

# An algorithm to generate two-dimensional critical lattice models using competing anyon condensation

Corresponding Author: Professor Ling Yan Hung

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

This manuscript develops a systematic construction of 2D critical lattice models expected to be described by a conformal field theory in the continuum. The authors define a special tensor network state whose overlap with the ground state of a 2+1D string-net flows to the partition function of a 2D critical model under RG. The unit cell of this state is assembled from competing Frobenius algebras that share a common module. From the algebra/module data they derive a sufficient criterion for second-order phase transitions and develop a refined condensation tree to predict the minimal symmetries present at criticality. The framework reproduces known results, including the A-series critical lattice models and the Ashkin–Teller phase diagram. Crucially, it also predicts new critical points in models constructed from the Haagerup fusion category.

Overall, this is an ambitious and timely contribution that combines category theory data with state-of-the-art tensor network techniques to search for new 2D conformal field theories. The Haagerup-based critical models are particularly interesting, given that no single-universe CFT with Haagerup symmetry is currently known.

The construction of the critical tensor network state from the competing Frobenius algebras is well motivated and methodologically novel. The numerical data is convincing and reproduces established results (though locating tricritical points numerically as e.g. in the Ashkin–Teller model is more challenging, as the authors note). The manuscript is clearly written and well structured, and the appendices provide a useful review of HGW string-nets. Several figures would benefit from larger fonts. For the Haagerup models, it would help to report uncertainty estimates for the central charge fits (also see suggested improvements). The bibliography appears adequate, but a brief comparison with prior work on critical 2d lattice models / anyon chains would help situate the contribution in the literature (also see suggested improvements).

## Suggested Improvements

1. Could you clarify how your tensor network models are related to the anyon chain/MPO models by Lootens et al. (arXiv:2112.09091) and the 2D stat-mech models by Aasen, Fendley, Mong (arXiv:2008.08598)? Can these earlier models be regarded as special cases of your framework?
2. Which of your Haagerup models (if any) correspond to the two Haagerup anyon chains studied in arXiv:2410.16356?
3. I'm confused by the statement that the minimal amount of symmetry in the critical state is  $Z(K)$ , where  $K$  is the subcategory of the first common ancestor node. In the anyon chain / 2d stat mech models mentioned above, the minimal amount of symmetry is the input category (or the Morita dual of the input fusion category w.r.t the module category), not the Drinfeld center of the input category.
4. For each Haagerup critical point in the two- and three-condensate sections, it would be very helpful to state the preserved symmetry explicitly and indicate whether it is the full Haagerup symmetry or a subgroup.
5. In the discussion section, line 326, it is stated that the anyon creation operator in a unit cell should correspond to the Landau order parameter. Could you elaborate more on this statement and clarify which creation operator you mean?
6. In the section on Generalised Kramer–Wannier duality: does the critical point obtained by the equal sum of normalized algebra states always match the self-dual point?
7. Could your numerics extract other CFT data beyond the central charge (e.g. Fermi velocity, scaling dimensions of primary fields)?
8. Uncertainty estimates for the central charge fits would be helpful, in particular for the Haagerup examples for which the central charges are not analytically known.
9. Regarding Eq. (8a): does the state  $\langle C_{ij} \rangle$  refer to the equal-sum state from Eq. (7)?
10. In line 206, it is stated that only 3 out of 4 Frobenius algebras sharing the module  $M=2$  emerge in Ashkin–Teller – why is this the case?

11. About Fig. 9 vs line 402 in the text: The right node in the figure is labeled by A1, but the text says A2.
12. In line 505, it is stated that  $D_c = 1$  for the A-series models. This is not obvious to me, a short justification would be helpful.
13. In line 506, it is mentioned that the Potts model can be created from the ZN string-net. Why not use the Tambara-Yamagami ZN string-net, which already includes the duality object?
14. The  $c \sim 1.3$  Haagerup example (competition between A0 and A4/A5/A6) appears in both “two competing” and “three competing” sections on pages 7 and 10. I would keep it only under “two competing” or justify the duplication.
15. In line 209, it is stated that the 3-state Potts model arises from competition between the A0 and A2 algebras of Haagerup, and that this matches the model by Bottini & Schafer-Nameki. However, Bottini & Schafer-Nameki realize the Haagerup symmetry in two decoupled Potts models, not a single Potts model. Please clarify this.
16. In the Appendix, line 81, a short definition of the full center of a Frobenius algebra would be helpful.

(Remarks on code availability)

I was able to install and run the code without any problems. I think the code would be a great resource for the community.

Reviewer #2

(Remarks to the Author)

This is an extremely interesting paper, which uses a set of very abstract ideas about generalized symmetries to construct very concrete 2d lattice models and predict analytically the locations of their critical points. The authors rediscover in a systematic way the RSOS models that realize the unitary minimal models, and by the same methods discover new classes of models that are not yet known. It should be published.

The prediction of the location of the critical points is similar in spirit to the prediction of Kramers and Wannier for the square lattice Ising model, in the sense that the authors identify two dual and competing sets of variables that are trying to condense, and the critical point arises from a balance between them. The examples the authors study, however, are not (as far as I know) known examples of lattice dualities. It would be useful and very interesting if the authors say a bit more about the question of whether there is actually a duality relation between the two sides of their transitions and what is its relation to known examples of Kramers-Wannier duality.

One interesting question is why the critical points realized this way always enjoy Lorentz invariance. Is it just because of lattice symmetries plus the irrelevance of the leading rotation-breaking operator?

(Remarks on code availability)

I was able to run the code.

Reviewer #3

(Remarks to the Author)

In this manuscript, the authors provide a systematic way to construct classical (stat-mech) lattice models in 2 dimensions that flow to 2d CFTs at low energies. This is done by using the SymTFT picture (which is related to the “strange correlator”) where the “bulk TQFT” is the Drinfeld center of an input fusion category, the “topological” boundary is a minimal gapped boundary of the bulk associated with an anyon condensation pattern, and the “physical” boundary is chosen to be a competing superposition of incompatible anyon condensations. With this algorithm, the authors reproduce various known CFTs (e.g., the A-series) and uncover new CFTs with Haagerup symmetries. The symmetries (not necessarily all of them) of the resulting CFTs are determined by a “refined condensation tree”. In addition to constructing new CFTs, this work provides a sufficient condition for a second-order transition. Moreover, many properties of the phase boundaries are reduced to the algebraic data in the input fusion category.

I strongly believe that this paper would appeal to a broad audience. The results are quite interesting, especially the sufficient condition for second-order transition and the new CFTs with Haagerup symmetries, and represent a significant advance in the exploration of 2d CFTs. They are presented clearly in well-chosen and familiar, as well as new, examples. As the authors mention, the connection between critical lattice models and strange correlators was already discussed in some examples in the literature, but that does not take the novelty away from this paper.

I do have some questions/comments for the authors:

1. The authors provide a nice candidate for  $|\Omega\rangle$  as the equal weight superposition of normalized condensed states that share a module, but it is not clear to me why this should always correspond to a phase transition point (first or second or even higher order). I see that this does agree with all the examples studied, but a proof that this is always the case would be nice. If this is hard, the authors should at least comment on why. As they note, when there is a generalized Kramers-Wannier duality, then the resulting critical coupling matches some of the states in their proposal. But without such a duality, there is no obvious reason why the other states in the proposal should be at the transition point. Should I understand their proposed candidate for  $|\Omega\rangle$  as a starting point near which one can hope to find the actual transition point?
2. Similar comments apply to the phase boundary where there is now a third state in the mix.
3. The argument for the sufficient condition (17) for a second-order transition is not very satisfactory. It is based on the naïve expectation that if we explore a phase space of dimension lower than the number of gapped phases, then the transitions are all second order. But this is certainly not true in general. One could, for example, construct a 2d lattice model with  $Z_2$  symmetry and with only one coupling constant such that the transition between the broken and unbroken phases is of first order. I understand that the given sufficient condition seems to be consistent with all the examples studied in this manuscript,

but there is no proof of this.

4. Continuing the last point, it is not clear why excluding a first-order transition implies a second-order transition. Why is a higher-order transition, say with dynamical exponent larger than 1, ruled out?

Some more technical points:

1. Why does the unit cell in (3) have only vertical links and diagonal links but no horizontal links of the square-octagon lattice? Based on what I understood, the vertical link in (3) could be vertical or horizontal, in which case the term "unit cell" is inappropriate.

2. In (6), the authors use a single  $r_i$  for each  $A_i$ , suggesting that the number of couplings  $D_c$  is the same as the number of gapped phases  $n_M$ . But in the discussion around (13), the counting of the number of coupling constants,  $D_{\text{unit}}$ , includes a coupling constant for each object  $a$  in  $A_i$ . This inconsistency should be explained.

Overall, I think the manuscript is suitable for publication in Nature Physics. I particularly like that fact that one can simply write down an explicit critical lattice model. However, the above questions/comments should be addressed before I can recommend its publication.

(Remarks on code availability)

I will start by saying that I have done little large-scale numerical computations before. The README.md file was descriptive enough to understand what the individual files are for. The main.py code has enough documentation in the form of comments that help navigate the code. However, the key technical code in rg\_methods.py is far less readable. Some of the comments left there were in a language other than English. Including more documentation of this fairly technical code would definitely help the reader in understanding the code and reproducing the results.

I ran the code successfully, and the output looks consistent with that of the run by the authors on September 26, 2025. I should note that I have not noticed any discussion of uncertainties or error bars in the code or in the manuscript.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Thank you for the careful revision and point-by-point response. I have read both and feel that my questions/comments, as well as those raised by the other reviewers, have been addressed. In particular, the new plots of the numerical central charge under RG flow are informative, and the section on cutting down phase-space is much clearer.

