

Roughening and dynamics of an electric flux string in a (2+1)D lattice gauge theory

Corresponding Author: Mr Francesco Di Marcantonio

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)

The Manuscript by Di Marcantonio and coauthors investigates the roughening transition of a pure lattice gauge theory (LGT) in (2+1) dimensions via numerical tensor network (TN) simulations. Making profit of the fact that a lot of aspects of such transition are universal, the Authors study the simplest gauge group, i.e., Z_2 , and they do so in a Hamiltonian formulation. They also exploit the quasi-(1+1)-dimensional character of the fundamental object in the process, i.e., the electric flux string, to employ powerful TN numerics based on matrix product states (MPS) and to avoid a full-fledged simulation in spatial dimension two, where other TN ansätze would probably be more suitable. Some noticeable results include the calculation of the universal correction to the confining potential (first proposed by Lüscher) and the observation of the restoration of (discrete) rotational symmetry at the transition, as well as the description of the different regimes of entanglement growth during the out-of-equilibrium evolution — a quantity that constitutes an added value intrinsic to the chosen method, and was therefore not tackled before. The results add consistency to the description of the string excitations by a massless bosonic theory. A quick hint is then given in the Conclusion about the perspective of observing the whole phenomenology in table-top quantum simulation experiments.

Overall the Manuscript blends a meanwhile traditional topic in LGT with a technique which is widely spread in condensed matter, and obtains some interesting results which are presented in a fairly accessible way for a vast audience. In particular, it might possibly stimulate cross-community discussions and contamination of methods. It is also complemented by a solid set of appendices to explain technical details in the most self-contained way possible, which — together with code availability — facilitates the reproducibility of results. There is therefore no doubt from my side about this Manuscript being publishable in Comm. Phys. up to a few comments and suggestions for improvement, which I leave here below.

1) I found the use of the term “crossover” in Fig. 1 (and its description in the text) quite confusing, since I would instinctively associate to that the meaning of “there is not a real phase transition”. But this is not the case, since g_r is a well-defined quantum phase transition with its universality class, and so on. Is such nomenclature so spread in the LGT literature that is unthinkable to use other terms, or could the Authors render the same concept with clearer terms?

2) Indeed, in Sec. III, it is explicitly said that “It can be shown that the roughening transition is a Berezinskii-Kosterlitz-Thouless (BKT) transition [25]”. Incidentally, this contrasts a bit with the sixth paragraph in the Introduction (“The transition between these two behaviours [...] motivates the exploration of novel diagnostic tools rooted in quantum information theory and symmetry principles.” The latter seems to imply that the nature of the transition still have to be elucidated... Incidentally, given the overall very accessible and self-contained character of the Manuscript, it could be helpful to equip the reader with a sketchy reasoning of why a BKT transition — which is usually associated to some $U(1)$ degree of freedom — emerges here: good references in that direction are, e.g., [19] and [26], but I am sure that many others already in the bibliography also discuss this point.

3) A couple of times the Authors make reference (already in Sec. II) to “the” path connecting two static charges, as if it was unique. Since this is not the case, and it later becomes clear that they intend “the path(s) with minimal length” and that they have in mind a straight line between charges posed on-axis (when not differently specified, see Fig. 6a), it would be helpful to explain this much earlier in the text (i.e., in Sec. II A-B). Now it only comes at page 5 in Sec. III (with reference to Fig. 3).

Hopefully, I did not oversee that in a earlier place, in which case I apologize.

4) At the technical level, I was a bit surprised to read that the Authors decided for a large local dimension in their MPS representation ($d=2^N$), instead of opting for a longer snake of N tensors with local dimension d each. From experience, I would have said that the latter is cheaper computationally, but maybe I am overlooking some implementation detail that the Authors would want to share with the readership. Conversely, a bond dimension $\chi=256$ sounds at first not particularly large for simulations that involve a $N=6$ cylinder and moreover out-of-equilibrium. Maybe also here some more considerations would be helpful to guide the readers — to be clear, I have no doubt on the reliability and care of the results, I am only a bit surprised by how cheap they are.

5) While most of the Manuscript is fairly pedagogical and accessible to members of both main communities (LGT and cond-mat), there are a few passages where content is dense of jargon and assumes a deep familiarity. E.g., at the end of the first paragraph in Sec. III, “it has been shown that the string delocalizes and its width diverges logarithmically with R , while keeping a non-zero string tension, meaning that it is not a deconfining phase [18]. Therefore, in the massless case, an infinitely long string is fundamentally unstable [20].” Maybe a couple more sketchy lines with intermediate steps would help the non-familiar reader further. I agree however that this is matter of taste.

6) The Manuscript is made rich by a very informative collection of plots: some of them, however, could probably even better convey their key message. In particular, there are a few advocating for a certain scaling law but not having the properly rescaled axes to show this in a clearcut way — i.e., as straight lines. This should be amended. E.g., missing lin-log scales on 4b and 4d, and a bit more involved in 6c. Another general remark is that it could be useful to indicate in most of them the position of the two critical points g_c and g_r , which would considerably increase accessibility of information.

