Dear Eckart, other PRFluids editors, and anonymous referees,

We return a new draft of our manuscript, "Accelerated evolution in convective simulations," addressing the concerns of the referee reports. We would like to point out a few major changes in this version of the manuscript. A new section, "Computational time-savings of AE" has been added after the results section and before the conclusion section. In this section, we directly compare the computational cost of AE and SE for select simulations and we discuss the computational benefits of AE. Furthermore, the final section of our paper is now titled "Discussion & Conclusions;" we have removed a vast amount of speculation regarding extensions of AE from this section and have added a paragraph containing some caveats regarding challenges that users will face in extending AE to more difficult systems. In addition to these changes in the text, we have added a new Fig. 2 which compares the evolution of Nu and the mean temperature in AE and SE, and we have added some accompanying text for this figure to the results section. Small changes in the text throughout the paper have been made for clarity where these changes were requested by the reports.

We feel that the clarity and scientific content of the paper have been greatly improved by the referee reports, and we thank the referees for their honest critiques and attention to detail. We hope that the changes made have clarified and cleaned up the areas of concern raised in both referee reports. Below we include our responses to the reports. The original text of the reports is included in italics, and our responses follow inline in unitalicized blocks.

Sincerely,

Evan H. Anders, Benjamin P. Brown, & Jeffrey S. Oishi

Response to first referee:

This paper describes a technique for accelerating the convergence to thermal equilibrium in convective DNS simulations where such convergence from an initial conductive state would be long. The technique is essentially to evolve a DNS for for a while, collect the background state non-equilibrium fluxes, and then solve a mean field boundary value problem with scaled version of these fluxes for a new background state in thermal equilibrium, then use this to restart the DNS. These, or very similar techniques, have been used by many in the past, although to my knowledge, as the authors here also claim, they have not been explicitly written down in a paper. The authors have gone to the trouble of doing this, not only providing a fairly detailed prescription of the technique, but also some evidence of its usefulness, albeit in a very simple case where it will definitely work which is perhaps not totally representative. I guess I think this paper is overall worthwhile, since it provides a place for newcomers to the business to find some potentially time-saving help, even though the ideas are not original. I think the most important issue with this paper is that its claims of more universal applicability are somewhat naive. I would suggest they temper these assertions substantially.

Basic questions:

How confident can one be that the BVP puts you closer to a non-linear saturated state than where you were? For example, in highly nonlinear (turbulent) problems, there may be multiple states, maybe even multiple stable states, with different background properties. Is it possible to guarantee that you are not putting the system into an equilibrium but unstable state, from which an equally long evolution to a stable state is required? For the simple problems shown here, this is perhaps not likely, but for very high degrees of turbulence, or situations where unknown instabilities may exist, this process may not be so successful perhaps? This is again a factor in my hesitation about the total applicability of this technique across many problems beyond the one the authors present.

We are confident that the BVP puts us closer to a nonlinear saturated state than we were before. It is true that there may be multiple stable states, and there is no guarantee that the BVP puts the solution into the same stable state as SE. We have mentioned this in our conclusion.

> *Here are some more comments in chronological order, not order of importance.*
>
> *Abstract and Intro:*
>
> *The authors come from an astrophysical convection background where the Peclet number is generally high and where these techniques are useful. They need to be careful to be clear that the convection they are talking about is high Pe, the parameter that describes the separation of overturning and thermal diffusion timescales. These techniques will not be useful (or required) for low Pe number situations.*

We have reworked some text in the second paragraph of the introduction section in order to clarify that we are discussing high Pe convection.

> *Section II.*
>
> *After (4): The expression for ΔT_0 assumes that the background temperature gradient is constant, which has not been introduced yet.*

We have added a sentence before the description of our nondimensionalization stating that the initial temperature profile is linear.

