Referee Report

"Krylov Complexity for Time-Dependent Hamiltonians"

arXiv:2607.10454

Recommendation: Reject in the present form, with encouragement to resubmit after substantial reconstruction.

The paper contains several sound and potentially useful calculations, especially the dressed-frame treatment of the driven two-level system and the symplectic formulation of the one-mode oscillator. However, its central claim is broader than what is presently defined or demonstrated. In the non-periodic part, the manuscript computes a fixed Fock-basis spread after propagating the state with a Magnus integrator, but it does not yet establish in what precise sense this is a Krylov complexity associated with a time-dependent generator. The claimed error control is also transferred from a bounded Hamiltonian to an unbounded oscillator without a valid norm argument, and the numerical evidence does not contain enough information or benchmarking to support the advertised reliability. Finally, the present examples do not reveal physical information beyond familiar transition probabilities and Bogoliubov particle production. Addressing these points would require new analysis and probably a more substantive application, rather than a routine revision of the existing presentation.

Summary and overall assessment

The manuscript studies state spread complexity for explicitly time-dependent quantum systems in two settings. Sections 2–4 use Floquet theory to replace periodic evolution by a stroboscopic Floquet Hamiltonian. For a general two-level Floquet Hamiltonian, the paper obtains equation (33), and then applies it to a circularly polarized drive and to a linearly polarized drive. The latter is treated near resonance using the rotating-wave approximation and at high frequency using a periodic dressing transformation. The time average in equation (48) produces a Bessel-renormalized longitudinal term, while the next Magnus contribution is written in terms of a Struve function in equation (51). Equations (56) and (57) translate this effective Hamiltonian into a stroboscopic spread complexity.

Section 5 turns to a harmonic oscillator with time-dependent frequency. The quantum evolution is represented by a two-dimensional symplectic propagator, from which the Bogoliubov coefficient $\nu(t)$ is extracted. With the even Fock states ordered as $|K_n\rangle = |2n\rangle$, equation (79) gives $C_K = |\nu|^2/2$. The sudden-quench result in equation (89) provides a simple exact check. For a smooth frequency ramp, the interval is divided into steps and the first two local Magnus terms yield the effective matrix and Hamiltonian in equations (97)–(100). The ordered product (101) is then used to generate the curves in Figures 2–4.

Several parts of this chain are convincing. The general two-level expression (33), the circular-drive reduction, the symplectic-to-Bogoliubov formulas, and the sudden-quench expression (89) are mutually consistent. The periodic dressing used in equation (47) is also a sensible way to organize the strong-drive/high-frequency problem, and the leading Bessel average is standard. Appendix B gives a plausible derivation of the Struve-function term in equation (51). The paper is clearly motivated and could become a useful worked account of how structure-preserving Magnus propagation interfaces with state spread.

At present, however, the strongest conclusions do not follow from these calculations. The manuscript moves among three different notions: a Lanczos basis generated by a fixed Hamiltonian, a Floquet basis generated by a branch-dependent logarithm, and a fixed even-Fock basis used after a sequence of different local generators. Their relationship is not established. Moreover, the soft-quench example is not a quantitative validation: the discretization, truncation order, reference oscillator, and numerical tolerances are not reported, and no directly integrated solution or error curve is shown. Since piecewise Magnus methods are already standard numerical tools, both the precise definition and the comparative evidence are essential to the paper's novelty and publication case.

Major comments

1. **The time-dependent Krylov quantity must be defined unambiguously.**

   Appendix A defines a Krylov basis by applying Lanczos recursion to one time-independent generator. This definition works directly for a chosen Floquet Hamiltonian, but the non-periodic construction in Section 5.3 does something different: equations (90)–(101) approximate the state by composing several local symplectic evolutions, after which the state is expanded in the fixed even-Fock ordering introduced in equation (76). No single generator is used to produce that basis. Parity preservation by itself only shows that the state remains in the even sector; it does not prove that an arbitrary ordering of that sector is the Lanczos basis relevant to the stated definition.