7) In Sec. III A, it is not fully clear to me why the Authors consider only i) the horizontal links of the string and ii) the vertical displacement of the central point between the two charges. Is that a matter of maximizing the signal and minimizing the computational costs? Why should the total variance across the string be more difficult to compute? And why do variations in the electric field along the vertical lattice direction do not matter? I am most probably overlooking something easy here, but so would probably also the average reader...

8) By looking at Fig. 5, there are two aspects that stroke me and made me not as comfortable as I wished. First of all, the three panels have different horizontal axis-scales so that the features always appear in the same position... but they are actually features that are distinguishing two different points and a regime in between: more graphical consistency would help communicating this much more clearly. Second, there is a strange discrepancy in the resolution of the data points in panel (c) between the regions 0.8-1.0 and 1.0-1.5: why is that the case? If the latter is not relevant, why is that plotted? Again, apologies if I am missing anything trivial here.

9) Minor: at the bottom of page 5, should it not read “Indeed, both in the strong confinement and in the deconfined region, the results are independent of R but with different features.” I mean R instead of g , see Fig. 4b, indeed.

10) In Fig. 4d, I would have expected to see a plateau of the central charge at the value $c=1$ for the whole region between g_c and g_r , while it seems to deviate much earlier. Is there any reasoning or approximate scaling that the Authors could provide in this sense?

11) I found the summary of the derivation of the Lüscher correction, and also the argumentation about the rescaling of the Casimir energy by the speed of sound, very instructive. In this respect, I was wondering whether one could not compute such correction by other estimates, as often done in $(1+1)$ -dimensional bosonic theories, for example, by looking at the dependence of some excitation energies with the size of the system (in this case, probably, of the string), or similar. It would be simply great to have one more self-consistency loop in the presented data and analysis.

12) Possibly naive question: should Fig. 8, i.e., the visualization of the electric field dynamics, not come before Fig. 7, where the string width evolution is then evaluated quantitatively? Otherwise said, what are we learning from Fig. 8 that we did not observe quantitatively in Fig. 7c? On a different note: how would it be to visualize snapshots of the full string at different instants in time? Would it teach us something about its behaviour close to the endpoints, or so?

13) Finally, at the end of Sec. IV.B, the Authors highlight “a stark ” between the behaviour of the string width and the entanglement entropy after a quench: could they also provide any insight on the physical mechanism behind this behaviour? I think this would constitute a considerable added value beyond the simple observation of the fact. I understand, however, that this might even be a spin-off research question per se: therefore, it is not a “must” but a “nice-to-have”.

Reviewer #2

(Remarks to the Author)

This paper studies the roughening transition of the electric flux string in a 2+1D lattice gauge theory using tensor network simulations. The authors consider four different physical observables to characterize the transition: the string width, the entanglement entropy, the Lüscher term in the confinement potential, and the kink mass. Moreover, they investigate the time evolution of a stiff string under the Hamiltonian dynamics. These tensor network simulations confirm ideas proposed in high-energy physics, and it is particularly interesting that the authors show the kink mass can serve as an indicator of the recovery

of rotational symmetry, which in turn signals the roughening transition. The results are relevant to the fields of tensor network states, condensed matter physics, high-energy physics, and quantum simulation.

The paper is clearly written, the tensor network results are consistent with theoretical predictions, and the data analysis is careful. Overall, the results are convincing, and the paper meets the acceptance criteria of Communications Physics. Before recommending publication, I have a few questions for the authors to address:

1. On page 9, in the paragraph containing Eq. (22), the authors argue that the kink mass is not equivalent to the excitation gap of the string. However, in Eq. (22), they fit the kink mass using the gap formula of the BKT transition. Are these two statements contradictory?
2. It seems that the string tension, $\sigma_{\text{off}} - \sigma_{\text{on}}$, becomes zero when $g = 1$. However, at this point, the string is stiff, and one would expect $\sigma_{\text{off}} \neq \sigma_{\text{on}}$. Is this equality merely accidental, caused by numerical inaccuracy?

Reviewer #3

(Remarks to the Author)

The authors investigate the roughening transition in the (2+1)-dimensional pure Z2 lattice gauge theory using tensor network techniques, and they further study the dynamical properties. Their simulation strategy relies on the MPS ansatz by formulating the theory on narrow cylindrical geometries.

Regarding the roughening transition, the authors present a broad set of numerical results involving the string width, entanglement entropy, string tension and Luscher term in the confining potentials, and the kink mass. In an intermediate gauge-coupling regime, the string width exhibits a clear logarithmic dependence on the charge separation, which is a characteristic signature of the roughening transition. In the same regime, the entanglement entropy also shows logarithmic scaling, and the extracted central charge $c=1$ supports the interpretation that the transition is of BKT type. For the confining potential, the authors particularly focus on the Luscher term, which is a correction to the linear potential term. Based on their numerical findings, they point out a possible non-trivial rescaling of the Casimir energy in the Hamiltonian formalism (which is an intriguing claim that may have wider relevance to recent Hamiltonian-based studies). The kink mass is also analyzed as another diagnostic for the roughening transition, and the authors attempt to relate their results to the mass gap expected at a BKT transition.

