Reply to Referee #1

In this work, the authors give an interesting study a holographic duality between 2+1d theories and 1+1d theories. This paper provides some new contributions to an already very hot topics. Although I did not go through every details of this work, especially the explicit computations in Section 5 on examples, I believe that the main results are correct mainly because this holographic duality has already been discovered and confirmed by different groups of people. I think that some of the results in this work have already appeared in literature. At the same time, I also think that this work contains some interesting new results, including the study of critical points, phase diagram and interesting discussion in Appendix A. So I think that the paper is publishable in principle. However, the main problem of this paper is that it did not make it clear what is new (or not new) in this work. Therefore, I recommend a major revision before the second review.

We thank the referee for the interest in our work and noting the novelty of our results pertaining to the study of critical points and phase diagrams of symmetric quantum spin chains. We want to emphasize that our attempt was to present our work in a self-contained manner that would in particular be accessible to condensed-matter physicists without much technical/mathematical expertise as well as without a deep knowledge of some of the recent developments in the understanding of global symmetries which have admittedly stemmed from the high-energy physics community. Given this perspective, we were obliged to review some known material. Much of this is scattered over the literature, often in much more technical language. That being said, our work is by no means merely a review of known physics but contains many new and interesting results. In the introduction of our (originally submitted) manuscript, under *Summary of Results*, we provided an overview of new results in the paper. In particular, sections 4 and 5 are primarily new results. Section 3 we provide a systematic and pedagogical and systematic exposition that connects many physical and mathematical aspects of the subject in a simple coherent construction. This section contains new results as well as known results in a new language/point of view that complements recent constructions in particular within *Categorical Symmetries*, *Symmetry TFTs* and *Topological Wick rotation*. We had attempted to cite all the relevant references in the introduction of the paper. In order to clarify this further, we have now added several new paragraphs in the introduction and included more citations in the main body of the paper where relevant.

The following is a list of more detailed comments on the paper.

1. The authors provided some review of the topological phases, phase transitions beyond Landau and generalized symmetries. This brief review is very nice but not necessary unless it is used as part of the main idea of this work.

   We thank the referee for their positive feedback regarding the introduction. From a conceptual standpoint, one of the purposes of our paper is to promote working towards a unification of Landau and beyond-Landau physics, as is also reflected in the title of the paper.

   This necessitates an introduction to Landau and beyond-Landau phases and phase-transitions, in addition to the notion of generalized symmetries which connects these ideas. All these concepts (SSB, SPT, topological order, generalized symmetries etc) are used throughout our paper.

We have chosen to retain this introductory material as it provides a brief survey that contextualizes and motivates our work for a condensed-matter audience without immediately diving into the details of our approach.

At the end of the introduction (page 4), the authors suddenly shifted to the content of this work, which is, according to the authors, a "different approach towards the unification of Landau and beyond-Landau paradigms" and is, perhaps different (at least superficially) from all the previous discussion. This structure of the Introduction Section is quite puzzling to me. Normally, one explain in the Introduction section the motivations and the origins of the main idea of this work. But it seems to me that the authors did not do that at all, but used a lot of paragraphs to explain approaches that are different from this work.

We would like to reiterate that since one of the main aims of our work is to study, within a unified framework, both Landau and beyond-Landau physics, the broad overview of Landau and beyond-Landau physics was very much a part of the general motivation for our work. Additionally, we have now added several new paragraphs expanding on the motivation as well providing a context of previous related works. In particular, we have explained how our work compliments other recent works related to symmetry TFTs, topological Wick rotation and categorical symmetries. We append the modified/added paragraphs here for the referee's reference:

In this paper, we describe a different approach toward the unification of Landau and beyond-Landau paradigms. Our approach exploits the topological nature of global symmetries to decouple the global-symmetry-related features of a symmetric quantum system from its local physics/dynamics. The symmetry operators and their action on charged operators can be holographically encapsulated in a topologically-ordered system that lives in one higher dimension. The action of symmetry operators on charged operators is encoded in the braiding of topological defects (e.g., anyons in 2+1 dimensions) of the topologically-ordered system. From this point of view, different phases such as Landau symmetry-breaking phases, SPTs as well as (Abelian) topologically-ordered phases all arise on the same footing, i.e, as gapped boundaries of a topological order in one higher dimension. Furthermore, the bulk topological order can be thought of as a theoretical gadget that allows one to conveniently discover numerous non-perturbative statements about the space of symmetric quantum systems. These include the classification of gapped phases, order parameters of each phase as well as non-trivial dualities that act on the space of theories. The dualities descend from the internal symmetries of the bulk topological order, which act on boundary theories through topological domain walls, and reveal rich hidden structures of the space of theories. We develop tools to exploit these dualities to constrain and construct the phase diagram and compute its various features efficiently through a study of the associated topological order. In order to provide concrete realizations that exemplify the utility of this approach in the simplest physically-relevant setting, we restrict ourselves to the study of $1+1d$ quantum systems with finite Abelian group symmetry.

