nature portfolio

Peer Review File

Entanglement Properties of SU(2) Gauge Theory

Corresponding Author: Professor Andreas Schafer

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
This work presents a review and new results on the thermalization of non-Abelian gauge theories, specifically through exact numerical simulations of the real-time dynamics of a pure 2D SU(2) lattice gauge theory (no matter).
The study discusses several key findings including (1) confirmation of the Eigenstate Thermalization Hypothesis, (2) entanglement entropy analysis, area-to-volume scaling transition (3) absence of quantum many-body scars for j_{\max} > 1/2, (4) spectral form factor analysis, (5) consistency of the SU(2) entanglement Hamiltonian with the Bisognano-Wichmann theorem, and (6) magic.

The authors motivate the study by the need to explore the thermal properties of non-abelian gauge theories, which rule out important phenomena in particle physics, such as ion collisions and, more in general, scattering properties. The choice of focusing on the SU(2) model is reasonable in this direction. However, since the study does not consider dynamical matter, it is hard to connect the thermal properties of the pure SU(2) to theories where dynamical matter plays a fundamental role (as in scattering).
At any rate, the paper is interesting, and some of the results are rigorous, well-derived, and innovative.
My major concern regarding the publication of this manuscript is the strong similarity and the apparent overlap with Ref[22] (also partially with Ref[27]) (both written by some of the same authors). Since also the titles of the manuscripts are very similar (entanglement on pure SU(2) LGT), and some of the results of this study rely on some of the ones obtained there, it is not obvious to me the novelty of this study, and I would like to receive from the authors a clear comment about this, eventually changing the title or stressing the focus on a particular aspect of the results.
For instance, one of the major results, i.e., the observation of ETH in the thermal properties of SU(2) is very clear and well-written, but it seems to be close to Ref[27].
Before the publication, I think such an aspect should be clear.

On top of this major comment, I list below a set of comments that I think the authors should address before any eventual publication.

- In the abstract, the authors already mention $jmax$ without clarifying what it is. Although it will be clear from the manuscript, I would avoid introducing an undefined (and technical) quantity in the abstract. Maybe I would state that the scars do not survive on larger gauge field representations or a similar sentence.

- The introduction is well written, but often lacks references in some key points: at the end of the first paragraph, when the authors mention that LGTs thermalize faster than other theories, can they add some references?

- Page 2 at the end of the second paragraph: when they cite real-time studies of non-abelian LGT, they might want to consider https://doi.org/10.1038/nature18318, https://doi.org/10.1103/PRXQuantum.5.040309, https://doi.org/10.1103/PhysRevX.6.011023, https://doi.org/10.1103/PhysRevA.105.023322, https://doi.org/10.22331/q-2023-12-20-1213.

- At the end of page 2, when the authors say “The evolution of a highly ordered (initial) state of an isolated interacting quantum system into a quasi-thermal state through its internal dynamics has been investigated for a wide range of model systems, from Ising-type spin chains, many-fermion systems, random matrix models, to low-dimensional gravity models.” I think the authors should add some references.

- The same goes for the next sentence regarding the numerical tools for simulations.

- Page 3, when the authors say “The method of choice has been the diagonalization of the Hamiltonian in a suitable truncated basis” the authors might want to spend some words about the need to truncate the Hilbert space (the gauge link Hilbert space is infinite) and the choice of the electric basis. Regarding the truncation of non-abelian LGTs and specifically SU(2), they might want to mention the choice of subgroups https://doi.org/10.22331/q-2021-02-04-393 , q-deformed algebras https://doi.org/10.1103/PhysRevLett.131.171902, or energy truncation in the Casimir https://doi.org/10.1103/PhysRevResearch.6.033057, https://doi.org/10.1103/PhysRevD.91.054506.

- Could the authors comment on the choice of the honeycomb lattice? What does it add to the results provided by this study? What does change in the scaling and the convergence of the electric basis representation (namely increasing jmax) with respect to the square lattice?

- Page 4, at the end of the first chapter, the authors say: “In the physical subspace, the mL, mR dependence can be integrated out and each state is represented solely by the angular momentum quantum numbers j on the links.” The integration of the fields is known in (1+1)D. Does it hold in (2+1)D for an arbitrary geometry, e.g. for honeycomb lattices? Can they provide some references or clarify a bit more this step?

- Page 4, at the end of the second paragraph, the authors mention that their approach (jmax=1/2 for long ladders and jmax=1 for short ladders) aims to investigate continuum SU(2) properties. Do they mean the continuum limit? Taking the continuum limit is a challenging problem which at least requires to account the limit of lattice spacing to 0 and a sufficiently large system size. Can the authors clarify this phrase and their argument?

- Also, they say that jmax =1/2 can lead to artifacts such as QMB scars. Although the authors are restricted to the pure SU(2) case, I think it would be important to mention what happens when introducing dynamical matter and the abelian scenario (see the conclusions, last point).

- Page 7, first phrase: is there a reason why on the honeycomb lattice, the authors chose the momentum sector with kx,ky=1 instead of 0, as in the square lattice case?

- Page 7 discusses some important quantities to analyze the spectral properties of the model; however, some of their definitions are left to the reader in some references. This might prevent a full understanding of the section. For instance, they should report the restricted gap ratio r_{α} and the mean fluctuations Δ_{i} (see caption of FIG2), defined on EQ 19 ref[27] but not reported in the manuscript.

