Referee Report

Non-conformal obstructions to bubble expansion

Recommendation

I recommend **major revision**. The manuscript addresses a timely and important problem: how non-conformal thermodynamics changes the hydrodynamic wave patterns generated by a first-order phase transition. The separation between local wall obstructions and global flow obstructions is useful, the simple exclusion argument for detonations in QCD-like equations of state is illuminating, and the proposed "shocked detonation" patterns could be a substantial addition to the literature. The paper is well motivated and, in scope, is appropriate for JHEP or PRD.

The present version is not yet ready for publication, however. Its most novel claim depends on a shock-selection prescription that is asserted rather than derived, while the completeness of the proposed wave-pattern classification is not established. In addition, the central numerical phase diagrams cannot be reproduced from the information in the paper, and the implementation is not validated quantitatively. These issues affect the physical solution space and the gravitational-wave conclusions, rather than only the presentation.

Synopsis and assessment

The authors study spherically symmetric, self-similar solutions of relativistic ideal hydrodynamics for a two-branch equation of state. Section 2 first contrasts QCD-like thermodynamics with the bag model and then introduces the analytic reduced equation of state in Eq. (6). Its stable low- and high-energy branches terminate at e_*^{L} and e_*^{H}, where the sound speed vanishes. The entropy and temperature are reconstructed from Eq. (11), with one normalization constant per branch. Section 3 reviews the self-similar flow equations (14)–(15), the wall and shock matching conditions (16)–(17), and the transition-strength definition (18)–(20).

Section 4 gives a concise local argument for the absence of detonations when the two stable energy intervals do not overlap. Starting from Eq. (21), the matching conditions imply Eqs. (22)–(25); the ordering in Eq. (26) then rules out a supercooled detonation. The corresponding reversal for a superheated transition leads to the same conclusion. This part of the paper is logically clear. I have also checked that the bag-model necessary bound in Eq. (27) follows from Eq. (25), although the endpoint/equality case should be stated more carefully.

Section 5 identifies wall obstructions, Eqs. (29)–(30), and flow obstructions associated with singular rarefaction profiles. Parameterizing the flow by $\xi(v)$, the degenerate-critical-point condition is given in Eq. (31). The second relation in that equation is consistent with differentiating the sonic condition along the $d = 3$ flow: at $\mu = c_{\mathrm{s}}$, imposing both $d\xi/dv = 0$ and $d^2\xi/dv^2 = 0$ yields $dc_{\mathrm{s}}^2/de = -2(1 - c_{\mathrm{s}}^2)c_{\mathrm{s}}^2/(e + p)$. The manuscript then proposes to bypass the multivalued continuation of a detonation by inserting an internal shock, and selects one member of the resulting family by maximizing entropy production. Sections 6 and 7 use numerical scans to map the solution space and its single-bubble kinetic-energy fraction. Appendix A gives a holographic interpolation, while Appendix B describes the SNOBEX code and the regular flow system (49).

The work appears novel relative to the standard bag-model construction and the constant-sound-speed templates cited in the paper. It also connects naturally to earlier holographic studies and recent determinations of metastable Yang–Mills thermodynamics. The potential significance is therefore high. The main obstacle is that the physical admissibility and uniqueness of the new compound waves have not yet been demonstrated at the level required for the claims made.

Major comments

1. **Admissibility and selection of shocked detonations.**

   The central new construction in Section 5.2 joins two branches of a multivalued rarefaction profile by a discontinuity satisfying the Rankine–Hugoniot conditions. The text states that these discontinuities can connect states that are supersonic on both sides, and that positive entropy production is sufficient for their admissibility. This is precisely the regime in which the characteristic structure must be analyzed. For a non-genuinely-nonlinear system, an entropy inequality alone need not select a unique weak solution; undercompressive or rarefaction shocks can depend on the viscous/capillary regularization. The manuscript should give, for every class of internal shock in Fig. 10, the two characteristic speeds in the shock frame and test an appropriate Lax or Liu evolutionary condition. If the proposed shocks fail the standard condition, the authors must identify a microscopic or dissipative regularization that supports them and explain the associated kinetic relation.