Ideas similar to the ones developed in this work have been around for quite some time. For example, in [1] it was shown that there is an $\mathrm{SL}(2,\mathbb{Z})$ action on the space of three-dimensional conformal field theories with global U(1) symmetry, reminiscent of the dualities discussed in this paper. It was shown that these dualities can be understood as originating from the

$\mathrm{SL}(2,\mathbb{Z})$ electric-magnetic dualities in a four-dimensional $U(1)$ gauge theory [2, 3, 4, 5, 6]. More concretely, these dualities are implemented as topological domains walls that are brought to the boundary, giving rise to a mapping of boundary theories.

More recent ideas related to using topological orders in one higher dimension to study symmetric quantum systems have appeared in a number of recent works [7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29]. Below, we compare and contrast our work with these related directions.

In [10, 11, 12, 14, 15, 30], the notion of categorical symmetry was introduced which combines a finite symmetry and its dual symmetry which typically arises via the process of gauging the original symmetry. Relatedly, these approaches treat the order and disorder parameter on an equal footing. The similarity with the present work stems from the fact that the categorical symmetry can be organized into a topological order in one dimension higher. An important difference between the approach employed in these works and ours is that we do not consider the notion of "dual symmetry". Instead, we simply fix a finite global symmetry and study the space/algebra of local symmetric operators with respect to the given symmetry. Upon doing so, we naturally discover the algebra of topological operators in a topological order in one higher dimension. Furthermore, we describe a complimentary approach wherein we start from a topological order in 2+1 dimensions and derive symmetric pseudo-spin chains on its boundary. We show that the space/algebra of local operators acting on the pseudo-spin chain is precisely the algebra of local symmetric operators with respect to a symmetry structure that is read-off from the 2+1 dimensional topological order. This algebra is related to the algebra of patch operators discussed in [11, 14].

Another related direction is the so-called Topological Wick Rotation [24, 25, 26, 27, 31]. The physical picture of Topological Wick rotation is related to considering a topological order in $d+1$-dimensions and restricting the theory to a d-dimensional spatial slice at a fixed time. Moreover the spatial slice is considered to be open. Then the authors argue that one may "Wick rotate" one of the spatial directions and dualize the topological order to a quantum liquid in $(d-1)+1$ dimensions. The category of operators then has the same algebraic (more precisely categorical) structure as that of the operators in the topological order. Although our work bears some similarities with this series of works, the framework we use, the conceptual picture and approach are very different and do not rely on such a construction.

Recently, there has been a lot of interesting progress in a direction related to so-called symmetry topological field theories (SymTFTs) [32, 8, 9, 16, 18, 33, 34, 35, 29, 36]. The main idea is that a d-dimensional theory $\mathfrak{T}$ on a manifold M with a global symmetry $\mathcal{C}$ can be constructed as topological theory in $d+1$-dimensions defined on $M\times[0,1]$ with the original theory $\mathfrak{T}$ on one boundary say $M\times\{0\}$ and a topological boundary condition on the other boundary $M\times\{1\}$. An attractive feature of this construction is that the symmetry related properties can be dealt with by simply manipulating the topological boundary condition. Indeed various dualities can be implemented by simply altering boundary conditions. In our work, we do not consider an interval compactification with a topological boundary condition on one boundary and a generic non-topological boundary condition on the other. Instead, we consider the bulk to be a semi-infinite cylinder, which allows us to access all the symmetry twisted symmetry sectors directly using the semi-infinite line operator with one end on the boundary to toggle between these different sectors. Furthermore, our method is more oriented towards studying concrete lattice spin models as compared with general applications of symmetry TFTs. A condensed-matter-oriented study of symmetry TFTs was developed in the work of Lichtman et al [13]. In this work, the authors use a finite cylinder spatial geometry with a Dirichlet boundary condition for the electric line operators on one boundary. The other boundary is left free and the bulk magnetic line operator wrapping the non-contractible cycle projects on to the dynamical (i.e., non-topological boundary) as a symmetry operator. In this picture when the non-topological boundary is an electric condensate, one obtains the spontaneous symmetry-breaking phase, while when the boundary is a magnetic condensate, one obtains the symmetric phase.

