Referee Report

Type I/II Vortex Dynamics With Excited Normal Modes

Summary and recommendation

The manuscript studies Abelian–Higgs vortex dynamics away from critical coupling when the radial normal mode of each vortex is excited. The central physical idea is appealing. In the type I regime an out-of-phase excitation can provide a repulsive contribution that competes with static attraction, whereas in the type II regime an in-phase excitation can compete with static repulsion. The authors combine single-vortex and coincident-vortex spectral data, pinned static two-vortex energies, an empirical mode-dependent interaction energy, and full field simulations. They report reversal of the usual force, transient finite-separation equilibria, off-critical spectral-wall phenomena, multibounce scattering, and several orbit-like trajectories.

The force-reversal mechanism and the finite-separation states in Sections 6 and 7 are interesting and, if made quantitatively reliable, would constitute a useful contribution to the vortex and soliton literature. The comparison between type I and type II dynamics is particularly well motivated. However, the present version has several problems that prevent a reliable assessment of the headline results. Most importantly, the origin analysis and boundary conditions for the non-radial spectral problem are inconsistent with the coupled equations; the reduced orbital mechanics contains elementary factors of two (and consequent normalization errors); the effective mode energy and the intensity convention are mutually inconsistent; and the numerical results are presented without the convergence and constraint diagnostics needed to distinguish physical long-lived behavior from finite-grid, initial-data, and absorbing-boundary effects.

I therefore recommend **major revision**. The paper could become suitable for a journal such as Physical Review D or JHEP, but the spectral and orbital calculations must first be corrected or reconciled with the actual code, and the main dynamical conclusions must be supported by controlled numerical tests. If the Appendix B boundary conditions were used in producing Figure 2.1, the affected spectrum and every downstream calculation that uses it will need to be recomputed.

Major comments

1. **The Frobenius analysis for $k \neq 0$ does not treat the leading u–w coupling.**

   Equations (2.33) and (2.34) contain, near the origin, $-2Nk^2w/\rho^2$ in the u equation and $-2Nu/\rho^2$ in the w equation. Consequently the leading indicial problem is coupled. With $u, w \sim \rho^s$, its homogeneous part is
   $$\begin{pmatrix} k^2 + N^2 - s^2 & -2Nk^2 \\ -2N & k^2 + N^2 - s^2 \end{pmatrix} \begin{pmatrix} u_0 \\ w_0 \end{pmatrix} = 0.$$
   It gives $s^2 = (N \pm |k|)^2$, with correlated leading coefficients, rather than $s^2 = N^2 + k^2$ separately for u and w as stated in equations (B.9)–(B.13). In particular, when $k = N$ one regular branch has a constant combination of u and kw. Imposing $u(0) = w(0) = 0$ as in the discussion leading to equations (B.19)–(B.21) can remove this branch, which is precisely the situation relevant to the translation/splitting sectors plotted in Figure 2.1.

There are further algebraic inconsistencies in the same appendix. For $s = 1$ and $j = 2m$, the coefficient in equation (B.6) is $-4m(m + 1)$, not $-4(m + 1)$ as used in equation (B.7). Likewise the denominator in equation (B.15) should contain $-4m(m + |k|)$. Moreover, the positive powers found for both $k = 0$ fields imply Dirichlet data at the origin, whereas the $-4/3$ stencil in equations (B.17)–(B.18) is the stencil obtained after using a Neumann ghost value. The indexing statement that $i = 1$ is the origin also conflicts with the evaluation of coefficients at $\rho = h$.

Appendix A has a related problem: it presents the discretization of the $k \neq 0$ three-component equations and says that the $k = 0$ case is obtained by omitting w_i. This does not produce equations (2.25)–(2.26). For example, equation (A.1) would lose both the v/ρ^2 term and the v–u coupling of equation (2.25). A separate two-component discretization is required.

The authors should redo the coupled Frobenius analysis, state the independent regular branches and their coefficient relations, and derive a boundary stencil consistent with the grid indexing. They should then say explicitly whether the published MATLAB calculation used these appendix conditions. If it did, Figure 2.1 and the quoted seven-digit eigenvalues require recomputation. If the code used different conditions, those conditions and convergence tests should replace the present appendix.

2. **The gauge-fixed spectral problem and its normalization need a consistency check.**

   The bilinear in the gauge condition (2.21), $\overline{\psi}\phi_s - \overline{\phi_s}\psi$, is purely imaginary for physical perturbations, while $\partial_\mu \chi^\mu$ is real. A background gauge condition normally contains an explicit factor of i (with a convention- dependent numerical coefficient). It also does not, without an additional argument, imply the Lorenz gauge as claimed. The authors should derive the gauge-fixing term and the operator in equation (2.22) from the quadratic action with all factors displayed, and verify self-adjointness in the inner product actually used numerically.