   The further prescription in which the shock position is chosen to maximize entropy production is not a general consequence of the second law. The paper does not define whether the optimized quantity is the local entropy-flux jump, the total entropy production rate including the shock area, or another functional; nor does it derive why its extremum is equivalent to a Jouguet condition. Please provide the explicit entropy-production functional, the derivation of the extremum, and an argument that this criterion represents the dynamically selected and stable solution. Otherwise the manuscript should retain the full one-parameter family, refrain from calling one member "dominant" or physically selected, and propagate that non-uniqueness into Figs. 12–15. This point is essential to the claimed discovery, not a minor technical detail.

2. **Completeness of the wave-pattern classification.**

   The paper says that a flow obstruction of an ordinary detonation can always be repaired by an internal shock, whereas the corresponding obstruction for a hybrid is genuine. No proof is given that the same operation cannot produce a shocked hybrid, or that additional compound waves with more than one internal shock are impossible. The second-kind patterns near $v = 0$ make this question especially important. More generally, the analytic equation of state in Eq. (6) changes the characteristic geometry that underlies the standard deflagration/hybrid/detonation taxonomy.

   The authors should formulate the relevant relativistic Riemann problem and show that all entropy-admissible wave sequences compatible with the two asymptotic states have been enumerated. At minimum, they should explain why shocked hybrids, multiple-shock patterns, and alternative compound rarefaction–shock continuations are excluded. A local phase portrait around both the degenerate point of Eq. (31) and the fixed point $\xi = c_{\mathrm{s}}(e_C), v = 0$ would make the argument much more convincing. The derivation of Eq. (31), including its assumptions ($d = 3$, the sign of μ, and nonzero v), should also be included rather than only quoted. Until completeness is established, the colored domains in Figs. 12–14 should be described as the regions found within the wave ansatz that was searched, not as the full space of hydrodynamically allowed solutions.

3. **The numerical results are not reproducible from the manuscript.**

   Figures 11–15 contain the core numerical evidence, but the exact models used in the six panels are not specified. Please provide a table listing $p_1, n, \delta, e_*^{\mathrm{H}}, e_*^{\mathrm{L}}$, the branch domains, all normalizations, and the entropy parameter x for every panel. For plots with more than one zero-entropy curve, every value of x must be stated. The value described only as "close to unity" in the (ξ_w, α_N) plots must be given numerically. The input configurations and tabulated Yang–Mills equation of state should be archived with a tagged code version or commit so that each published figure can be regenerated exactly.

   Appendix B currently describes software modules, but it does not provide a numerical validation. The revised paper should report the ODE and root-finder tolerances, grid resolution, event/acceptance thresholds near singular points, matching-condition residuals, entropy-flux residuals, and convergence under all of these choices. It should explain how multiple roots of Eq. (17) are found and classified, how spline differentiation affects dc_{s}^2/de, and how integrations are initialized at the degenerate and $v = 0$ points. Useful benchmarks would include analytic bag-model profiles, recovery of the standard Jouguet curves, and a direct numerical check of Eq. (31). The boundaries and gaps in the phase diagrams should carry a numerical uncertainty or a documented resolution study.

   There is also an apparent branch error in the description of `find_shocked_detonation_jouguet`: Appendix B tests $\xi_w \geq c_{\mathrm{s}}^-(e_N)$, although e_N is the high-energy state ahead of the wall. The relevant sound speed appears to be that of the high-energy branch. Please check both the text and implementation and state the plus/minus and pre-/post-shock conventions consistently.

4. **Entropy production and the reconstruction of the full equation of state need an explicit treatment.**

   The physical exclusion of large parts of Figs. 12–14 rests on entropy production, yet the entropy-current jump condition is never written. Please display it with the velocity orientation used in Eqs. (16)–(17), show how it is evaluated at a phase-changing wall and at a same-phase shock, and explain why the sign for a same-phase shock is independent of the integration constants in Eq. (12). The statement that all shocks found are allowed should be supported by quantitative checks, not only asserted in a footnote.

   The map from the two constants in Eq. (12) to the single invariant parameter x in Eq. (32) should also be derived. One common rescaling merely changes the temperature unit, while the ratio fixes the relative temperature assignment of the two branches; spelling this out would clarify why x parameterizes the physically distinct full equations of state. Finally, the claim that the second law is always more restrictive than the wall obstruction for deflagrations and the flow obstruction for hybrids should be demonstrated over the full allowed range of x, or explicitly limited to the sampled models. Because the α_N contours themselves move with the temperature assignment, this is necessary for interpreting the physical gaps.