The present work provides a complimentary approach to these past works and contextualizes these approaches to concrete lattice models that are fairly standard in quantum magnetism. Moreover, we lay out a detailed and practical framework to use such abstract notions to study the generically complex phase diagrams of these models.

Much more detailed suggestions are given below for individual sentences in the last paragraph of Introduction.

- "Our approach exploits the topological nature of global symmetries to decouple the global-symmetry features of a symmetric quantum system from its local physics."

  Comments: This idea is clearly not new. Introduction section should contain an introduction of this idea and references.

  Yes, we agree this idea is not new. We now have added several paragraphs and references describing other approaches that also use similar constructions.

- "The symmetry operators and their action on charged operators can be holographically encapsulated in a topologically-ordered system that lives in one higher dimension."

  Comments: It seems to me that this sentence provides the main idea of this work, i.e. topological holography. Unfortunately, Introduction section does not contain a detailed explanation of the motivation and the origin of this idea and references. As far as I can tell, I do not see any connection between this idea and the discussion before this paragraph.

  As this paper aims to appeal to a wider segment of the condensed matter physics community rather than a specialized subset, the introduction focuses primarily on the conceptual motivations behind this work. The technical motivations and detailed constructions are given in sections 2 and 3. We believe this is more appropriate as it gives more space to explain the background needed to understand the motivations behind the construction, without making the introduction overly technical.

  In addition, just to clarify, we have detailed a construction in Section 3 which shows how the space of G symmetric spin chains as an algebra is isomorphic to the space of of line operators in a topological order in one higher dimension. Since the action of symmetry operators on charges is naturally contained within this algebra, it trivially follows that it can be encapsulated into the linking of topological operators in the higher dimensional topological order.

- "Independently, related ideas have recently appeared in the literature under the name of symmetry TFTs [107-112]. The action of symmetry operators on charged operators is encoded in the braiding of topological defects (e.g., anyons in $2 + 1$ dimensions) of the topologically-ordered system."

Comments: I do not know if the second sentence has anything to do with the first one because it seems that the second sentence make sense in a wilder context.

We thank the referee for pointing this out. It was indeed written in a way which was a bit confusing. We have now changed the order of the sentences in the modified paragraphs appended above.

- "Ideas related to using topological orders in one higher dimension to study symmetric quantum systems have appeared in a number of recent works [92,94,107-111,113-128]. The present work provides a complimentary approach to these past works."

  Comments: The motivation or the origin of the main idea of this work might be hidden in this sentence. Since you did not explain all these previous works, it is also not clear what you mean by "a complimentary approach". My suggestion is that (1) shorten the discussion in the previous paragraphs and focus on only those directly related to the main idea of this work; (2) the authors should rewrite the introduction by expanding these two sentences, and explain and emphasize what is new in this work.

  Above, we have argued about why the introduction has the structure it does. Furthermore, following the referee's suggestion, we have added several paragraphs describing the previous works and how our approach complements related recent works.

2. Let me add a remark towards the relation between Sym-TFT and topological holography. I have been exposed to both ideas several times through both arXiv papers and online talks. As far as I can tell, the idea of Sym-TFT is somewhat natural and quite obvious, but that of the holographic duality is very mysterious at least to me. In particular, the idea of Sym-TFT does not imply that of holographic duality in any way

   We would like to emphasise that our paper does not use the phrase *holographic duality* anywhere, instead we use *holographic construction* or *holographic framework*. Holographic duality suggests some sort of equivalence between theories of different dimensions, which is not the case for our work. We holographically relate (generalized) symmetries of theories in different dimensions, which in turn reveals relations between many concepts as the paper explains. One of these is that 0-form symmetries of the, say, $2+1d$ topological theory imply dualities amongst $1+1d$ theories. These are same-dimension dualities, not holographic dualities.