- Page 8, in the last paragraph, the authors introduce the “T” variable without specifying what it is. Is it a time? Of what? It is not clear at first sight.

- Page 9, section 3.2, regarding the hydrodynamic behavior pre-thermalization. When the authors say it applies to all observables without locally conserved quantities, how do they consider Gauss Law? Does it play a role here, being it is a local constraint? Can they comment a bit more about the hydrodynamization, as it is also discussed later on?

- Page 11, section 4.1 is titled “Entanglement Entropy and Page Curve.” However, the Page curve is never mentioned throughout the section. Either they remove it from the title, or they should explicitly mention it in the text.

- Page 12, when the authors say: “the continuum SU(2) gauge theory is known to be approximately conformal in the high-temperature regime”; can the authors provide some references about it? In how many spatial dimensions is this true: 1D or 2D?

- Page 12, when the authors mention the “black hole information paradox”. I find this paragraph not so clear. Could the authors improve it in connection with the study case?

- Page 13, final paragraph of section 4.1. The authors comment on the absence of outliers in the entropy of eigenstates for jmax=1, which would imply the absence of QMB scars on truncations larger than jmax=1/2. Is this just a special case of the chosen value of the gauge coupling, or do they have a more rigorous argument, e.g. some converging results on larger truncations jmax>1? Again they refer to Ref[22], but then I miss the relevance of new results in this analysis of the gauge field truncation. Is there something new with respect to Ref[22]?

- Page 13, section 4.2, the authors explore the (very interesting) behavior of the entanglement under time evolution. They consider an initial state corresponding to $\beta=0.005$ with a general non-vanishing entanglement entropy. However, they do not specify at which value of the coupling they are setting the Hamiltonian for the time evolution. Is the coupling the same as in section 4.1, i.e. $ag^{2}=1.2$?

- Page 14 Could the authors mention in the caption of FIG6(a) what is the inset panel about? It seems to be a zoom of the convergence at late time, but it would be better for the reader if it is specified in the caption. Overall, the comparison between this initial state and the ground state at large coupling is anyway robust and clear.

- Page 14, final paragraph. The connection of this result with heavy ion collisions is just sketched and not very clear at first read unless one is very familiar with such a topic. Could the authors write an equation that mimics the same behavior they have proven for the entropy eigenstates in eq.9?

- Page 19 second paragraph, when the authors say “There already exists a large body of results, primarily from the study of simple numerical models that provide guidance for future investigations.” Could they add some references?

- Page 20, in the last paragraph of the conclusions, the authors mention the need to extend their results by including fermions (dynamical matter) to the SU(2) LGT. I think they should mention the results found in https://arxiv.org/abs/2405.13112, where QMB scars are detected in the SU(2) Yang-Mills LGT with dynamical matter in (1+1)D (and they seem to be stable under larger truncations) but also in (2+1)D, and the results observed in the abelian scenario (https://quantum-journal.org/papers/q-2023-05-15-1004/).

Reviewer #2

(Remarks to the Author)
Report on Entanglement Properties of SU(2) Gauge Theory

The present work represents an interesting study of pure SU(2) lattice gauge theory in a Hamiltonian formulation. The authors primarily focus on the theory’s dynamical properties, assessing the complexity of its real-time dynamics through several well-developed quantifiers. These include tests of the eigenstate thermalization hypothesis and the spectral form factor, comparisons of subsystem entanglement entropies with thermal entropies, and studies of the entanglement spectrum, where they examine its flatness in relation to non-stabilizerness and demonstrate adherence to the Bisognano-Wichmann theorem.

I find this work particularly engaging, as it explores novel approaches to understanding the dynamical properties of SU(2) gauge theory through quantum state-based quantities such as entanglement entropies and non-stabilizerness. In the context of gauge-theory quantum simulations and computations, these quantities are especially relevant, as they could, in principle, be extracted. Given the challenges of studying gauge theory dynamics through conventional means, this approach has the potential to yield new insights into complex phenomena, such as those observed in heavy-ion collisions. The present study takes an important step in this direction. Additionally, as far as I can assess, the calculations appear to be correct, and the paper is overall well-structured and clearly presented.

That said, I have a few comments and questions for the authors:

1. Scope and Motivations:

While I appreciate the numerical results, the authors frame their study within the context of benchmarking quantum computers. In this regard, I believe the paper would benefit from a more detailed discussion of how these computations could be practically implemented. For instance, which of the quantifiers discussed can be efficiently extracted from quantum computers, and what would be the associated computational cost? Have the discussed quantities been measured previously, perhaps in other models? Furthermore, what implications do the findings have for quantum computation? For example, what does the non-stabilizerness in Fig. 9 tell us about the number of required T-gates in an error-corrected quantum computation and, consequently, its near-term feasibility? While some of these aspects are touched upon in section 3.7, I believe an expanded discussion would be valuable.

Additionally, I am curious about the implications of this study for the full gauge theory. Since numerical calculations often necessitate a relatively small representation cutoff, what can be inferred about the limit in which this cutoff is removed? If I understand correctly, the interpretation of the results suggests that SU(2) gauge theory behaves like a generic non-integrable model in relation to the discussed quantifiers. Is there further insight to be gained regarding how gauge symmetry —specifically, non-abelian gauge symmetry—affects real-time dynamics?

2. Clarification on Novelty: The authors could more explicitly highlight which results have previously appeared in the literature and which contributions are new.