For completeness, I note two minor points where I remain slightly unconvinced by the rebuttal. First, my Question 1 is not directly answered: the response compares 2D statistical-mechanics models and 1+1D anyon chains, but does not explicitly situate the tensor-network models studied here relative to either class. Second, I still find the manuscript's use of "symmetry" potentially confusing, insofar as it includes "hidden symmetries" that can be condensed or broken. These are minor issues, however, and I am happy with the manuscript in its present form.

(Remarks on code availability)

Thanks for improving the documentation of the code.

Reviewer #2

(Remarks to the Author)

This extremely interesting paper should be published. I have no further comments.

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)

The referees raised a lot of concerns and the authors addressed all of them. Most of their responses are satisfactory. However, I am not convinced that the revised manuscript is suitable for publication.

1. The authors clarified what they mean by  $D_c$ , which is helpful, but I am not convinced with their argument for continuous transition when  $n_M > D_c$ . As I said in my last report, just because the number of parameters/couplings is smaller than the number of gapped phases, the transition need not be continuous in general. The authors reason that a first-order transition requires "extra degrees of freedom to allow for degeneracy", but I do not see why this should be the case. It is very interesting that the statement " $n_M > D_c \Rightarrow$  continuous transition" seems to hold in the examples studied in this paper (based on numerics), but that doesn't constitute a proof.

2. Table 1 in the authors' response lists the changes in central charges. These changes seem very drastic and are alluded to

their improved RG algorithm. Moreover, some of the transitions which were categorized as continuous in the previous version are now deemed first-order. The authors now provide uncertainty estimates based on an eight-step plateau in their RG procedure, which suggests that the new values are more stable. My question is, how trustworthy are the new results? Looking at the plots of C in the supplementary material, the plateaus in Figure 11 aren't as flat as those in Figure 10. For example, the purple curve in the top-left panel of Figure 11 seems to be on a downward trend instead of stabilizing around 0.779 as quoted in the text.

3. In Figure 4 in the main text, I think A\_2 and A\_3 should be exchanged to be consistent with the discussion in the text. I urge the authors to verify other figures too.

(Remarks on code availability)

I could not test run the code but I can see that the authors have addressed the issues I raised.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Thank you to the authors for addressing my previous minor comments. I have no further comments and would be pleased to see the manuscript published.

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)

I am happy with the authors' responses to all the questions and I recommend the current manuscript's publication.

(Remarks on code availability)

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# Reply

Dear Editor,

We would like to thank the referees for their meticulous reading of our paper and for raising many thoughtful questions and suggestions. In the following we will reply in detail to these points raised, and detail the changes that we made in the manuscript accordingly.

## 1 Reply to Reviewer 1

1. Could you clarify how your tensor network models are related to the anyon chain/MPO models by Lootens et al. (arXiv:2112.09091) and the 2D stat-mech models by Aasen, Fendley, Mong (arXiv:2008.08598)? Can these earlier models be regarded as special cases of your framework?

**Reply:** The anyon chain model such as those studied in (arXiv:2112.09091 or originally in [5]) is in the Hamiltonian formulation and the strange correlator, a term coined in [8]; however, what is identical to [6,7 (arXiv:2008.08598)] lies in the partition function formulation, i.e., the Hamiltonian formulation is the inhomogenous limit of the statistical models. This is also described in section 3.5 of 2008.08598. The Hilbert space of the anyon chain correspond to a zigzag cauchy slice in the 2D statistical partition function (Figure 4 in 2008.08598). Indeed this is not widely discussed in the literature. We have included a footnote in the introduction in the main text to emphasize this point.

2. Which of your Haagerup models (if any) correspond to the two Haagerup anyon chains studied in arXiv:2410.16356?

**Reply:** There are three models in 2410.16356. Their Model 1 is expressible as linear combinations of projectors as in their (7). These Hamiltonians automatically preserve all Haagerup topological symmetries and allow only for spontaneous symmetry breaking. Their model 1 however does not appear to correspond to a CFT, as it is checked in the computation of entanglement entropy. It is unclear if that model is right at criticality, but it seems to be fairly close in phase space to our model where the module is chosen to be  $\rho$ . Their model 2 is engineered to preserve integrability but is made to explicitly break some of the Haagerup symmetries, as explained in the text there. It is not constructed from terms in (7). The critical model that has an approximate central charge of  $c=1.5$  constructed there is therefore outside of the space of models obtainable in this paper.

3. I'm confused by the statement that the minimal amount of symmetry in the critical state is  $Z(K)$ , where  $K$  is the subcategory of the first common ancestor node. In the anyon chain / 2d stat mech models mentioned above, the minimal amount of

symmetry is the input category (or the Morita dual of the input fusion category w.r.t the module category), not the Drinfeld center of the input category.

**Reply:** Indeed the different species of topological operators are enumerated via the input fusion category, or its Morita equivalent ones thereof, the Drinfeld center encapsulates the full collection of *hidden* symmetries. This was explained in Ji and Wen [14] (arXiv:1912.13492), which they termed "categorical symmetry". The idea is that for a theory with a given symmetry, there exists other duality frames, and these different duality frames are best understood using the Drinfeld center of the topological symmetries. They take the Ising model as example, where the  $\mathbb{Z}_2$  symmetry is accompanied by a magnetic  $\mathbb{Z}_2$  symmetry which is hidden, but made visible by gauging the  $\mathbb{Z}_2$  symmetry. The action of the magnetic symmetry is trivial in the original frame, but non-trivial in the dual frame, where the electric symmetry action becomes trivial. In the topological holographic principle framework/symTFT/sandwich, each of the symmetric model is constructed by taking the path-integral of the TQFT corresponding to the Drinfeld center of the topological symmetry, and imposing a dynamical boundary and a topological boundary condition respectively, on the two sides of the sandwich. The topological symmetry of the final model is reduced to the "input category" because the topological boundary corresponds to spontaneous symmetry breaking of part of the symmetries (either electric condensation or magnetic condensation describing the two different duality frames). Now it is observed that at criticality, these models become 'self-dual' at least for cases bearing an obvious Kramers-Wannier duality. In the sandwich construction, it corresponds to the "dynamical boundary" becoming one that *preserves both electric and magnetic symmetries* encoded in the Drinfeld center. This was proved in Ising by Levin [28] that both symmetries are preserved underlies criticality. This is the physics that we make use of – i.e. preserving a pair of non-commuting symmetries at the dynamical boundary of the sandwich – in engineering critical models. In our construction, the ultimate collection of topological symmetries preserved in the models are given by the collection of bulk anyons that did not condense in both the dynamical and the topological boundaries. Note however that the change of the topological boundary does not change the dynamics of the theory – that is a change of the duality frame as described above, such as the change between the electric and magnetic  $\mathbb{Z}_2$  frame in the Ising spin model. Therefore by analysing the dynamical boundary we were able to determine what the model is. We have included in the introduction a brief discussion of these statements.

4. For each Haagerup critical point in the two- and three-condensate sections, it would be very helpful to state the preserved symmetry explicitly and indicate whether it is the full Haagerup symmetry or a subgroup.

**Reply:** As described in the previous question, the full set of topological symmetries are determined by both the dynamical and topological symmetries. In our engineering

of gapless dynamical boundary conditions, we focused on the symmetries preserved (i.e. anyons that remain un-condensed) there. As discussed in the main-text, the minimal amount of symmetries guaranteed are determined by the first common ancestor of the competing condensates in the condensation tree. We have included in the table the corresponding set of symmetries in each critical point there. This information is independent of the choice of the topological boundary encoded in the specific choice of  $|\Psi\rangle$ .

5. In the discussion section, line 326, it is stated that the anyon creation operator in a unit cell should correspond to the Landau order parameter. Could you elaborate more on this statement and clarify which creation operator you mean?

**Reply:** Our model is formulated in the extended Hu–Geer–Wu (HGW) version of the string-net construction, which enlarges the Hilbert space relative to the original Levin–Wen model so that all anyonic excitations can be distinguished explicitly. In this framework, an “anyon creation operator” means an operator that creates an anyon–anti-anyon pair (possibly carrying different gauge fluxes) respectively in two plaquettes of the lattice. In the present context in line 326, a creation operator in a unit cell refers specifically to the operator that creates such a pair in the two octagonal plaquettes separated by this unit cell.

An anyon-condensation process can be understood as forming a coherent combination of the creation operators of the anyons that condense. This combination defines a projector that maps the ground state of the parent (uncondensed) string-net model to the ground state of the condensed theory [ArXiv:2506.05319]. The resultant condensed ground state may be viewed as a coherent state populated by arbitrarily many condensed anyons throughout the lattice, directly analogous to Cooper-pair condensation in a superconductor or Higgs condensation in a gauge theory. From this viewpoint, the expectation value of the creation operator for a condensed anyon changes qualitatively across the transition: in the parent topological order, where that anyon labels a nontrivial topological sector, the corresponding expectation value is effectively zero, while in the condensed phase it becomes nonzero. Therefore, the creation operator (more precisely, the appropriately defined local/pair creation operator in a unit cell) of the condensed anyon plays the role of a Landau order parameter that tracks the condensation-driven phase transition, in close analogy with the Higgs order parameter in the Standard Model.

6. In the section on Generalised Kramer-Wannier duality: does the critical point obtained by the equal sum of normalized algebra states always match the self-dual point?

**Reply:** Yes it does. In the infinite set of examples of the A-series and the N-state potts model for all N, and also in the examples where the generalised Kramer-Wannier duality can be defined, the self-dual point always coincide with the equal-weight sum of normalized algebras. We feel that on the one hand, self-duality is

seen as an explanation of phase transition. On the other, which seems to be logically independent, this point coincides with a point where we quantitatively have an *equal* amount of two different condensates in the same cell – equality defined by the notion of trace in the fusion tensor category. This indeed was what allowed one to generalise to cases without obvious notion of Kramer-Wannier duality, where the phase transition for every case checked remains at the equal-weight point for every pair of algebras sharing a module. We have emphasized this point in the results section.

