Referee Report

Non-relativistic Floquet Conformal Field Theory

Synopsis

The manuscript studies periodically driven quantum systems with exact non-relativistic conformal symmetry. Its essential input is the $SO(2,1)$ algebra generated by the untrapped Hamiltonian H, the special conformal generator C, and the dilatation D. The authors first consider a two-step protocol alternating between $H + \mu_1^2 C$ and $H + \mu_2^2 C$. In the two-dimensional representation of the algebra, the one-cycle propagator is an $SL(2, \mathbb{R})$ matrix. Its half-trace

$$\mathcal{T} = \tfrac{1}{2}\,\mathrm{Tr}\,U_T$$

is used to divide parameter space into elliptic, hyperbolic, and boundary regimes. Equations (5)–(9) give the effective Floquet generator and the matrix entries after ℓ cycles. Conjugating C, H, D with this matrix then gives their stroboscopic expectation values in a trapped primary state. In particular, equation (17) predicts bounded oscillations of the cloud radius in an elliptic regime and exponential growth in a hyperbolic regime.

The manuscript supplements this exact square-pulse analysis in several directions. Equations (11)–(13), developed further in supplemental equations (74)–(83), relate a general time-dependent $SO(2,1)$ Hamiltonian to an Ermakov–Pinney scaling problem and a time-independent oscillator. Equations (19)–(20) give the return fidelity. Supplemental equations (33)–(47) treat primary-field two-time functions and autocorrelators of conformal generators. A continuously modulated trap is analyzed by a Trotterized monodromy calculation in supplemental equations (51)–(57), with a proposed high-frequency Magnus approximation in equations (58)–(61). The resonant protocol of a recent unitary-Fermi-gas experiment is mapped to a hyperbolic instability tongue in supplemental equations (67)–(73). Finally, a linear combination of Schrödinger Killing vectors is studied in a fixed bulk geometry. The hyperbolic class is associated with a timelike stationary-limit surface, while a subset of the parabolic class is associated with a zero-surface-gravity Killing horizon.

Assessment, novelty, and recommendation

The paper contains a useful and potentially publishable idea. The direct matrix multiplication behind equations (5)–(9) is sound, and combining it with equation (15) does yield the radius formula (17). This gives a compact, representation-independent explanation of stable and unstable breathing dynamics in an ideal scale-invariant trap. The resonant-monodromy numbers in supplemental equations (71)–(73) are also internally consistent. The work therefore makes a plausible connection between Floquet conjugacy classes, exact scaling dynamics, and cold-atom observables.

The conceptual ingredients themselves are largely established. Exact scaling evolution and the Ermakov–Pinney/Lewis construction are represented in the cited literature, notably Refs. [79] and [96]–[100], while the authors' earlier Refs. [75]–[76] develop related Floquet-conformal classifications. The novelty here lies in transporting that structure to non-relativistic CFTs, identifying experimentally accessible radius diagnostics, producing a continuous-drive phase map, and proposing a Schrödinger-holographic interpretation. That synthesis could be significant, particularly if the experimentally distinct elliptic response is made quantitatively testable.

In its present form, however, the manuscript is not reliable enough for publication. The displayed Magnus expansion has incorrect normalization, Hermiticity, and dimensions; the trace-boundary classification conflates nontrivial parabolic matrices with the central matrices $\pm I$; several operator-transformation signs disagree; and explicit expectation-value and autocorrelation formulas contain algebraic errors. These are not merely typographical because they affect the claimed high-frequency boundary, correlator behavior, and holographic interpretation. The reproducibility material also does not presently generate the phase scan described in the supplement.

I therefore recommend **major revision before reconsideration**. A corrected full-length treatment could be suitable for a venue such as Physical Review D. Suitability for JHEP would require a sharper separation between the established scaling mechanics and the new result, together with a substantially firmer holographic statement. The following issues should be resolved before the conclusions can be assessed.