3. Page 7, at the end of the Introduction section, the authors wrote "The idea that anyons and anyonic symmetries are related to global symmetries and dualities in one lower dimension has been discussed in [130] and also more thoroughly in [131] (see chapter 7 and section 7.5.5 of [132])." Comments: If this is the origin of the idea, the authors should expand this paragraph and provide more details. On the other hand, I found it puzzling because the paper [130] discuss relation between 2+1d topological orders and their 1+1d gapped/gapless boundaries. This lower dimension (i.e. 1+1d theory) is anomalous because it has a nontrivial 2+1d bulk. Naively, I do not see any relation between [130] and the holographic duality studied in this work. The reference [131] was not published nor online. [132] is available online, but I do not see anything new in the Section 7.5.5. Moreover, if you say that "anyons and anyonic symmetries are related to global symmetries and dualities in one lower dimension" is the key motivation, then I claim that "anyons and anyonic symmetries in 2+1d is related to global symmetries and dualities in one lower dimension" was well known long ago for non-chiral 2+1d topological orders and its gapped boundaries. The fact that an invertible domain walls in (or an automorphisms of) the 2+1d bulk gives a duality of boundary theory is obvious,

right? It was known long ago, at least no later than the well-known work arXiv:1104.5047 by Kitaev and Kong. Actually, what is non-trivial is that these invertible domain walls (or automorphisms) one-to-one correspond to the "dualities" (also called Morita equivalences) between two (potentially identical) gapped boundaries (see Eq. (32) in Kitaev-Kong's paper). As far as I know, there are more papers discussing this issue afterwards. However, the real problem is that I do not see how all of these early works can motivate the holographic duality studied in this work.

We want to reiterate that our paper does not have any *holographic duality*, as explained in a previous response above. In the paper we clearly state that we are presenting a holographic construction, and that the dualities are a form of *generalized Kramers-Wannier dualities.* The paper [130] has Kramers-Wannier duality in the title and connects in a lot of details the relation between bulk-boundary symmetries, $\mathbb{Z}_2$ dualities, conformal defects etc. in a simpler setting. In [131] (appears as a chapter in [132]), a notion of *fermionic gapped boundaries* are studied where the effective bulk model has a non-ablian S_3 anyonic symmetry. It is shown that this gives rise to a non-abelian generalization of Kramers-Wannier dualities and a detailed phase-diagram of an effective boundary spin-chain is computed similar to our examples in section 5. This phase-diagram is part of our $\mathbb{Z}_2 \times \mathbb{Z}_2$ example (compare figure 17 to figure 7.5.5 of [132] and figure 20 of our paper to figure 7.5.6 and 7.5.7 of [132]). In [131,132], even the relation to Ashkin-Teller model and compact orbifold CFT is discussed. In appendix C of our paper, we present a simple toy model of our construction using the $\mathbb{Z}_N$ Wen-plaquette model. The approach is exactly the same as in [130] and [131], we even use a slight modification of the graphical notation that is also used in those works. In chapter 7 of [132] is it also stated that TQFTs in $d+1$ dimensions are related to symmetries and generalized Kramers-Wannier dualities in d dimensions. These observations were our primary motivations. We do not understand why the referee sees no relation between the earlier works [130], [131] and [132], and this work.

We thank the referee for pointing out equation (32) of Kitaev-Kong. While we are familiar with this paper and have cited it, the details around equation (32) we did not know. This paper is very mathematical and equation (32) is a very abstract category theory statement. These statements are not easy to understand by even vast majority of people working on Topological Order. We do not even understand all the details ourselves without further digging into the mathematics. One of the authors (H.M.) had discussed the idea of the relation between generalized Kramers-Wannier dualities and anyonic symmetries with experts such as Xiao-Gang Wen and Davide Gaiotto years ago, and they did not point out any early works on it, in particular the results around equation (32) of Kitaev-Kong paper. So we do not believe that this has been a well-known and widely-appreciated result in the field.

Even if the Kitaev-Kong paper contains some of these ideas in the form of very abstract mathematical statements, our work exposes these relations from a completely different point of view and develops tools to use these in the study of phases of matter (as in section 4) and concretely applies these ideas to non-trivial examples (as in section 5).