7. Could your numerics extract other CFT data beyond the central charge (e.g. Fermi velocity, scaling dimensions of primary fields)?

**Reply:** Indeed one can try to extract the spectrum numerically from the transfer matrix. This is illustrated for example in our previous work Phys.Rev.X 15 (2025) 1, 011073 by one of the authors, and they would give more data about the spectrum of the purported CFT. Particularly the spectrum with lower conformal dimensions can be extracted, alongside their spins. Accuracy depends on the bond dimensions and in general require a lot more computing power, which will be the focus in an up-coming work. The spectrum of the transfer matrix is indeed very important and powerful in deciphering the details of the purported CFT than the central charge alone.

8. Uncertainty estimates for the central charge fits would be helpful, in particular for the Haagerup examples for which the central charges are not analytically known.

**Reply:**

There is no widely accepted method to quantify the numerical uncertainty in central charges computed using TNR-based approaches. We propose estimating the uncertainty  $\delta C$  by considering the maximal deviation within a selected plateau along the tensor RG flow of the numerical central charge.  $\delta C$  quantifies the numerical stability, and are included in the summary tables. We include  $\delta C$  in the summary tables. Plots of the numerical central charge against RG steps are also provided in the supplementary materials.

9. Regarding Eq. (8a): does the state  $\langle C_{ij} |$  refer to the equal-sum state from Eq. (7)?

**Reply:** Yes. We have changed the notation in Eq. (7) to  $\langle C_{ij} |$ .

10. In line 206, it is stated that only 3 out of 4 Frobenius algebras sharing the module  $M=2$  emerge in Ashkin-Teller – why is this the case?

**Reply:** The three algebra satisfy  $A_0 \subset A_2 \subset A_3$ , and therefore they naturally share the same module  $M = 2$ . The other frobenius algebra is not a sub-algebra of any of these 3 algebras. In our lattice, the module lines in the dual graph always forms a closed loop (a small square). Using the property of module actions reviewed in

the supplementary material, one can show that this graph can be related (using F moves) to one without the modules loops but with only the algebra. Each algebra is already a *stable RG fixed point* in that the vertex would remain invariant under any further F-moves or removing of loops of the algebra. i.e. putting one algebra in a unit cell is only one F-move step away from an exact RG fixed point defined by a given algebra. This point was mentioned near equation (4) in the manuscript. Similarly, one can show that if all the interpolating algebras share this module, these algebras would all remain interpolating in the new boundary state in  $\langle \Omega |$  after the F moves. It is equivalent to say that the fixed point defined by an algebra *not sharing* this module is very far in the space of RG. This means that the IR phase could not involve this other condensate when we interpolate the algebras with these positive coefficients, each algebra defining a nearby RG fixed point. We mention this in the paragraph between equation 16 and 17 in the section "Cutting Down Phase Space to Avoid First Order Phase Transitions". We have tried to improve that paragraph too.

11. About Fig. 9 vs line 402 in the text: The right node in the figure is labeled by A1, but the text says A2.

**Reply:** We thank the referee for his/her meiticolous reading. We have corrected this typo.

12. In line 505, it is stated that  $D_c = 1$  for the A-series models. This is not obvious to me, a short justification would be helpful.

**Reply:** The number of degrees of freedom is computed as follows. This counting lies inside a given "unit-cell". The idea of the current paper is to engineer a comensurate combination of competing condensate in individual cell before putting them together and ensuring that the equilibrium is not destroyed when different cells are connected. This is achieved by choosing module of the competing algebras at the boundary of the unit cell. The non-degenerate module action of each algebra ensures that different vertices where algebra meets module are independent. As a result, we can focus on tuning an equilibrium point in individual unit cell. In a unit cell, it is described by a finite number of edges, and thus accomodates a finite number of degrees of freedom. This number of degrees of freedom is essentially roughly  $D_c$ . In the case of the A-models, where the module is chosen to be the  $j = 1/2$  object, the middle edge in the unit cell can either be  $j = 0, 1$ . Therefore the total vector space is 2dimension. Each cell is a linear combination of  $j = 0$  and  $j = 1$  in the middle edge, and the overall normalisation only changes the overall normalisation of the resultant partition function. Therefore  $D_c = 2 - 1 = 1$ , where the overall normalisation is a redundant dof that is removed from  $D_c$ . More generally,  $D_c$  counts the number of independent vertices that appear in the unit cell up to normalisation. We note that this is different from counting the number of couplings we introduced into the interpolation. What is important is the number of different vertices that are actually

available in the unit cell, which is the maximum dimension of vector space describing this little world under the confines of the module. This is described in some detail in the method section. We have included extra clarifications in the main text (right after the section "Critical States with Two Competing Condensates") and a newly added extended clarification in method sections (in the sub-section Cutting Down Phase Space to Avoid First Order Phase Transitions).

13. In line 506, it is mentioned that the Potts model can be created from the ZN string-net. Why not use the Tambara-Yamagami ZN string-net, which already includes the duality object?

**Reply:** We can indeed use the Tambara-Yamagami ZN string-net. A special example is the recovery of the Ising spin model using the  $A_{2+1}$  category (which is the special case of the Tambara-Yamagami Z2 string net) and the Z2 model. In either input category, we obtain a corresponding pair of Frobenius algebra. It is possible to recover the same model through different input categories, but from either perspective one could read off from the condensation tree that it is the e-m competition of the D(Z2) (sub-) category that is at play, allowing one to see that they produce the same model. This discussion of the Ising model would readily generalises to ZN vs Tambara-Yamagami ZN models.

14. The  $c \sim 1.3$  Haagerup example (competition between A0 and A4/A5/A6) appears in both "two competing" and "three competing" sections on pages 7 and 10. I would keep it only under "two competing" or justify the duplication.

**Reply:** Yes, we have made the change in the revised manuscript by keeping it only under "two competing".

15. In line 209, it is stated that the 3-state Potts model arises from competition between the A0 and A2 algebras of Haagerup, and that this matches the model by Bottini & Schafer-Nameki. However, Bottini & Schafer-Nameki realize the Haagerup symmetry in two decoupled Potts models, not a single Potts model. Please clarify this.

**Reply:** We thank the referee for pointing this out. The referee is correct that this critical point is realized as two decoupled direct sum of 3-state Potts models. The number of direct sum of a given model depends on both boundaries of the "sandwich", where the strange correlator is a realisation. For example, the realisation of the Ising spin model via the  $A_{2+1}$  also produces a *direct sum* of two copies of the Ising models. The technical reason is related to the fact the Ising model gauged the e-m duality. (In this case a detailed explanation is given for example in Verstraete et al our ref [8], near their equation 3) The competition of the pair of condensates leads to the Ising critical point, but the number of copies in the direct sum depends on some other details. We did not expand on that in this paper since it involves details of the

tensor network basis chosen for  $|\Psi\rangle$ , as opposed to universal data in the Frobenius algebra (We included more such discussion in our previous arxiv version section 3.5), and that would involve a digression about how different  $|\Psi\rangle$  can be constructed, which is a topic discussed in Fuchs, Verstraete et al Ref [27] – this is what controls the topological boundary of the "sandwich" mentioned above. To avoid clutter and to keep the discussion focused on the Frobenius algebra. But to avoid confusion particularly since we mentioned comparison with Bottini & Schafer-Nameki in the main text, we have included a clarification there about the number of copies involved.

16. In the Appendix, line 81, a short definition of the full center of a Frobenius algebra would be helpful.

**Reply:** As suggested, we have included a short definition of the full center of Frobenius algebra.

## 2 Reply to Reviewer 2

1. The prediction of the location of the critical points is similar in spirit to the prediction of Kramers and Wannier for the square lattice Ising model, in the sense that the authors identify two dual and competing sets of variables that are trying to condense, and the critical point arises from a balance between them. The examples the authors study, however, are not (as far as I know) known examples of lattice dualities. It would be useful and very interesting if the authors say a bit more about the question of whether there is actually a duality relation between the two sides of their transitions and what is its relation to known examples of Kramers-Wannier duality.

**Reply:** We thank the referee for making this important point, which is similar in spirit to question 1 of Referee 1 above too. Our understanding is that for any pair of Lagrangian condensates containing non-commuting anyons, they are analogues of electric-magnetic pairs in some generalised sense. In that sense, these equilibrium points should be some kind of Kramers-Wannier self-duality. As explained in our reply to referee 1 question 1, in all our examples where a conventional KW duality is defined, our equilibrium point is automatically coinciding with the known self-dual point as expected. (This point is now emphasized in the discussion section.) It was suggested to us by Prof. Xie Chen (Caltech), that perhaps this implies the existence a finite depth circuit in general to convert between one condensate to another, which would perhaps be an ultimate way of formulating this duality in complete generality. We will leave these ideas to the future.

2. One interesting question is why the critical points realized this way always enjoy Lorentz invariance. Is it just because of lattice symmetries plus the irrelevance of the leading rotation-breaking operator?

**Reply:** This is indeed a very good question. It seems that the square lattice with discrete translation and  $\pi/2$  rotation symmetry might be promoted to an emergent continuous Lorentz invariance (or rotation/translation in Euclidean space) at criticality, it is not guaranteed a priori. Well known classes of models such as the A-models also appear to suggest that emergent Lorentz invariance is quite natural in a square lattice. In particular  $\pi/2$  rotation would make Lifshitz type theories with  $t$  distinguished from  $x$  unlikely. Nonetheless, we do not rule out the possibility of some other gapless phases in principle.

