Referee Report

Dilatonic states, phase transitions, and criticality in holography

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Summary of the manuscript

This manuscript is a topical review of a program aimed at identifying light composite dilatons in confining quantum field theories by means of gauge-gravity duality. Its central proposal is that a zero-temperature line of first-order transitions ending at a critical point can provide a mechanism for making one scalar bound state parametrically lighter than the confinement scale. The review compares this critical-endpoint mechanism with two related patterns: models in which a scalar becomes parametrically light only on a metastable branch beyond a strong first-order transition, and flux-driven models in which a scalar can be numerically light on the stable branch but the putative transition is approached together with large curvature or a singular limiting geometry.

After a broad introduction to dilaton effective field theory, lattice motivation, and previous holographic work, Section II reviews the holographic dictionary, Wilson-loop confinement, and the Van der Waals analogy for global and local stability. Section III develops a common technical framework: one- and two-circle dimensional reductions, domain-wall and soliton backgrounds, UV expansions, gauge-invariant spin-0, spin-1, and spin-2 fluctuation equations, a probe approximation intended to diagnose a dilatonic state, and holographic free energy. Sections IV and V then survey nine top-down and bottom-up constructions. The presentation emphasizes the comparison between free-energy branches and fluctuation spectra rather than the detailed microscopic interpretation of every model.

The examples fall into a useful three-part taxonomy. In Sections IV.A-IV.C and V.A, a scalar becomes very light on a metastable soliton branch and turns tachyonic farther along that branch, while the known competing saddle is a singular domain wall. In Sections IV.D-IV.E and V.B, circle holonomies and bulk fluxes generate confinement/deconfinement-like phase diagrams and numerically suppressed scalar masses, although some limiting solitons become singular or strongly curved. Finally, Sections IV.F and V.C describe, respectively, top-down and bottom-up systems with a first-order confinement/confinement line ending at a critical point. A scalar eigenvalue vanishes at that endpoint and the corresponding mode is not reproduced by the probe approximation. The manuscript interprets this as an exactly massless dilaton and regards the nearby hierarchy as a general stabilization mechanism.

The comparative organization is valuable. In particular, placing the free-energy analysis, the tachyonic branches, and the probe spectra side by side makes clear which light states occur on physically realized branches and which occur only after a first-order transition. The review also assembles a large and relevant bibliography and brings together results that are otherwise distributed across many papers. Its scientific value is therefore primarily the synthesis and proposed physical interpretation, rather than a new numerical calculation.

Assessment of correctness, novelty, and significance

The background-field and gauge-invariant fluctuation strategy is well suited to the problem, and the broad logical progression from global stability to local spectra is convincing. Within the retained classical supergravity sectors, the evidence for a scalar gap closing at the two critical endpoints is compelling. The distinction between stable, metastable, and tachyonic branches is one of the strongest aspects of the article, and the top-down critical example is sufficiently unusual to merit a clear review treatment.

The main physical interpretation is not yet established at the same level. Failure of the probe approximation demonstrates that scalar-metric mixing is important; it does not, by itself, prove that the state is the pseudo-Nambu-Goldstone boson of spontaneously broken approximate scale invariance. A generic order-parameter mode becomes massless at a second-order endpoint, and in the top-down example the response variable itself is a component of the stress tensor. The manuscript must therefore distinguish a generic critical scalar, a scalar with overlap onto the stress-tensor trace, and a genuine dilaton satisfying the relevant Ward identities. This distinction affects the main conclusion, the wording of Sections IV.F, V.C, and VI, and the classifications in Table I.

There are also several definite technical errors in the pedagogical formalism, including the two-torus analytic solution and the general probe equation, together with inconsistent dimensions and numerical values in the critical example. These are not merely stylistic defects because Section III is presented as the common basis for all later calculations.

As a review, the work is timely, interesting, and potentially useful to the holography and composite-dynamics communities. It is suitable for publication as a topical or invited review once the central interpretation is made more precise and the formal errors are corrected. If assessed as an ordinary research article, its novelty would be limited because the figures and model results are drawn from prior publications; the new contribution is the taxonomy and synthesis.

Recommendation

Major revision. I would support publication after the authors correct the displayed formalism, separate controlled results from conclusions that rely on singular saddles, and either provide stronger evidence for the dilaton assignment or consistently describe the light state as a dilaton-like critical scalar. The requested changes are substantial, but they would turn the manuscript into a strong and reliable reference.