   For a one-mode quadratic Hamiltonian there is a useful statement available: when the pair-creation coefficient is nonzero, Lanczos recursion from the reference vacuum produces $|2n\rangle$ up to phases. The authors should prove this for the most general local Hamiltonian appearing in equation (100), including the $xp + px$ term, and explain whether the phases and possible vanishing of the pair term matter as the interval changes. They should then state exactly which generator defines the basis. If the intended observable is instead the fixed-reference Fock spread, equivalently one half of the reference occupation number, it should be named and interpreted as such rather than identified without qualification with the static Lanczos construction.

   The dependence on the reference frequency ω_r is also physical for this definition. Section 5.2 sets $\omega_r = \omega_0$ for the sudden quench, but Section 5.4 does not state the reference used for Figures 2–4. This choice must be explicit, and the paper should discuss how C_K changes under a different reference oscillator. In particular, the nearly constant adiabatic result is a statement about spread relative to the initial oscillator basis, not a basis-independent amount of nonadiabatic complexity.

   There is a related Floquet issue. A Floquet Hamiltonian is defined only after choosing a time origin and quasienergy branches. In a general multilevel system, changing individual quasienergy branches is not merely adding an identity operator and can change the Lanczos basis even though the one-period unitary is unchanged. The concrete two-level probability in equation (33) may avoid this problem, but the general statements in Sections 2–3 require either a branch/gauge-fixed definition or a proof of invariance for the class being claimed.

2. **The convergence criterion used for the oscillator is not presently applicable.**

Equations (58)–(60) use an operator-norm bound for a bounded Hamiltonian. The oscillator Hamiltonian in equation (61) is unbounded, so $h_{\max}$ is infinite on the full Hilbert space and the step-size assertion below equation (90) does not follow from equation (59). Later formulas actually propagate the finite-dimensional classical generator $M(t)$, not the Hilbert-space Hamiltonian $H(t)$. A correct analysis should therefore be formulated directly for the matrix Magnus series of $M(t)$.

This reformulation requires care. The entries of the raw (x, p) matrix in equation (63) carry different units, so its Euclidean operator norm is not meaningful until dimensionless quadratures, for example those determined by ω_r, have been chosen. The authors should specify the norm, derive a local condition for each interval, and distinguish that matrix bound from the bounded-Hamiltonian result quoted in Section 4.3. The constant δ_ξ should be defined numerically or in a way sufficient to reproduce Figure 1.

There is also a problem with the status assigned to equation (60). The discussion of the cited Ref. [15] states that the global constant entering its theorem was extrapolated from coefficients computed through order 24 and that verifying the same constant in general remains open. The present manuscript nevertheless plots equation (60) through $N = 100$ and describes it as a guaranteed universal bound. The authors should either use a fully proved estimate, prove the needed constant, or label the displayed expression and Figure 1 as conjectural/empirical with their precise domain of verification. This issue is separate from, and additional to, the failure of the bounded-Hamiltonian norm to apply to equation (61).

The accumulated error also needs an actual bound. Products of unitary approximants admit a simple telescoping estimate, but the symplectic matrices used here need not have norm one and can amplify earlier local errors, particularly in unstable or strongly squeezed regimes. The phrase "up to stability constants" below equation (90) hides precisely the behavior that controls the long-time reliability of $\nu(t)$ and hence of $C_K(t)$. A useful revision would bound the error in the full symplectic product, propagate it to ν, and state the dynamical assumptions under which the advertised $O(t\Delta^m)$ behavior is uniform. Without such qualifications, the statement that the construction tracks evolution over arbitrarily long times is too strong.