The second half of the paper concerns the dynamics of electric flux strings. The authors simulate the real-time evolution of the entanglement entropy, string width, and electric-field occupation using TEBD, an algorithm well-suited for short-time dynamics. While the linear growth rate of the entanglement entropy appears insensitive to the gauge coupling in the roughening phase, no such universality is found in the dynamics of the string width, whose growth rate retains a non-trivial coupling dependence. The electric flux dynamics is also visualized via the time evolution of the electric-field occupation.

Although the Z2 gauge theory is the simplest gauge theory, it contains several features highly relevant to the community: despite its simplicity, it exhibits a confinement–deconfinement transition, and it serves as a key testbed for both quantum computing approaches and classical tensor network methods. Additionally, the confinement phase can be further classified into the strongly confined and weakly confined phases, and the latter is the roughening phase, that has been an interesting topic not only in condensed matter physics but also in high-energy physics. Indeed, recent tensor network studies have revisited the roughening transition in this model, as in refs. [73, 83].

A brief comparison with these works [73, 83] is in order. Although all three studies use tensor network techniques, their methodologies differ substantially: ref. [73] employs iDMRG on an infinite cylinder, and ref. [83] uses a two-dimensional tree tensor network ansatz, whereas the present work focuses on finite cylindrical systems with MPS. In addition, the authors of this manuscript provide detailed analyses of the string width and the Luscher term in the confining potential. The former seems to be a natural target to be simulated in the study of the roughening transition. Their discussion of the Luscher term is particularly noteworthy, as Hamiltonian-based studies may indeed feature a non-trivial speed of sound. Further investigation in the community would certainly be valuable. For these reasons, this work can be meaningfully distinguished from the earlier studies.

However, an additional point of comparison with refs. [73, 83] would be helpful for the community. Therefore, I recommend publication after the authors address the following question, including some minor ones.

1) I would like to encourage the authors to add some further comments on the comparison of the roughening transition points obtained in their paper and those reported in refs. [73, 83]. In the current manuscript, the authors only state that "This yields a value $g_r \simeq 0.91$, which is comparable with the recent literature [73, 83]." I think it would be beneficial for the readers if the manuscript briefly discussed how these transition points were determined in these previous works, including the numerical values obtained in refs. [73, 83], the diagnostics used to locate the transition. Such a comparison would help clarify the consistency among different methods.

2) Although the authors estimate $g_r=0.91$ from the fit based on eq. (22), with some remarks, is it possible to extract the transition point using another diagnostic? If so, providing such an alternative estimate could help validate their determination. For example, looking at Fig. 15, is there any possibility of identifying g_r from the central charge or its dependence on g ?

3) Can the authors report the resulting critical exponents γ and ν obtained in Appendix B?

Reviewer #4

(Remarks to the Author)

The authors investigate the phenomenon of string roughening in a Z_2 lattice gauge theory using tensor network methods applied to cylindrical geometries. Their work provides a thorough numerical analysis, elucidating several aspects associated with string roughening. In particular, they examine the logarithmic divergence of the string width as the separation between static charges increases, the logarithmic scaling of the entanglement entropy, the Lüscher correction to the confining potential, and the restoration of rotational symmetry that is broken by the lattice. The latter is especially interesting, as it enables them to estimate the transition point between the confined and rough-string regimes. Finally, they study the real-time dynamics of the string following quenches to different regions of the phase diagram.

Overall, I find the paper technically solid and the results significant, especially in light of rapid progress in quantum simulators capable of probing gauge-theory physics in two dimensions. I recommend the article for publication, provided the authors address the following points:

1. In Fig. 4, the authors show that the growth of the string width with respect to R is compatible with a logarithmic dependence. However, it is unclear whether Eq. (13) is fully satisfied. In particular, I would like to see whether the prefactor of the logarithm in their fit is consistent with the $1/\sigma$ dependence predicted by the equation.

2. I am somewhat confused about the logarithmic dependence of the entanglement entropy on R . Could the authors clarify why such a dependence is expected specifically as a function of R , and how the central charge emerges in this context? I would expect the scaling to depend sensitively on how the bipartition is defined for the entropy calculation. Could the authors explain precisely how the cut is implemented? This point is also important because the duality transformation they employ may alter the structure of the bipartition.

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

I thank the authors for responding to my detailed questions and for revising the manuscript accordingly. My concerns have been clarified. Therefore, I recommend this manuscript for publication in Communications Physics.

Reviewer #3

(Remarks to the Author)

I thank the authors for addressing all the points I raised. I recommend publication.

Minor comment: Part of the newly added sentence in the caption of Fig. 10 should be in text mode.

Reviewer #4

(Remarks to the Author)

The authors have addressed the concerns raised. I recommend publication.