Major comments

1. **A probe-spectrum mismatch is not a sufficient definition of a dilaton.**

   Section III.D compares the physical gauge-invariant scalar problem in equations (55)-(56) with the probe problem in equations (59)-(60), where mixing with the scalar metric fluctuation is suppressed. A large change in an eigenvalue is evidence that gravitational mixing cannot be neglected and that the state has overlap with the trace sector. This is a useful diagnostic, but it is only a necessary ingredient of a dilaton interpretation. A generic singlet scalar can mix with the metric, and a critical order-parameter mode will generally affect stress-tensor correlators.

This point is especially important in Section IV.F. The massless mode occurs at a conventional critical endpoint, while the plotted response function is $\langle T_{22}\rangle$. Gap closing and a divergent correlation length are generic there, without implying spontaneous breaking of scale invariance. Hence the statements that this mode is "undoubtedly" a dilaton, that the bottom-up result is "clear evidence," and that the mechanism has been "proven explicitly" are stronger than what the probe comparison establishes.

The authors should formulate and apply a uniform physical criterion. Ideally the review should quote or provide the pole residue in the properly renormalized $\langle T^{\mu}{}_{\mu} T^{\nu}{}_{\nu}\rangle$ correlator, a normalized matrix element defining a dilaton decay constant, and an appropriate trace Ward identity or PCDC-type relation. The discussion should separate explicit breaking by sources and compactification from spontaneous breaking by the vacuum. It would also be useful to state whether the light eigenmode dominates the trace correlator and whether its low-energy couplings have the dilaton form. If these quantities are not available, the accurate conclusion is that the state is a *dilaton candidate* or a *dilaton-like critical scalar with substantial trace-sector mixing*. The same standard should be used in Sections IV.A-IV.E, V.A-V.C, Section VI, the figure captions, and Table I.

The motivation in Section I should be brought into line with this careful definition. In the minimal Standard Model the Higgs excitation is not the PNGB of spontaneously broken dilatations: the Higgs mass term explicitly breaks scale invariance, and Higgs couplings follow from gauge symmetry, Yukawa interactions, and the electroweak vacuum expectation value. A composite-dilaton interpretation of the observed scalar is an interesting beyond-the-Standard-Model hypothesis, not an identity. The passages that call the observed Higgs a dilaton should be rewritten.

2. **The critical hierarchy, critical exponents, and meaning of "stabilization" should be quantified.**

   Figures 8 and 12 show a scalar eigenvalue reaching zero at a single point, but the review does not display how the hierarchy develops as that point is approached. To support the term "parametric," the authors should summarize the behavior of

$$\frac{m_0}{m_{\text{gap}}} \quad \text{or} \quad \frac{m_0}{\Lambda}$$

   as a function of distance from the critical surface, give the fitted critical exponent or expected universality class, and show that the reference gap, string tension, and heavier states remain finite. The associated susceptibility or nonanalytic derivative of the free energy should be related explicitly to the massless pole. Numerical uncertainties, cutoff convergence, and the direction of approach through parameter space should be stated at least in summary form.

   There is also a conceptual distinction between a tunable hierarchy and a dynamically selected or technically natural hierarchy. Reaching an arbitrarily small mass by tuning one or more control parameters close to a critical surface is an important result, but it does not by itself explain why a microscopic theory lies close to that surface. The implications for the Higgs hierarchy problem should therefore distinguish protection of a small eigenvalue from the tuning required to reach criticality.

   The approximation regime deserves explicit discussion here. Both critical examples describe confining theories in fewer than four uncompactified dimensions, and the calculations are performed at large N in a classical gravity limit. The review should explain what is known about finite-N critical fluctuations, whether the relevant higher-dimensional curvature and coupling remain controlled, and whether flux quantization leaves the apparently continuous tuning available. These qualifications are necessary before generalizing the two examples to a universal four-dimensional mechanism.

3. **Several equations in Section III require correction or derivation.**

First, the analytic two-circle soliton in equations (41) and (42) contains

$$\log\sinh\left(\frac{D(\rho-\rho_o)}{2}\right).$$

As printed, the large-ρ slopes are

$$\chi' = \frac{3}{2(D-2)}, \qquad A' = \frac{2D-1}{2(D-2)},$$

whereas equation (32), with $\hat{A}' = 1$, requires

$$\chi' = \frac{2}{D-2}, \qquad A' = \frac{D}{D-2}.$$

The constants in the stated IR expansion are also not recovered. Replacing D by $D+1$ in both hyperbolic-sine arguments appears to restore both limits. The authors should correct the expressions and verify them against equations (26)-(27).

Second, the paragraph after equation (45) assigns the symbol Δ_J both to the source falloff and to the operator dimension. In standard quantization, with d the boundary dimension,

$$\Delta_\pm = \frac{d}{2} \pm \sqrt{\frac{d^2}{4} + m^2}, \qquad \Delta_{\text{op}} = \Delta_+, \qquad \Delta_J = d - \Delta_{\text{op}} = \Delta_-.$$

In the notation of equation (45), the plus-root exponent should be Δ_V, not a second Δ_J. The Breitenlohner-Freedman bound is a stability bound, and the alternative-quantization window and its endpoints should be stated with a single unambiguous boundary-dimension symbol.