4. At the same time, there are a lot of closely related works on this holographic duality in the literature. I wonder what the motivations are in these vast literature on the same topics, and what the key observations are? I think that it is unfair, unreasonable and irresponsible not to explain the possible the relation with the earlier literature, some of which are a few years earlier than this work. It is the responsibility of the authors to make it clear what is new in

this work and what is not. If otherwise, a referee can also irresponsibly or superficially claim that this work is not new.

We appreciate the referee's point that in the originally-submitted version, we did not expand enough on the relation between our work and the related recent works. As we mentioned in a previous response above, the introduction has been modified so as to include a long discussion about related works and contextualizing our work in relation to these recent works. Furthermore, we have now added several references which can be found in the list of changes below. In addition, there is long discussion at the end of the introduction section (an unnumbered subsection called "Summary of Results") summarizing what is new in our paper. That being said, we find the tone of the referee accusatory, which we believe is unwarranted.

5. Page 14, " can symmetry-related aspects of a 1 + 1d theory be described by a topologically-ordered system in one higher dimension? " One way to think about this is the following: 0-form symmetry in 1 + 1d are given by a collection of line-like operators, therefore which 2 + 1d topological orders have a (subset of) similar line-like topological operators? Comments: I found both questions unnatural here. If you only need line-like operators, then they are ubiquitous in theories in all dimensions higher than 2. Why consider 2+1d (not higher)? What I am saying is that these claims do not motivate the holographic duality at all. It seems more like making up a motivation after knowing the answer.

   The logic of the first sentence is that general theories in $1+1d$ have topological (symmetries) and non-topological content. It is natural to ask whether the topological content can be described by purely topological theories that we are very familiar with in higher dimensions. In $1+1d$ symmetries are line-like and act on charged operators which are point-like, through linking. In the simplest approach, if one seeks to recreate the properties of symmetries in $1+1d$ using only line-like operators in a higher dimension, then one is forced to consider $2+1d$. This is because a non-trivial linking between lines are necessary, for the action of symmetry on charged point operators in $1+1d$ (which would appear as ends of lines). In dimensions higher than $2+1d$, linking between lines are trivial and this is thus not possible.

   In principle one could imagine more complicated constructions using higher dimensions. For example topological lines and charged points in $1+1d$ could be seen as boundaries of surfaces and lines of a $3+1d$ theory. The non-trivial surface-line linking might do the same thing in this case. However, we choose the simplest direction to proceed.

   These are however the first few lines of the section. The construction is more naturally motivated by starting from higher dimensions, which is exactly how we proceed in the rest of the section. This addresses why the first question is natural.

   Given this, the second question we ask is "which $2+1d$ topological theory should be considered for a given $1+1d$ symmetry?" This seems like the most natural followup question.

6. Page 14 Is it possible to say something about the similarities and differences between AdS/CFT and Topological Holography?

   On page 14, we tangentially mention that the holographic relation between symmetries bears resemblance to the AdS/CFT correspondence. While this is not a crucial aspect of our paper, we will now briefly elaborate on the AdS/CFT for the sake of clarity.

   The main **difference** between the AdS/CFT Correspondence and what we call topological holography is that the former is a holographic duality in the sense that two theories in different dimensions are **dynamically equivalent quantum-mechanically.** In particular, this implies that the bulk and boundary theory must have the same number of degrees of freedom which is only possible if the bulk theory is non-local (the number of degrees of freedom in a volume scales with surface area). In other words, the bulk theory must contain gravitational degrees of freedom. This means that one, in principle, can map all the dynamical information of one theory, encoded in its correlation functions, to the other theory. Furthermore, in principle, one can identify their Hilbert spaces, space of operators (operators of one theory are mapped to operators of the other theory), etc. On the other hand, **topological holography is a holographic construction rather than a holographic duality**, in the sense that there is no a priori relation between the dynamical contents of the two theories involved. In particular, the theory in d dimensions could be a non-trivial interacting theory while the $d+1$-dimensional bulk theory is a purely topological field theory whose dynamics are trivial since there are no dynamical degrees of freedom in the bulk. We draw the attention of the referee to the fact that the bulk theory is an **auxiliary** theory which facilitates the construction and the study of various symmetry related aspects of the lower-dimensional theory.

The **similarities** we briefly point out is regarding the relation between bulk and boundary symmetries. In traditional AdS/CFT context this is stated as bulk gauge symmetry becomes boundary global symmetry. In the language of our work we say that bulk $1-$form symmetry (a global symmetry of the bulk gauge theory) becomes $0-$form boundary symmetry.