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In the following response to the referees we adopted as a color convention BLACK for the Referees comments and TEAL for the Authors answers.

I. REMARKS TO THE AUTHOR

A. Reviewer #1

The Manuscript by Di Marcantonio and coauthors investigates the roughening transition of a pure lattice gauge theory (LGT) in $(2+1)$ dimensions via numerical tensor network (TN) simulations. Making profit of the fact that a lot of aspects of such transition are universal, the Authors study the simplest gauge group, *i.e.*, Z_2 , and they do so in a Hamiltonian formulation. They also exploit the quasi- $(1+1)$ -dimensional character of the fundamental object in the process, *i.e.*, the electric flux string, to employ powerful TN numerics based on matrix product states (MPS) and to avoid a full-fledged simulation in spatial dimension two, where other TN ansätze would probably be more suitable. Some noticeable results include the calculation of the universal correction to the confining potential (first proposed by Lüscher) and the observation of the restoration of (discrete) rotational symmetry at the transition, as well as the description of the different regimes of entanglement growth during the out-of-equilibrium evolution — a quantity that constitutes an added value intrinsic to the chosen method, and was therefore not tackled before. The results add consistency to the description of the string excitations by a massless bosonic theory. A quick hint is then given in the Conclusion about the perspective of observing the whole phenomenology in table-top quantum simulation experiments.

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We thank the referee for the careful and detailed description of our work and for considering the manuscript is publishable after a few improvements. We are welcome for the rich discussion originated by the comments of the referee.

1) I found the use of the term “crossover” in Fig. 1 (and its description in the text) quite confusing, since I would instinctively associate to that the meaning of “there is not a real phase transition”. But this is not the case, since g_r is a well-defined quantum phase transition with its universality class, and so on. Is such nomenclature so spread in the LGT literature that is unthinkable to use other terms, or could the Authors render the same concept with clearer terms?

We acknowledge the point raised by the referee. Indeed, we use the term “crossover” to indicate that it is not a phase transition of the bulk of the $(2+1)\text{D } Z_2$ gauge theory, but of the effective $(1+1)\text{D}$ string. It is the string that exhibits a BKT transition, not the whole $(2+1)\text{D}$ quantum system. Hence, we have a situation where a massive $(2+1)\text{D}$ system develops a massless $(1+1)\text{D}$ interface. That being said, far away from the string, the excitations of the LGT are still massive even in the crossover region. In order to make this point more clear for the reader, we have included an explanation of the meaning of the term crossover in Sec. I within the main text. This kind of nomenclature, we have found, is rather common in the LGT literature.

2) Indeed, in Sec. III, it is explicitly said that “It can be shown that the roughening transition is a Berezinskii-Kosterlitz-Thouless (BKT) transition [25]”. Incidentally, this contrasts a bit with the sixth paragraph in the Introduction (“The transition between these two behaviours [...] motivates the exploration of novel diagnostic tools rooted in quantum information theory and symmetry principles.” The latter seems to imply that the nature of the transition still have to be elucidated... Incidentally, given the overall very accessible and self-contained character of the Manuscript, it could be helpful to equip the reader with a sketchy reasoning of why a BKT transition — which is usually associated to some $U(1)$

degree of freedom — emerges here: good references in that direction are, e.g., [19] and [26], but I am sure that many others already in the bibliography also discuss this point.

We thank the referee for this comment on the BKT transition. We agree on the contrast between Sec. I sixth paragraph, and Sec. III second paragraph. We better pointed out in the main text that the tools we develop are to study the BKT transition “embedded” in the bulk (2+1)D system. This being said, we are not doubting its nature. Following their suggestion, we have added in the introduction an explanatory paragraph outlining why an effective U(1) description emerges for the string fluctuations, leading to a BKT transition, with explicit references to Refs. [19, 26]. In particular, we referenced the fact that the string can be treated through a Solid-on-Solid model, which is in the same universality class of the planar XY model, known to have a BKT transition.

3) A couple of times the Authors make reference (already in Sec. II) to “the” path connecting two static charges, as if it was unique. Since this is not the case, and it later becomes clear that they intend “the path(s) with minimal length” and that they have in mind a straight line between charges posed on-axis (when not differently specified, see Fig. 6a), it would be helpful to explain this much earlier in the text (*i.e.*, in Sec. II A-B). Now it only comes at page 5 in Sec. III (with reference to Fig. 3). Hopefully, I did not oversee that in a earlier place, in which case I apologize.

We agree with the referee’s comment. Closing Sec. II A, we have added a clarification stating that we focus on the minimal-length path (the “straight” string) as the reference state, as illustrated in Fig. 3.

4) At the technical level, I was a bit surprised to read that the Authors decided for a large local dimension in their MPS representation ($d = 2^N$), instead of opting for a longer snake of N tensors with local dimension d each. From experience, I would have said that the latter is cheaper computationally, but maybe I am overlooking some implementation detail that the Authors would want to share with the readership. Conversely, a bond dimension $\chi = 256$ sounds at first not particularly large for simulations that involve a $N = 6$ cylinder and moreover out-of-equilibrium. Maybe also here some more considerations would be helpful to guide the readers — to be clear, I have no doubt on the reliability and care of the results, I am only a bit surprised by how cheap they are.