   This is especially important for $k \neq 0$. The variables in equations (2.27)–(2.34) enter the physical gauge and Higgs perturbations through derivatives and factors of k and ρ. It is not evident that the physical quadratic norm reduces to the simple expression (2.35). The authors should derive the radial norm after angular integration and show that their discrete matrix is Hermitian (or identify the appropriate generalized eigenvalue problem). Direct angular integration of the displayed ansatz instead produces terms of the form $s(\rho)^2 + k^2 v(\rho)^2/\rho^2 + u(\rho)^2 + k^2 w(\rho)^2$, with $s = v' - \rho f w$, rather than simply $v^2 + u^2 + w^2$. If a field redefinition makes (2.35) correct, it must be shown. This would also provide an independent diagnostic of the Appendix B implementation.

3. **Several formulas defining the interaction model are internally inconsistent.**

   First, the tail coefficients in equation (3.1) are called the scalar charge q and magnetic dipole strength m. The interaction of two identical point sources then contains products of the two charges, conventionally

   $$E_{\text{int}}(s) = \frac{1}{2\pi}\left[m^2 K_0(s) - q^2 K_0(\sqrt{\lambda}s)\right],$$

   not the terms linear in q and m written in equation (3.2), unless the symbols are silently redefined between the two equations. The plotted asymptotic curves should be checked after correcting this point, and the method used to extract q, m, and $\beta(\lambda)$ should be reported.

   Second, equation (3.6) subtracts $\max_a \omega^2(a)$ but the text says that the mode energy is normalized to zero at infinite separation. These coincide for the type II branch but not for the type I branch in Figure 6.1, whose frequency rises toward the wall. Figure 6.5 visibly uses zero at infinity and a positive wall contribution, not the normalization displayed in equation (3.6). An additive constant does not change the force, but the mismatch shows that the quantity plotted has not been defined correctly.

Third, every trajectory caption defines $I = \frac{1}{2}(\epsilon\omega)^2$, whereas the sentence below equation (3.6) uses $\epsilon = \sqrt{I}/\omega$. The stated definition instead gives $\epsilon = \sqrt{2I}/\omega$. Equations (5.1)–(5.2) then multiply mode vectors by $I/2$, although normalized in-phase and out-of-phase vectors would carry a $1/\sqrt{2}$ combination and a separate amplitude. These are not cosmetic issues: the effective force is proportional to ϵ^2, so the missing factor changes every intensity-dependent potential and predicted equilibrium. The authors should establish one unambiguous convention for eigenvector norm, oscillation amplitude, mode energy, and the plotted "intensity," and recompute the relevant figures.

4. **Equation (3.6) is not yet a controlled effective potential.**

   For a harmonic mode whose frequency varies with the collective coordinate, the force depends on what is held fixed. At fixed instantaneous amplitude the quadratic term is proportional to $\epsilon^2\omega^2(s)$; in adiabatic motion the approximately conserved quantity is instead the oscillator action, so the amplitude changes and the energy is proportional to $\omega(s)$. The simulations themselves show a strongly time-dependent intensity. The paper does not state which reduction is intended, derive it from a collective- coordinate Lagrangian, or estimate the nonadiabatic corrections.

   There is also some circularity: $\omega(s)$ is extracted from a dynamical trajectory through equation (3.5), and a potential built from that function is then used to explain the same trajectory. Above a continuum crossing there is no normalizable discrete mode, so a dashed continuation of a measured oscillation frequency cannot automatically be inserted into a conservative potential. This affects the interpretation of Figures 6.7, 7.12, and 7.13 in particular.

   The paper would be substantially stronger if the authors computed the relevant instantaneous two-center Hessian independently at pinned separation, derived the collective-coordinate force, and compared predicted and measured turning or equilibrium positions with uncertainties. At minimum they should explain the fixed-amplitude versus fixed-action choice, report how equation (3.5) separates mode oscillations from translational and radiative changes in the static energy, and restrict equation (3.6) to the domain in which a discrete mode exists.

5. **The largest excitations are outside the stated linear regime.**

   Using the intensity definition in the captions, the runs with $I(0) = 0.7$ and 0.75 have ϵ of order one or larger. Even the $I(0) = 0.3$ examples are not parametrically small. Section 4 acknowledges that cubic and higher terms then matter, yet the interpretation continues to use a linear eigenmode, a quadratic mode energy, and a single instantaneous frequency. These large-intensity runs support important claims, including the type II multibounce state in Figure 7.3 and the $\lambda = 2$ spectral-wall example in Figure 7.11.