7. Page 14, the title of Section 3.1 seems too big and does not provide the correct information. Maybe "Dijkgraaf-Witten theories" or "finite gauge theories" is better.

   In Section 3.1, we describe topologically-ordered phases in terms of **generalized symmetries**, which is a crucial aspect of our construction. Since we aim to emphasize the conceptual content and being accessible, rather than highly technical, we have for simplicity concentrated the discussion to Dijkgraaf-Witten theories. However, note that everything discussed in Section 3.1 naturally generalizes to any topologically-ordered phase, in particular the existence of line-like and surface-like symmetries. The points made in the section are thus not unique to Dijkgraaf-Witten theories. We chose the title "Symmetries of topologically-ordered phases" to illustrate the kind of symmetries that topologically-ordered systems exhibit. We believe that a more technical title would make it harder to connect the step-by-step logic of the paper.

8. Page 18, I wonder what the relation between $\mathbb{SOA}[G]$ defined in (3.17) and ribbon operators in Kitaev quantum double models is? I have trouble to identify $\mathbb{SOA}[G]$ with the notions I have learned from other references. Even if it is new, its relation to other familiar notions should be clear. For example, the "patch operators" in Ji and Wen's work (e.g. 1912.13492) and more, and perhaps already in Levin and Wen's original paper on Levin-Wen models as those string operators? The term "super-selection sector" also appeared in this subsection. Then one should state "super-selection sector" of what (certain net of local operator algebras). Are operators defined in Eq. (3.18) the operator of sectors? I also want to point out that there are even a few rigorous studies on the super-selection sectors in finite gauge theory in 2+1d (or 1+1d, depending on your taste). See for example arXiv:1012.3857 and arXiv:2201.05726.

   The ribbon operators in Kitaev quantum double models or in the Levin-Wen models are topological line (technically ribbon) operators in the bulk, constructed for those particular lattice realizations. We think of these operators more abstractly based on their algebraic properties, rather than specific lattice models. $\mathbb{SOA}[G]$ is essentially the algebra of such operators ending on the boundary (ie. creating no bulk excitations). See equations (3.4) and (3.9). A more directly relevant concept is that of bond algebra. The relation between this and the string operator-algebra has been elaborated in Section 3.5 in great detail and we have cited the relevant references. Despite their different construction, the recently developed notion of patch operators is also directly related to our string operator algebras. We have now added a sentence clarifying this similarity in the introduction of our paper.

What we mean by *super-selection sector* in this section are just sectors with well-defined anyonic flux in the cylinder (see equations (3.15) and (3.16)). The name is used in anticipation of the discussion in section 3.3. We show that $\mathbb{SOA}[G]$ is isomorphic to the algebra of G-symmetric operators in $1 + 1d$. Super-selection sector here has the traditional meaning: $\langle\psi|\mathcal{O}|\phi\rangle = 0$ for any local operator $\mathcal{O}$ and states $|\psi\rangle$ and $|\phi\rangle$ in different sectors. Here by local operator we mean any operator in $\mathbb{SOA}[G]$ (G-symmetric operator) with finite support. The operators in (3.18) are not "local" in this sense, as they do not belong to $\mathbb{SOA}[G]$ and they are infinitely long strings. They can map between different superselection sectors as shown in (3.23). As briefly discussed in section 3.7, one can see these infinitely long operators acting locally on the boundary. However, as G-breaking operators and thus not in $\mathbb{SOA}[G]$.

9. In Section 3.2, the discussion is model independent. The so-called "Hilbert space" is not the Hilbert space associated to the boundary of a lattice model. Instead, it is the space of spanned by boundary operators. I found the term "boundary Hilbert space" quite misleading. The term "the space of boundary operators" is not much longer. I guess that the authors might want to imitating the usual dualities in physics, which require two theories to have the same Hilbert space. But it might not be true (nor necessary) in this topological holography, right? I don't know. Maybe the authors should say something about it in the paper. About the terminology, I have a comment. As far as I can tell from other relevant references, some authors used "a map from 2+1d theories to 1+1d theories" and some authors used "holographic duality". I think that both are fine. However, it is better to make it clear what one mean by the term "holographic duality" and explain this subtle difference if you want to use it.

