Referee Report

Gauge Symmetry Degeneration in Lorentzian Deformed Light-Cone Null Reduction

Summary

The manuscript studies the fate of gauge symmetry in a Lorentzian deformed light-cone null reduction of a complex Maxwell field. The starting point is a $(d+1)$-dimensional Maxwell-type parent action written in deformed light-cone coordinates, followed by compactification in the x^- direction, a single-mode ansatz with $p_- = mc$, and a Carrollian limit $c \to 0$. The paper argues that this procedure does not produce Carrollian electrodynamics. Instead, the limiting theory is the free complex-scalar theory found in the author's earlier work, with $\tilde{A}_-$ and $\tilde{A}_i$ behaving as independent scalar degrees of freedom and $\tilde{A}_\tau$ decoupled.

The core claim is that the degeneration is structural. Section 2 reviews the parent action, the single-mode reduction, and the reduced Lagrangian in equation (2.7). The reduced canonical momenta in equation (2.8) are then used to motivate the reduced Gauss-law expression in equation (2.9). Section 3 presents three diagnostics: the Gauss law degenerates into the conservation statement in equation (3.1), the number of real physical degrees of freedom increases from $2(d-1)$ to $2d$, and the limiting action in equation (3.2) has Carrollian conformal symmetry in arbitrary dimension, unlike genuine Carrollian Maxwell theory. The section also gives a useful explanation of gauge-symmetry loss as a cross-order cancellation problem: the $O(c^0)$ variation of the surviving term $\mathcal{L}^{(0)}$ is cancelled in the full theory only by terms that are removed by the truncation. Section 4 gives a power-counting no-go theorem for repairing the result by assigning arbitrary powers of c to $\tilde{A}_\tau$, $\tilde{A}_-$, and $\tilde{A}_i$ within the same single-mode ansatz.

The question addressed by the paper is timely and relevant for work on Carrollian limits, null reductions, light-front formulations, and celestial-holography-motivated constructions. The main message is also physically plausible: a limiting procedure may preserve the equations of motion of some variables while destroying the first-class constraint structure that made them gauge variables in the parent theory. If correct and sufficiently documented, the observation would be a useful cautionary result for the community.

Evaluation of Correctness and Clarity

The manuscript has a coherent overall narrative and identifies a real issue: the limiting action (3.2) plainly has no spatial derivative structure and therefore cannot be equivalent to either the electric or magnetic Carrollian Maxwell systems discussed in the cited literature. The comparison with the dimensionally restricted conformal invariance of Maxwell theory is a helpful diagnostic, and the cross-order cancellation discussion around equations (3.3)–(3.5) is one of the strongest parts of the paper. It makes clear why checking invariance after truncation is not the same as taking a controlled limit of a gauge symmetry.

However, the paper currently leaves several central derivations too compressed for the main conclusion to be considered fully established. The most important gap is the passage from the parent first-class Gauss law in equation (2.4) to the reduced expression in equation (2.9), and then to the limiting statement (3.1). Since the whole paper turns on whether the first-class constraint survives the reduction, this step needs to be derived in a way that fixes all normalizations, complex conjugations, mode restrictions, and powers of c. In particular, equation (2.4) contains a ∂_- derivative. Under a single Fourier mode this derivative should produce a factor proportional to $-ip_- = -imc$. It is therefore not immediately transparent from the manuscript how the terms in equation (2.9), especially the unsuppressed $\partial_\tau \tilde{A}_-$ and $\partial_\tau(\partial_i \tilde{A}_i)$ pieces, arise from the parent constraint rather than from the Euler-Lagrange equation obtained by varying the reduced action with respect to $\tilde{A}^*_\tau$. These two routes may of course agree after the reduction is performed carefully, but the manuscript should show the agreement explicitly because the reduced constraint algebra is the object under scrutiny.

The degrees-of-freedom discussion is also suggestive but too terse. The parent count $2(d+1)-4 = 2(d-1)$ for a complex Maxwell field is reasonable if one treats the complex gauge parameter as two real first-class gauge parameters. After the limit, the manuscript states that $\tilde{A}_\tau$ decouples and that $\tilde{A}_-$ and $\tilde{A}_i$ become d independent free complex scalars, giving $2d$ real degrees of freedom. This is consistent with the limiting action (3.2), but the argument would be more convincing if it were formulated directly in Hamiltonian language after the limit. For example, the author should state whether equation (3.1) is meant to be an equation of motion, a residual constraint, or a condition imposed only on a subclass of solutions. If it is not a constraint, the paper should explain why it does not remove phase-space data and how this is reflected in the Poisson bracket analysis of the limiting theory.