Major comments

1. **The boundary $|\mathcal{T}| = 1$ is not uniformly a parabolic phase.**

   Equations (18), (57), and (63)–(66) classify every matrix with $|\mathcal{T}| = 1$ as parabolic and identify the boundary with $\rho = 0$. For $SL(2,\mathbb{R})$ this is incomplete. A matrix with trace ± 2 can be a non-diagonalizable parabolic element, but it can also be the central element $+I$ or $-I$. Only the former has a nonzero nilpotent part and hence polynomial growth under repeated application. For $U_T = \pm I$, all stroboscopic observables simply recur.

   The manuscript supplies a concrete counterexample to its own classification. At $\beta = 0$ in supplemental equation (70), $U(\pi) = -I$. The text following that equation and Table 1 call this a parabolic point, although $U(\pi)^\ell = (-1)^\ell I$ and there is no linear or quadratic growth. It is the degenerate tip of an instability tongue, not a nontrivial parabolic monodromy. Similarly, $\mathcal{T} = -1$ corresponds to $\rho = \pi$ modulo 2π, not to $\rho = 0$. The factors $\rho/\sin\rho$ in equations (6)–(9) also require an explicit logarithm branch and separate limits at both signs of the trace.

   The authors should classify the Jordan form at $\mathcal{T} = \pm 1$, distinguish $\pm I$ from nontrivial parabolic elements, give the correct limiting propagators, and revise every assertion of universal power-law boundary growth. This distinction must also be carried into the fidelity, experimental, and holographic discussions.

2. **Several advertised expectation values and autocorrelators are algebraically inconsistent.**

   Equation (15), together with equation (3), immediately gives

   $$ {}_{\mu_1}\langle \Delta | H(\ell) | \Delta \rangle_{\mu_1} = \frac{\Delta}{2} \left(\mu_1 d_\ell^2 + \frac{c_\ell^2}{\mu_1} \right). $$

   The second line of equation (16) instead prints $\frac{\Delta}{2}(\mu_1 c_\ell^2 + d_\ell^2/\mu_1)$. The latter does not follow from equation (15) and is dimensionally inconsistent with the matrix entries. Although the central radius result uses the first line of equation (16), this error undermines the claimed simultaneous treatment of all three generators.

   There is a related error in supplemental equation (46). From equations (42)–(44), the fourth auxiliary correlator should be

   $$ \mathcal{F}_4 = \frac{\Delta}{4} \left[(\Delta - 1) d_\ell^2 + \frac{(\Delta + 1) c_\ell^2}{\mu_1^2} - \frac{2i}{\mu_1} c_\ell d_\ell \right], $$

   whereas the manuscript inserts an extra factor μ_1^2 in the first term. That error propagates to the energy autocorrelator (47). Moreover, equation (45) has a_ℓ, b_ℓ growing exponentially in the hyperbolic class, so $|\mathcal{A}_C|$ generically grows rather than decays. This agrees with the large upward range displayed in supplemental Fig. 2(b) and with the statement before equation (41), but contradicts the claim after equation (47) of “exponential decay” and “linear decay.”

All one- and two-point formulas should be rederived from a single convention, checked at $\ell = 0$, checked dimensionally, and compared with the plotted data. The revised paper should state whether each plotted correlator is raw, normalized, connected, inverted, or logarithmic.

3. **The Magnus analysis in supplemental equations (58)–(61) is not a valid Floquet-Magnus expansion.**

   With $U_T = \mathcal{P}\exp[-i\int_0^T dt\,\mathcal{H}(t)] = \exp(-iH_F T)$ and $\hbar = 1$, the second and third terms have the standard structure

$$H_F^{(2)} = -\frac{i}{2T}\int_0^T dt_1 \int_0^{t_1} dt_2, [\mathcal{H}_1, \mathcal{H}_2],$$
$$H_F^{(3)} = -\frac{1}{6T}\int_0^T dt_1 \int_0^{t_1} dt_2 \int_0^{t_2} dt_3([\mathcal{H}_1,[\mathcal{H}_2,\mathcal{H}_3]] + [\mathcal{H}_3,[\mathcal{H}_2,\mathcal{H}_1]]),$$

   up to an explicitly stated alternative convention. Equation (58) instead uses $1/(2T^2)$ and $1/(6T^3)$ and omits the factor required to make the second term Hermitian. Its terms therefore do not have common dimensions.