3. Technical Explanation in Section 2.1: The authors integrate out the eigenvalues of the third component of the gauge field for the left and right parts of the links. I think this step is not straightforward for non-experts, and a slightly more detailed explanation would be helpful, e.g. through a reference to related work.

4. Growth of the Hilbert Space in Section 2.1: The authors state a “polynomial growth with jmax”. Could they clarify precisely what they mean here?

5. Missing Reference: There appears to be a missing reference to section 3.3 on page 6.

6. Universality in Section 3.4: The apparent “universality” in the exponent κ is quite intriguing. Have similar behaviors been observed in other models?

7. Connection to Heavy-Ion Collisions: Perhaps the authors could elaborate on the relation between the results in section 3.4 and entropy production in heavy-ion collisions?

8. Clarification in Section 3.6: The authors state, “This implies that the quantum correlations in energy eigenstates in this

range of the spectrum are quite far from Gaussian distributed, an observation which suggests that the system is far from being semiclassical." Could they expand on this statement in more detail?

9. Comparison of Entanglement Spectra: Could the authors provide a comparison between the entanglement spectra and the spectra of the optimized modular Hamiltonians shown in Fig. 7?

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I thank the authors for their detailed and thoughtful responses to my comments. I am satisfied with the effort they made to improve the clarity and rigor of the manuscript, including the addition of appropriate references, clarification of technical aspects, and explicit definitions of key observables. I especially appreciate the clarification regarding the use of the honeycomb lattice and the entanglement structure in connection with QMB scars for different truncations and in the presence of fermionic matter.

Regarding my main concern on the novelty of the study in comparison to Ref. [22] and Ref. [27], I acknowledge the authors' clarification that this work was invited for a special focus issue and that it is intended to combine a pedagogical review with new, previously unpublished results. I find this reasonable, especially now that the introduction clearly delineates which parts are new and which are revisited.

All my other comments have been addressed appropriately. The manuscript is now clearer, better contextualized, and more accessible to readers unfamiliar with some of the subtleties of thermalization in non-Abelian lattice gauge theories.

I therefore recommend the publication on Communications Physics.

Reviewer #2

(Remarks to the Author)
Report on Entanglement Properties of SU(2) Gauge Theory:

I appreciate the authors' efforts in addressing my previous comments. Overall, I am satisfied with the revisions made. I have included a few additional, optional comments for the authors to consider, which I believe could help further strengthen the manuscript.

Please find my (few) remaining comments below:

Authors: We suspect that the Referee report may be based on a later version of the manuscript than the one we submitted to the journal.

I apologize for any confusion I may have caused regarding the different versions of the manuscript.

Authors: […] Thermalization in these models is driven by fermion pair production, often called "string breaking", whereas in the pure (2+1)-dimensional nonabelian gauge theory thermalization is driven by the chaotic nonlinear dynamics of the gauge field, see e.g. [?]. In full QCD with three light flavors of quarks probably both mechanisms are at work. We note however, that it is currently believed that the early approach to local equilibrium in relativistic heavy ion collisions is primarily driven by the nonlinear kinetics of gluons (see e.g. [? ? ?]). […]

Thank you to the referees for their additional clarifications. However, I was unable to read the references—perhaps they should also be cited in the manuscript, if not already included.

Authors: […] We have also found that quantum many-body scars, which are ubiquitous inSchwinger models and in the jmax =1/2 cut-off version of SU(2) lattice gauge theory, seem to be absent as soon as higher color representations are included in the computational basis in the region of couplings that weak breaking of ETH might happen. […]

In this context, Ref. arXiv:2502.03533 might also be relevant.

Authors: […] We have little doubt that much more remains to be discovered and hope that our review-style article will help others interested in pursuing answers to the Referee's questions get started on their own discovery journey.

I agree with the authors that, at the current stage of research, much remains to be discovered. However, I am still left wondering about their views on what truly distinguishes gauge theories from generic quantum many-body systems. What specific features set them apart, and how might these be tested, either qualitatively or quantitatively? What directions or scenarios do the authors see as most promising to explore next? Furthermore, what do they anticipate could realistically be uncovered using near-term quantum devices? I recognize that these are complex and open-ended questions, and I offer them as optional points for consideration, should the authors wish to expand on them.

Reply to referee reports

We would like to thank all referees for the comments. In the following we address the points raised by the referees one by one. We have marked all changes in the manuscript in red. Nearly all of them are in reaction to comments by the referees. However, in Fig. 3(b) we have corrected a small bug in the computation such that now the Λ^T measures of the "electric energy" and "Plaquette" (magnetic energy) agree, as it should be and as we have explained in lines 317-323.

1 Referee A

This work presents a review and new results on the thermalization of non-Abelian gauge theories, specifically through exact numerical simulations of the real-time dynamics of a pure 2D SU(2) lattice gauge theory (no matter). The study discusses several key findings including (1) confirmation of the Eigenstate Thermalization Hypothesis, (2) entanglement entropy analysis, area-to-volume scaling transition (3) absence of quantum many-body scars for $j_{\max} > 1/2$, (4) spectral form factor analysis, (5) consistency of the SU(2) entanglement Hamiltonian with the Bisognano-Wichmann theorem, and (6) magic.

The authors motivate the study by the need to explore the thermal properties of non-abelian gauge theories, which rule out important phenomena in particle physics, such as ion collisions and, more in general, scattering properties. The choice of focusing on the SU(2) model is reasonable in this direction. However, since the study does not consider dynamical matter, it is hard to connect the thermal properties of the pure SU(2) to theories where dynamical matter plays a fundamental role (as in scattering).

