Referee Report

"Iterative Gauging is Deconstruction"

Recommendation

Major revision. The paper contains a simple and potentially useful observation: the Abelian phase-only action of a deconstruction quiver becomes, after dualizing every link phase, the same local quadratic action that is generated by an alternating sequence of higher-form gaugings. The note is concise and the proposed node/link dictionary is intuitive. In its present form, however, the manuscript repeatedly calls the relation "exact" while establishing only a local, normalization-insensitive equivalence of Gaussian actions. Global data, compactness, charge and flux quantization, boundary conditions, and even several relative signs are not controlled. Since the iterative construction is itself defined by which generalized symmetry is gauged at each stage, the inconsistent statements about transformations and currents are central rather than cosmetic.

I would be willing to reconsider a substantially revised version. Such a version could be suitable as a short high-energy theory note if it either proves the stronger equivalence, including the global data, or explicitly narrows its claim to a local noncompact effective-field-theory correspondence and then demonstrates a controlled deconstruction limit.

Synopsis and assessment

The manuscript begins with a product of Abelian gauge groups and bifundamental link scalars. Freezing the radial modes and retaining the link phases gives the Stueckelberg action (2). For homogeneous couplings, unitary gauge produces the graph-Laplacian mass matrix (4), which is the familiar finite-lattice starting point for dimensional deconstruction. The authors then introduce a first-order form of each compact-looking link phase in (5) and integrate out its one-form field strength. This gives the dual action (6), with one-form gauge fields on nodes, $(d-2)$-form gauge fields on links, and incidence-matrix BF couplings.

Section III attempts to reconstruct (6) without first postulating the quiver. Starting from Maxwell theory (7), the manuscript gauges a claimed magnetic $(d-3)$-form symmetry by introducing a $(d-2)$-form field in (8), gauges an emergent zero-form symmetry by introducing the next one-form field in (9), and iterates to obtain (10). The electric-gauging subsection starts instead with the two-form field in (11), performs two dualizations, and arrives at the dual-frame chain (17), which is recast in the Stueckelberg-like form (18). The discussion extrapolates this construction to nonuniform lattices, different graph topologies, a topological BF limit, and possible non-Abelian generalizations.

At the level of local equations of motion for noncompact differential forms on a closed, topologically trivial spacetime, the link-phase dualization and incidence-matrix structure are standard and essentially correct, up to normalizations and field-sign conventions. This supports the paper's basic intuition. It does not yet support equivalence as a quantum theory, nor does the displayed derivation consistently reproduce its own BF signs. The novelty is therefore presently an interpretive identification, not a new duality theorem. Its significance for a journal such as PRD or JHEP will depend on making the precise content of that identification substantially sharper.

Major comments

1. **State and prove the actual scope of the duality in (5)–(6).** The condition $dF_{i,i+1} = 0$ implies only that $F_{i,i+1}$ is locally closed; it does not imply globally that $F_{i,i+1} = d\theta_{i,i+1}$. For a compact $U(1)$ link phase, the parent functional integral must also reproduce the periods of $d\theta$, the winding sectors, and the charge-one normalization. This normally requires a compact Lagrange-multiplier field, a quantized BF coefficient (with the relevant 2π factors), and an explicit sum over topological sectors. Boundary terms generated while passing between the two forms of the BF coupling must also be retained or excluded by a stated assumption.

   The footnote following (5) says that factors of 2 and π will not be tracked because they are irrelevant. They are irrelevant for a local classical Gaussian exercise, but they determine compactness, line-operator charges, large-gauge invariance, and the claimed inversion of couplings. They therefore cannot be omitted in an "exact" dictionary. There is already a factor-of-two issue at the purely local level: with the coefficient $v_{i,i+1}^2$ in (2) and (5), rather than $v_{i,i+1}^2/2$, direct Gaussian elimination does not give the displayed $1/(2v_{i,i+1}^2)$ coefficient in (6) without an additional convention for the norm.

   The revision should do one of two things. Preferably, formulate the compact master action with all periods and normalizations, integrate it in both orders, and give the map of genuine line/surface operators and flux sectors. Alternatively, declare all fields noncompact, restrict to a closed topologically trivial spacetime and local correlators, fix the norm convention, and replace "exact equivalence" throughout by the narrower claim that has actually been demonstrated. The latter option should also distinguish the full scalar theory (1) from its frozen-radial, phase-only effective action (2).