### 3 Reply to Reviewer 3

1. The authors provide a nice candidate for  $|\Omega\rangle$  as the equal weight superposition of normalized condensed states that share a module, but it is not clear to me why this should always correspond to a phase transition point (first or second or even higher order). I see that this does agree with all the examples studied, but a proof that this is always the case would be nice. If this is hard, the authors should at least comment on why. As they note, when there is a generalized Kramers-Wannier duality, then the resulting critical coupling matches some of the states in their proposal. But without such a duality, there is no obvious reason why the other states in the proposal should be at the transition point. Should I understand their proposed candidate for  $|\Omega\rangle$  as a starting point near which one can hope to find the actual transition point? Similar comments apply to the phase boundary where there is now a third state in the mix.

**Reply:** In known cases of Kramers-Wannier self dual points where phase transition occurs, the self-dual point is located at the symmetric point between “electric” and “magnetic” symmetries. Explicitly in the case of the Ising spin model for example, the transfer matrix has a  $Z_2$  symmetry (flipping of spins) that is spontaneously broken below the critical temperature. The magnetic  $Z_2$  symmetry is hidden, unless we make use of the “domain wall” degree of freedom and in the high temperature limit it is spontaneously broken, where as the original  $Z_2$  symmetry is preserved. The electric and magnetic symmetry can be captured by the  $Z_2$  quantum double model in 2+1 dimensions, where the  $Z_2$  electric and magnetic symmetries correspond to non-commuting electric and magnetic anyons in the 3D tqft. That Kramers-Wannier self-duality lead to phase transition follows from the physics that non-commuting anyons, the electric and magnetic anyon in this case, cannot be spontaneously broken (acquires expectation value /condense) at the same time. Self-duality is a way to ensure that neither can condense. One input in this paper is to phrase this self-duality in terms of data of the 3D tqft. At the self-dual point in known examples, it turns out to correspond to the equal weight sum of a pair of normalised frobenius algebras – a normalisation that is naturally defined using the trace of the fusion category data. Each algebra describes a maximal condensate so that two algebras would describe condensates with non-commuting anyons. Therefore, our input gives a *new physical interpretation* of the Kramers-Wannier self-dual point in terms of anyonic data – and

this is the key to generalising this physics to cases where Kramers-Wannier duality is not obviously defined. The underlying physics is that whenever two non-commuting condensates normalised according to the trace of the fusion category are summed with equal weight, neither of them can win over the other. As a result, neither can condense, leading to a phase transition. It is possible that there are more generalisations of the notion of KW duality to encapsulate all the pair-competition cases discussed in this paper – but our construction is more physically fundamental than the notion of KW duality. We propose that equation (7) is the precise location of phase transition between a pair of condensate which is confirmed in all cases (particularly those integrable models with known KW duality) and checked numerically when KW duality is not known. We now include further emphasis and clarifications of this point in the discussion section.

In the presence of more than 2 algebras allowed in a given unit cell, the phase space accommodated in the cell is higher. Tricritical point is not simply the equal weight sum of three normalised algebras - that is understandable because the three algebras have different distances from each other in the condensation tree, and an algebra may prefer some condensate more than others. In that case, our state  $\langle \Omega |$  becomes more of an ansatz that helps us to search for the tricritical point – what makes it more powerful is that the phase boundaries can also be largely (but not completely) determined in equation (8a), which also admits a natural interpretation in the 3D tqft. In the competition of more than 3 condensates therefore, our interpolation in equation (6) is more of an ansatz. The meeting point of three phase boundaries traced out in (8a) is, in all cases checked, very close to the numerically determined tri-critical point, making it an excellent ansatz for locating these higher transition points. We have also included clarifications of this point in the discussions.

2. The argument for the sufficient condition (17) for a second-order transition is not very satisfactory. It is based on the naïve expectation that if we explore a phase space of dimension lower than the number of gapped phases, then the transitions are all second order. But this is certainly not true in general. One could, for example, construct a 2d lattice model with  $Z_2$  symmetry and with only one coupling constant such that the transition between the broken and unbroken phases is of first order. I understand that the given sufficient condition seems to be consistent with all the examples studied in this manuscript, but there is no proof of this.

**Reply:** We thank the referee for raising this point about the counting. We were cutting down the length of our paper when we submitted our paper and we realised indeed the discussion of the engineering of higher order phase transition as it stands is not clear. Let us clarify the explanation in the text. The counting  $D_c$ , as the referee correctly observed, is not the naive number of couplings we introduced into the interpolation in equation (6). The number of couplings we introduced in the interpolation is equal to the number of phases we considered competing in the unit cell. Our

argument is thus not related to this number. Rather, we made the following observation – the ansatz we have chosen surrounds each unit cell with the shared module of competing algebras. As we have also made a similar explanation to referee 1 above who raised a related point, the module ensures that the condensates in different unit cells are essentially decoupled, since the module action of the algebra at each vertex is non-degenerate. Our strategy of engineering critical point is thus to engineer a commensurate point at each unit cell, and the effect of the module at the boundary is to ensure that the equilibrium is not affected by the background formed by all the other cells. In each of this isolated cell, the dimension of phase space is given by the aggregate dimension of different vertices allowed in this unit cell. Concretely in the A-model where the module edge is chosen to be  $M = 1$ , the central edge can only be 0, 2. The unit cell has a 2 dimensional space. This counting is a restriction of how “descriptive” a unit cell is. In this cell, it happens that also exactly 2 algebras corresponding to two gapped phases share this module  $M = 1$ . As we interpolate between the object 0 and 2, the coupling is only changing the distance we are from either of the gapped phase. At the critical point, we are supposedly at equidistant from either condensate and now to describe a “first order phase transition” *inside this tiny world of the unit cell* (an abuse of the terms because the unit cell is small), we need extra degenerate degrees of freedom to express this distinguishability of the two coexisting condensates – but we do not have any more degrees of freedom since the total dimension of space is 2, and yet overall normalisation does not matter. Therefore we see that *within* the unit cell, the two condensate right at the commensurate point is not distinguishable. This is analogous to the proof of Levin in [29] that Ising model has to be either gapless or degenerate when the electric/magnetic symmetry are both preserved – we argue that degeneracy should be observed in these individual isolated unit cell. This is very different from the number of couplings that we introduced into the interpolation, which forms a sub-space of the phase space *describable inside the unit cell*. The dimension of the latter is an upper bound to the variety of scenarios that can possibly happen. Whenever the number of independent distinct condensates accommodated in the unit cell exceeds this maximal dimension, there is no room left for degeneracy needed for first order phase transition inside this cell. We have included extra clarifications in the section where the unit cell is introduced, and dramatically expanded the discussion in the Method section where this cutting down of degrees of freedom is explained.

3. Continuing the last point, it is not clear why excluding a first-order transition implies a second-order transition. Why is a higher-order transition, say with dynamical exponent larger than 1, ruled out?

**Reply:** The referee is right. We really meant continuous phase transition rather than strictly 2nd order phase transition. We have corrected this misleading use of the term in the revised manuscript, and replaced 2nd order phase transition by

continuous phase transitions.

4. Why does the unit cell in (3) have only vertical links and diagonal links but no horizontal links of the square-octagon lattice? Based on what I understood, the vertical link in (3) could be vertical or horizontal, in which case the term “unit cell” is inappropriate.

**Reply:** Certainly, local configurations with horizontal links are used too. In this paper we restrict our attention to models where these horizontal links are obtained from the vertical ones by  $\pi/2$  rotation. It is indeed an abuse of language to use the term “unit cell”. We have clarified this point and included a warning in the introduction – while preserving this notation to avoid clutter.

5. In (6), the authors use a single  $r_i$  for each  $A_i$ , suggesting that the number of couplings  $D_c$  is the same as the number of gapped phases  $n_M$ . But in the discussion around (13), the counting of the number of coupling constants,  $D_{\text{unit}}$ , includes a coupling constant for each object  $a$  in  $A_i$ . This inconsistency should be explained.

**Reply:** We thank the referee for his/her careful reading of the manuscript. This is related to point 2 the referee raised above. What we call “number of couplings” in a unit cell  $D_c$  is more appropriately the (effective) dimension of phase-space of a single unit cell, which is the upper bound of the number of couplings one can introduce into the small world of the unit cell. We have since clarified this point in multiple places, and expanded and improved our discussions in the Method section that explains  $D_c$ .

6. The key technical code in `rg_methods.py` is far less readable. Some of the comments left there were in a language other than English. Including more documentation of this fairly technical code would definitely help the reader in understanding the code and reproducing the results.

**Reply:** We have updated the codes and provided more comments.

7. I should note that I have not noticed any discussion of uncertainties or error bars in the code or in the manuscript.

**Reply:** We added the estimation of uncertainties using the RG flow of the numerical central charge.

## 4 A summary of the changes to the manuscript

We have made a number of changes to both our main-text and supplementary materials. We have highlighted these changes in red. (When a section is changed substantially, we highlight in red the section title.) We have corrected a number of typos, some of which pointed out by the referees. In the following, we will summarise more important changes to the manuscript.

