results. The description of the modelling used is perplexing and misleading, and the major results are buried. I will try to explain my issues below. Overall, my feeling is that this paper needs substantial re-writing to be publishable.*

We thank referee B for their honesty regarding the confusing structure of the paper. We have tried to restructure the text in a more sequential and thoughtful manner.

> *My first and major issue is that this paper presents the model as though the Mach number is a parameter. The Mach number is a diagnostic of compressible convection. Compressible convection is governed by 4 parameters: a measure of the density stratification, a measure of the superadiabaticity, and two measures of the diffusivities (viscous and thermal). These can all be considered as timescales. Since there is a further timescale (the sound crossing time) available, these 4 parameters can be non-dimensionalised. Some of these non-dimensionalisations can be cast in terms of a Rayleigh number and a Prandtl number for convenience of comparison with Boussinesq models, for example. The Mach number, on the other hand, is a derived, diagnostic quantity that depends on the choice of these 4 parameters. This is easily seen from the results in the paper, e.g. Figure 1. Here the Mach number is a measured quantity plotted as a function of (a) the Rayleigh number, and then (b) the superadiabaticity. The paper here is therefore confusing because it casts the results in terms of 'low or high Mach number', as if this were a parameter. There are many examples of this throughout the paper, but see the abstract, and, for instance, in the introduction ' the two control parameters of RB convection are joined by the degree of stratification, n_ρ, across the domain and the characteristic Ma of the convective flows'. What the authors generally mean in their writeup is that they are either choosing a small or large superadiabaticity (epsilon) at fixed Ra, or small or large Ra at fixed epsilon. The former is used for most of the results section. ...*

It is true that ϵ is the primary control parameter we are referring to when we mentioned "high" or "low" Mach number. This is because Ma is a very strong function of ϵ, and a weaker function of Ra, as shown in Fig. 1. At the ranges of Ra which we are reasonable to examine via DNS (up to nearly $10^7\mathrm{Ra}_{\mathrm{crit}}$), the Ma increases by less than a factor of 100 from simulations at onset to simulations near the state-of-the-art. Thus, the choice of very low ϵ (10^{-4}, 10^{-7}) forces all simulations at that parameter into the Ma $\ll 1$ regime, regardless of Ra. Whereas the choice of $\epsilon \approx 1$ forces all simulations into the Ma ≈ 1 regime.

This being said, it is understandable that the manner in which we presented Ma and ϵ was somewhat confusing. We have changed the text to try to more explicitly state that ϵ is the control parameter, and that setting ϵ to be very small allows us to study low Ma convection, whereas setting ϵ near 1 allows us to study high Ma convection. We have chosen to retain the title of the letter while adding clarification to the text.

> *...However, it is true that the interesting change in transport results appear when the flow becomes supersonic, but this could mean high superadiabaticity (epsilon) *or* high Ra (for fixed Pr). The results should all really be cast as 'at high enough Ra or high enough superadiabaticity, a high Ma flow results and this changes the transport characteristics'. Ultimately, the scaling results exhibited are in terms of Ra, and this makes perfect sense, although these results are only confined to a small paragraph at the end of the results section without much elaboration or explanation. I personally would like a much more causal relationship explained between the shock heating and 'spinners' and the heat transport results.*

It appears that the "spinners" are not the primary cause of the change of the Nu scaling at about Ma $= 1$. We see a similar change in the scaling of the Reynolds number, and this suggests that a large fraction of the increase in energy transport with Ra is caused by increasing velocities. Once the mean flow speed is at Ma $= 1$, the convective transport can no longer benefit from increases in velocities, and only changes in the size of thermodynamic variables or changes in the strength of the diffusivity are left to increase the Nusselt Number. This is mentioned in the second paragraph of the Discussion section.

> *Beyond this main issue, there are a lot of small things that I don't understand, which I will try and list here, in chronological order.*
>
> *Introduction*
>
> *'Numerical constraints ... to moderately high Ma': What is the numerical constraint of low Ma flow?*

When the convection is low Ma, the convective dynamics are much slower than the linear acoustic waves. By stepping this linear component implicitly, we bypass the high Ma component of these waves and timestep on the order of the convective dynamics. This is noted in the second to last paragraph of the Experiment section.

> *'RB' By 'Rayleigh-Benard problem' here I assume that the authors are referring to Boussinesq dynamics? It might be a good idea to make this clear.*

We have reworded this sentence at the beginning of the second paragraph of the Introduction to clarify that we are referring to Boussinesq convection.

> *'the Ma is controlled by the superadiabatic excess': It is clear from Fig 1 that this is not a complete statement.*

The third paragraph of the introduction has been restructured. We note that ϵ is a control parameter whose primary job is to modify the Ma of the resulting solution.

> *Experiment*
>
> *Eqn (1): Better define co-ordinates, especially z and z_0*

$z_0 = L_z + 1$ was confusing notation and has been removed. We define z shortly below Eqn (1).