2. **Correct the generalized-symmetry transformations and currents.** The iterative argument depends on the existence and degree of a symmetry at each step, but the definitions in Section III are not internally consistent. Immediately after (7), the manuscript writes $dA_1 \to dA_1 + a$ with $da = 0$. A shift of the field strength by a generic closed two-form is not a symmetry of the Maxwell kinetic term. If the intended electric one-form transformation is $A_1 \to A_1 + a$ for a closed one-form a, that must be written instead. The electric and magnetic currents must then be stated with a single convention for whether "current" denotes J or $\star J$, with the appropriate factor of the gauge coupling.

   The inconsistency becomes explicit after (9): the initial magnetic current is written as $\star dA_1$, whereas the new magnetic current associated with A_2 is written as dA_2. These have different form degrees for general d. Moreover, once BF couplings are present, the naive Maxwell currents and their conservation equations can mix. The authors should list, after each of (7), (8), and (9), the gauge transformations, the surviving global symmetries, their form degrees, their normalized currents, and the Ward or Bianchi identity that proves conservation. They should also explain whether there is a mixed electric/magnetic anomaly and what becomes of it upon gauging. Only then is the recursive step in (10) well defined.

3. **Resolve the relative-sign mismatch between (6), (8)–(10).** For a $(d-2)$-form H and a one-form A, integration by parts on a closed spacetime gives

   $$\int H \wedge dA = \int A \wedge dH.$$

   Thus, the coupling in (9), $+iH_{1,2} \wedge d(A_1 - A_2)$, is $+i(A_1 - A_2) \wedge dH_{1,2}$. It has the opposite orientation to the $i(A_2 - A_1) \wedge dH_{1,2}$ term in (6) and (10). In addition, starting from the

$+iH_{1,2} \wedge dA_1$ term in (8) and adding the $+iA_2 \wedge dH_{1,2}$ coupling described in the prose produces a sum $A_1 + A_2$, not a difference.

A redefinition of a newly introduced field can repair an isolated sign, but the manuscript presently changes conventions without saying so. The revision should choose an orientation for every edge, derive the incidence matrix with that convention, and keep it through (5), (6), and (8)–(10). The gauge transformation of the dual scalar then supplies a simple check of the sign.

4. **Supply the omitted derivation of the electric-gauging chain.** The steps from (12) to (14) and from (17) to (18) contain the nontrivial part of the dual-frame claim, yet they are summarized by "dualizing in the standard way" and "when the dust settles." This is not adequate when the paper's conclusion is that all couplings invert.

   There is also a visible sign problem. Both squares in (13) contain $dA_i + \widetilde{H}_{1,2}$. Dualizing them with the same convention naturally gives BF terms of the same orientation, whereas (14) contains $\widetilde{H}_{1,2} \wedge d(\widetilde{A}_1 - \widetilde{A}_2)$. If a field redefinition or an opposite sign inherited from the C_2 master action is intended, it should already appear in (13). Please give a first-order master action and integrate it explicitly in both orders, including Hodge-star signs in Euclidean signature, all 2π factors, and the boundary terms.

   The form degrees should be displayed alongside the calculation: $\widetilde{H}$ is a two-form, C_i and $\widetilde{A}_i$ are $(d-3)$-forms, and $\widetilde{\theta}$ in (18) is a $(d-4)$-form. The phrase "couplings inverted" also needs a dimensionally meaningful definition and the conventional quantized dual-coupling relation. In $d > 4$, (18) is a higher-form Stueckelberg system, not the conventional scalar-link deconstruction theory; the manuscript should state exactly in what sense it describes the same emergent geometry. The special role of $d = 4$ should be separated from the general-d statement.

5. **Make the quiver topology and boundary conditions consistent.** The prose and Figure 1 introduce a linear chain, but the corner entries in (4) are those of a circular quiver. The footnote says that closing the quiver plays no role, while the Discussion later imposes $A_{N+1} = A_1$ and also emphasizes possible boundary sectors and graph topology. These alternatives have different endpoint matrix entries, spectra, zero modes, winding sectors, and topological limits. They therefore do matter to the claimed exact correspondence.

   The iterative procedure as written naturally grows an open chain by adding a new node. Identifying the last new node with the first is an additional operation, not merely a boundary condition that follows from the recursion. Please specify the ranges of every node and link sum, write the incidence matrix for both the path and cycle graphs, and explain how the cycle is closed in the gauging construction. The same precision is needed for (17), whose unqualified sum and $(i-1, i)$ indexing otherwise appear to introduce a link with index zero.