   Full nonlinear evolution from such initial data can of course be meaningful, but it no longer validates the proposed linear-mode potential without further evidence. The authors should show amplitude scans into a genuinely perturbative regime, project the evolution onto the relevant instantaneous mode, quantify higher harmonics and radiated energy, and demonstrate that the force reversal or wall signature persists. Alternatively, they should construct nonlinear periodically excited vortex initial data and reformulate the analysis accordingly.

6. **The numerical evidence is not reproducible or sufficiently validated.** Section 4 gives a 601×601 grid and $h = 0.1$, but omits the time step, Courant or stability check, evolution gauge,

treatment of Gauss' law, damping profile and layer width, vortex-position algorithm, definition of $I(t)$, initial phases for most runs, and termination criteria. No spatial, temporal, domain-size, or damping-layer convergence study is shown. Nor are total energy loss, interior energy balance, angular-momentum loss, or gauge-constraint violations reported. These diagnostics are essential when the phenomena of interest are small velocity changes, long trapping times, multiple bounces, and slowly decaying orbits.

The claimed fractal structure in Figures 7.9–7.10 is also not established by one finite-resolution color map. The paper should give the parameter-grid spacing and classification rules, repeat selected cuts at finer parameter and field resolution, and provide nested zooms or another quantitative self-similarity test. Otherwise "fractal" should be replaced by the more limited observation of a banded multibounce structure. The quoted critical velocity $v_{\text{in}} = 0.11$ likewise needs an uncertainty and a definition of the intensity range over which it is claimed.

7. **The evidence for off-critical spectral walls should be separated from evidence for radiation and ordinary effective barriers.**

   At $\lambda = 0.9$, coincidence between a continuum crossing and a trajectory turning point is suggestive, but the manuscript describes energy as being "transferred to the spectral wall." A wall is not an independent reservoir; when a bound mode reaches the continuum, the relevant questions are how its spectral weight and energy enter radiation and how this back-reacts on the collective coordinate. At $\lambda = 0.5$, Section 6 explicitly says the wall is difficult to confirm, while Section 9 states that spectral walls appear. At $\lambda = 2$, an irregularity near $d \simeq 2$ correlated with a frequency crossing is again evidence consistent with a wall, not by itself a confirmation.

   The authors should track radiated flux and instantaneous spectral projection, vary phase and amplitude, and demonstrate convergence of the turning location toward the independently determined continuum crossing. The $\lambda = 0.5$ and $\lambda = 2$ conclusions should remain qualified unless these tests are performed. The signed or imaginary coordinate used after right-angle scattering must also be defined: equations (3.2)–(3.6) use a real nonnegative separation s, whereas Figure 6.1 and the text use negative or imaginary d.

8. **The orbital formulas following equation (8.1) do not follow from that Lagrangian.**

   From
   $$L_{\text{red}} = \frac{m}{4}(\dot{s}^2 + s^2\dot{\theta}^2) - V(s),$$
   one obtains $p_\theta = (m/2)s^2\dot{\theta}$, not $(m/4)s^2\dot{\theta}$, and
   $$\frac{m}{2}\ddot{s} = \frac{m}{2}s\dot{\theta}^2 - \frac{dV}{ds},$$
   not the equation displayed below (8.1). The subsequent expressions for L_z, centrifugal force, and centrifugal energy therefore have incorrect factors. In addition, equation (3.4) defines an interaction energy per vortex, whereas the kinetic term in equation (8.1) is the total relative kinetic energy; Section 8 mixes these conventions.

   This error propagates directly into the force curves and predicted velocities in Figures 8.1, 8.4, and 8.8. The authors should redo the two-body reduction with a single total-energy convention, distinguish each vortex's tangential speed from the relative speed, and recompute all centrifugal curves and target velocities before comparing with field simulations. With L_z denoting the physical total angular momentum, the total centrifugal potential is $L_z^2/(ms^2)$ and the per-vortex value is $L_z^2/(2ms^2) = L_z^2/(8md^2)$. If $v_{\rm in}$ is the speed of each vortex at radius d, then $L_z = 2mdv_{\rm in}$, not $mdv_{\rm in}/2$ as used in the figure captions.

9. **The orbit and stability claims are stronger than the evidence.**

   The critical-coupling excited pair in Figure 8.2 escapes after about six turns, the close type I trajectory in Figure 8.6 is followed only until the numerics fail, and the type II pair in Figure 8.9 completes about two turns before escaping. None is accompanied by a linear stability analysis, a parameter window, or a boundary/domain convergence test. "Stable circular orbit" and "semi-stable" should therefore be replaced by "long-lived" or "metastable orbit-like trajectory" unless stability is demonstrated.