1. A footnote in the introduction emphasizing the connection to the anyon chain model.
2. An added brief discussion of the notion of a hidden categorical symmetry described by the drinfeld center of the fusion category describing the topological defects of the theory in the introduction.
3. Added items in the tables in the results section of the first common ancestor of the competing condensates
4. Added emphasis that the KW self-dual point of known models match with the equal weight point of a pair of competing condensate in the results section, and also in the discussion section. We also emphasized that our results at present produces approximate location of higher critical points, which are important starting point for search for them.
5. Included lots of clarifications of the counting of degrees of freedom in the unit cell and its relation to continuous phase transition in the Method section.
6. Corrected the use of "second order phase transition" to "continuous phase transitions" in the text.
7. Included a clarification in the introduction regarding the meaning of a "unit cell".
8. Increased the font size in all figures.
9. Added uncertainty estimations  $\delta C$  for all numerically extracted central charges. It is defined as the maximal deviation within a plateau.
10. Added a new section in the Supplementary Material that exhibits the RG flows of numerically extracted central charges.
11. Added a subsection in the Supplementary Material to justify the use of non-converged central charges in the phase diagrams.
12. Updated the author order from alphabetical to reflect contributions, with the agreement of all authors.
13. We have carried out a systematic upgrade of the numerical central charge calculations by increasing the bond dimension and extending the RG flow depth. This improvement was enabled by implementing customized sparse tensor contraction routines

and additional computational resources. Importantly, these upgrades do not alter any of the main ideas of the paper.

For families of simple lattice models corresponding to minimal CFTs, including those in the A-series (such as the Ising model) and the  $N$ -state Potts models, the numerical central charges are already highly stable and accurate. We now explicitly provide their numerical central charges. The upgrade was however important in improving the accuracy of these exotic and potentially irrational critical points with  $c > 1$ , where the analysis with low bond dimensions, or those based on entanglement entropy was less reliable.

We summarize the changes observed for these exotic CFT candidates in Table 1. Accordingly, we have updated the related discussions in the sections **Critical States with Two Competing Condensates** and **3-algebra competition in the Haagerup  $H_3$  strange correlator**, including the analysis of the critical points, the phase diagrams, and the summary table of critical models. In line with the referee’s suggestions, we have also added an explicit estimate of the central-charge uncertainty and identified the symmetries preserved at each phase transition.

**Table 1:** Summary of substantial numerical changes.

| Cat   | $\mathcal{A}_i, \mathcal{A}_j(, \mathcal{A}_k)$           | Type                      | $C$                      |
|-------|-----------------------------------------------------------|---------------------------|--------------------------|
| $A_4$ | $0, 0 \oplus 1 \oplus 2 \oplus 3$                         | Cont. $\rightarrow$ Weak  | $1.2 \rightarrow 1.15$   |
| $A_4$ | $0 \oplus 2, 0 \oplus 3$                                  | Cont. $\rightarrow$ Weak  | $1.2 \rightarrow 1.15$   |
| $A_5$ | $0 \oplus 2, 0 \oplus 4$                                  | Cont. $\rightarrow$ Weak  | $1.3 \rightarrow 1.2$    |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_1$                            | Cont. $\rightarrow$ First | $2.1 \rightarrow -$      |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_{4,5,6}$                      | Cont. $\rightarrow$ Weak  | 1.3                      |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_1, \mathcal{A}_2$             | Cont.                     | $1.8 \rightarrow 0.79$   |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_2, \mathcal{A}_{4,5,6}$       | Cont.                     | $1.8 \rightarrow 0.779$  |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_1, \mathcal{A}_{4,5,6}$       | Cont.                     | $2.1 \rightarrow 1.217$  |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_{4,5,6}, \mathcal{A}_{5,6,4}$ | Cont.                     | $1.8 \rightarrow 1.3424$ |
| $H_3$ | $\mathcal{A}_2, \mathcal{A}_{4,5,6}, \mathcal{A}_{5,6,4}$ | Cont. $\rightarrow$ First | $2.1 \rightarrow -$      |

We thank the referees again for the many thoughtful comments. We hope that our changes would render our manuscript now suitable for Nature Physics.

Best regards,

The Authors

# Reply

Dear Editor,

We would like to thank the referees for reading our replies carefully, and referees 1,2 for their encouragements. We understand that there are some other questions and issues, and in the following we would like to try our best to respond to them.

## Reply to Reviewer 1

1. First, my Question 1 is not directly answered: the response compares 2D statistical-mechanics models and 1+1D anyon chains, but does not explicitly situate the tensor-network models studied here relative to either class.

**Reply:** The tensor network model studied here in the special case of the two condensate competition in the A-series between the algebra 0 and 0+2 reproduces the A-series integrable lattice models (bottom of page 6). We also have the N-state Potts model using the  $Z_N$  TQFT. These are also partially studied in Aasen, Fendley and Mong in [7-8] as boundaries of 3D TQFT. As explained in [7-8], the anyon chain models correspond to taking the inhomogenous limit of the models in [7-8]. Some of these limits are known but not all have been done in detail explicitly. However many of the models discussed in this paper are generalisations of [7-8], and has never been seen in the literature. e.g. most of the models involving 3 algebra competitions, and also many of those with Haagerup symmetries.

2. Second, I still find the manuscript's use of "symmetry" potentially confusing, insofar as it includes "hidden symmetries" that can be condensed or broken. These are minor issues, however, and I am happy with the manuscript in its present form.

**Reply:** We agree that this is potentially confusing although we have not been able to come up with very succinct way of referring to them. We thank the referee for his/her comments, encouragements and understanding.

## Reply to Reviewer 2

1. This extremely interesting paper should be published. I have no further comments.

## Reply to Reviewer 3

1. The authors clarified what they mean by  $D_c$ , which is helpful, but I am not convinced with their argument for continuous transition when  $n_M > D_c$ . As I said in my last report, just because the number of parameters/couplings is smaller than the number of gapped phases, the transition need not be continuous in general. The authors reason that a first-order transition requires "extra degrees of freedom to allow for degeneracy", but I do not see why this should be the case. It is very interesting

that the statement “ $n_M > D_c \Rightarrow$  continuous transition” seems to hold in the examples studied in this paper (based on numerics), but that doesn’t constitute a proof.

**Reply:** We thank the referee for pressing on this point, which is important. We agree with the referee that the number of parameters/couplings introduced in the theory has no bearing on the order of phase transition. This is indeed the case. The counting that we have is not this number, but rather the dimension of *phase space* in this tiny unit of two triangles. It is shielded from the other units by the surrounding shared module of the competing algebras. The size of phase space, however is important for determining the presence of higher order phase transitions. In first-order phase transitions (such as water-ice coexistence), the phase transition occurs at a point where the free energy of water and ice happens to be the same. But these 2 phases are distinguishable – meaning that there must exist parameters so that when we vary them slightly, we get super-cooled water or super-heated ice, in which the two phases become distinguishable. When it comes to continuous phase transition, it means that in the vicinity of the critical point the two phases remain indistinguishable. In the unit cell, the phase space does not contain any extra parameters to distinguish the phases – and thus the transition is continuous. The small cell does not carry enough degrees of freedom to describe co-existence of 2 distinguishable phases. Therefore, the crucial ingredient in the above discussion is the role of module in separating the condensates in different cells, without which the number of couplings has no bearing on the continuity of the transition. This property follows from the non-degenerate action of the algebra on the module. To illustrate the utility of the modules, we considered in the simple case of bulk Z2 model, picking different modules for the two interpolating algebras. (We still keep it a module for individual algebra so that when we are at either end of the interpolation it is a topological fixed point. If we replace the module altogether by any other object that is not a module of the condensing algebra, then we lost sight of the topological symmetric phases involved in the competition. ) We numerically tested the interpolation, and the phase transition at the critical interpolation parameter becomes a sharp first order phase transition. In that case, we can in fact show that by choosing different modules at a unit cell for the electric algebra ( $A_e = 0$ ) and magnetic algebra ( $A_m = 0 \oplus 1$ ), it is clear that different unit cells are correlated (e.g. an  $A_e$  on one cell would force neighbouring cells to be  $A_e$  as well), and so the true "unit" in that lattice is the entire global lattice. In that case, the number of interpolation parameter remains 1, but what we call  $D_c$  approaches infinity since the space in which the condensate lives approaches infinity when there is no effective separation between the unit cells. The number of topological gapped phases remain only 2 in the 3D bulk Z2 model. Therefore the phase transition should be a first order transition, instead of the second order transition corresponding to the Ising model observed when the unit cell is surrounded by a shared module of the two algebras. This first order transition is confirmed by the numerical check. In fact, in this case the correlation between unit cells are so simple the numerical

value of the critical coupling matches with equal weight of  $A_e$  and  $A_m$ , but now with normalisation defined on the global lattice that approaches infinite size instead of a single unit cell! These results were presented in the appendix of our arxiv version of our manuscript (Appendix E). We were intent on cutting down our length before submission to Nature Physics and this is a section we didn't include initially. We have now included in the supplementary materials section 4.

It is indeed true that we have not yet come up with a more rigorous mathematical proof of this result, and relied on some physical arguments. This is one reason to have relegated this to the method section rather than keeping it in the most prominent location. However, we feel that the counting of degeneracy is an important insight that assisted in the engineer of models, and we felt obliged to include this section in the paper. A possible path to proving this partially could be in the following line: many of the models we studied are related to  $Q$ -state Potts models. This is obvious in the case of the infinite family of the  $Z_N$  models for all positive integer  $N$ . In some cases, our colleagues and collaborators discovered that it is possible to map them to Potts model with  $Q$  analytically continued to some real  $Q$  (We have more to say about these examples below when we talk about the accuracy of the numerics.) The value of  $Q$  will be mapped to the quantum dimension of one of the interpolating Frobenius algebra in these special cases. It is shown that for  $Q > 4$  the Potts model experiences a first order phase transition at the critical temperature (For Planar Potts model we understand that a rigorous proof of this result was given by the Fields medalist Hugo Duminil-Copin and collaborators relatively recently in 2016/17, although Baxter's work came decades earlier. ). Our assertion of the condition for phase transition should thus be mapped to the property of the Frobenius algebra verses the number of objects in the algebra in these cases. From our knowledge we do not know of a case violating the condition from the perspective of Frobenius algebra, and we believe in these subset of cases a rigorous proof is quite likely. But I am afraid the precise mapping to these models and the considerations of the properties of Frobenius algebra involved a large body of works (including works in progress of our colleagues and collaborators) and we could not quite include this body of work in this work (although we will say a bit more in the following). We hope that we could report in detail these new findings in a forth-coming publication.