6. **Demonstrate a controlled extra-dimensional continuum limit.** The graph Laplacian in (4) is evidence for deconstruction, but the manuscript does not state the lattice spacing, circumference, canonically normalized mass spectrum, or scaling of g, v, and N that yields a finite Kaluza–Klein tower. These data are necessary to distinguish an exact finite-quiver duality from the approximate emergence of a continuous extra dimension. For the homogeneous circle, the revision should diagonalize (4), identify the zero mode and the finite-N eigenvalues, and then give the $N \to \infty$ scaling and the resulting higher-dimensional coupling.

   The assertion that arbitrary positive g_i and $v_{i,i+1}$ describe a "generic latticization" also needs qualification. A sequence of arbitrary inhomogeneous weights defines a weighted graph Laplacian, but only suitably scaled, slowly varying families need converge to a local differential operator on a smooth extra dimension. Please give the map to a nonuniform lattice and state sufficient continuum conditions. Likewise, a general graph does not by itself discretize a higher-dimensional geometry; additional locality, spectral, and scaling conditions are required. The broader claims in the Discussion should be presented as conjectural unless such conditions are supplied.

7. **Clarify novelty and the relation to the cited literature.** The duality between a compact Stueckelberg or Abelian-Higgs phase and a higher-form BF description is standard. The manuscript's potentially novel contribution is recognizing the iterative-gauging construction of the manuscript's Ref. 4 as repeated assembly of this familiar quiver structure. The paper should cite the relevant Abelian Higgs/Stueckelberg–BF duality and topological-mass literature, and state clearly which new statement goes beyond it.

   Ref. 4, as described by its title and by the Introduction, concerns emergent topological orders from iterative gauging. The present paper gives a continuum quadratic action but does not map the discrete/global operator algebra, constraints, sectors, or excitations of that construction. A side-by-side dictionary should identify the seed theory, symmetry and gauging operation, Hilbert-space constraints, boundary condition, and observables on both sides. If only the Abelian Gaussian skeleton is shared, the words "is" and "universal mechanism" should be softened accordingly. This comparison is important both for establishing novelty and for deciding whether the conclusion extends beyond the special free Abelian setting.

Minor comments

1. Define $|F|^2$ explicitly, including the Hodge star, factorial convention, and Euclidean-signature convention. This will remove several otherwise ambiguous factors in (1)–(18).

2. State at the beginning whether every $U(1)$ gauge field is compact, what the minimum electric charges are, and how all gauge parameters are periodically identified. If a noncompact treatment is intended, use notation that does not imply compact $U(1)$ data.

3. The map following (5) should include the gauge transformations of $H_{i,i+1}$ and explain how the original link-phase periodicity is encoded. The word "roughly" in the comparison below (10) is incompatible with the paper's exactness claim; replace it by a normalized equation or explicitly label it as a local heuristic.

4. In (5), (6), (10), and (17), place all terms unambiguously inside the sum and state the sum ranges. As typeset, it is not always clear whether the last BF term is summed over links.

5. Explain the gauge fixing used between (2) and (3), count the remaining diagonal $U(1)$ zero mode, and distinguish the open-chain and circle cases.

6. In the electric-gauging subsection, specify the gauge transformation of the two-form $\widetilde{H}_{1,2}$ that makes (11) invariant. Then track that transformation through (12)–(14). This is an efficient independent check of the relative signs.

7. The topological limit below (18) is particularly sensitive to compact normalization and to the

rank and integral cokernel of the graph incidence matrix. Its claimed topology dependence should be reconciled with the earlier assertion that open versus closed quivers plays no role.

8. The manuscript should distinguish a finite weighted quiver, a nonuniform lattice approximation, and a smooth extra-dimensional continuum. At several points these three notions are used interchangeably.

9. Please correct the following wording: "with generic $v_{i,i+1}, g_i$ corresponds" should read "correspond"; "look in closer detail to" should read "look more closely at"; "for latter convenience" should read "for later convenience"; and "a deconstructed the action" should read "a deconstructed action." Also add the missing space in "Ref. [4]" wherever needed and use one term consistently for "Lagrangian" versus "action."

10. The statement after (3) calls (4) a harmonic-oscillator mass matrix. "Graph Laplacian" is more precise, and it makes the subsequent graph generalization transparent.

Conclusion

The manuscript has a clear pedagogical core, and a careful revision could turn it into a useful short note. The minimum requirement is a conventionally consistent local derivation: correct symmetry transformations and currents, fixed BF signs, explicit electric-frame dualization, and well-defined boundary conditions. For the stronger and more interesting claim of an exact equivalence of compact quantum theories, the authors must additionally match flux sectors, large-gauge transformations, operator charges, and topological data. A short but explicit continuum-limit calculation and a sharper comparison with the iterative-gauging literature would then establish both the physical content and the journal-level significance of the result.