We acknowledge the point raised by the referee. Indeed, we motivate our choice of $d = 2^N$ (blocking a vertical slice of the cylinder into a single MPS site) following two main reasons. On one hand, it offers a particular ease in the implementation of the Gauss law constraints and local symmetries within a single tensor. Further, the vertical compression of ladders has an easier interpretation. In the blocked anstaz, tensors correspond to the vertical displacement of the string with respect to the equilibrium position. Hence, blocking a vertical strip of the lattice as just a single tensor, captures all the *in-rung* entanglement. The other reason is related to the cost. While a “snake” (1D path) could seem computationally cheaper per site, none of the two approaches solve the exponential complexity of increasing the vertical dimension. We have added a subsection in the technical Appendix E regarding the MPS mapping of 2D systems.

5) While most of the Manuscript is fairly pedagogical and accessible to members of both main communities (LGT and cond-mat), there are a few passages where the content is dense with jargon and assumes a deep familiarity. E.g., at the end of the first paragraph in Sec. III, “it has been shown that the string delocalizes and its width diverges logarithmically with R , while keeping a non-zero string tension, meaning that it is not a deconfining phase [18]. Therefore, in the massless case, an infinitely long string is fundamentally unstable [20].” Maybe a couple more sketchy lines with intermediate steps would help the non-familiar reader further. I agree, however, that this is a matter of taste.

We thank the referee for this comment on jargon and accessibility. We have revised the indicated passage to include additional explanatory sentences clarifying the meaning of the string broadening, its coexistence with a finite string tension, and why this does not correspond to deconfinement. We agree that these additions improve accessibility without compromising conciseness.

6) The Manuscript is made rich by a very informative collection of plots: some of them, however, could probably even better convey their key message. In particular, there are a few advocating for a certain scaling law but not having the properly rescaled axes to show this in a clearcut way — *i.e.*, as straight lines. This should be amended. E.g., missing lin-log scales on 4b and 4d, and a bit more involved in 6c. Another general remark is that it could be useful to indicate in most of them the position of the two critical points g_c and g_r , which would considerably increase accessibility of information.

We thank the referee for these observations. In response we have modified the axes on Figs. 4b, 4d and 6e accordingly, in order to better visualize results and make them more accessible.

7) In Sec. III A, it is not fully clear to me why the Authors consider only i) the horizontal links of the string and ii) the vertical displacement of the central point between the two charges. Is that a matter of maximizing the signal and minimizing the computational costs? Why should the total variance across the string be more difficult to compute? And why do variations in the electric field along the vertical lattice direction do not matter? I am most probably overlooking something easy here, but so would probably also the average reader...

We thank the referee for exposing their doubt. In ref. [18] Lüscher defines the string width taking the chromo-electric energy density. Hence, the expectation value of the field at point x in its components (along different directions). Then, performing an actual computation he uses simply the links parallel to the electric flux (evidently lying on one of the space axes). In this sense, we follow this definition for the sake of simplicity and use the electric energy (linear) density, again parallel to the horizontal axis of our lattice where the flux tube lies. In addition, we choose only the links in the middle of the string as they correspond to the ones showing the largest width on average. The endpoints where the charges lie are indeed fixed so we do not expect much oscillations to occur. As the referee pointed out, it is cheaper to compute the expectation values of just one column of links, but it is not the main reason behind our choice.

8) By looking at Fig. 5, there are two aspects that stroke me and made me not as comfortable as I wished. First of all, the three panels have different horizontal axis-scales so that the features always appear in the same position... but they are actually features that are distinguishing two different points and a regime in between: more graphical consistency would help communicating this much more clearly. Second, there is a strange discrepancy in the resolution of the data points in panel (c) between the regions 0.8-1.0 and 1.0-1.5: why is that the case? If the latter is not relevant, why is that plotted? Again, apologies if I am missing anything trivial here.

We thank the reviewer for their comments. We have taken into account the suggestions and we agree that a better graphical consistency will communicate better our results. In the plots of Fig. 4, 5 and 6 we have indicated with dashed lines the position of both the deconfining and the roughening points. Moreover, we also shaded the different regimes, each with its color (as in the phase diagram in Fig. 1), in order to better visualize the features of each phase. In Fig. 5c we have also added more points of resolution, in order to have a better consistency. For all the graphs in Fig. 5, in the end, we have decided to keep the current x-axis domains, as we believe that the features we want to demonstrate each have their own scale of couplings.

9) Minor: at the bottom of page 5, should it not read “Indeed, both in the strong confinement and in the deconfined region, the results are independent of R but with different features.” I mean R instead of g , see Fig. 4b, indeed.

We thank the referee for noticing the typo. We corrected it and highlighted it in the main text.