2. Table 1 in the authors' response lists the changes in central charges. These changes seem very drastic and are alluded to their improved RG algorithm. Moreover, some of the transitions which were categorized as continuous in the previous version are now deemed first-order. The authors now provide uncertainty estimates based on an eight-step plateau in their RG procedure, which suggests that the new values are more stable. My question is, how trustworthy are the new results? Looking at the plots of  $C$  in the supplementary material, the plateaus in Figure 11 aren't as flat as those in Figure 10. For example, the purple curve in the top-left panel of Figure 11 seems to be on a downward trend instead of stabilizing around 0.779 as quoted in the text.

**Reply:** We thank the referee for pressing on this very important issue, and we appreciate that this is a crucial point to clarify. As we explained in our previous reply, the main improvement of our code at that time is implementing the sparse tensor contraction. It significantly speeds up the computation and grant the ability to increase bond dimensions in the computation of phase diagram. The need to increase bond dimension is to reduce finite size effects for us to acquire more accurate tri-critical points/RG flows. We also carefully adjusted the sweeping of the learning rate for the LBFGS gradient descent to obtain a more stable minimal point for each TNR step.

Then comes the very important question the referee rightfully posed – why is there such a drastic change in the central charge in certain cases in our last reply to the referee? The reason has to do with that we used to directly read off the central charge in the phase diagram.

In our initial estimation, the values were extracted at a fixed RG step for computational efficiency over large phase diagrams that involve thousands of points. Preliminary benchmarks on the A-series models seem to suggest the renormalization flow typically stabilizes within 6 to 8 steps. Either it has settled at 0 if it is far from the critical point, or stabilises at the actual central charge if it reaches a plateau. As one can see in the resultant phase diagram the phase boundaries would light up - the width of the phase boundary is very sharp (only sufficiently close to the phase boundary would it light up) and matches very accurately with directly comparing with diagram of following the end point of tensor network renormalisation.

On the other hand, when it comes to accurately reading off the central charge of a critical point and determining carefully if the phase transition is continuous at all, it is indeed by far inadequate, particularly when we get out of the simplest benchmark cases such as the Ising model, where this choice is good enough to produce a central charge of 0.499. We also want to give extra thanks to the referee for suggesting that we should estimate the error of the central charges. We inspected in detail into individual RG flows, and realised particularly with the new examples concerning the Haagerup models, the naive choice of taking the 8th RG step is not adequate – there are examples when the system hasn’t settled on a stable plateau at the 8th step and fluctuate up and down for a couple more steps, and some where the plateau starts to destabilise at that point. In addition, to locate precisely tri-critical points we depended on the phase diagrams. Those points might take particularly more RG steps to stabilise. When we initially inspected the tricritical points, we naturally picked up points in the phase diagram that appeared brightest, which gives a bias to larger  $c$  even though at the 8th step carrying the largest “ $c$ ” may only be a result of fluctuations. As one can see in various examples, the truncation errors can also produce an abnormally high  $c$ . These less accurate  $c$ ’s are however more likely to be caught by reading off the phase diagram.

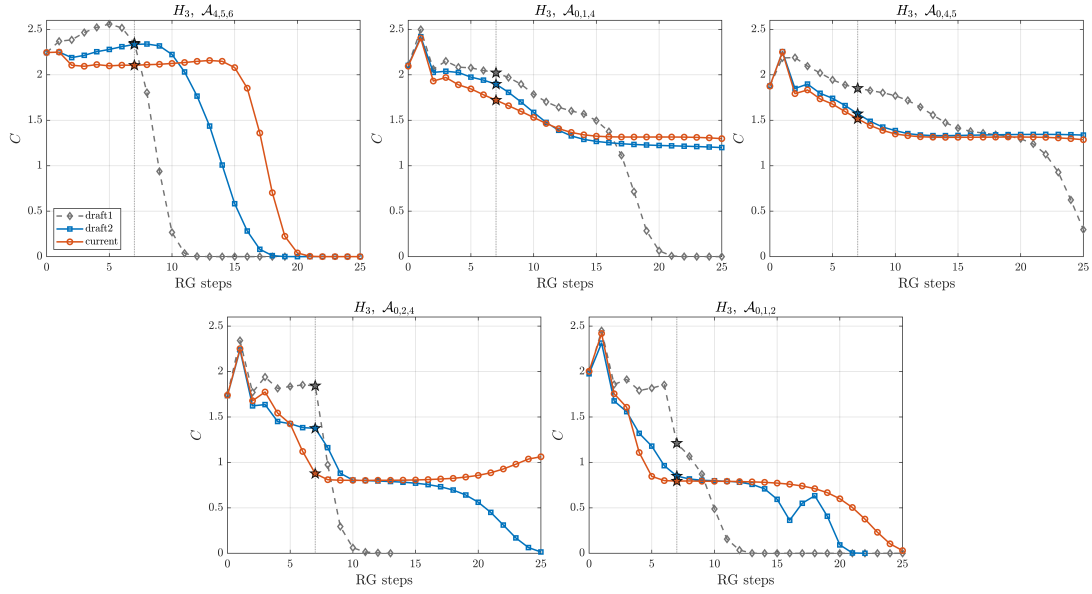
To cure these problems, we spent much more effort to locate the tri-critical points and inspect each RG flows. Starting from the previous draft, the tri-critical points

are acquired by the intersection of phase transition lines, which were traced by the converged tensor. The central charges are then extracted by the plateaus of individual RG flows at these tri-critical points. These are illustrated in Figure 1. We use the data in our RG diagram in the first draft of our paper and indicate the location of the 8th step. This diagram is superposed on the data of the first reply where the RG diagram has improved slightly, but where we read off the central charge by locating the plateau manually. In other words, for each estimate of the central charge that we produce in our last reply and also now, we identify the location of the plateau case by case and taking average there. This is the main reason for the changes, and we owe it to the referee to discover the problems with our initial automated estimate algorithm. We have also emphasized in our revised manuscript that the  $c$ 's appearing in the phase diagrams are not the converged values because of these observations.

The only exception is the competition of  $\mathcal{A}_{0,2}$  in  $H_3$ , which is unstable as already noted in the first draft. The reported value in the first draft is given by the fitting of entanglement entropy. This will be explained in more details below.

Figure 1 is further superposed on the latest RG result. Our latest improvements for the last two months focused on optimising the index contraction in some intermediate steps. When computing the loop loss between intermediate tensor  $M$  and the new coarse-grained  $t$ , all legs are meant to contract in a loop. However, due to limited computational resources, we chose some indices (specifically  $k_1$  and  $k_2$ ) to be contracted only between neighbouring indices. With sparse tensor contraction, we found it now possible to do a full loop contraction. We contrast the results of the new full loop contraction and old RG diagrams of the most important examples in Figure 1 and 2. The most important observation that should be clear is that the change in the numerics did not lead to drastic change of the results. The optimisation has however increased the stability of the plateau in most cases. We would be very happy to subject our code to any scrutiny or produce any data on request to show transparently the workings of the codes. The changes are summarized in Table 1.  $Z_n$  models have little changes because the indices  $k_1$  and  $k_2$  are redundant for group categories. The converged tensors and thus the phase diagram have little changes, too. For these data, we keep the results reported in the previous draft.

Then comes the important question of identifying a continuous phase transition. As it is evident, the main testing stone used in the current paper is the appearance of fixed points, that show up as plateau in tensor network renormalisation which combines multiple tensors into one. The plateau cannot be perfect because we are working with finite bond dimensions which necessarily introduces irrelevant deformations to the theory, even if we are sitting right at the critical point. Keeping higher bond dimension should in principle produce a better approximation to the fixed point, although it in turn is restricted by our computational power and accumulation of errors in the presence of more variables. Therefore in identifying fixed points, we look into the size and stability of the plateau under different bond dimensions. In a high-likelihood continuous phase transition we would generally observe an



**Fig. 1:** RG flow for tri-critical points in  $H_3$ . 'Draft1' are the RG flows which gave the reported values at the starred eighth step (7 at the axis because it starts at 0) in our initial submission. 'Draft2' labels the last submission, while 'current' labels this submission which uses full contraction for indices  $k_1, k_2$ .

approximate plateau that grows in size and improve in stability as we increase bond dimension until accumulation of other errors and computational complexity bogs it down. The stability and size of the plateau is indeed key. The referee rightfully pointed out that the plateaus in Figure 11 are not as flat as those in figure 10. In figure 10, the models displaying flat and extended plateaus correspond to the Ising model ( $c=0.5$ ), the tri-critical Ising model ( $c=0.7$ ), the 3-state Potts model ( $c=0.8$ ), the 4-state Potts model ( $c=1$ ). These lattice models are very well studied and are known for their smaller finite size effects. They are most often used as benchmarks to test the accuracy of one's numerics. The models we created from the  $A_k$  categories presented in figure 10 falls into this category of theories. A top notch tensor network renormalisation algorithm should be able to reach the order of 20 steps in the plateau. Figure 10 is thus how we gauged the level of our numerics to be in the same league as the best tensor network renormalisation algorithms. Figure 11 contains models following from the Haagerup symmetries. To start with, the background bond dimension is much greater: The category involved has many representative simple objects, and/or the modules may contain many basis elements in each unit cell. So the naive counting of bond dimensions already make it much more challenging. Then it does have more finite size effects. This is an issue that also arises in many treatments of models beyond the simple ones we listed above. For example, the Fibonacci model (or the Z3 parafermion model) are known to suffer from a lot more finite size effects, and it takes quite many tricks to extract accurate numerics (For a list of references it can be found in the paper by two of us Phys.Rev.B 107 (2023) 24, 245146

<https://arxiv.org/pdf/2212.02937>. Our previous work is an example of tackling such effects in resolving the spectrum for example.) Prior studies of the Haagerup model had also found many subtleties following from the high bar of bond dimensions and finite size effects. For example, in the Haagerup anyon chain model constructed in Phys.Rev.Lett. 128 (2022) 231603 (eprint:2110.03008 ), it corresponds to a choice of a single object  $\rho$  as the module in our language, and so we suspected that it is the same as the  $A_0 - A_1$  competition that we considered (Although this is not certain until we inspect the spectra in the thermodynamic limit, a test that has not been performed by either of our groups since that takes more preparation in expanding computational power. ). We initially also attempted to use logarithmic scaling of the entanglement entropy as a test, and we saw a decent match with logarithmic scaling and extracted a central charge close to 2, similar to their results See the plot below in figure 3. However it is observed that there is some small weird oscillations in the numerics also in Phys.Rev.Lett. 128 (2022) 231603, such as the plot of the spectral gap against system size  $L$ , and that could also be indicative of a weak first order phase transition.