   There are also parameter contradictions that presently prevent quantitative comparison. Figure 8.2 uses $I(0) = 0.025$, while Figure 8.3 describes the same run with $I(0) = 0.05$. The text predicts $v = 0.038246$ at $d = 4$, while Figure 8.5 uses 0.03230815. The images and text surrounding Figure 8.8 use intensities $0.05, 0.10, 0.15, 0.20$, but its caption lists $0.10, 0.20, 0.30, 0.40$. These discrepancies, together with the centrifugal factor error, require a complete audit of Section 8's plotted parameters.

10. **The vortex–antivortex subsection does not support the claim made for it.**

    Section 8.4 does not state λ, initial radius, impact parameter, domain size, or any normal-mode amplitude or phase. It therefore cannot be reproduced and does not show that a normal-mode force induces the orbit advertised in the abstract. The displayed pair completes only about one orbit, and the authors then state that its escape is an artifact of the damping boundary. This is not evidence for a stable physical orbit. The assertion that escape is impossible because the static force is attractive is also incorrect in a short-range attractive potential when the total energy is positive, while damping would ordinarily remove kinetic support rather than explain outward escape.

    This subsection should either be replaced by a controlled calculation on a larger domain, with explicit initial data, conserved-energy/angular-momentum budgets, boundary tests, and an actual excited-versus-unexcited comparison, or be reduced to a clearly labeled preliminary observation. The abstract and conclusion should not attribute a mode-induced long-lived vortex–antivortex orbit to the present evidence.

11. **The novelty boundary should be stated more precisely.**

    Section 2.2 and Figure 2.1 largely review or reproduce known spectral results; equations (3.2)–(3.3) use established long- and short-range interactions; and Section 5 reviews the authors' and others' critical-coupling results. The genuine advance appears to be the systematic off-BPS competition of the normal mode with the nonzero static force, especially the force reversal and finite-separation transient equilibria in Figures 6.4–6.6 and 7.2–7.8. That advance is worthwhile, but it is obscured by the amount of baseline material.

    The cited 2025 doctoral thesis already advertises type I/type II off-critical quasi-stationary states, spectral walls away from critical coupling, and orbiting two-vortex and vortex–antivortex solutions. The manuscript should identify explicitly which derivations, datasets, scans, and conclusions are new beyond the thesis, rather than saying only that it extends that analysis. It should also compare its coupling range, observables, and conclusions directly with the contemporaneous work on wobbling-vortex scattering cited in Section 1. A concise result-by-result comparison would make the paper's publication case much clearer.

Minor and editorial comments

1. The statement in Section 1 that type II vortices have no static solutions for $|N| > 1$ should say that there is no stable separated multivortex minimum. The paper itself later uses the unstable radially symmetric $N = 2$ static saddle.

2. Equation (3.5) should specify whether Δt is measured between two successive maxima, two successive minima, or adjacent extrema. Because the static energy is quadratic in a linear oscillation, this distinction can produce a factor-of-two ambiguity. The frequency-extraction uncertainty and the smoothing or fitting procedure should be given.

3. Several captions describe the top-left panel as a "static force" even though the vertical axis and equation (3.4) define a static energy. Energy and force terminology should be corrected consistently. In particular, an energy correction proportional to d^4 gives a force proportional to d^3, not d^4 as stated in the small-separation discussion.

4. The continuum boundary is variously called the gauge threshold or mass threshold. The paper should display the two thresholds $\omega^2 = 1$ and $\omega^2 = \lambda$, state which is the lowest continuum in each regime, and label each shaded region consistently.

5. The relation for $a_\pm$ stated below equations (2.25)–(2.26) does not follow from the Cartesian components written below equation (2.16). With the given convention $a_+ = a_1 + ia_2$, the phases and signs should be checked.

6. The product/sum initial data in equation (4.3) are justified only at large separation, yet several orbit calculations start at $d = 1$ or below. The authors already note relaxation of the data; they should quantify the resulting spurious radiation and constraint error or construct relaxed pinned initial data before applying tangential velocity.

7. Figure 7.10's normalization and the meanings of t_{max}, t_{end}, and t_{min} are not clear. Please give the precise observable plotted and its range.

8. At $\lambda = 0.9$ the text gives the continuum crossing as $d \approx -1i$. If this is an imaginary moduli-space coordinate used to encode the outgoing axis, it must be defined explicitly; if not, it is a typographical error.

9. The manuscript would benefit from a careful proofread. Examples include "Abikrosov" for Abrikosov, "indical" for indicial, "majority if this work," and visibly corrupted character encodings in the vortex–antivortex caption and the discussion of the Schrodinger–Chern–Simons model.

In summary, the off-critical force reversal is sufficiently interesting to merit reconsideration, but the present analytic and numerical problems leave the main conclusions uncertain. I recommend major revision, including recalculation of every result affected by the spectral and orbital errors.