5. **Kinetic-energy normalization and the gravitational-wave inference.**

   There is an internal mismatch between Eqs. (38) and (39). Equation (38) writes the velocity-dependent term as $\frac{1}{4}w\gamma^2v^2$ and the text calls it the kinetic energy, while Eq. (39) uses $w\gamma^2v^2$ in the numerator. The standard decomposition consistent with Eq. (39) is

   $$T^{tt} = \frac{3}{4}w + w\gamma^2v^2 + \theta.$$

   The authors should correct the decomposition or explain a different convention and ensure that the plotted quantity has the advertised normalization.

The control-volume step in Eqs. (40)–(42) should be derived for a moving outer boundary. In particular, show explicitly how the junction conditions at an outer shock or wall imply that the energy in the disturbed region equals $e_N V_{\text{bubble}}$, and state that wall/surface energy is neglected. It would also be useful to verify numerically that this identity holds for each wave class. Finally, Fig. 15 deliberately retains solutions with negative entropy production. Conclusions about the maximum physically attainable kinetic fraction should therefore be supported by a second version or an envelope after the entropy condition is imposed. Otherwise the advertised suppression of the gravitational-wave source remains qualitative.

6. **Status and uncertainty of the QCD-like benchmark.**

   The pure $SU(3)$ result in Fig. 7 is a prominent concrete claim, but its input near the transition combines published results with private data and a chosen normalization. The relevant tabulated e, p, s, T data, interpolation scheme, metastable endpoints, and normalization procedure should be made available. Since the allowed energy interval lies close to the spinodal endpoints and the paper acknowledges lattice artifacts, the authors should vary the input within its uncertainties and show how stable the upper wall-velocity boundary is. The analytic non-overlap argument for excluding detonations is robust, but the quantitative deflagration region need not be. Claims concerning real QCD, neutron-star matter, and supernovae should remain clearly qualified because the present system has no conserved charge and uses a perfect-fluid, single-bubble description.

Minor comments

1. Equation (27) is presented with a strict inequality. Please discuss the saturated case $2\epsilon = e_*^{\mathrm{L}} - e_*^{\mathrm{H}}$, where the candidate states sit at both branch endpoints and the limiting velocities may be degenerate.

2. The labels in Figs. 11–15 should identify each model consistently, and the captions should state which solid, dashed, and dot-dashed curves correspond to which obstruction. At present, some of this information must be assembled from several paragraphs.

3. The discussion below Fig. 15 considers the equations of state in panels (c) and (d), but refers to the would-be maximum of the green curve in panel (f). This appears to be a panel-reference error.

4. In Section 5.2, define "supersonic on each side" explicitly in the shock rest frame and specify which side is upstream. The same symbols $v_\pm$ are used for a phase boundary and an internal shock, which makes the Jouguet statement ambiguous.

5. Appendix A gives $s = \pi g(r_H)^{3/2}/(r_H^{3/2}\phi_A^3)$. For the displayed metric, the horizon area density scales as $g(r_H)^{3/2}/r_H^3$; the exponent of r_H therefore appears to be a typo. It is hidden numerically by the choice $r_H = 1$, but should be corrected.

6. Appendix A would be more useful if Fig. 16 were supplemented by $c_s^2(e)$ or $p(e)$ for the same values of χ. An entropy-versus- temperature plot alone does not fully exhibit the claimed approach to the reduced bag-model equation of state.

7. Please state whether all reported solutions obey $0 \le c_s^2 \le 1$, $e + p > 0$, and the other local stability/causality conditions throughout every branch segment actually sampled by the flow.

8. Several strong statements use “always” or “all” where the numerical scan or a restricted wave ansatz is the evidence. These should either be supported by the completeness argument requested above or narrowed to the models and configurations explicitly tested.

Conclusion

The manuscript contains an appealing organizing idea and potentially important new hydrodynamic phenomena. The local detonation obstruction is transparent, and the numerical results suggest that non-conformality can qualitatively reshape the conventional solution diagram. A publishable revision should put the shocked-detonation construction on a firm admissibility and selection footing, establish the completeness of the wave patterns, and make the numerical claims fully reproducible and quantitatively validated. If those points are resolved, I would view the paper as a strong candidate for a high-energy theory journal.