Response to the Author

I appreciate the author's efforts in addressing the reviewers' comments. However, after carefully evaluating the responses, I find that several major concerns remain insufficiently resolved.

1. **On the use of gravitational acceleration as an analogy.** Even when restricting the discussion to Newtonian mechanics, gravity does not seem to constitute an appropriate analogy for the Lorenz reference state (LRS). The gravitational acceleration g is related to a scalar potential field Φ through $g = |\nabla\Phi|$, and this potential satisfies

$$\nabla^2\Phi = 4\pi G\rho, \tag{1}$$

   where G is the gravitational constant and $\rho(x, y, z, t)$ is the mass distribution. In this sense, the gravitational field can be described by a Poisson equation, which is formally *local in space*. In the oceanographic context, a possible explicit analogue of Φ would be the pressure p. Although the pressure force in an incompressible flow acts nonlocally, a similar Poisson equation for p can be written to illustrate how hydrodynamic motion at a given point influences adjacent fluid elements. These processes are conceptually straightforward.

   The LRS, by contrast, is a far more intricate construct. Determining the LRS corresponding to a given spatial distribution of S and θ requires a global rearrangement of fluid parcels, which is inherently difficult to express in a local form. This is precisely why the physical interpretation of the LRS has remained a challenging issue.

2. **On the observability of the Lorenz reference state.** The theoretical argument presented through equations (2)–(5) is not convincing. While the mathematical manipulations themselves are correct, the reference profiles $\rho_0(z)$ and $p_0(z)$ introduced here may be chosen arbitrarily; there is no inherent reason to identify them with the LRS.

   Furthermore, the statement that "while $p_0(z)$ enters as a passive reference, its choice determines N_0 and is thus constrained by observations" is not meaningful. The authors may be implicitly assuming that N_0 corresponds to the in-situ Brunt–Väisälä frequency, but this is not the case. The true Brunt–Väisälä frequency is determined by the vertical gradients of salinity and entropy. In the present notation, it is given by

$$N^2 = b_S(S, \theta, z)\, S_z + b_\theta(S, \theta, z)\, \theta_z. \tag{2}$$

   In contrast, the quantity N_0 defined in the author's reply depends solely on the arbitrarily chosen reference state and is therefore not observable.

3. **On the formulation using the static energy function.** As I noted in my previous comments, the decomposition of the static energy function into $\Sigma = \Sigma_{\text{dyn}} + \Sigma_{\text{heat}}$ and the use of the LRS to define the latter is an interesting and potentially useful idea. However, I remain unconvinced by the strengthened arguments presented in the author's response.

   The author now discusses a specific example involving a diabatic process at the ocean surface. In this situation, defining Σ_{heat} based on the LRS makes expression (6) represent the APE production rate. While this observation is valid, it is not unexpected, since APE is fundamentally defined relative to the LRS. This simple example thus reiterates a well-known result: destabilization of the stratification and the onset of convection are linked to the APE budget. It does not, however, substantively support the central claim of the manuscript that "the Lorenz reference state enters the equations in the way an external constraint would."