At any rate, the paper is interesting, and some of the results are rigorous, well-derived, and innovative. My major concern regarding the publication of this manuscript is the strong similarity and the apparent overlap with Ref[22] (also partially with Ref[27]) (both written by some of the same authors). Since also the titles of the manuscripts are very similar (entanglement on pure SU(2) LGT), and some of the results of this study rely on some of the ones obtained there, it is not obvious to me the novelty of this study, and I would like to receive from the authors a clear comment about this, eventually changing the title or stressing the focus on a particular aspect of the results. For instance, one of the major results, i.e., the observation of ETH in the thermal properties of SU(2) is very clear and well-written, but it seems to be close to Ref[27]. Before the publication, I think such an aspect should be clear.

Reply: Our manuscript was written in response to an invitation by the guest editors to submit a contribution to the special focus volume of Nature Communications Physics entitled *Quantum Simulation of Gauge Theories: Steps and Strides*. The guest editors informed us that a manuscript that reviews previously published results in combination with new, unpublished results would be appropriate for this focus volume. For the benefit of the Referee and the general reader, we have added a brief list at the end of the Introduction that explains which sections contain new results and which are mainly reviews (lines 110-127).

On top of this major comment, I list below a set of comments that I think the authors should address before any eventual publication.

In the abstract, the authors already mention jmax without clarifying what it is. Although it will be clear from the manuscript, I would avoid introducing an undefined (and technical) quantity in the abstract. Maybe I would state that the scars do not survive on larger gauge field representations or a similar sentence.
Reply: We followed the Referee's advice and modified the abstract, which now reads: "...(3) the absence of quantum many-body scars when higher gauge field representations are taken into account;... (lines 20-23)"

The introduction is well written, but often lacks references in some key points: at the end of the first paragraph, when the authors mention that LGTs thermalize faster than other theories, can they add some references?
Reply: We added several references to thermalization studies in various quantum field theories which indicate that nonabelian gauge theories are especially effective thermalizers (lines 40-41).

Page 2 at the end of the second paragraph: when they cite real-time studies of non-abelian LGT, they might want to consider https://doi.org/10.1038/nature18318, https://doi.org/10.1103/PRXQuantum.5.040309, https://doi.org/10.1103/PhysRevX.6.011023, https://doi.org/10.1103/PhysRevA.105.023322, https://doi.org/10.22331/q-2023-12-20-1213.
Reply: We added the references suggested by the Referee (line 50).

At the end of page 2, when the authors say "The evolution of a highly ordered (initial) state of an isolated interacting quantum system into a quasi-thermal state through its internal dynamics has been investigated for a wide range of model systems, from Ising-type spin chains, many-fermion systems, random matrix models, to low-dimensional gravity models." I think the authors should add some references.
Reply: We have added many references to relevant papers. While our bibliography is still far from a complete list of all relevant papers, we now cite more than 100 and we did not want to add many more (lines 70-71).

The same goes for the next sentence regarding the numerical tools for simulations.
Reply: We combined this with the list of references related to the previous comment as we found it unnatural to separate tools and systems (lines 70-71).

Page 3, when the authors say "The method of choice has been the diagonalization of the Hamiltonian in a suitable truncated basis" the authors might

want to spend some words about the need to truncate the Hilbert space (the gauge link Hilbert space is infinite) and the choice of the electric basis. Regarding the truncation of non-abelian LGTs and specifically SU(2), they might want to mention the choice of subgroups https://doi.org/10.22331/q-2021-02-04-393 , q-deformed algebras https://doi.org/10.1103/PhysRevLett.131.171902, or energy truncation in the Casimir https://doi.org/10.1103/PhysRevResearch.6.033057, https://doi.org/10.1103/PhysRevD.91.054506.
Reply: We added the references suggested by the Referee (lines 76-82).

Could the authors comment on the choice of the honeycomb lattice? What does it add to the results provided by this study? What does change in the scaling and the convergence of the electric basis representation (namely increasing jmax) with respect to the square lattice?
Reply: The advantage of the honeycomb lattice over the square lattice for (2+1)-dimensional SU(2) lattice gauge theory is that each vertex has only three links. If the j states on these three links are given, one can determine whether they can combine to form a SU(2) singlet state. If so, the SU(2) singlet state is uniquely determined. In the construction of the SU(2) singlet, the m quantum numbers are projected onto physical states that obey the Gauss law. Thus, the basis of physical states is unambiguously labeled by the j states on each link.

On a square lattice each vertex has four links. SU(2) singlet states cannot be uniquely defined by the four j states on these links. For example, consider four links with $j = \frac{1}{2}$. There are two singlet states, corresponding to the first two $j = \frac{1}{2}$ links forming symmetric or antisymmetric linear combinations. To uniquely label all physical states in the basis, one has to introduce one auxiliary quantum number at each vertex corresponding to an intermediate j state. This is often referred to as "point-splitting" because it effectively splits the tetravalent vertex into two coupled trivalent ones. This procedure introduces additional overhead in the computation at a fixed lattice size by effectively doubling the number of vertices. The honeycomb lattice allows one to study a larger lattice with fewer computational resources.

Convergence of the electric basis representation scales as $\frac{EV}{\epsilon g^2}$ where E is the energy of the state, V the lattice size, ϵ the accuracy and g^2 the coupling. Whether the lattice is honeycomb or square does not affect these parametric scalings, but only affects the coefficient in front. This can be seen from the proof of this scaling in the appendix of arXiv: 2402.04221 (lines 148-154).