> *The non-dimensionalization is very confusingly written. Can you write out the non-dimensionalised polytope? This section also says that the 'timescale is the isothermal sound crossing time of the layer' and then two sentences later says that 'we use buoyancy time units', so which is it? The definition of the latter does look like the sqrt(epsilon) so maybe these are the same?*

Ben, what is he asking us to write out? In our numerical simulations, one time unit is the isothermal sound crossing time at the top of the layer. However, convective dynamics have a typical overturn time which can be a few simulation time units (at high ϵ) or it can have an overturn time of many simulation time units (at low ϵ). The buoyancy time allows us to estimate this.

> *'The scaling of the entropy gradient with epsilon ... evolved values ': I really have no idea what these two sentences mean, sorry!*

The initial entropy gradient of the polytrope is $\nabla S \propto -\epsilon$, and as a result evolved variables are $O(\epsilon)$. We have removed this sentence, as it is confusing, and mentioned the scaling of thermodynamic variables with ϵ in the second paragraph of the Results section, where we now describe Fig. 1.

Eqns: I don't understand why these equations are formulated with gradients of nu when mu is constant. The stress tensor only depends on mu and so this can be pulled out of all derivatives. Or am I missing something? The case of constant nu and therefore variable mu is much harder. Similarly for the formulation in terms of chi and not the thermal conductivity; variable thermal conductivity is hard but variable chi is easy. Why not write in terms of mu and k not nu and chi?

μ is initially constant, which means that $\mu = \nu\rho_0$ must be constant. Since we are in a stratified atmosphere, this means that $\nu \propto 1/\rho_0$. A similar relation between κ and χ exists. Thus, while ν and χ are constant in time, they are *not* constant with height, and their gradients must be included in the equation set. μ and κ are allowed to evolve with the density profile, and we have discussed this more clearly in the second paragraph of the Experiment section.

'IMEX': I am assuming this acronym stands for implicit-explicit? Maybe write out?

We have spelled this out in the second to last paragraph of the Experiment section.

'extended to include variable nu and chi' : see above. This seems very unnecessary!
Results
$d_z T_{ad} = 0$*: This is only true for liquids not gases. See Speigel and Veronis 1960.*

This is a good point that we had not appreciated. Most of the studies of Boussinesq convection with which we are familiar make the assumption that the fluid is incompressible. It is true that in an ideal gas which is compressible but barely stratified, the adiabatic temperature gradient is what we found and what is reported by Spiegel and Veronis, $\partial_z T_{ad} = -g/c_P$, [2]. We have updated the text following Eqn 8 to reflect this.

'While it has been suggested that pressure forces...': I do not understand the discussion here. Do the authors regard the breakup of the down flows as an extreme version of asymmetry?

In our low Ma simulations, pressure fluctuations are small and tend to not be positive in the upper atmosphere and negative in the lower atmosphere regardless of whether we are observing an upflow or a downflow. The classical argument [3] of "buoyancy breaking accelerates downflows and deccelerates upflows" appears to break down at low Ma.

'exhibit states in which the flux entering...': Surely in a stationary state the flux in has to equal the flux out. Are the authors saying that this is not a stationary state?

We are saying this is a non-stationary state. The combination of fixed-temperature boundary conditions and no-slip boundary conditions allows the system to fluctuate between roll states of convection and shear states of convection. These states persist over long time scales (hundreds of buoyancy times) and the atmosphere is in flux disequilibrium in both states. We have updated the text in the third paragraph of the Results section to make this clearer.

Discussion
Can the authors demonstrate the basic principles of whatever balance is described by the Grossman-Lohse theory?

We believe that the Grossman-Lohse theory, which models the energy dissipation rate of both kinetic and potential energies, could be extended to stratified, compresible flows which also have potential energy. At low ϵ, where the density profile is simple and very similar to the initial polytrope in the evolved state, it seems that this extension would be easier to develop. However, we are inexperienced in applying boundary layer theory and will need to educate ourselves further on the development of the Grossman-Lohse theory before we can say with certainty that an expansion of this theory to stratified convection is tractable. We have removed this suggestion from the Discussion section and made suggestions towards further experiments which we are certain our work can inform.

> *'under solar conditions ... we expect that epsilon* $\approx 10^{-20}$ *...': Is this small epsilon a reflection of efficient mixing of convection in the ultimate nonlinear state or of the initial linear convective driving?*

These approximations in the initial letter were a representation of the choices of parameters necessary to achieve the desired Mach number and Rayleigh number of convective motions in the deep solar interior. There, Ma $\approx 10^{-4}$ and Ra $\approx 10^{20}$. We made an estimate of what ϵ would be necessary to achieve low Ma at such large values of Ra based on the observed scaling of Mach number with ϵ and Ra in Fig. 1.

Closing

We hope that these modifications to the text have sufficiently clarified our work for experts in the field while also keeping it broad enough to remain interesting for non-experts. Thank you for considering our newly revised manuscript for publication.

Sincerely,

Evan H. Anders & Benjamin P. Brown

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[1] G. Ahlers, S. Grossmann, and D. Lohse, Rev. Mod. Phys. **81**, 503 (2009).

[2] E. A. Spiegel and G. Veronis, Astrophys. J. **131**, 442 (1960).

[3] N. E. Hurlburt, J. Toomre, and J. M. Massaguer, Astrophys. J. **282**, 557 (1984).