When we resorted to our tensor network renormalisation algorithm, we found that the central charge in our model gradually falls as we increase bond dimensions and the number of RG steps, and we end up deciding that it is a first order phase transition in our model. Now the question is – how good is our numerics? Can we trust what we see? In this case, we are very lucky that it is possible to have exact analytic check! As mentioned in the reply to the previous question, this is a case that can be mapped to an analytically continued Q-state Potts model with some real value of Q! The competition between  $A_0 - A_1$  in Haagerup falls into special cases involving competition of an algebra  $A_1$  with  $A_0$  – the latter refers to the trivial algebra that is found in all categories. It is observed that in the case of exactly a pair of algebra  $A, A'$  competing, with one of the algebra fixed to  $A = A_0 = 0$ , it is possible to map the model we have written down to the random cluster model [First pointed out to us by a collaborator, and we have independently checked]. This can be shown explicitly by using the property of the Frobenius algebra. Roughly speaking in the random cluster model each edge on a lattice can take either two values 0,1. In our model, we can package  $A$  as 0, and  $A'$  as 1 – the latter is possible using the associativity condition of a Frobenius algebra. As it is known, the random cluster model can be further mapped to the Q-state potts model. The parameter Q is mapped to the quantum dimension of the algebra  $A'$ . Therefore these special cases of our models can produce Q-state potts model with real irrational values of Q. As discussed above, the Q state potts model acquires first order phase transition at  $Q > 4$ . Mapped to the competition of  $A_0 - A_1$  in Haagerup, we get a  $Q = \dim A' = (11 + 3\sqrt{13})/2 = 10.9083 > 4$ . This thus turns out to correspond to a first order phase transition. But the story does not end there. While this is a first order phase transition, in the case of the Q-state potts model, one can still compute the “central charge” corresponding to this value of Q. (The formula is given by  $c = 1 - 6/(t(t + 1))$ , where  $Q = 4 \cos^2(\pi/(t + 1))$ .)

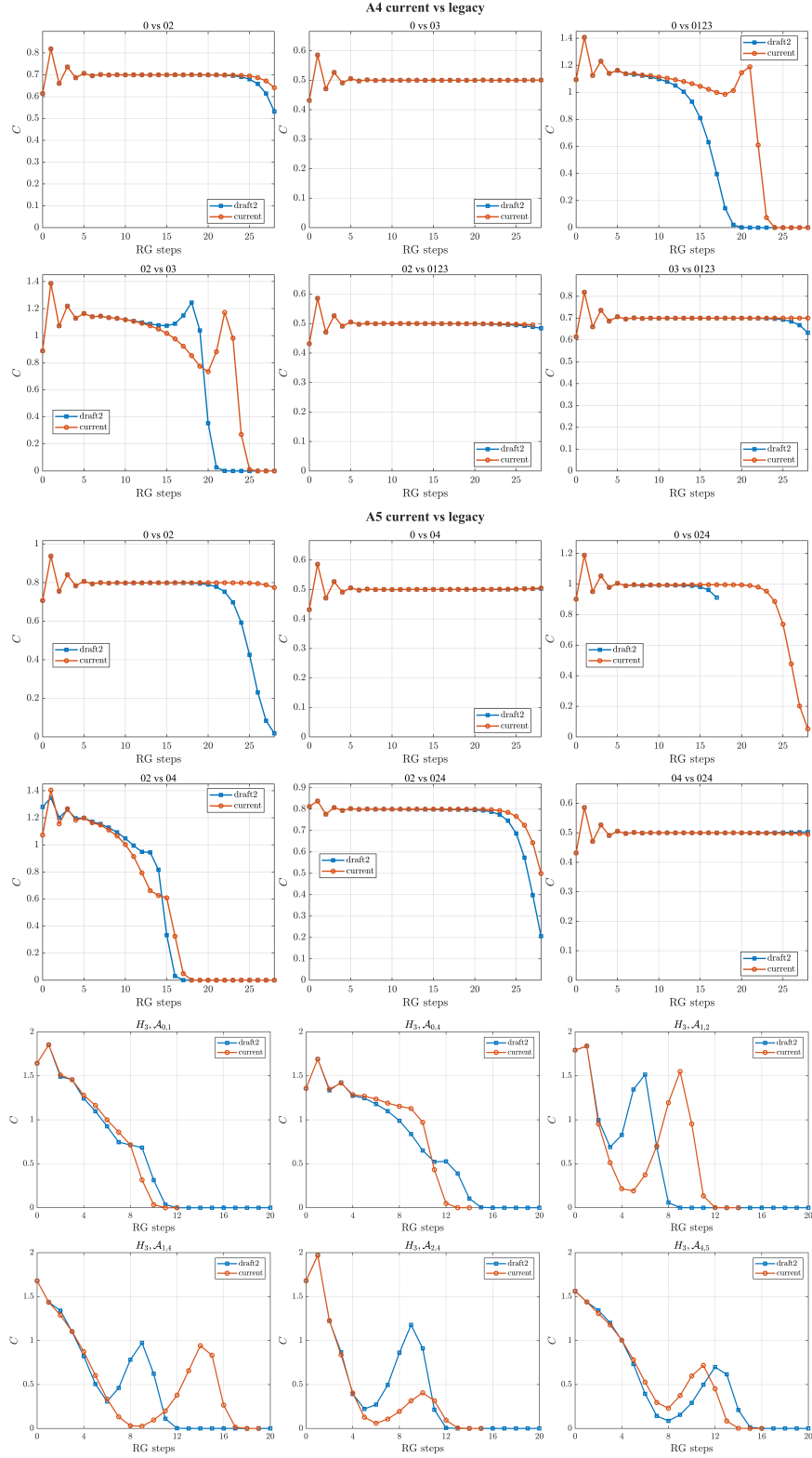
With our value of  $Q > 4$ , one gets a complex central charge. In fact, it is given by  $1.641 + 0.222i$ . (This is the same branch of the solution that gives the correct central charge for true second order phase transition with  $Q \leq 4$ .) The imaginary part of the central charge is much smaller than the real part of the central charge, and thus this is probably a reason why an almost logarithmic scaling behaviour was initially observed in the entanglement entropy computation. The exact analytic mapping confirms the conclusion of our numerics that identify this model to be a first order transition! Contrast this with the case of the 5-state Potts model in which it is known to be a very weak first order transition and its almost logarithmic scaling behaviour is very well explained by a complex CFT. The central charge of this complex CFT is indeed again given by analytically continuing the above expression. (These results are also discussed in some of the references we have cited in our manuscript.) For  $Q = 5$ ,  $c_5 = 1.138 + 0.022i$ . One can see that the imaginary part in the  $A_0, A_1$  competition is way bigger than the case of the 5 state potts and thus behave more like a first order phase transition on closer inspection. Our numerics indeed can see a quasi-plateau for the 5-state potts model with  $c \approx 1.13 \approx \text{Re}[c_5]$  in the 5 state potts case which is mentioned in the text as a check of our numerics. That means **even where it is a first order phase transition, if it is sufficiently weak our numerics accurately recover the real part of the central charge of the underlying complex CFT**. Now as it is visible in figure 11, the competition between  $A_0, A_4$  in the Haagerup model shows a much slower decay compared to  $A_0, A_1$ . The quasi-plateau gives an approximate central charge of about 1.3. This is yet another example that falls into the special class that can be mapped to the generalised  $Q$ -state potts model like above, with  $Q = 4 + \sqrt{13} = 7.606 > 4$ . Now it is again greater than 4, but smaller than the  $A_0, A_1$  competition. This explains the clearly slower decay. The predicted complex central charge from the  $Q$ -state potts model is  $1.405 + 0.109i$ . The imaginary part of the central charge is way smaller than that in the  $A_{0,1}$  competition, confirming that the model is much closer to a continuous phase transition. Moreover, from the quasi plateau one can estimate a central charge to be around the value of 1.3 – which is indeed quite close to the analytically predicted value. We note that in these cases, the needed bond dimension is still smaller than cases involving the tri-critical points just because there are fewer objects involved. But we are glad that we have analytic understanding at least in some sub-class of models that back up our numerics, giving us confidence that we are still in control.