Page 4, at the end of the first chapter, the authors say: "In the physical subspace, the mL, mR dependence can be integrated out and each state is represented solely by the angular momentum quantum numbers j on the links." The integration of the fields is known in (1+1)D. Does it hold in (2+1)D for an arbitrary geometry, e.g. for honeycomb lattices? Can they provide some references or clarify a bit more this step?
Reply: As mentioned in the previous point, at each vertex on a honeycomb lattice the m quantum numbers are projected onto physical states that obey the Gauss law, and SU(2) singlet states can be uniquely represented by the three j numbers on the three links. This can only be done for lattices with trivalent vertices. An explicit construction of SU(2) singlets on such lattices can be found in Appendix A of 2103.08661. For (3+1)

dimensions, the hyper-honeycomb lattice that only has trivalent vertices has recently been proposed [2503.09688]. Alternatively, the triamond lattice could be used for the (3+1)-dimensional gauge theory [**?**] (lines 148-154).

Page 4, at the end of the second paragraph, the authors mention that their approach (jmax=1/2 for long ladders and jmax=1 for short ladders) aims to investigate continuum SU(2) properties. Do they mean the continuum limit? Taking the continuum limit is a challenging problem which at least requires to account the limit of lattice spacing to 0 and a sufficiently large system size. Can the authors clarify this phrase and their argument?
Reply: We apologize for this mistake; we meant the infinite volume limit. We have corrected this and also added a sentence saying that we could not perform a controlled continuum limit (lines 165-177).

Also, they say that jmax =1/2 can lead to artifacts such as QMB scars. Although the authors are restricted to the pure SU(2) case, I think it would be important to mention what happens when introducing dynamical matter and the abelian scenario (see the conclusions, last point).
Reply: We added a sentence, commenting on the QMBS in (1+1)-dimensional U(1) and SU(2) lattice gauge theories with dynamic fermions and referred to the relevant papers (lines 165-177).

Page 7, first phrase: is there a reason why on the honeycomb lattice, the authors chose the momentum sector with kx,ky=1 instead of 0, as in the square lattice case?
Reply: The level statistics of non-integrable systems follow the Wigner-Dyson shape. But this is only manifest when all discrete symmetries are lifted. The $k_x = 0$ and $k_y = 0$ sectors have remaining reflection symmetries. In order to show the Wigner-Dyson statistics and level repulsion, one has to pick one reflection symmetry sector in the $k_x = k_y = 0$ momentum sector, which can be done but comes with additional computational overhead. For example, see the last page of 2303.14264. The $k_x = k_y = 1$ momentum sector is better suited for the use of level statistics to test for non-integrability, since this sector has no remaining discrete symmetry.

Page 7 discusses some important quantities to analyze the spectral properties of the model; however, some of their definitions are left to the reader in some references. This might prevent a full understanding of the section. For instance, they should report the restricted gap ratio r_α and the mean fluctuations Δ_i (see caption of Figure 2), defined in Equation (19) in reference [27] but not reported in the manuscript.
Reply: In the revised version, we have written down the explicit formula for r_α and mentioned the Δ_i measure at the appropriate place in the text (lines 265-266 and 283-285).

Page 8, in the last paragraph, the authors introduce the "T" variable without specifying what it is. Is it a time? Of what? It is not clear at first sight.
Reply: We added the clarification that the variable T corresponds to a time and controls the energy range of the contributing matrix elements (lines 308-310).

Page 9, section 3.2, regarding the hydrodynamic behavior pre-thermalization. When the authors say it applies to all observables without locally conserved quantities, how do they consider Gauss Law? Does it play a role here, being it is a local constraint? Can they comment a bit more about the hydrodynamization, as it is also discussed later on?

Reply: We have added a sentence about hydrodynamical modes before eq. (8) and a footnote explaining that local gauge symmetries do not result in hydrodynamic modes (lines 379-382).

Page 11, section 4.1 is titled "Entanglement Entropy and Page Curve." However, the Page curve is never mentioned throughout the section. Either they remove it from the title, or they should explicitly mention it in the text.

Reply: We have inserted a brief explanation of the Page curve and added two references (lines 407-418).

Page 12, when the authors say: "the continuum SU(2) gauge theory is known to be approximately conformal in the high-temperature regime"; can the authors provide some references about it? In how many spatial dimensions is this true: 1D or 2D?

Reply: The Referee is right that this argument is invalid for (2+1) dimensions. Therefore, we removed it.

Page 12, when the authors mention the "black hole information paradox". I find this paragraph not so clear. Could the authors improve it in connection with the study case?

Reply: We have extended the discussion of the Page curve in the opening paragraph of Section 3.3 and tried to clarify the paragraph mentioned by the Referee by explaining that the thermal appearance of subsystems smaller than half the total quantum system has relevance to understanding (hypothetical) observations of black hole radiation as well as (real-world) measurements of particle emission from relativistic heavy ion collisions (lines 407-418 and 446-456).