> *The sentence "Our choice of the thermal boundary conditions in Eqn. (9) was motivated by the fact that accelerated evolution is simpler when both the thermal profile and the flux through the domain are fixed at a boundary" is never revisited or explained, unless I missed it?*

We have clarified this in the second to last pargraph of section III (and added a forward reference to this part of the text).

> *Section III.*
>
> *The assumption after equation (10) that the convective region will NOT change substantially is a BIG assumption that allows this technique to work. In a situation where nonlinearities affect*

the extent of the convective region (e.g. penetrative convection), or anywhere else where the active region varies (e.g. an instability), this technique will have problems. More on this later.

Equations (12-13): These are essentially steady state, mean field equations, with approximated fluxes for the later (solved) state. Is the prescription for the approximated fluxes in (11) going to work for all problems? It does so for the case examined, but this case has a simple background temperature gradient. Would this work for more complicated problems, for example, with a non-constant $\partial T_0/\partial z$? What about the cases that are NOT symmetric (i.e. periodic in the horizontal where the mean field equations contain some other terms?

We are fairly confident that this prescription will work for more complex initial temperature profiles (F_B will need to pick up some height dependence in the defintion of χ, for simple internally heated systems, for example). It would be useful to test AE in such a case, but such studies are beyond the scope of this work, and we leave it to future work to determine if this method is valid in such cases.

We are uncertain of how AE will work for non-symmetric, horizontally variant cases. As it fundamentally adjusts the mean profile of the whole domain in a uniform manner, it may not be ideal for such cases.

Refer to the revised final section of the paper for some discussion on these issues.

Last para: the authors say proper profile, but this is an approximate thermodynamic background profile, essentially the first iterate in an iteration scheme. Perhaps proper is not the right word?

We have replaced "proper" with more appropriate descriptors.

IV:
I dont understand "$10^{3+2/3}$"? Why is written like this?

To determine the Ra of our simulations, we take even sized steps in log space (3 per decade). We have changed $3 + 2/3$ to 3.67 for easier reading.

I am not convinced that the whole "2D and 3D scaling laws" section adds anything at all to this paper. It shows that this technique can be used but so does the single example that is interrupted

by this piece. It is somewhat interesting for the scaling laws found themselves, but it sounds like this should be in a different paper because there is not enough explanation to understand things totally here. I feel like the examples before and after this section are sufficient to make the point about the AE scheme.

We feel that it is important to show that AE works across some swath of parameter space, in order to demonstrate its robustness for the problem presented here. We have trimmed the text of the scaling laws section and reduced speculation in order to keep the paper on topic. We have also replaced Re with Pe in this section in order to reinforce the point that we are interested in high Pe convection (since Pr = 1, Re = Pe numerically, so only labels have changed).

When discussing the PDFs, the author refer to the "modes" of the PDF? This sounds a bit odd to me. Do they mean the peaks? The phrase "temperature fluctuations off of the modes" is particularly baffling!

We have replaced "modes" with "maxima" for clarity.

The fact that the mean temperature profile is off by a constant factor almost everywhere is a bit disturbing, even if the factor is small. Does continued iteration of AE not get rid of this? If not, then this is significant, because it would take a thermal relaxation time again to correct this issue.

We have added a paragraph to the "accelerated evolution recipe" appendix. In it, we discuss the fact that AE can be iteratively solved until you converge to a desired fractional percentage, and that we were only aiming for O(1%) in this paper. It is true that it will likely take a thermal relaxation time to evolve from the 1% off state to the true state, but that slow evolution does not appreciably change the dynamics.

I felt like I wanted a section at the end of this section that described how much the AE saved in computational time and wall time over SE. There is a bit of it here, and some of it in the discussion, and some more details in the appendix, which seems a bit scattered. I would be tempted to agglomerate it all here in a separate section.

We have added a new section V., "Computational Time-savings of AE." We have combined the relevant information from the results section and the conclusion section previously here, and we have added some information on a few additional runs in a new table.

V.

This is the section I have most issues with.

