Comment on "Is the Lorenz reference state global or local and observable?"

The manuscript addresses an important conceptual question in ocean energetics: whether the Lorenz reference state (LRS) should be understood as a global or local quantity, and to what extent it is observable. The LRS, defined as the adiabatic rearrangement of water parcels minimizing potential energy, has become a fundamental construct in diagnosing available potential energy. However, its physical interpretation remains debated, as it depends on the global distribution of entropy and salinity, while the governing equations of motion are local. The author aims to reconcile this apparent inconsistency through analogies and illustrative examples.

Overall, I found the manuscript stimulating and appreciated several original and thought-provoking insights. Nevertheless, I am unable to recommend publication in its current form, for the reasons outlined below.

1. **On the use of gravitational acceleration as an analogy.** The manuscript treats gravitational acceleration as an example of a field with a global origin that is locally observable. However, this analogy is problematic. The present formulation implies that gravitational acceleration can be described as a function of space and time related to a scalar potential, $g = |\nabla\Phi|$, with $\Phi(x, y, z, t)$ determined instantaneously by the mass distribution of the universe via Newton's law. This view contradicts the modern understanding of gravity in general relativity.

   In general relativity, gravitational acceleration is frame-dependent; one can always find a freely falling frame in which gravity vanishes locally. A familiar example is the International Space Station (ISS): although Earth's gravitational attraction at the ISS is nearly the same as at the surface, astronauts experience weightlessness because their orbital motion balances the gravitational pull with centrifugal acceleration. Hence, the concept of a universally measurable g is not meaningful in a local experiment.

   More fundamentally, gravity is a manifestation of spacetime geometry. The gravitational field is expressed through the metric tensor and propagates at the finite speed of light, analogous to waves in an elastic medium. Thus, although gravity originates from the distribution of mass and energy in the universe, it is best regarded as a transient local property rather than a static global one.

   I understand that the author introduces gravity as a pedagogical analogy to frame the subsequent discussion of ocean energetics. Nevertheless, even for illustrative purposes, the description should remain scientifically accurate. While such an analogy might be acceptable in an informal essay, a refereed article requires stricter adherence to established physical theory.

2. **On the observability of the Lorenz reference state.** The manuscript suggests that the LRS can, in principle, be reconstructed from locally measured buoyancy frequency if the system is sufficiently close to rest. However, this assumption of near-equilibrium conditions is rarely met in the real ocean. In particular, the pronounced tilting of isopycnal surfaces in high-latitude regions indicates significant departures from equilibrium. Consequently, the practical and theoretical usefulness of the arguments presented in Section 3 is unclear.

3. **On the formulation using the static energy function.** Section 4 presents a reformulation of the Navier–Stokes equations in terms of a static energy function, following the

author's earlier work (Tailleux and Dubos, 2024). This framework is indeed elegant and potentially valuable for numerical modeling. However, the manuscript's interpretation appears overstated.

As written in equations (5)–(7), the equations of motion are expressed using a static energy function Σ, decomposed into a dynamically relevant component $\Sigma_{\rm dyn}(\eta, S, p, \Phi)$ and a passive component $\Sigma_{\rm heat}(\eta, S)$. The author defines $\Sigma_{\rm heat}$ based on the LRS and argues that the LRS "enters the equations in the way an external constraint would." Yet, this statement is questionable, as the choice of $\Sigma_{\rm heat}$ in equation (8) is not unique. One could equally adopt another reference state $z_r'(\eta, S)$ without altering the subsequent formulation. This arbitrariness weakens the claim that the LRS directly shapes the dynamics through its deviations. Although the LRS remains a useful diagnostic construct, its role as a physically necessary ingredient of the governing equations is not convincingly demonstrated.

In summary, while the manuscript raises important conceptual questions and offers stimulating reflections, the arguments are not fully justified by the proposed analogies and theoretical formulations. Despite these concerns, I found the paper intellectually engaging and potentially valuable as a discussion piece. I would encourage the author to make it publicly available as a non-refereed contribution, as it may inspire future theoretical advances in the energetics of the general ocean circulation.