10) In Fig. 4d, I would have expected to see a plateau of the central charge at the value $c = 1$ for the whole region between g_c and g_r , while it seems to deviate much earlier. Is there any reasoning or approximate scaling that the Authors could provide in this sense?

We thank the referee for the comment about the central charge. We added at the end of Sec. III that this trend is due to the finite size of the strings used in our computations. We take advantage of this comment to point out another striking observation and refer the reader to Appendix E where these comparisons are studied in detail. As can be seen also in other plots as in Fig. 5b-c for the Lüscher term and the restoration of rotational invariance, there are some oscillations with respect to the expected flat behavior of these quantities in the roughening region. In Appendix E, we show that by increasing the R s used in the fit of the entropy (for the central charge) or the potential (for the Lüscher term or string tension difference) we can obtain more flat behavior of the above-mentioned observables in the rough region. Jointly, by increasing the transverse direction N , we show a broadening of the roughening region extending toward the bulk critical point g_c (which is reached only in the limit of $N \rightarrow \infty$, a true 2D scenario).

11) I found the summary of the derivation of the Lüscher correction, and also the argumentation about the rescaling of the Casimir energy by the speed of sound, very instructive. In this respect, I was wondering whether one could not compute such correction by other estimates, as often done in $(1+1)$ -dimensional bosonic theories, for example, by looking at the dependence of some excitation energies with the size of the system (in this case, probably, of the string), or similar. It would be simply great to have one more self-consistency loop in the presented data and analysis.

We acknowledge the point raised by the referee. Indeed, we plan to extract the speed of sound in a follow-up project. Even if our model does not exhibit translational invariance, we tried to get the transfer matrix in order to get the correlation length ξ , and the first excited state energy e_1 to use this formula $e_1 - e_0 = v/\xi$ of the gap, common in the literature. The results were inconclusive, thus this analysis focuses on the information extracted from the ground states only, avoiding further complexity to the project which is already numerically intensive. We have added a comment in Sec. V about inclusion of a better characterization in a future work.

12) Possibly naive question: should Fig. 8, *i.e.*, the visualization of the electric field dynamics, not come before Fig. 7, where the string width evolution is then evaluated quantitatively? Otherwise said, what are we learning from Fig. 8 that we did not observe quantitatively in Fig. 7c? On a different note: how would it be to visualize snapshots of the full string at different instants in time? Would it teach us something about its behaviour close to the endpoints, or so?

We welcome the referee suggestions about the order of the plots. We have exchanged the position of Fig. 8 and Fig. 7 and their relative paragraphs in the main text in Sec. IV accordingly. Moreover, we have also added some snapshots of the whole system at different times, corresponding to the time evolution in the crossover region. We have observed that outside the endpoints of the string, the system is basically unaffected, while it is clear that the string is delocalized in the inter-charge space.

13) Finally, at the end of Sec. IV.B, the Authors highlight “a stark difference” between the behaviour of the string width and the entanglement entropy after a quench: could they also provide any insight on the physical mechanism behind this behaviour? I think this would constitute a considerable added value beyond the simple observation of the fact. I understand, however, that this might even be a spin-off research question per se: therefore, it is not a “must” but a “nice-to-have”.

We agree with the referee on this comment about the different observables behavior in the dynamical setting. Indeed, the nature of these observables, the entropy and the string width, is different with the string width being a “local” operator, whereas the entropy being sensitive to long range correlations. Thus we do not expect the same qualitative trend for a quench. This being said, we acknowledge the necessity of a physical description for the interpretation of our dynamical results. As pointed out by the referee, we surely envision to dedicate the follow-up on this description, as it was already mentioned in the outlooks of our manuscript.

B. Reviewer #2

This paper studies the roughening transition of the electric flux string in a 2+1D lattice gauge theory using tensor network simulations. The authors consider four different physical observables to characterize the transition: the string width, the entanglement entropy, the Lüscher term in the confinement potential, and the kink mass. Moreover, they investigate the time evolution of a stiff string under the Hamiltonian dynamics. These tensor network simulations confirm ideas proposed in high-energy physics, and it is particularly interesting that the authors show the kink mass can serve as an indicator of the recovery of rotational symmetry, which in turn signals the roughening transition. The results are relevant to the fields of tensor network states, condensed matter physics, high-energy physics, and quantum simulation.

The paper is clearly written, the tensor network results are consistent with theoretical predictions, and the data analysis is careful. Overall, the results are convincing, and the paper meets the acceptance criteria of Communications Physics. Before recommending publication, I have a few questions for the authors to address:

We are grateful to the referee for their general comment on how our work bridges different communities of condensed matter physics, high-energy physics, and quantum simulation. We thank the reviewer for considering our manuscript in line with Communication Physics criteria after answering the referees comments and queries.

1) On page 9, in the paragraph containing Eq. (22), the authors argue that the kink mass is not equivalent to the excitation gap of the string. However, in Eq. (22), they fit the kink mass using the gap formula of the BKT transition. Are these two statements contradictory?