   We suspect that there might be a few misunderstandings here, that we will attempt to clarify. As we clarified in our response to previous points raised, there is no holographic duality in our construction. The bulk and boundary theories do not have to have the same Hilbert spaces.

   We want to address the point: *"The so-called "Hilbert space" is not the Hilbert space associated to the boundary of a lattice model. Instead, it is the space spanned by boundary operators."*

   What we call "boundary Hilbert space" is not a space of operators. As we state before equation (3.17), it is a representation space of $\mathbb{SOA}[G]$, a space where "boundary operators" act. We show that this space has the structure of a generalized pseudo spin-chain where $\mathbb{SOA}[G]$ acts as G-symmetric operators.

   In appendix C, we essentially employ the same construction in a much more concrete setting: the Wen plaquette model. There, we parameterize in great detail how this "boundary Hilbert space" is embedded in the full Hilbert space. Here, it can be thought of as the boundary Hilbert space very concretely.

10. Section 3.3. "Mapping to generalized spin-chains" contains the main idea of the holographic duality. I am not sure if the explanation in this subsection is successful. Instead, I found Appendix A very important. It is the key to understand this holographic duality, right? Maybe it is worthwhile to move it to the main text. I also recommend the following two papers 2112.09091 and 1801.05959, which I found very helpful in my own understanding of

the subject. However, I have to confess that this map or the holography is still very mysterious to me.

Appendix A is quite technical and we think putting it in the main text will digress the reader from the main message of the section. We believe that the reader who wish to know the details of the construction would consult Appendix A for further elaboration of the ideas spelled out in Section 3.3. The approach taken in section 3 is to derive most relations using simple topological properties and pictures, as it captures more universal aspects of the underlying construction.

In other words, while the technical details in Appendix A are specific to the 2+1d/1+1d construction, the logic laid out in section 3 naturally generalized to any dimension and setting.

11. Section 3.5, as I have already mentioned before, I think that the main idea of this subsection has already appeared in arXiv:1104.5047. In Eq. (3.38), g[G] is defined in terms of S-, T-matrices. I want to warn the authors that it was known that, in general, S-, T-matrices do not encode all the information of a modular tensor category as shown in this mathematical paper arXiv:1708.02796. I am not so familiar with these mathematical literature. I do not know if the examples of this insufficiency exist for finite abelian groups or not.

    The aim of this section is two-fold

    (1) To explain the action of dualities on the building blocks of the G-symmetric theories introduced in our paper, i.e. $\mathbb{SOA}[G]$. We derive the properties of these dualities when acting on $\mathbb{SOA}[G]$ (equation (3.47));

    (2) To construct a unitary operator that implement these dualities and the full Hilbert space containing all twisted superselection sectors.

    The purpose of this section is therefore very different from the work in arXiv:1104.5047.

    Regarding the second point, we emphasize that the construction works for finite Abelian groups and additional details might need to be considered for other groups. As it is established in Corollary 2.2 of the reference [154], all Abelian TQFTs (which we use to construct our spin-chain models) are described by Dijkgraaf-Witten theories for finite Abelian groups. Moreover, to the best of our knowledge, all untwisted finite gauge theories of Abelian groups can be distinguished as modular tensor categories by their modular data.

12. Section 3.7, the discussion around Eq. (3.61) is similar to the works by Fuchs et al in cond-mat/0404051 and hep-th/0607247 if SOA is viewed in 1+1d and to arXiv:1104.5047 if SOA is viewed in 2+1d.

    This short section was actually inspired by the works of Fuchs et al, as we wanted to connect some of their results from our approach. However, we would like to draw the attention of the referee to the point that this section is a **generalization** of the idea of Fuchs et al. Their works focus on conformal field theories (critical transitions), while our approach derives these properties for any G-symmetric theory. Thus generalising from critical points to any other gapped or gapless phase. Not to mention that the starting point is completely different. We however noticed that we forgot to refer to Fuchs et al's works in this section (they were cited in other parts of the paper). We have therefore added these references here as well, and thank the referee for pointing this out.

13. Section 3.8, as far as I know, all non-invertible symmetries in all non-chiral 2+1d topological orders were first constructed in arXiv:1104.5047.