Page 13, final paragraph of section 4.1. The authors comment on the absence of outliers in the entropy of eigenstates for $j_{\max} = 1$*, which would imply the absence of QMB scars on truncations larger than* $j_{\max} = 1/2$*. Is this just a special case of the chosen value of the gauge coupling, or do they have a more rigorous argument, e.g. some converging results on larger truncations* $j_{\max} > 1$*? Again they refer to Ref[22], but then I miss the relevance of new results in this analysis of the gauge field truncation. Is there something new with respect to Ref[22]?*

Reply: Regarding the novelty of the results, please see the answer to the first comment. The coupling constant is fixed by the requirement of equal partition of electric and magnetic energy, which is a property of the continuum SU(2) gauge theory in thermal equilibrium. Furthermore, the construction of the scar states for $j_{\max} = 0.5$ explained in Ref[22] is no longer supported for $j_{\max} > 0.5$, which we can already see in the entanglement entropy for $j_{\max} = 1$.

Finally, for states up to a given energy E, after some critical $j_{\max}$, a further increase $j_{\max}$ will not significantly modify their wave functions. Thus the calculated entanglement entropies of these states will not change much as $j_{\max}$ further increases. This can be seen from the proof of convergence in the appendix of 2402.04221. We added a sentence explaining this convergence (lines 475-483).

Page 13, section 4.2, the authors explore the (very interesting) behavior of the entanglement under time evolution. They consider an initial state corresponding to $\beta = 0.005$ with a general non-vanishing entanglement entropy. However, they do not specify at which value of the coupling they are setting the Hamiltonian for the time evolution. Is the coupling the same as in section 4.1, i.e. $ag^2 = 1.2$?
Reply: In addition to the caption of Fig. 6 we included the coupling constant in the main text, clarifying that the system is in the ergodic regime, i.e., $ag^2 = 1.2$ (lines 489-490).

Page 14 Could the authors mention in the caption of FIG6(a) what is the inset panel about? It seems to be a zoom of the convergence at late time, but it would be better for the reader if it is specified in the caption. Overall, the comparison between this initial state and the ground state at large coupling is anyway robust and clear.
Reply: The Referee correctly surmised that the inset panel showed a zoom of the late time evolution. Since the main massage is already apparent in the full figure, we removed the inset panel to avoid confusion.

Page 14, final paragraph. The connection of this result with heavy ion collisions is just sketched and not very clear at first read unless one is very familiar with such a topic. Could the authors write an equation that mimics the same behavior they have proven for the entropy eigenstates in eq.9?
Reply: We have added a sentence at the beginning of this paragraph citing a few representative articles from the vast literature on this topic. Many of the investigations relating to relativistic heavy ions have been more focused on the approach to fluid dynamics ("hydrodynamization") than on the problem of entropy production and thermalization, which is addressed in the cited articles.

We wish we were able to "write an equation that mimics the same behavior we have found for the energy eigenstate", as the Referee would like us to do. Unfortunately, we are not aware of the existence of such a equation. One of the goals of the current research reviewed in this article is to establish the fundamental basis for the desired equation (532-534.

Page 19 second paragraph, when the authors say "There already exists a large body of results, primarily from the study of simple numerical models that provide guidance for future investigations." Could they add some references?
Reply: We only refer to some very recent work, because a comprehensive list of references would be far too long (lines 723-732.

Page 20, in the last paragraph of the conclusions, the authors mention the need to extend their results by including fermions (dynamical matter) to the SU(2) LGT. I think they should mention the results found in https://arxiv.org/abs/2405.13112, where QMB scars are detected in the SU(2) Yang-Mills LGT with dynamical matter in (1+1)D (and they seem to be stable under larger truncations) but also in (2+1)D, and

the results observed in the abelian scenario (https://quantum-journal.org/papers/q-2023-05-15-1004/).
Reply: We added appropriate references to the manuscript (lines 763-770).

2 Referee B

The present work represents an interesting study of pure SU(2) lattice gauge theory in a Hamiltonian formulation. The authors primarily focus on the theory's dynamical properties, assessing the complexity of its real-time dynamics through several well-developed quantifiers. These include tests of the eigenstate thermalization hypothesis and the spectral form factor, comparisons of subsystem entanglement entropies with thermal entropies, and studies of the entanglement spectrum, where they examine its flatness in relation to non-stabilizerness and demonstrate adherence to the Bisognano-Wichmann theorem.

I find this work particularly engaging, as it explores novel approaches to understanding the dynamical properties of SU(2) gauge theory through quantum state-based quantities such as entanglement entropies and non-stabilizerness. In the context of gauge-theory quantum simulations and computations, these quantities are especially relevant, as they could, in principle, be extracted. Given the challenges of studying gauge theory dynamics through conventional means, this approach has the potential to yield new insights into complex phenomena, such as those observed in heavy-ion collisions. The present study takes an important step in this direction. Additionally, as far as I can assess, the calculations appear to be correct, and the paper is overall well-structured and clearly presented. That said, I have a few comments and questions for the authors:

We suspect that the Referee report may be based on a later version of the manuscript than the one we submitted to the journal. This later version had a slightly different section structure and contained a few additional insights gained after the original submission. We apologize if this created confusion. We have incorporated these changes into the revised, resubmitted manuscript.

1. Scope and Motivations: While I appreciate the numerical results, the authors frame their study within the context of benchmarking quantum computers. In this regard, I believe the paper would benefit from a more detailed discussion of how these computations could be practically implemented. For instance, which of the quantifiers discussed can be efficiently extracted from quantum computers, and what would be the associated computational cost? Have the discussed quantities been measured previously, perhaps in other models? Furthermore, what implications do the findings have for quantum computation? For example, what does the non-stabilizerness in Fig. 9 tell us about the number of required T-gates in an error-corrected quantum computation and, consequently, its near-term feasibility? While some of these aspects are touched upon in section 3.7, I believe an expanded discussion would be valuable.
Reply: We added brief discussions of computational cost for time evolution and entanglement entropy measurement in Section 3.7. We also mentioned a recent experimental work performing the entanglement entropy calculations on quantum hardware. For the

relation between magic and T-gates, we added some explanations into the Introduction and moved some old texts there, too (lines 677-681 and 684-687).