We thank the referee for this comment on the gap formula. As stated in the main text, right before Eq. (22), the kink mass coincides with the mass gap of the on-axis string $\Delta E_{\text{on}} = 2m_k$ in the $g > g_r$ region. Once $g < g_r$, the kink mass definition $m_k = V_{\text{off}} - V_{\text{on}}$ does not coincide with ΔE_{on} . Nonetheless, the mass gap formula Eq. (22) conventionally used to describe the gap in BKT phase transition is valid for $g > g_r$, hence allowing us to use the kink mass with caution. The referee is right that this might seem contradictory but we take this result with attention since in the limit of $g \rightarrow g_r$ (from above) the kink mass only approximate the energy gap. Following the comment, we have rephrased this concept in the main text trying to avoid the contradiction.

2) It seems that the string tension difference, $\sigma_{\text{off}} - \sigma_{\text{on}}$, becomes zero when $g = 1$. However, at this point, the string is stiff, and one would expect $\sigma_{\text{off}} \neq \sigma_{\text{on}}$. Is this equality merely accidental, caused by numerical inaccuracy?

We thank the referee for their observation. As the referee suggested, this difference $\sigma_{\text{off}} - \sigma_{\text{on}}$ going to zero (and the previous oscillations) are due to numerical inaccuracy. We demonstrated in Appendix E.3 that more than the transverse size N , the inaccuracy is due to the finite R s we are using. In Fig. 15 we show these results. We expect the difference of string tensions to behave more smoothly as we increase the string lengths more. We integrated a comment on this matter at the end of Sec. III D.

C. Reviewer #3

The authors investigate the roughening transition in the (2+1)-dimensional pure Z2 lattice gauge theory using tensor network techniques, and they further study the dynamical properties. Their simulation strategy relies on the MPS ansatz by formulating the theory on narrow cylindrical geometries.

Regarding the roughening transition, the authors present a broad set of numerical results involving the string width, entanglement entropy, string tension and Luscher term in the confining potentials, and the kink mass. In an intermediate gauge-coupling regime, the string width exhibits a clear logarithmic dependence on the charge separation, which is a characteristic signature of the roughening transition. In the same regime, the entanglement entropy also shows logarithmic scaling, and the extracted central charge $c = 1$ supports the interpretation that the transition is of BKT type. For the confining potential, the authors particularly focus on the Luscher term, which is a correction to the linear potential term. Based on their numerical findings, they point out a possible non-trivial rescaling of the Casimir energy in the Hamiltonian formalism (which is an intriguing claim that may have wider relevance to recent Hamiltonian-based studies). The kink mass is also analyzed as another diagnostic for the roughening transition, and the authors attempt to relate their results to the mass gap expected at a BKT transition.

The second half of the paper concerns the dynamics of electric flux strings. The authors simulate the real-time evolution of the entanglement entropy, string width, and electric-field occupation using TEBD, an algorithm well-suited for short-time dynamics. While the linear growth rate of the entanglement entropy appears insensitive to the gauge coupling in the roughening phase, no such universality is found in the dynamics of the string width, whose growth rate retains a non-trivial coupling dependence. The electric flux dynamics is also visualized via the time evolution of the electric-field occupation.

Although the Z2 gauge theory is the simplest gauge theory, it contains several features highly relevant to the community: despite its simplicity, it exhibits a confinement–deconfinement transition, and it serves as a key testbed for both quantum computing approaches and classical tensor network methods. Additionally, the confinement phase can be further classified into the strongly confined and weakly confined phases, and the latter is the roughening phase, that has been an interesting topic not only in condensed matter physics but also in high-energy physics. Indeed, recent tensor network studies have revisited the roughening transition in this model, as in refs. [73, 83].

A brief comparison with these works [73, 83] is in order. Although all three studies use tensor network techniques, their methodologies differ substantially: ref. [73] employs iDMRG on an infinite cylinder,

and ref. [83] uses a two-dimensional tree tensor network ansatz, whereas the present work focuses on finite cylindrical systems with MPS. In addition, the authors of this manuscript provide detailed analyses of the string width and the Luscher term in the confining potential. The former seems to be a natural target to be simulated in the study of the roughening transition. Their discussion of the Luscher term is particularly noteworthy, as Hamiltonian-based studies may indeed feature a non-trivial speed of sound. Further investigation in the community would certainly be valuable. For these reasons, this work can be meaningfully distinguished from the earlier studies.

However, an additional point of comparison with refs. [73, 83] would be helpful for the community. Therefore, I recommend publication after the authors address the following question, including some minor ones.

We thank the reviewer for his/her thorough analysis of our work. In particular, we thank for the emphasis put on the novelties in our work as addressing the Lüscher term study and non-trivial speed of sound in Hamiltonian-based quantum simulations. Also we appreciate the comment on the dynamical study of the string width. We made a good use of the referee comments to further improve our manuscript which was already considered a distinguished work.