3. **The numerical soft-quench evidence is not reproducible and does not validate the central claim.**

   Figures 2–4 give only the protocol parameters ω_0, ω_1, and τ. The paper does not report the number of intervals, the step-selection rule, the retained Magnus order, the value of ω_r, the evaluation of the integrals in equations (98)–(99), or numerical tolerances. Figure 1 likewise omits the parameters used to determine $h_{\max}$, δ_ξ, and the displayed convergence time. These omissions prevent the results from being reproduced.

   More importantly, the figures do not compare the approximation with an independently obtained reference solution. For this oscillator, a high-accuracy benchmark is straightforward: directly integrate the classical equation $\ddot{x} + \omega^2(t)x = 0$ (or the equivalent first-order system), construct the exact numerical symplectic matrix, and compare S, ν, and C_K. For several values of τ, the revision should show errors versus step size for first- and second-Magnus truncations, demonstrate the expected convergence order, and monitor $\det S = 1$. It should also compare a global truncation and the piecewise method on the same genuinely time-dependent interval so that the claimed advantage is actually visible.

   The statement that the $\tau = 0.1$ curve "agrees well" with equation (89) is not established by Figure 3: no exact sudden-quench curve, difference plot, or norm of the discrepancy is shown.

Likewise, for $\omega_r = \omega_0$ the adiabatic limit should approach

$$C_{\mathrm{ad}} = \frac{1}{8}\left(\frac{\omega_1}{\omega_0} + \frac{\omega_0}{\omega_1} - 2\right),$$

which is 1/16 for the parameters $(\omega_0, \omega_1) = (1, 2)$. This gives a direct benchmark for Figure 4. These quantitative tests are necessary before the conclusions in Section 5.4 and the abstract can be sustained.

4. **The two-level approximation regimes and their accuracy need sharper control.**

   Equations (15) and (16), which state the first two Magnus terms, are malformed in the rendered manuscript: equation (15) has an incorrect upper integration limit, and equation (16) lacks the subscript on Ω_2. These equations introduce the principal tool of the paper and must be corrected.

   For the linearly polarized drive, near resonance alone is not a sufficient statement of the rotating-wave regime. The smallness of the counter-rotating contribution relative to the carrier scale must also be imposed, and the leading Bloch–Siegert correction should at least be discussed. Thus the asserted identity with the circular-drive result is an approximation with a stated error, not an unconditional equality.

   In Section 4.2.2 the periodic dressing is exact in Ω_0/ω, while the Magnus hierarchy in the dressed frame is controlled primarily by $\omega_0 T$. This useful distinction should be stated explicitly. Equations (51), (56), and (57) should then be tested against direct numerical evaluation of the exact one-period unitary over a representative parameter grid. Such a test would establish the domain in which the gauge-dependent transverse term is accurate. It should also establish a stroboscopic time window: a small omitted quasienergy correction produces a phase error that grows with the integer n, so a fixed-order Hamiltonian approximation is not uniformly accurate for arbitrary n. Special care is required near zeros of J_0: there the retained longitudinal and transverse terms vanish together, equation (33) is formally of the 0/0 type, and higher terms can become the leading generator. Saying that the dynamics is frozen "through this order" is algebraically true, but it is not a controlled physical prediction unless the omitted terms are bounded in that neighborhood and the limiting prescription is stated.

5. **The novelty relative to existing time-dependent Krylov and Magnus methods must be made explicit.**

   The component methods used here–Floquet reduction, Magnus and Floquet–Magnus expansions, piecewise Magnus integration, symplectic propagation of quadratic systems, and the squeezed-vacuum number distribution–are established. The exact circular drive and sudden quench are useful checks but do not themselves provide new dynamics, and the smooth one-mode ramp is a minimal test problem. The clearest potentially original analytical result is the dressed-frame Struve expression in equation (51), together with its consequence for stroboscopic spread. The paper should identify precisely whether this formula, the proposed definition, an error theorem, or some other result is its main new contribution.

