Dear Editor,

We thank the two referees for their careful read and detailed and constructive critique of our letter. In responding to these two reports, the science within our paper has been significantly strengthened. In addressing the questions raised by report A, we have undertaken our first 3D runs and we report on the results of these in the revised draft. In responding to report B, we realized that our previous definition of the Nusselt number (Nu) 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. We have streamlined our discussion of the control parameters and have shown how evolved properties (e.g, Ma) depend on the control parameters of our experiment. Our previous presentation of the experiment was confusing, and following the report's comments we feel that we have improved the presentation.

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.

We include the revised version of our paper, "Convective heat transport in stratified atmospheress at low and high Mach number," in which we have addressed the critique contained in both reports. We think the science has been substatially improved by the critque, as has the presentation of the experiment and the results. The text and figures have been substantially revised in addressing the critique. Below we include a detailed response to the reports; we include the text of the reports inline in their entirety along with our responses. We address major points of the critiques first and then address minor points.

Sincerely,
Evan H. Anders & Benjamin P. Brown

I.   RESPONSE TO REPORT A:

A.   Major Concerns

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 now 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 many fluid properties (e.g., the density profile, Mach number, and Reynolds number) of the evolved 3D state are similar to the 2D simulations. We now report on these limited 3D results as well as our more extensive 2D results. These results are shown in Figs. 1, 3, and 4, with large symbols, and are discussed in the text. Broadly, there seems to be some agreement between the 2D and 3D dynamics.

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.

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

II. RESPONSE TO REPORT B:

A. Major Points

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 appreciate the honesty in report B, and we have come to agree that the structure of our previous draft was confusing. We have restructured the text to clarify the presentation of both the experiment and the results. We have highlighted the major results of flow properties and how those scale in low- and high- Mach number convection. As can be seen in the rest of our response and the revised letter, under our new analysis supersonic flows behave substantially similarly to other flows. Surprisingly, compressible convection at high and low superadiabaticity is more similar than different, in contrast to our initial analysis.

> *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. ...*

We agree with the report. We now present the experiment with ϵ as the control parameter, and we show that Ma is a strong function of ϵ and a weaker function of Ra (Fig. 1). In our studies, we find and show that ϵ and Ra largely determine Ma and Re in the evolved state, respectively (Fig. 1 & 3).

> *...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.*

In revisiting our results, we found that the substantial differences between low and high ϵ in heat transport arose from a poor definition of Nu and from changes to the overall stratification. We have determined a more consistent version of Nu (based on current properties rather than initial properties). Now, high and low ϵ transport is substantially similar. We do see a small change in transport when 2D simulations achieve a mean Ma of $O(1)$, and to test this we have conducted an

aditional suite of experiments at higher $\epsilon = 1$. In these solutions, all of the flow properties show a change of behavior at the point at which the flows enter the sonic regime. This is shown in Figs. 1, 3, and 4 and discussed in the text.

We have addressed the major points in the critique; we now continue with the minor points and clarifications.

B.   Minor Points

> 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.
> '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. In explicit timestepping schemes, these fast acoustic waves set the Courant Friedrich Lewy (CFL) limit. By employing modern IMEX schemes, we are able to implicitly involve these fast linear waves and timestep on the timescale of the slow convective dynamics. This is detailed 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 clarified that we are referring to incompressible Boussinesq convection in the Introduction.

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

We have clarified the discussion of how Ma scales with ϵ and Ra throughout the text.

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

We agree that z_0 was confusing notation and this 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?

We have clarified that the equations are non-dimensionalized on the isothermal sound crossing timescale. We have also clarified how the evolution time of the flow is linked to the buoyancy time. This appears in the final paragraph of the experiment section.

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

We agree that this sentence was confusing and have moved the discussion of the magnitude of evolved fluctuations to the discussion of 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?*

Our equations are formulated as the evolution of velocities rather than momentum, and in this formulation ν and χ are the natural variables. If ν and χ are constant in space, the problem becomes a constant coefficient problem and is numerically simple. However, a spacially constant ν and χ does not satisfy thermal equilibrium within the initial polytrope, which is only satisfied when $\kappa = \rho\chi$ is constant. To satisfy thermal equilibrium required a gradient in χ, and to satisfy a constant Pr through the domain we must also have a gradient in ν. ν and χ are now nonconstant coefficients, and this approach, as we take here, is much more challenging numerically. We have clarified and detailed the profiles of $\nu, \chi, \mu,$ and κ 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?*

Being unable to determine how ϵ affects symmetries in the flow, we have removed this discussion.

> *'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?*

As detailed in the revised text, the combination of fixed-temperature boundary conditions and no-slip boundary conditions allows the system to alternate 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. A sensible equilibrium and average emerges over long times. This discussion appears with the discussion of Fig. 3a.

> *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, it seems the extension would be easier to develop. However, we are inexperienced in boundary layer theory and presently cannot say with certainty that an expansion of this theory to stratified convection is tractable. We have removed this suggestion from letter. We have also combined the Results & Discussion sections.

> *'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?*

This is a good question which we do not know the answer to. As a result, we have removed this large extrapolation of our results from Fig. 1 from the discussion entirely.

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

[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).