Third, equation (59) is not the general fixed-metric or probe limit of equation (55). Removing the terms generated by metric mixing should leave the sigma-model curvature contribution:

$$\left[\mathcal{D}_r^2 + (D-1)A'\mathcal{D}_r - e^{-2A}q^2\right]\mathfrak{p}^a - \left[V^a{}_{|c} - \mathcal{R}^a{}_{bcd}\Phi'^b\Phi'^d\right]\mathfrak{p}^c = 0.$$

The printed equation omits the Riemann term. This is immaterial only for a flat target space or a special background contraction, neither of which is true generically for the multi-scalar reductions under review.

Fourth, equation (57) contains the covariant radial combination $\partial_r\mathcal{A}_\mu^B - Z^B{}_{Ca}\Phi'^a\mathcal{A}_\mu^C$, whereas equation (58) imposes the plain condition $\partial_r\mathcal{A}_\mu^A = 0$. Variation of equation (14) appears instead to give the corresponding H_{AB}-weighted covariant Neumann condition. The authors should derive equation (58), identify the field basis or limiting assumption that reduces it to the printed form, or correct it. This matters in the flux models for which $Z^A{}_{Ba}$ is nonzero.

Finally, the boundary condition (56) contains $1/q^2$, precisely while the critical discussion relies on an exact $q^2 = 0$ mode. The limiting boundary-value problem used to establish that zero mode should be stated.

4. **The holographic dictionary, dimensions, and calculated sector must be stated accurately.**

   Section II presents equation (1) as applicable to a generic "asymptotically safe" field theory and says that the equality is valid in general. The statement must instead be restricted to QFTs that admit a holographic dual. A weakly coupled classical gravity saddle captures an additional large-N, strong-coupling regime in which quantum-gravity and string or M-theory corrections are suppressed. Asymptotically AdS backgrounds ordinarily encode a UV conformal fixed point and its deformations, not a generic asymptotically safe theory. These qualifications are part of the correctness of the review, particularly because several examples approach large curvature.

   The dimensional bookkeeping also needs revision. The coordinate ranges in Section III.A are off by one if indices start at zero. A reduced D-dimensional spacetime has $M = 0, \ldots, D-1$, with boundary indices $\mu = 0, \ldots, D-2$ and radial coordinate $M = D-1$. The paragraph following equation (30) calls the vacuum $\mathrm{AdS}_{\hat{D}-1}$, although equation (30) is a $\hat{D}$-dimensional metric; this should be $\mathrm{AdS}_{\hat{D}}$.

   In Section IV.F, the parent theory is not a four-dimensional boundary QFT in the sense stated there. The calculation uses a four-dimensional gravity truncation with an eleven-dimensional uplift, followed by circle reduction, and the confining field theory is lower dimensional. Table I compounds the ambiguity by listing the uplift dimension $D = 11$ for this row while using truncation dimensions in other rows. The review should consistently list the parent/uplift dimension, effective bulk dimension, compactification, and uncompactified boundary-QFT dimension.

   Relatedly, the spectra displayed are spectra of the retained bosonic, circle-zero-mode sector of particular truncations. A consistent truncation does not establish that omitted supergravity fields, Kaluza-Klein harmonics, fermions, or string/M-theory modes are heavier. Section III.C explicitly discards the circle KK towers without demonstrating scale separation. Statements such as "the lightest state in the theory" and claims of complete local stability should either be supported by the larger fluctuation spectrum or qualified as applying to the calculated sector. The separation from KK modes is particularly relevant near shrinking circles and in assessing a phenomenologically useful hierarchy.

5. **Transitions involving singular saddles should not be assigned the same status as transitions between controlled regular solutions.**

   In Sections IV.A-IV.C and V.A, a regular soliton is compared with a singular domain wall, in some cases one that fails the Gubser criterion. A lower renormalized on-shell action for such a configuration orders the known classical solutions; it does not establish that the singular saddle is an admissible QFT phase or that the true ground state has been found. The location and even the nature of the transition can change if a regular completion or an additional branch exists. The locally stable continuation of the soliton is a meaningful result, but calling it definitively "metastable" presumes knowledge of a physical lower-energy phase.