Additionally, I am curious about the implications of this study for the full gauge theory. Since numerical calculations often necessitate a relatively small representation cutoff, what can be inferred about the limit in which this cutoff is removed? If I understand correctly, the interpretation of the results suggests that SU(2) gauge theory behaves like a generic non-integrable model in relation to the discussed quantifiers. Is there further insight to be gained regarding how gauge symmetry—specifically, non-abelian gauge symmetry—affects real-time dynamics?

Reply: The Referee raises what are perhaps the crucial questions for studies of the real-time dynamics of nonabelian gauge theories: What are the specific features that depend on the nonabelian nature of the gauge symmetry, and what can be learned about the gauge theory in the continuum limit from studies utilizing small lattices? We do not profess to have comprehensive and conclusive answers to these questions at the current time.

However, a few aspects are clear: Nonabelian gauge theories with (2+1)-dimensional, and thus dynamical, gauge fields exhibit a different mechanism driving thermalization than the strictly (1+1)-dimensional abelian and nonabelian Schwinger models with fermions. In the Schwinger models, the gauge fields are nondynamical and can be integrated out. Thermalization in these models is driven by fermion pair production, often called "string breaking", whereas in the pure (2+1)-dimensional nonabelian gauge theory thermalization is driven by the chaotic nonlinear dynamics of the gauge field, see e.g. [**?**]. In full QCD with three light flavors of quarks probably both mechanisms are at work. We note however, that it is currently believed that the early approach to local equilibrium in relativistic heavy ion collisions is primarily driven by the nonlinear kinetics of gluons (see e.g. [**? ? ?**]).

We have also found that quantum many-body scars, which are ubiquitous in Schwinger models and in the $j_{\max} = \frac{1}{2}$ cut-off version of SU(2) lattice gauge theory, seem to be absent as soon as higher color representations are included in the computational basis in the region of couplings that weak breaking of ETH might happen. We have little doubt that much more remains to be discovered and hope that our review-style article will help others interested in pursuing answers to the Referee's questions get started on their own discovery journey.

2. Clarification on Novelty: The authors could more explicitly highlight which results have previously appeared in the literature and which contributions are new.

Reply: We added an outline of the structure of the article and a list of new results at the end of the Introduction (lines 110-127).

3. Technical Explanation in Section 2.1: The authors integrate out the eigenvalues of the third component of the gauge field for the left and right parts of the links. I think this step is not straightforward for non-experts, and a slightly more detailed explanation would be helpful, e.g. through a reference to related work.

Reply: We added the following explanation: "Physical states of the plaquette system are formed by the individual link states such that each vertex satisfies Gauss's law, i.e., each vertex state is a color singlet. On lattices containing only sites with not more than three adjoining links, each physical state is represented unambiguously by the irreducible representations j of the link variables. Since the plaquette operator itself is a singlet under the color gauge group, it can be shown that its action can be expressed solely in terms of the quantum numbers j of the links that form the plaquette [**?**] (lines 148-154)."

4. Growth of the Hilbert Space in Section 2.1: The authors state a "polynomial growth with jmax". Could they clarify precisely what they mean here?
Reply: In the revised version, we included a footnote that gives a rough upper bound for the dimension of the Hilbert space as $(2j_{\max}+1)^{3P+1}$, which grows exponentially with the number of plaquettes P and polynomially with the angular momentum cutoff $j_{\max}$ (footnote 2, page 5).

5. Missing Reference: There appears to be a missing reference to section 3.3 on page 6.
Reply: We corrected this typo.

6. Universality in Section 3.4: The apparent "universality" in the exponent κ is quite intriguing. Have similar behaviors been observed in other models?
Reply: We are not aware of similar findings in other models, but we suspect that the universal time dependence may be related to the quantum Lyapunov exponents of the system, similar to the observation that in classical chaotic systems the Kolmogorov-Sinaï (KS) rate of entropy production equals the sum of all positive Lyapunov exponents. We plan to investigate this in future work by studying the (coupling dependent) quantum Lyapunov exponents of the system using OTOC. As this is rather speculative we prefer not to put it in print before having real results. Therefore, we just mention that the exponent is coupling dependent (which we have found in numerical simulations).

7. Connection to Heavy-Ion Collisions: Perhaps the authors could elaborate on the relation between the results in section 3.4 and entropy production in heavy-ion collisions?
Reply: The relationship between the results of Section 3.4 and the process of entropy production is explained in the last two paragraphs of this section. We added a few additional references at the beginning of the last paragraph pointing the reader to a variety of approaches to the entropy production process in quantum field theory, especially in nonabelian gauge fields (lines 532-534).

8. Clarification in Section 3.6: The authors state, "This implies that the quantum correlations in energy eigenstates in this range of the spectrum are quite far from Gaussian distributed, an observation which suggests that the system is far from being semiclassical." Could they expand on this statement in more detail?
Reply: We have tried to make this statement clearer and removed the extraneous statement on semi-classicality (lines 626-630).

