Referee Report

Manuscript: "Non-self-dual nontopological soliton in a pure Chern-Simons gauge model"

Summary

The manuscript studies nontopological, rotationally symmetric solitons in an Abelian pure Chern-Simons-Higgs model in 2+1 dimensions. The scalar potential is a sixth-order potential depending on a parameter ε, with the self-dual Chern-Simons-Higgs point recovered at $\varepsilon = 0$ and $\tau = m_A/(2m) = 1$. The paper focuses on the non-self-dual regime, derives the reduced radial equations for the ansatz functions $f(r)$, $\Omega(r)$, and $a(r)$, and discusses the resulting charge, flux, angular momentum, asymptotics, virial identity, and fixed-charge variational relation.

The main physical claims are that non-self-dual nontopological solitons exist only in a bounded region of the (τ, ε) plane, that their charge and energy are bounded when $\varepsilon \neq 0$, and that arbitrarily large charge and energy occur only at the first-order transition point $\varepsilon = 0$, where the potential has two degenerate zero minima. The numerical section then compares the nonzero-ε and zero-ε cases, displaying the behavior of $Q_N(\Omega_\infty)$, E/Q_N, and the ratio to the Bogomol'nyi bound, and the appendix gives a thick-wall expansion near $\Omega_\infty = m$.

The topic is appropriate for a high-energy theory journal. The paper addresses a natural gap between the well-studied self-dual Chern-Simons-Higgs solitons and the more general non-self-dual regime. The analytic relations among charge, flux, angular momentum, the virial identity, and the large- and small-radius asymptotics are useful and mostly convincing. The numerical findings also appear plausible and physically interesting, especially the contrast between the finite-charge behavior at $\varepsilon \neq 0$ and the large-charge limit at $\varepsilon = 0$.

Recommendation

I recommend publication after major revision. The manuscript contains publishable material, but several central conclusions are currently stated more strongly than the evidence presented in the paper supports. In particular, the numerical determination of the existence region, the nonexistence statement for $\tau > 1$, and the stability claims need more detail, qualification, or additional checks. The paper would also benefit from a sharper comparison with earlier non-self-dual Chern-Simons soliton work, since the novelty is currently visible but not stated with enough precision.

Major Comments

1. **Numerical existence region and the claim of no solutions for $\tau > 1$.**

   The existence region shown in Figure 1 is one of the central results of the manuscript. At present, however, the paper gives only a qualitative description of the boundary curve $\varepsilon = \varphi_b(\tau)$ and says that the authors were unable to find solutions for $\tau > 1$. This is not enough to support the stronger conclusion that no soliton solution exists in that region. The branch-point argument based on the observed behavior $\varphi_b(\tau) \propto (1-\tau)^{1/2}$ is suggestive, but it is not a proof of nonexistence for real solutions at $\tau > 1$.

   The numerical section should describe the boundary-value computation in more detail. The paper should specify how the semi-infinite interval was truncated or compactified, what boundary conditions were imposed at the numerical endpoint, which continuation parameters were used, how the upper boundary curve was located, and what precision or residual checks were applied. The statement that the differential identity $dE/dQ_N = \Omega_\infty$ and the virial identity were checked is useful, but the reader needs representative error estimates. A small table of numerical values for $\varphi_b(\tau)$, $\Omega_\infty^{\min}$, and, for $\varepsilon = 0$, $\Omega_\infty^{\lim}$ would make Figure 1 and the later figures substantially more reproducible.

Unless an analytic nonexistence argument is added, the conclusion should be phrased as a numerical conclusion: for example, that no solutions were found for $\tau > 1$ within the explored ansatz and numerical method, and that the fitted behavior of the boundary curve is consistent with the disappearance of the nontopological branch at the self-dual endpoint. If the authors intend the statement as a theorem, the proof should not rely on analytic continuation of a numerically determined boundary curve.