   There are direct internal contradictions in the flux examples. Section IV.D states that the limiting soliton is singular on the square boundary and then describes the transition as occurring between two regular backgrounds. Section IV.E places coexistence at another singular soliton limit and alternates between saying that two states become parametrically light and that their suppression is only numerical. The authors should give a controlled-curvature window, show how the scalar masses scale before that window is lost, and classify the endpoints as limiting or candidate transitions if no controlled saddle exists.

The discussion of scheme dependence should also be sharpened. Finite local counterterms can shift a one-point function and analytic contact terms, but a common counterterm evaluated at the same sources cancels in free-energy differences between branches. Branch crossings, discontinuities between branches, and divergent nonanalytic susceptibilities should be separated from scheme-dependent analytic pieces. This would make the use of the Van der Waals analogy more precise.

6. **The review should provide a more uniform comparative synthesis.**

   Because the individual numerical results have appeared previously, the main novelty of this article is the comparison among them. Table I is a good starting point, but it currently combines unlike notions of dimension and does not encode the qualifications that control the conclusion. A revised table should give, for each model:

   - the gravity truncation and uplift dimensions and the boundary-QFT dimension;
   - the control parameter and dual operator;
   - the evidence for confinement;
   - whether the competing saddles and the transition limit are regular;
   - whether the light state occurs on a stable, metastable, or uncontrolled branch;
   - the smallest quoted mass ratio and its reference scale;
   - whether suppression is numerical or parametric; and
   - the evidence supporting the dilaton assignment.

   The conclusion should then state separately what has been established by regular top-down solutions, what is suggested by bottom-up models, and what depends on singular limits. The broad literature already cited in Section I on dilaton EFT, fixed-point merger, walking dynamics, and lattice light scalars should be used to compare physical criteria, not only to establish historical context. In particular, the review should explain how a critical-endpoint scalar differs from the dilaton candidates in these other mechanisms.

Minor and specific comments

1. Section IV.F and Figures 7-8 use inconsistent values of the critical parameter. The consistent value is $b_0^{\mathrm{CP}} \simeq 0.6815$, not 0.9815, and the representative value in the Figure 8 caption should be $b_0 = 0.6836$, not 6836.

2. In Section IV.F and the Figure 8 discussion, the plotted free-energy normalization is $\mathcal{F}\ell^3\alpha^{-1}$, as shown in the figure, not $\mathcal{F}\ell\alpha^{-1}$.

3. In Section IV.A, the transition point corresponds to $\phi_I = \phi_I^c \simeq 0.039$. The inequality $\phi_I < \phi_I^c$ describes one side of the transition and is not "equivalent" to the quoted critical source.

4. In Section V.B, the soliton is favored for small $|\mathcal{A}_6^U|$, not for small $\mathcal{A}_6^U(\max)$, which is a fixed endpoint.

5. Section V.A states generally that the stable-branch scalar has no clear dilaton signature, whereas Section IV.C assigns dilaton character to the stable D5 branch. This should be made model dependent rather than presented as a universal feature of the first three examples.

6. The statement in Section II.B that a second-order transition has a discontinuous second free-energy derivative is too restrictive and is inconsistent with the later claimed divergence. “Nonanalytic” is the general statement; the relevant susceptibility and critical behavior should be specified model by model.

7. The Wilson-loop discussion should state that the trace of the closed path-ordered holonomy is gauge invariant. Path ordering alone does not ensure gauge invariance.

8. A negative extremum of the potential, rather than specifically a local maximum, is what gives an AdS vacuum. Stability and admissible scalar masses then require the BF condition.

9. The quantity plotted as $M^2/|M|$ should be defined explicitly as a signed square root when $M^2 < 0$, so that the treatment of tachyonic points is unambiguous.

10. The Table I preamble declares six columns, while the header and every row contain five entries, leaving a spurious empty column. This should be corrected when the table is expanded.

11. The bottom-up cigar geometries are explicitly acknowledged in Section II.A not to possess a top-down Wilson-loop probe. Later statements should retain the distinction between Wilson-loop confinement and the pragmatic “regular cigar plus mass gap” criterion.

12. The duplicate occurrence of the citation key corresponding to Cata et al. in the long dilaton-EFT citation list should be removed. A careful technical proofread is also needed for visible errors such as “parameter spce,” “metastable brunch,” “first-oder,” and “one one-parameter.”

Final evaluation

The manuscript has the ingredients of a valuable review: a coherent technical framework, a broad survey of examples, and an interesting attempt to isolate the role of weak zero-temperature transitions in producing light scalars. The massless mode in the regular top-down critical construction is an important result to highlight. The present draft, however, conflates a critical scalar with a PNGB dilaton, treats several singular-saddle phase diagrams too definitively, and contains errors in equations intended to make the treatment self-contained. Addressing the major comments above would substantially strengthen both the correctness of the article and the significance of its synthesis.