9. Comparison of Entanglement Spectra: Could the authors provide a comparison between the entanglement spectra and the spectra of the optimized modular Hamiltonians shown in Fig. 7?
Reply: We added four new plots (Fig. 8), in which we compare the entanglement spectra with the spectra of the optimized modular Hamiltonian, as well as the corresponding eigenvectors. We hope that this addresses the Referee's request /Fig.8 and lines 586-598).

Reply to the final referee reports

Statements from the referee reports are in boldface. Our answers are in italic.

The first referee had no remaining concerns:

Reviewer 1 (Remarks to the Author)

I thank the authors for their detailed and thoughtful responses to my comments. I am satisfied with the effort they made to improve the clarity and rigor of the manuscript, including the addition of appropriate references, clarification of technical aspects, and explicit definitions of key observables. I especially appreciate the clarification regarding the use of the honeycomb lattice and the entanglement structure in connection with QMB scars for different truncations and in the presence of fermionic matter.
Regarding my main concern on the novelty of the study in comparison to Ref.[22] and Ref.[27], I acknowledge the authors' clarification that this work was invited for a special focus issue and that it is intended to combine a pedagogical review with new, previously unpublished results. I find this reasonable, especially now that the introduction clearly delineates which parts are new and which are revisited.
All my other comments have been addressed appropriately. The manuscript is now clearer, better contextualized, and more accessible to readers unfamiliar with some of the subtleties of thermalization in non-Abelian lattice gauge theories.
I therefore recommend the publication on Communications Physics.

The 'optional' comments of the second referee, however, stimulated quite some discussion in our group.

Reviewer 2 (Remarks to the Author)

I appreciate the authors' efforts in addressing my previous comments. Overall, I am satisfied with the revisions made. I have included a few additional, optional comments for the authors to consider, which I believe could help further strengthen the manuscript.
Please find my (few) remaining comments below:

Authors: We suspect that the Referee report may be based on a later version of the manuscript than the one we submitted to the journal.

I apologize for any confusion I may have caused regarding the different versions of the manuscript.

no comment

Authors: [...] Thermalization in these models is driven by fermion pair production, often called "string breaking", whereas in the pure (2+1)-dimensional nonabelian gauge theory thermalization is driven by the chaotic nonlinear dynamics of the gauge field, see e.g. [1]. In full QCD with three light flavors of quarks probably both mechanisms are at work. We note however, that it is currently believed that the early approach to local equilibrium in relativistic heavy ion collisions is primarily driven by the nonlinear kinetics of gluons (see e.g. [2–4]).

Thank you to the referees for their additional clarifications. However, I was unable to read the references - perhaps they should also be cited in the manuscript, if not already included.

In fact, we are working on a paper devoted to this subject, with highly relevant new results. As the subject is quite non-trivial we do not think that we should try to absorb it into the Communications physics paper. We could add a sentence informing the reader that we work on this new paper, but, the editorial request table states explicitly that one should not do that. So, in the end we decided to leave this section as it is.

Authors: [...] We have also found that quantum many-body scars, which are ubiquitous in Schwinger models and in the $j_{\max} = \frac{1}{2}$ cut-off version of SU(2) lattice gauge theory, seem to be absent as soon as higher color

representations are included in the computational basis in the region of couplings that weak breaking of ETH might happen. [...]. In this context, Ref. arXiv:2502.03533 might also be relevant.

We added a citation of [5].

Authors: [...] We have little doubt that much more remains to be discovered and hope that our review-style article will help others interested in pursuing answers to the Referee's questions get started on their own discovery journey.

I agree with the authors that, at the current stage of research, much remains to be discovered. However, I am still left wondering about their views on what truly distinguishes gauge theories from generic quantum many-body systems. What specific features set them apart, and how might these be tested, either qualitatively or quantitatively? What directions or scenarios do the authors see as most promising to explore next? Furthermore, what do they anticipate could realistically be uncovered using near-term quantum devices? I recognize that these are complex and open-ended questions, and I offer them as optional points for consideration, should the authors wish to expand on them.

In fact, your suggestion has spurred intense discussion in our group. However, we feel that we are not yet able to give a really satisfactory answer to this deep question.

References

[1] B. Müller, L. Ebner, A. Schäfer, C. Seidl, X. Yao, The Nonabelian Plasma is Chaotic (2024). arXiv:2409.01311 [hep-lat]

[2] X.G. Huang, J. Liao, Kinetic evolution of the glasma and thermalization in heavy ion collisions. Int. J. Mod. Phys. E **23**, 1430003 (2014). https://doi.org/10.1142/S0218301314300033. arXiv:1402.5578 [nucl-th]

[3] A. Kurkela, A. Mazeliauskas, J.F. Paquet, S. Schlichting, D. Teaney, Effective kinetic description of event-by-event pre-equilibrium dynamics in high-energy heavy-ion collisions. Phys. Rev. C **99**(3), 034910 (2019). https://doi.org/10.1103/PhysRevC.99.034910. arXiv:1805.00961 [hep-ph]

[4] A. Kurkela, A. Mazeliauskas, Chemical equilibration in weakly coupled QCD. Phys. Rev. D **99**(5), 054018 (2019). https://doi.org/10.1103/PhysRevD.99.054018. arXiv:1811.03068 [hep-ph]

[5] A.N. Ciavarella, C.W. Bauer, J.C. Halimeh, Generic Hilbert Space Fragmentation in Kogut–Susskind Lattice Gauge Theories (2025). arXiv:2502.03533 [quant-ph]