2. **Stability assertions need a more careful qualification.**

   The manuscript distinguishes stability against emission of elementary scalar quanta from decay into multiple solitons, which is the right starting point. The condition $E/Q_N < 1$ in the dimensionless notation is a reasonable energetic test against decay into free scalar particles. However, the later statement that the absence of a cusp in the $E(Q_N)/Q_N$ curves implies classical stability is too compressed for such an important claim.

   For solitons of Q-ball type, classical stability is usually tied to the fluctuation spectrum, and turning points in $Q(\omega)$ or $E(Q)$ often signal changes in the number of unstable modes. In the present model the gauged curves have upper and lower branches connected at turning points, and the nonzero-ε case also allows energetic fission for part of the branch. The paper should either provide a genuine linearized fluctuation analysis, or clearly state that the work establishes energetic stability against elementary-particle emission and gives only indirect evidence for classical stability. If the Lee-Pang cusp criterion is being applied, the authors should explain why its hypotheses apply to this pure Chern-Simons gauged system and to the particular branch structure shown in Figures 3 and 4.

   This point is important because the conclusion currently says that the non-self-dual solitons are absolutely stable against emission of elementary particles, while the numerical discussion also finds a range where decay into smaller solitons is energetically possible. The paper should separate the notions of stability more explicitly: elementary-particle stability, fission stability, classical linear stability, and quantum tunneling stability are not equivalent.

3. **The analytic derivations are useful, but several assumptions should be made explicit.**

   The derivation of the reduced equations, the charge-flux-angular-momentum relations, the fixed-charge relation $dE/dQ_N = \Omega_\infty$, and the virial identity are among the strongest parts of the paper. I found these arguments broadly convincing. Still, several steps deserve clarification.

   First, the matching-counting argument for existence, based on the number of small- and large-radius parameters, should be presented as a consistency check rather than evidence close to a proof. Parameter counting cannot rule out obstructions or multiple disconnected branches. Second, the monotonicity argument for $\Omega(r)$ and $a(r)$ assumes control over possible zeros of $f(r)$. Since the paper later restricts attention to the nodeless solution, it would be clearer to state explicitly that the monotonicity result is being used for the nodeless branch and to explain how the argument changes, or fails, for radially excited solutions with nodes.

Third, the derivation of the variational identity should spell out the class of allowed variations and boundary terms. Since Ω_∞ is both a gauge-invariant boundary value and the Lagrange multiplier for fixed charge, the reader would benefit from a slightly more formal explanation of which quantities are held fixed in the variation and how the gauge condition $a_0(\infty) = 0$ enters.

These are not fatal problems, but they affect the precision of the presentation. The analytic section will be much stronger if the authors distinguish exact consequences of the field equations from plausibility arguments and from numerical observations.

4. **Relation to previous Chern-Simons soliton literature should be sharpened.**

   The introduction cites the classic self-dual Chern-Simons-Higgs papers and earlier work on non-self-dual nontopological solitons. However, the manuscript does not make sufficiently clear which results are new relative to the papers by Bazeia and Lozano and related earlier studies. The reader is told that a number of issues were considered previously, but not which issues and not how the present analysis extends them.

   Before publication, the authors should add a concise comparison paragraph. For example, the paper should state whether the novelty lies in the full numerical mapping of the (τ, ε) existence region, the use of the interpolating potential with the parameter ε, the finite-versus-infinite charge distinction between $\varepsilon \neq 0$ and $\varepsilon = 0$, the virial and differential identities, or the thick-wall asymptotics. If some of these elements already appear in earlier work, that should be acknowledged directly. This will make the significance of the manuscript clearer and will prevent the paper from appearing as a mostly numerical reanalysis of known solutions.