The example performed was the most likely to succeed. The 2D cases were essentially rolls. Were the 3D cases still essentially 2D rolls too (since the scalings came out the same)? What about larger aspect ratio and more turbulence cases where the dynamics would be more complex? Keeping things smooth certainly enhances the likelihood of a simple scaling of the nonlinear fluxes working.

I think the projections for more complicated problems mentioned are naive. If there is any nonlinear adjustment to the convective region, e.g. penetrative convection, then the technique is far less likely to be simply successful in the current form. The assumption of fully convective and thermal boundary layers remaining in place is a big one, and this is not really acknowledged. The authors do mention penetrative convection and do indeed say that only a stiff case would work under these premises, but this is a bit hidden, I think, and needs to be emphasized more. For example, the penetrative ice-water problem even in Boussinesq would not work well with this technique I think, since the penetrative region can deepen dramatically, and adjust the radiative flux to match the substantially changed convective flux.

Furthermore, the compressible case is much more danger prone than envisioned here. I suspect that in the iteration to accelerate, any mismatch in the fluctuations and the estimated means in the DNS will lead to major transients in the form of sound waves, which can totally destroy a compressible evolution by reducing the tilmestep substantially, so this technique may have drawbacks if there is not filtering of sound waves somehow. The authors have glossed over these problems in their eagerness to extol the virtues of their scheme.

I think the claims of this method working in many more situations need to be examined more thoroughly before being made so boldly here.

We agree that our intial conclusion section was over-eager. We have greatly reduced the amount of text spent on speculation surrounding extensions of the AE method, and have added a paragraph warning the reader that not all extensions will be as simple (or guaranteed to work) as the case studied here.

Response to second referee:

1. *The authors should provide clarification on what the following terms mean: "thermal equilibration", "thermally relaxed", and "thermal convergence". Also, are these terms related to statistically steady state?*

All three of these phrases are equivalent, and we have replaced all mentions of thermal equilibration, relaxation, and thermal convergence with "thermal equilibration" or "thermally equilibrated" throughout the paper. This term is explained in pargraph 2 of the introduction.

2. *What is the "Kelvin-Helmholtz timescale"?*

The Kelvin-Helmholtz timescale is

$$t_{\rm KH} = \frac{E_g}{L},$$

where E_g is the gravitational energy in a system, and L is the luminosity (energy output / time). Basically it is a timescale that astronomers and astrophysicists use to estimate the timescale of thermal evolution of a system (e.g., a star). Since the point we were trying to make is that "the mean thermodynamic structure of the Sun evolves *really* slowly," we have removed this jargon and said this more clearly in paragraph 2 of the intro.

3. *The authors note that the bootstrapping method is susceptible to hysteresis effects. They should provide examples of where such effects have been observed.*

We misspoke. We have not specifically seen hysteresis effects (nor have we seen this in the literature). We meant to say that in simulations where there are multiple solution branches (e.g., a convective roll and a shearing solution), bootstrapping a solution on one branch to different parameters with the same solution branches will result in a solution on the same solution branch. Particularly, in our own work in stratified atmospheres, atmospheres trend toward shearing states, and then stay stuck in those states as we bootstrap to higher Ra.

We have corrected our error and made our meaning clearer.

4. *It is true that direct numerical simulation of turbulent thermal convection is expensive. However, the highest Ra simulation run by Stevens et al. reaches a stationary state in a few hundreds of free-fall times. Hence, what is the necessity of running simulations for "thousands or millions of free-fall times"?*

We have added a new Fig. 2 to address this question. In general, the convective flows and heat transport (e.g., Nu) reach their final, time-stationary mean value very quickly (see panels (a) and (b) of Fig. 2). However, the thermal structure of the system can still be slowly evolving for long timescales despite the flows being relatively statistically stationary. The purpose of running for many, many free-fall times is to converge the thermal profile of the domain to ensure that the full system is in a steady state.