Next, let us discuss the tri-critical points. We admit that the quality of the numerics are lower there in comparison to the benchmarks. There are two reasons. First, the number of objects involved is much larger with more condensates involved, making the base-line bond dimension much larger. Second, unlike the two-phase competition where we have an analytic value of the transition point, we only have an approximate location of the tri-critical point from the intersection of the bisection lines. Therefore, to extract the central charge, not only do we need to maximise the bond dimension, we need also to do a search near to the intersection point of the bi-section lines of

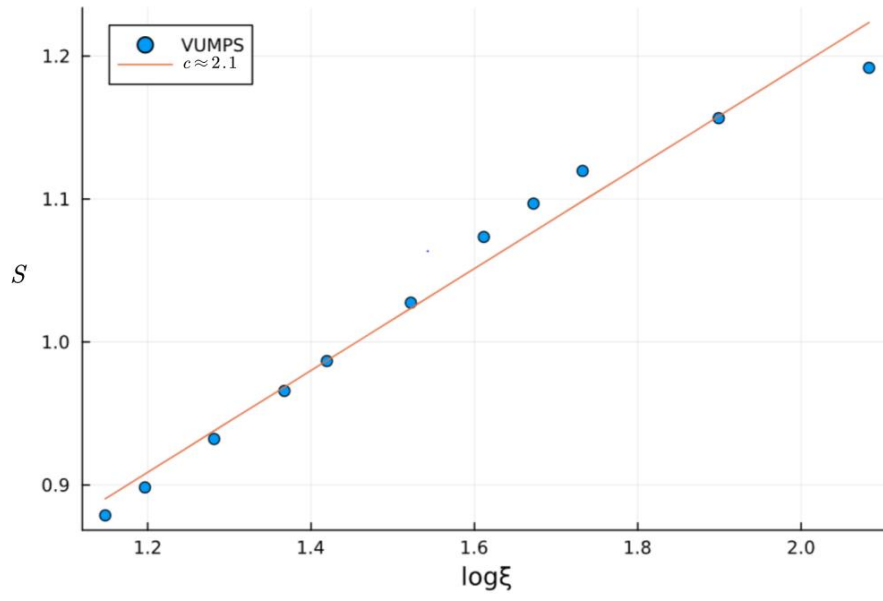
the triangles. The more accurate we could locate the tri-critical point, there are fewer possible relevant perturbations left over in the model, in addition to irrelevant perturbations that are already there due to finite size effect. This is the reason why the numerics are more tricky here. As the referee pointed out that the quality of the plateau for the competition between  $A_{0,1,2}$  appears to be lower, this is because we have not made use of our limited resources in greatly refining the location of the critical point. The reason is that the plateau appears to have central charge  $c = 0.8$  and we have good reason to conclude that this is the 3-state Potts model – since it looks like that this 3-state Potts model is the end point of the critical line corresponding to the  $A_{0,2}$  competition which is shown to be a 3 state Potts model (by us here, and also earlier by the Oxford group). With the given central charge 0.8 it also cannot be a new CFT since minimal models are classified and tightly constrained. Therefore, we found the other models with  $c > 1$  a lot more interesting and potentially new, and we spent most of our resources in improving the location of the tri-critical points and improving the tensor network renormalisation procedure. As shown in the figure 1 above, we observed that the plateau gradually improves for the candidate new CFTs with Haagerup symmetries. This is what gives us confidence that the tri-critical points are connected to a continuous phase transition. Could there be a possibility that this is still a first order phase transition? At present, particularly for the cases of  $A_{0,1,4}$  and  $A_{0,4,5}$  the plateaus look long and stable. The plateau for  $A_{4,5,6}$  is of a lesser quality, but its size is significantly longer than the shoulders that are otherwise formed for other provably “weak” first order phase transition such as the  $A_{0,4}$  case. In fact after the small optimisation we performed in the past two months, the quality of the plateau of this case has visibly improved, distinguishing it more clearly from first/weak first order phase transitions. This gives us confidence that the plateau follows from true scaling behaviour from a real CFT. In any event, we hope to convey the message that appearance of a relatively flat and extended plateau is a physical scaling behaviour, and that signals the presence of a CFT behind, even if it may be a complex one. i.e. Weak first order phase transitions look like CFT because there is a (complex) CFT behind. This perspective becomes clear to us when we began to delve into the literature of the 5-state Potts model, and witnessing in our numerics that the properties of complex CFTs do show up in our numerics. That the location of this critical point can be tightly constrained by topological data remains shocking to us.

3. In Figure 4 in the main text, I think  $A_2$  and  $A_3$  should be exchanged to be consistent with the discussion in the text. I urge the authors to verify other figures too.

**Reply:** We checked and the labels are correct. For example, in Figure 4, the horizontal axis  $y$  is the coefficient of object 2 while the vertical axis  $z$  is the coefficient of object 4. For large  $z$  (up left) one gets  $\mathcal{A}_2 = 0 \oplus 4$ ; for large  $y$  and  $z$  (up right) one gets  $\mathcal{A}_3 = 0 \oplus 2 \oplus 4$ .



**Fig. 2:** RG flow for pair competition points in  $A_{4,5}$  and  $H_3$ . ‘Draft2’ labels the last submission, while ‘current’ labels this submission which uses full contraction for indices  $k_1, k_2$ .



**Fig. 3:** Finite entanglement scaling of the critical point from  $A_0 - A_1$  competition in <sup>1</sup>

## A summary of the changes to the manuscript

We have made a number of changes to both our main-text and supplementary materials and highlighted these changes in red.

1. The results of the central charges of  $H_3$  tri-critical points in both the main-text and supplementary;
2. The central charges and uncertainties in the summary table;
3. The plots for RG flows of  $A_4$ ,  $A_5$  and  $H_3$  in the supplementary.

**Table 1:** Summary of numerical changes in continuous rows after recomputing plateau values from the selected current-reference flows, using the final minimum-standard-deviation plateau selector.

| Cat   | $\mathcal{A}_i, \mathcal{A}_j, \mathcal{A}_k$                 | $C$                            | $\delta C$                     |
|-------|---------------------------------------------------------------|--------------------------------|--------------------------------|
| $A_2$ | $0, 0 \oplus 1$                                               | $0.49982 \rightarrow 0.49977$  | $0.00053 \rightarrow 0.00044$  |
| $A_3$ | $0, 0 \oplus 2$                                               | $0.49997 \rightarrow 0.50000$  | $0.00035 \rightarrow 0.00042$  |
| $A_4$ | $0, 0 \oplus 2$                                               | $0.69945 \rightarrow 0.69976$  | $0.00033 \rightarrow 0.00050$  |
| $A_4$ | $0, 0 \oplus 3$                                               | $0.49997 \rightarrow 0.49996$  | $0.00017 \rightarrow 0.00018$  |
| $A_4$ | $0 \oplus 2, 0 \oplus 1 \oplus 2 \oplus 3$                    | $0.49976 \rightarrow 0.49988$  | $0.00051 \rightarrow 0.00038$  |
| $A_4$ | $0 \oplus 3, 0 \oplus 1 \oplus 2 \oplus 3$                    | $0.69949 \rightarrow 0.69990$  | $0.00020 \rightarrow 0.00013$  |
| $A_5$ | $0, 0 \oplus 2$                                               | $0.79825 \rightarrow 0.79949$  | $0.00087 \rightarrow 0.00079$  |
| $A_5$ | $0, 0 \oplus 4$                                               | $0.50011 \rightarrow 0.49994$  | $0.00041 \rightarrow 0.00053$  |
| $A_5$ | $0, 0 \oplus 2 \oplus 4$                                      | $0.9926 \rightarrow 0.9954$    | $0.0031 \rightarrow 0.0015$    |
| $A_5$ | $0 \oplus 2, 0 \oplus 2 \oplus 4$                             | $0.7988 \rightarrow 0.79990$   | $0.0010 \rightarrow 0.00052$   |
| $A_5$ | $0 \oplus 4, 0 \oplus 2 \oplus 4$                             | $0.500007 \rightarrow 0.49998$ | $0.000084 \rightarrow 0.00047$ |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_1, \mathcal{A}_2$                 | $0.79 \rightarrow 0.793$       | $0.14 \rightarrow 0.014$       |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_2, \mathcal{A}_{(4,5,6)}$         | $0.779 \rightarrow 0.8047$     | $0.074 \rightarrow 0.0077$     |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_1, \mathcal{A}_{(4,5,6)}$         | $1.217 \rightarrow 1.3154$     | $0.034 \rightarrow 0.0065$     |
| $H_3$ | $\mathcal{A}_0, \mathcal{A}_{(4,5,6)}, \mathcal{A}_{(5,6,4)}$ | $1.3424 \rightarrow 1.3141$    | $0.0067 \rightarrow 0.0042$    |
| $H_3$ | $\mathcal{A}_4, \mathcal{A}_5, \mathcal{A}_6$                 | $2.28 \rightarrow 2.106$       | $0.12 \rightarrow 0.022$       |

We thank the reviewers once again for their comments and encouragement. We have tried our very best to improve the manuscript, and we hope it has been rendered suitable for Nature Physics.

Best regards,

The Authors

# Reply

Dear Editor,

We would like to thank you and the reviewers again for your careful reading. We are very pleased that the reviewers are satisfied with the revised version. We have made the following revisions.

1. We have reorganized the Methods and the previous Supplementary Information into a single Supplementary Information PDF. The Methods now appear as Sections 1 to 5, followed by the previous Supplementary Information as Appendices A to F.
2. We have prepared a separate main-text LaTeX file and vector PDF files for the main figures. For compilation, the figure files should be placed in the same directory as the LaTeX file.
3. We have incorporated the previous footnote text into the main text, and likewise moved the explanatory notes for Table 1 into the main text.
4. We have completed the accompanying editorial forms and prepared the source-data and supplementary-code files. These files are supplied as separate upload files.
5. We have also updated the schematic figures in Supplementary Information Section 4, which illustrate the strange-correlator construction and the tensor-network RG procedure, adjusted wording including uses of the word novel, and corrected small typos throughout the manuscript.

We hope that the revised files now satisfy the editorial and formatting requirements. Thank you again for your consideration of our manuscript.

Best regards,

The Authors