   Equation (59) then introduces an undefined μ_0, and the quoted k_1, k_2 lack the time or drive-frequency dependence needed to repair the dimensions. Consequently equations (60)–(61) and the claimed high-frequency threshold do not follow. There is also a logical error in stating that $\mu_2^c > \mu_1$ implies $N_m^2 > 0$ whenever $\mu_2 > \mu_1$; the implication would require $\mu_2 > \mu_2^c$.

   The authors should redo the Magnus calculation from the logarithm of the time-ordered propagator, state the Floquet gauge/time origin, give its dimensionless expansion parameter, and benchmark the truncated result against the exact monodromy over a range where the approximation is controlled. Until then, the analytic interpretation of Fig. 4 should be removed.

4. **The primary-operator transformation and the driven Green function use mutually inconsistent signs.**

   Immediately after main equation (1), a primary at the origin is defined by $[D, \mathcal{O}] = +i\Delta\mathcal{O}$. Supplemental equation (4) instead gives $[D, \mathcal{O}] = -i\Delta\mathcal{O}$ at the origin. These two choices generate opposite finite scale weights. Likewise, main equation (10) and supplemental equation (32) contain a negative particle-number phase, while supplemental equation (34), stated for the same $U^{\dagger\ell}\mathcal{O}U^{\ell}$ transformation, contains a positive phase.

   There is also a direct calculus error in supplemental equation (28): the chain rule is $dv(t(\sigma))/d\sigma = v'(t)\,dt/d\sigma$, without the additional minus sign printed there. With the printed equation, the conservation law in equation (27) does not follow. Since equations (39)–(40) use these transformations, their phases and branch factors cannot be accepted without a fresh derivation.

   The revision should fix one convention for U, U^{-1}, the charge of $\mathcal{O}^\dagger$, and all algebra generators; verify the result for pure time translations and pure dilatations; and specify the $i0$ prescription and complex-power branches in equations (33), (39), and (40). The assumed vacuum invariance should also explicitly include $H|0\rangle = 0$ if it is needed in equation (36).

5. **A return amplitude is not, by itself, a scrambling diagnostic.**

   Equations (19)–(20) compute a Loschmidt-type fidelity of a single primary state. Its exponential decay in an unstable representation is interesting, but it does not establish many-body information scrambling. No out-of-time- order correlator, operator-size distribution, entanglement diagnostic, or separation of dephasing from scrambling is provided.

   The stated critical asymptotics are also inconsistent with equation (20). For a nontrivial parabolic matrix the entries grow linearly in ℓ, hence $\Lambda_0 \sim \log \ell$ and $\mathcal{F} \sim \ell^{-\Delta}$; for $U_T = \pm I$, the fidelity is constant. Thus the statement that "scrambling turns out to be linear in ℓ" does not follow from the displayed result. The authors should either describe $\mathcal{F}$ strictly as a return fidelity and state its correct asymptotics, or add an accepted scrambling observable and demonstrate the claimed connection.

6. **The continuous-drive computation is not reproducible from the linked material as described.**

   The supplemental text states that Fig. 4 scans $0 \leq \mu_2 \leq 2$ and $0.1 \leq \omega_D \leq 6$ on a 401×591 grid, and it gives a convergence table relative to $N_0 = 2048$. The repository linked in the Data and code availability statement, `https://github.com/diptarkadas/cosine-drive-response`, presently sets the phase-diagram ranges to $0 \leq \mu_2 \leq 1$ with 241 points and $0.1 \leq \omega_D \leq 3$ with 361 points. Its default output is a different, smaller phase diagram, and it does not provide the script or stored values underlying the convergence table in the supplement.

   The exact code version used for every manuscript figure should be archived and tagged. It should reproduce the stated grids, captions, representative points, $\max|\mathcal{T}|$, heating fraction, and convergence table with one command. Convergence only relative to the same first-order endpoint scheme at $N_0 = 2048$ is not an external accuracy check. The phase boundaries should also be compared with an adaptive ODE solver or a higher-order structure-preserving integrator, with errors reported for $\mathcal{T}$ and for boundary locations rather than only the grid classification.