1) I would like to encourage the authors to add some further comments on the comparison of the roughening transition points obtained in their paper and those reported in refs. [73, 83]. In the current manuscript, the authors only state that "This yields a value $g_r \simeq 0.91$, which is comparable with the recent literature [73, 83]." I think it would be beneficial for the readers if the manuscript briefly discussed how these transition points were determined in these previous works, including the numerical values obtained in refs. [73, 83], the diagnostics used to locate the transition. Such a comparison would help clarify the consistency among different methods.

We thank the referee for their insight on the comparison with the literature. We have added a small paragraph in Sec. III D, following the referee suggestion, pointing out the different diagnostics and values found.

2) Although the authors estimate $g_r = 0.91$ from the fit based on eq. (22), with some remarks, is it possible to extract the transition point using another diagnostic? If so, providing such an alternative estimate could help validate their determination. For example, looking at Fig. 15, is there any possibility of identifying g_r from the central charge or its dependence on g ?

We thank the referee for this comment on the roughening point. We agree that different diagnostic would strengthen our results and we add a comment in Sec. V. As matter of fact, we looked at other methods as the fidelity susceptibility or, as suggested by the referee, the g dependence of the central charge c . Nonetheless, with the available data, the method following Eq.(22) in the main text was the most effective one, hence we presented it stand-alone.

3) Can the authors report the resulting critical exponents γ and ν obtained in Appendix B?

We thank the referee for this request. Nonetheless we regret to inform we have not been able to obtain an accurate estimation of γ , while the fit in Fig. 10d of Appendix B have yielded a value of $\nu = 0.57 \pm 0.17$ (which have been added to the text). This is most certainly due to the lack of sufficient data. An accurate calculation of the critical exponents, especially for a 2D quantum system, is a computational heavy task and it requires the computation of observables at many different system sizes. However, we wish to remark that the objective of Appendix B is to get an approximate value of the deconfining transition point, so that we are able to designate a confinement regime and to be able to distinguish it from the roughening point.

D. Reviewer #4

The authors investigate the phenomenon of string roughening in a Z2 lattice gauge theory using tensor network methods applied to cylindrical geometries. Their work provides a thorough numerical analysis, elucidating several aspects associated with string roughening. In particular, they examine the logarithmic divergence of the string width as the separation between static charges increases, the logarithmic scaling of the entanglement entropy, the Lüscher correction to the confining potential, and the restoration of

rotational symmetry that is broken by the lattice. The latter is especially interesting, as it enables them to estimate the transition point between the confined and rough-string regimes. Finally, they study the real-time dynamics of the string following quenches to different regions of the phase diagram.

Overall, I find the paper technically solid and the results significant, especially in light of rapid progress in quantum simulators capable of probing gauge-theory physics in two dimensions. I recommend the article for publication, provided the authors address the following points:

We thank the referee for his general overview of our work and which aspects makes it recommended for publication, as the kink mass for the determination of the roughening point and the general robustness of our numerics. We addressed the referee's points in the following text:

1) In Fig. 4, the authors show that the growth of the string width with respect to R is compatible with a logarithmic dependence. However, it is unclear whether Eq. (13) is fully satisfied. In particular, I would like to see whether the prefactor of the logarithm in their fit is consistent with the $1/\sigma$ dependence predicted by the equation.

We thank the referee for this comment. Indeed, we made the plot axes lin-log to better show the logarithmic dependence on R . However, we would like to point out that in most of the literature, the relation with the string tension as $1/\sigma$ arises from taking it as an energy scale (or building *ad hoc* the action of the model). Updating Eq. (13), we can think of the pre-factor to the logarithm as an intrinsic string width w_0^2 .

2) I am somewhat confused about the logarithmic dependence of the entanglement entropy on R . Could the authors clarify why such a dependence is expected specifically as a function of R , and how the central charge emerges in this context? I would expect the scaling to depend sensitively on how the bipartition is defined for the entropy calculation. Could the authors explain precisely how the cut is implemented? This point is also important because the duality transformation they employ may alter the structure of the bipartition.

We thank the referee for their insightful comment on the entanglement entropy. As pointed out in the second paragraph of Sec. III B, we know the string is effectively a (1+1)D system. Thus, we follow the work of Calabrese and Cardy, e.g. [98], to describe the entropy of our flux tube. In the reference, they fix the length of the chain L and take a (unique) bipartition A of length l . As we show in the first paragraph: “We consider a transverse cut of the cylinder in two equal parts of length $L/2$ and compute the EE for this bipartition...”, thus we fall into Eq (2) of the reference. By substituting $l = R/2$ in that formula we obtain Eq. (15) in the main text. As the referee suggested, extra care should be put on the structure of the bipartition. We think our MPS mapping grouping one column of the cylinder is ideal in this setting as, when performing an mps bipartition, we are effectively doing vertical cuts in the 2D dual lattice. On the latter, vertical cuts cross horizontal links, while on the direct lattice, a bipartition lies on vertical links. But, as shown in Appendix A, these relations are taken care of in our duality transformation.