5. *Presumably, the most important step in the Accelerated convergence method involves decreasing the heat flux through the top boundary so as to match it with the heat flux at the bottom boundary. This is achieved by introducing a function* $\xi(z) \equiv F_B/F_{tot}$. *The following questions arise:*
   (a) *What is the functional form of* $\xi(z)$*? And where does the z-dependence come from?*
   (b) *Here,* F_{tot} *is not really a constant, but depends on time. Hence,* $\xi(z)$ *should also be a function of time.*
   (c) *The "evolved" quantities are obtained by multiplying* $\langle u \times \omega \rangle$ *and* F_E *by* $\xi(z)$. *Can this construction be rigorously justified? I would like the authors to provide more details on how they arrived at this step.*

(a) In the simple case studied in this problem, where F_B is constant in time and space, the functional form of $\xi \propto F_{tot}^{-1}$, and depends only on the fluxes of the convective system. Since F_{tot} can have a large z-dependence (e.g., Fig. 1b), ξ, too, has a z-dependence. ξ is simply a height-dependent scaling that we use to appropriately adjust our velocity, temperature, etc.

(b) We have changed $\xi(z) \rightarrow \xi(z,t)$.

(c) Physically, ξ is a scaling factor that tells us how much larger the flux is in the interior than it should be. We assume that $F_{tot} \sim F_{conv} \sim w(T - \langle T \rangle)$. As such, we assume that $\xi \propto (w^{-1/2}, (T - \langle T \rangle)^{-1/2})$, and that scaling both

the temperature fields and the temperature fluctuations around the mean profile will reduce the convective flux in the proper way. We scale $\langle u \times \omega \rangle$ by ξ, as it is nondimensionally of order u^2/L, and so if the velocities must be multiplied by $\sqrt{\xi}$, this term must be multiplied by ξ.

We cannot rigorously derive ξ from first principles. Its form, and the use of it here, are the result of our experience and familiarity with convection.

6. *The assumption that convection at early times occupies roughly the same volume as convection in the stationary state is acceptable. However, there is also the possibility that instead of decreasing the heat flux at the top one could increase it at the bottom? The boundary condition for T1 at the bottom surface is*

$$\frac{\partial T_1}{\partial z} = 0$$

So, the imposed heat flux at the bottom is 0.

Our boundary condition is that *fluctuations* in the temperature derivative at the bottom of the domain are zero. Which is to say that $\partial T_0/\partial z = -1 =$ constant. We could increase the heat flux at the bottom to match the heat flux at the top, but in that case we would be solving a different system with a different set of boundary conditions than the ones studied here.

7. *Could the authors construct plots of Nu vs. t (like in figure 2) for both AE and SE starting from t = 0? I would like to understand how Nu(t) evolves with time in the AE cases.*

We have created this plot as part of a reponse to point 4. Please refer to the new Fig. 2 in the text.

8. *I diasgree with the authors statement that Previous studies in 2D convection may have avoided these time-varying Nu states by using bootstrapping techniques... If one were to measure Nu(t) in the interior, then the sign of Nu would fluctuate between positive and negative. However, the time average would be always be positive. (This I know from my own work.)*

The purpose of the previous Fig. 2 (now Fig. 3) was merely to show why we have such large error bars on Nu in our Nu v. Ra plot (Fig. 4). Our aim is to show the nature of the flow morphologies at times of high and low heat transport; the instantaneous sign of Nu (positive or negative) is not what we are trying to point out.

We have clarified the discussion around the scaling laws, as requested by the other referee report. We have removed speculative statements which aimed to compare the scaling law of Nu found here with Nu elsewhere in order to keep the focus of our paper on AE rather than Nu scaling for our system setup.

9. *Have the authors studied convection with fixed temperature and no-slip conditions at the top and bottom surfaces? Have they compared the AE and SE results for that case?*

We have not. We've used no-slip at both surfaces, but only mixed thermal boundary conditions. We don't forsee major difficulties in AE being applied to fixed T boundary conditions, but felt that such a study was outside the scope of this work.