   The discussion in Section 6 arrives too late and remains descriptive. In particular, the manuscript should compare its definition and scope directly with the extended-Hilbert-space construction in Ref. [25] and with the recent Lie-algebra treatment cited as Ref. [29], not list them only as related work. It should also explain what the present method adds to standard structure-preserving Magnus integrators. For publication in a high-energy theory journal, a quantitatively nontrivial example, a rigorous error result tied to complexity, or a broader multi-mode application would materially strengthen the significance. As written, the evidence supports a pedagogical and computational illustration more readily than the broad methodological advance claimed in the abstract.

The physical interpretation needs the same sharpening. In the two-dimensional examples, equation (33) is simply the transition probability out of the initial state. In the oscillator example, equation (79) is one half of the produced occupation number in the chosen reference basis. The authors should demonstrate what the Krylov language reveals that is not already contained in these standard observables. A parametric-resonance regime, a genuinely many-body or field-theoretic example, or a new scaling or universality result would provide a much stronger test of both the method and its relevance to the intended community.

6. **Several logical claims should be narrowed or proved.**

   The opening of Section 5.2 suggests that closure of nested commutators in a finite-dimensional Lie algebra can make the global Magnus series terminate. Lie-algebra closure keeps every term inside the algebra but does not, by itself, imply termination or convergence; the text should separate these statements. The sudden quench is exact simply because the post-quench generator is constant, and therefore it does not test a global Magnus treatment of genuinely time-dependent evolution.

   Similarly, preservation of symplecticity by exponentiating the truncated generator is valuable, but it does not alone imply accuracy of the resulting squeezing or complexity. The manuscript should distinguish structural preservation, convergence of the Magnus logarithm, and accuracy of a finite product. The conclusion should then be rewritten to reflect what has actually been established after the quantitative tests requested above.

Minor comments

1. The notation $H_{\text{eff}}^{(m)}$ in equation (19) denotes the truncation through order m, whereas $H_{\text{eff}}^{(2)}$ in equations (50)–(51) is used for the second contribution alone. Distinct notation for an individual Magnus term and for a truncated sum would prevent confusion.

2. Appendix B states that $\mathcal{I}_2$ gives the opposite result to $\mathcal{I}_1$ and then proceeds to the final coefficient. Since this factor of two is essential to equation (51), the short missing calculation should be displayed explicitly.

3. Sections and appendices are repeatedly cited with equation-reference formatting, producing constructions such as “Appendix (B)” and “Sec. (4.2.2).” Ordinary section references should be used.

4. Figures 1–4 should label all dimensionless conventions, state the numerical method in their captions or nearby text, and use legends whenever an approximation and a benchmark are compared. Error or residual panels would be more informative than qualitative agreement alone.

5. The manuscript should state whether the squeezing phase is omitted only in the displayed state or also in intermediate basis choices. It drops out of equation (79), but making this invariance explicit would connect the Bogoliubov calculation more cleanly to the proposed complexity.

6. The terms “Krylov complexity” and “spread complexity” should be used consistently. The paper concerns states, while several references in Section 6 concern operator Krylov complexity; the distinction matters when comparing definitions and universality claims.

7. The bibliography contains a literal `"‘bibtex` fence, duplicate entries for the same Floquet review, encoding problems, and an “Unpublished” entry that cannot serve as a verifiable reference. These should be cleaned up, and the unused numerical-Magnus references should either be discussed or removed.

Recommendation

I recommend **rejection in the present form**, while encouraging the authors to resubmit a substantially reconstructed manuscript. The analytical examples contain useful material, but a publishable version would need a precise, basis-aware definition; a correct matrix-level convergence and stability analysis in place of the inapplicable oscillator norm argument; quantitative validation of the piecewise algorithm; and a clearer scientific advance over existing methods, preferably through a nontrivial application or new physical result. These additions go beyond a normal revision of the current manuscript. Until they are supplied, the claims of generality, controlled long-time accuracy, physical significance, and journal-level novelty are not sufficiently supported.