7. **The holographic conclusion is both kinematic and conditional.**

   Supplemental equations (84)–(92) compute the norm of a chosen Killing vector in an undeformed Schrödinger background. This is a useful geometric classification, but it is not a bulk solution dual to the driven state, and it does not establish an ergoregion produced by Floquet energy absorption. The manuscript acknowledges the absence of backreaction in the body, yet the abstract and conclusion use substantially stronger language.

   Even the kinematic parabolic statement needs a qualification. Equation (93) divides by α_- and assumes a finite t_0. The generator H itself, with $\alpha_+ = 1$ and $\alpha_- = \gamma = 0$, is parabolic, but equation (87) then has no finite norm-zero locus in this coordinate patch. Thus a parabolic algebra element does not automatically yield the extremal horizon constructed in equations (93)–(96). The central monodromies $\pm I$ pose an additional problem because their logarithm, and hence the associated Killing generator, is not unique.

   The authors should state the necessary conditions for the horizon to exist, analyze patch and global issues, distinguish a stationary-limit surface from a dynamically generated ergoregion, and temper the holographic claims unless a backreacted construction is supplied.

8. **The physical domain of the claimed universality needs to be quantified.**

   The exact result assumes an anomaly-free $SO(2,1)$ representation, zero-range scale invariance, isotropic harmonic confinement, and ideal unitary evolution. Real trapped gases have detuning, finite effective range, anisotropy, losses, and possible scale-anomaly effects. The brief statement that the formulas hold for a finite number of cycles near unitarity does not provide an error estimate or an experimentally usable criterion. The authors should identify the leading symmetry-breaking operators and compare their timescales with the Floquet exponent and proposed observation window.

   In addition, equation (24) shows that C measures a second spatial moment. Growth of $\langle C \rangle$ does not imply, as the End Matter states, that the number density becomes large at a system boundary; an expanding cloud normally dilutes, and an ideal harmonic trap has no hard boundary. Equation (26) should also clarify whether A_0 is a dimensionless bracket amplitude, the physical oscillation amplitude including $\Delta/(4\mu_1^3)$, or a peak-to-peak value. These points are essential for turning the otherwise interesting group-theory result into a falsifiable experimental prediction.

Minor comments

1. The manuscript alternates among H_F, $\mathcal{H}_F$, Δ, and Δ_0 without consistently distinguishing the operator, the lowest primary dimension, and a general primary dimension. Please standardize this notation.

2. Equation (4) redundantly writes $\mathcal{H}_1 = H + \mu_1^2 C \equiv \mathcal{H}_1$, and the two time intervals share an endpoint. A half-open convention would avoid ambiguity at the pulse switch.

3. The left caption of main Fig. 1 says $T_2 = T - T_1$ while also saying that T_1 and T_2 are varied. State clearly whether the total period is fixed and label both axes and color thresholds explicitly.

4. Supplemental equation (43) is missing closing parentheses in the printed formulas for H and C, and it cites main equation (3) although the $L_0, L_\pm$ definitions are in main equation (2).

5. In supplemental equation (55), the smooth limit of $\sinh(s\delta t)/s$ is taken as $s \to 0$, not as $\delta t \to 0$. The missing parenthesis in equation (58) and the double comma in its summation should also be corrected.

6. The procedure after supplemental equation (50) says singular points are "omitted or regularized" but gives neither the threshold nor the regularization. These choices should be specified because the elliptic correlator deliberately contains repeated singularities.

7. In the discussion of equation (88), the quadratic is in r^2. It has one positive and one negative root for r^2 under the stated conditions, not simply one positive and one negative physical root for r. The role of the deformation parameter β should also be retained consistently between the main-text and supplemental norm formulas.

8. A thorough copyedit is needed. Recurrent examples include "alegbra," "ellptic," "opscillatory," "an universal," "a systems," "similar ot," and subject–verb disagreements in several figure captions.

Conclusion

The central observation that $SO(2,1)$ monodromy controls exact radius dynamics in an ideal non-relativistic CFT is worthwhile, and the connection to a tunable cold-atom protocol could become important. At present, however, the paper combines this correct core with several incorrect or mutually inconsistent extensions. Reclassification of the trace boundary, correction of the operator algebra, replacement of the Magnus calculation, exact reproducibility, and a more conditional holographic interpretation are all required before the work is suitable for publication.