The no-go theorem in Section 4 is a valuable attempt to sharpen the conclusion, but its assumptions need more careful presentation. Requirements (R1)–(R3) rule out a particular class of regular power-law rescalings within the single-mode $p_- = mc$ ansatz. This is a meaningful result, but the manuscript sometimes phrases it as if it excludes any possible repair of the reduction. The proof also treats divergent kinetic terms under (R1) as acceptable because the inequalities are $\beta \leq 0$ and $\gamma \leq 0$ rather than $\beta = 0$ and $\gamma = 0$ or a stated finite-action normalization prescription. The author should clarify whether divergent overall factors are to be removed by an additional normalization of the action, whether such a normalization affects the constraint comparison, and why field redefinitions involving longitudinal and transverse components separately are outside the intended scope. With these qualifications added, the theorem would be a clearer and more defensible statement.

Novelty and Significance

The manuscript appears to make a focused contribution relative to the cited literature. Earlier works developed Carroll contractions of Lorentz-invariant theories, Carrollian electrodynamics from equations of motion or action principles, Bargmann-based null reduction, and Lorentzian light-cone approaches. The present paper's novelty is not the construction of a new Carrollian theory, but rather the diagnosis that one Lorentzian deformed light-cone route produces scalar-like dynamics because the $c \to 0$ step removes the Gauss constraint and the spatial derivative terms that distinguish Maxwell theory from a collection of scalars.

This is a useful result if stated with the correct scope. It helps separate three notions that are sometimes conflated: null reduction as a geometric operation, the Carrollian limit as a contraction, and the preservation of first-class gauge constraints under singular limits. The paper would be of interest to readers working on Carrollian field theory and light-front/null reductions, especially because it points out that a theory may still have Carrollian conformal symmetry while failing to carry the gauge structure expected from its parent field. The result is probably not broad enough in its present form to support a sweeping no-go theorem for all Lorentzian reductions, but it is publishable as a concise diagnostic note once the central derivations are made explicit.

Recommendation

I recommend publication after major revision. The main idea is interesting and likely useful, and the conclusions are plausible. The present version is nevertheless too compressed at precisely the technical points on which the claim depends. A revised version should include a transparent derivation of the reduced Gauss law, a sharper Hamiltonian explanation of the limiting degrees of freedom, and a more carefully scoped statement of the scaling no-go theorem. These revisions would not necessarily require changing the main conclusion, but they are needed before the paper can serve as a reliable reference.

Major Comments

1. The derivation of equation (2.9) should be expanded substantially. The reader needs to see how the parent constraint $\mathcal{G} = \partial_i \Pi^i + \partial_- \Pi^-$ becomes the displayed reduced expression after the single-mode ansatz and the $x^+ = c\tau$ rescaling. This is especially important because ∂_- acting on a single mode gives a factor $-imc$, while equation (2.9) contains terms that appear without such a suppression. The manuscript should specify whether the displayed expression is obtained by reducing the parent Hamiltonian constraint, by varying the reduced Lagrangian with respect to $\tilde{A}_\tau^*$, or by an equivalent intermediate procedure. If these procedures are equivalent, showing the equivalence would strongly reinforce the paper's main claim.

2. The constraint interpretation of equation (3.1) needs to be made precise. The text says that it is not a constraint but only states that the longitudinal part $\sum_i \partial_i \tilde{A}_i$ is conserved in τ. This is plausible, but it should be backed by a phase-space statement. Does equation (3.1) arise as a secondary constraint in the limiting Hamiltonian system, or as an ordinary Euler-Lagrange equation? If it is an equation of motion, what are the canonical coordinates and momenta of the limiting theory, and how many independent initial data remain after imposing it? Since the asserted increase from $2(d-1)$ to $2d$ is one of the three diagnostics, this point should not be left at the level of an intuitive sentence.