5. **The large-charge distinction between $\varepsilon = 0$ and $\varepsilon \neq 0$ is interesting and should be supported quantitatively.**

   The argument near the end of the numerical section, explaining why arbitrarily large energy and charge require $\varepsilon = 0$, is physically appealing. It correctly identifies the need for an extended region in which f sits near a minimum of the effective potential and for the virial relation to remain compatible with small Ω. This is one of the most interesting parts of the manuscript.

   At the same time, the argument remains qualitative. For $0 < \varepsilon < 1/\sqrt{3}$ the potential still has a local asymmetric minimum, and the paper argues that its nonzero value prevents arbitrarily large solitons. It would be useful to quantify this statement. For example, does the maximal charge diverge with a definite scaling as $\varepsilon \to 0$ at fixed τ? Do the numerical curves show a scaling law near the boundary between the finite and infinite charge behavior? A table or asymptotic estimate would substantially strengthen the conclusion that the qualitative change is controlled precisely by the degeneracy of the two zero minima.

   The appendix on the thick-wall regime is a useful addition, but it addresses the lower branch near $\Omega_\infty = m$. It would help to connect that approximation more directly to the main large-charge conclusions, especially to explain which asymptotic branch controls the upper-charge endpoint for $\varepsilon \neq 0$ and which controls the divergent-charge behavior for $\varepsilon = 0$.

6. **Figures and numerical data need more self-contained presentation.**

   The figures carry much of the evidence in the paper, but their captions and the surrounding text do not give enough quantitative information. The curves in Figures 3–7 should be labeled with the actual values of τ used, either in the plot or in the caption. Figure 1 should state how the boundary was sampled and whether the displayed curve is an interpolation, a fit, or a direct numerical continuation. Since the axes in Figure 1 are logarithmic, the caption should mention whether the plotted region includes $\varepsilon > 1/\sqrt{3}$ and how that relates to the disappearance of the local asymmetric minimum of the potential.

The checks of the virial identity and of $dE/dQ_N = \Omega_\infty$ should also be reported quantitatively. Even a sentence giving typical relative errors would make the numerical conclusions more credible.

Minor Comments

1. The manuscript contains several typographical and grammatical errors that should be corrected before publication. Examples include “Chern-Simon term”, “Figire”, “turning poins”, “an usual nongauged two-dimensional Q-ball”, and “the bvp problem depends on four parameters three of which , ...”. There is also an extra space in one citation command in the introduction.

2. The word “nontopological” is sometimes hyphenated and sometimes not. The authors should choose one convention and use it consistently.

3. The notation $\tau = e^2 v^2/(m\kappa)$ is central. It would be helpful to remind the reader more explicitly that the discussion assumes positive κ and positive τ, and to state how the sign conventions would change if κ were chosen negative.

4. The paper should define all dimensionless electric and magnetic fields used in Figure 2 before the figure is encountered, or include the definitions directly in the caption.

5. The statement that the model has no topological solitons for $\varepsilon \neq 0$ is correct for the boundary condition $|\phi| \to 0$, but the discussion would be clearer if it explicitly contrasted this with the $\varepsilon = 0$ case, where the broken minimum is degenerate with the symmetric one.

6. The bibliography is adequate, but the entries would be more useful if arXiv identifiers or DOIs were added consistently where available. Some punctuation in the bibliography also needs cleanup.

7. The discussion of type-I and type-II superconductivity by analogy with the Maxwell-Higgs system is suggestive. Since the gauge dynamics here are pure Chern-Simons rather than Maxwell, the analogy should be phrased cautiously, as the authors mostly do, and perhaps tied explicitly to the mass ratio $\tau = m_A/m_H$.

Conclusion

The manuscript presents a worthwhile study of non-self-dual nontopological solitons in a pure Chern-Simons-Higgs model. I expect the main results to be publishable after revision. The most important revisions are to document the numerical procedure, qualify or prove the nonexistence and stability claims, and clarify the relation to previous non-self-dual Chern-Simons soliton literature. With these changes, the paper would be a useful contribution to the literature on gauged Q-balls and Chern-Simons solitons.