3. The discussion of gauge transformations after the reduction should be more explicit. The parent gauge parameter is an arbitrary complex function of x^+, x^-, and x^i. Under the single-mode truncation, what class of gauge parameters is retained? Is the gauge parameter also restricted to a compatible Fourier mode, and if so how does this interact with $\delta\tilde{A}_- = \partial_- \varepsilon$ and $\delta\tilde{A}_\tau = c^{-1}\partial_\tau \varepsilon$? The cross-order cancellation argument around equations (3.3)–(3.5) is persuasive, but it would benefit from a clear statement of the reduced gauge transformation laws and their c-scaling before the limit is taken.

4. The no-go theorem should be carefully scoped. As written, the theorem rules out preserving nontrivial dynamics and a first-class Gauss law through componentwise power-law rescalings of $\tilde{A}_\tau$, $\tilde{A}_-$, and $\tilde{A}_i$ within a single-mode $p_- = mc$ ansatz. That is a useful theorem, but it does not by itself exclude more general field redefinitions, separate longitudinal/transverse scalings, multi-mode constraints, gauge-parameter rescalings, or modified reductions that keep auxiliary constraint variables. The abstract and conclusion should be adjusted so that the result is not read as a no-go statement beyond the stated ansatz.

5. Requirement (R1) in Section 4 needs clarification. The inequalities $\gamma \leq 0$ and $\beta \leq 0$ ensure that the kinetic terms do not vanish, but for negative exponents the kinetic terms diverge before any further normalization. If such divergences are allowed because the action may be

divided by an overall power of c, this should be stated. The author should then explain why the same normalization does not change the finite-constraint requirement (R3). If the intended limiting theory must have finite nonzero kinetic terms without an extra normalization, then (R1) should probably be formulated with equalities rather than inequalities. Either route may still support the desired conclusion, but the assumptions should be explicit.

6. The comparison with Carrollian Maxwell theory would be stronger if the paper displayed the relevant electric and magnetic Carrollian Maxwell actions or constraint structures, at least schematically. Section 3 states that those theories contain spatial derivative terms and have Carrollian conformal symmetry only in $d = 4$. Since this comparison is used as a diagnostic of genuine gauge-field dynamics, including the representative actions and their degrees-of-freedom counts would make the paper more self-contained.

7. The manuscript should distinguish more carefully between the loss of local gauge redundancy and the possible survival of global symmetries. The conclusion says that the scalar theory has a genuine global $U(1)$ current, while the parent theory had local $U(1)$ gauge redundancy. This distinction is important and should be emphasized earlier, perhaps immediately after equation (3.2), to avoid the impression that all $U(1)$ structure disappears.

Minor Comments

1. The abstract is dense and would benefit from one sentence that states the setup more slowly: a complex Maxwell field is reduced using the Lorentzian deformed light-cone single-mode prescription, and the resulting $c \to 0$ theory is compared with Carrollian electrodynamics.

2. The introduction contains several grammatical issues that should be corrected. For example, "Dimensional unrestricted Carrollian conformal symmetry(CCS)" should be rewritten as a complete phrase, and spaces should be inserted before parenthetical abbreviations such as "symmetry (CCS)".

3. The notation Π^τ, Π^*_i, and Π^*_- should be standardized. In Section 2 the parent constraint is written with upper-index momenta, while equation (2.8) gives lower-index momenta conjugate to complex conjugate fields. This is manageable, but the relation between these conventions should be stated.

4. The sentence after equation (2.7) says that the overall factor $c^2/2$ is the structural origin of the degeneration. This is an important claim and should be tied immediately to the power counting of the four terms $T_1, \ldots, T_4$.

5. The phrase "The parent Gauss law is an elliptic PDE–a Poisson equation for $\tilde{A}_-$ and $\tilde{A}_\tau$" should be checked. If this statement refers to the reduced but finite-c theory, it would help to display the elliptic operator explicitly.

6. The bibliography contains two unrelated astronomy/deep-learning entries at the beginning. These should be removed before publication.

7. Several references are cited as adjacent citation commands, for example the citations to Bagchi et al. and to Basu and Chowdhury in the introduction. These should be combined into a single citation command where possible.

8. The acknowledgement quotation is fine in a preprint, but for a concise journal letter the author may wish to shorten or omit the poetic sentence.