    There might be a misunderstanding regarding this section, which we will attempt to elaborate on. This section is not about non-invertible symmetries of 2+1d topological orders. It is about emergence of non-invertible symmetries in G-symmetric $1+1d$ theories. Not all Hamiltonians in a given phase will have these emergent symmetries, but certain submanifold will (and this requires some fine-tuning). We relate the existence of such symmetries to edges of invertible bulk domain walls and give a simple prescription to construct Hamiltonians with these enriched symmetries.

    Later in sections 4.1 and 4.2 we discuss a bit further about these submanifolds of theories with enhanced emergent symmetries. For example we argue that if the Lagrangian subgroup associated to a gapped phase is self-dual under a subgroup of dualities (stabilizer subgroup), then by fine-tuning one can construct Hamiltonians with enhanced symmetries. Often these are emergent non-invertible symmetries.

14. Page 3.4, "We will call the group structure of a Lagrangian subgroup L the fusion structure of the corresponding gapped phase." I found this terminology very confusing. It was known that each of the gapped phases (or rather the gapped boundaries) can be described by a fusion category. Clearly, your fusion structure of the corresponding gapped phase is not the fusion structure of the associated fusion category. I think the full story is the following. A Lagrangian subgroup L "determines" (with some non-trivial convention) a Lagrangian algebra AL in the modular tensor category C associated to the bulk of finite gauge theory. According to the general anyon condensation theory (arXiv:1307.8244), the category CAL of right AL-modules in C is the fusion category of the gapped boundary. The fusion product in CAL should be the fusion structure of the gapped phase, which does not have a direct relation to the group structure on L. Moreover, it is the choice of subgroup L that determines the gapped phase. The group structure of the Lagrangian subgroup L is not a variable, it is already fixed by the definition of G.

    Reading the referees point here, made us realize that our terminology might be slightly confusing for the more mathematical reader. Our notion of *"fusion structure"* is not referring to any underlying fusion category describing a gapped boundary phase.

    In the case we are working with, global symmetries that are finite abelian groups G, any gapped phase is associated to a Lagrangian subgroup $\mathcal{L}$ of (what we call) the fusion group of Anyons $\mathcal{A}$.

    "The group structure of the Lagrangian subgroup L is not a variable, it is already fixed by the definition of G."

    Each gapped phase has an associated subgroup $\mathcal{L}$. But $\mathcal{L} \subset \mathcal{A}$ and $\mathcal{L}' \subset \mathcal{A}$ for two different gapped phases, are not necessarily isomorphic as groups. For example, when $G = \mathbb{Z}_2 \times \mathbb{Z}_4$ there are 10 gapped phases. However, eight of these $\mathcal{L}_1, \ldots, \mathcal{L}_8$ are isomorphic to $\mathbb{Z}_2 \times \mathbb{Z}_4$ while the other two $\mathcal{L}_9, \mathcal{L}_{10}$ are isomorphic to $\mathbb{Z}_2 \times \mathbb{Z}_2 \times \mathbb{Z}_2$. See Figure 10 on page 36, equation (4.11) on page 35 or Table 5 on page 87.

    We call these group structures (up to isomorphism) the ***fusion structures*** of each gapped phase. In this example, eight phases have the fusion structure $\mathcal{L}_1, \ldots, \mathcal{L}_8 \simeq \mathbb{Z}_2 \times \mathbb{Z}_4$, while two phases have the fusion structure $\mathcal{L}_9, \mathcal{L}_{10} \simeq \mathbb{Z}_2 \times \mathbb{Z}_2 \times \mathbb{Z}_2$ (note that these are up to isomorphism). The word fusion structure refers to the fact that these are the fusion rules of the anyons that are condensed in the given gapped phase.

Why is it important to associate each gapped phase with this structure? In the paper we show that $\mathcal{L}$ is the group of order-parameters, whose correlation functions can be used to detect a gapped phase (see equation (3.37)). In section 4 we present many results where the combined knowledge of global symmetry, the set of gapped phases and Lagrangian subgroups, web-of-dualites etc can be used to constraint and build the structure of the phase-diagram of a given Hamiltonian. Knowing how dualities map between gapped phases is crucial for this. There are generally many dualities and it is difficult to find explicit dualities that map between two different specific phases.

We show that there are ***no dualities between two gapped phases with different fusion structures***. In this sense, a fusion structure is an ***obstruction*** to dualities and is an inherent property of each gapped phase. Conversely, we believe that there exists at-least one duality between any two gapped phases with the same fusion structure. We do not have a proof of this, but this is true in all the examples we looked at.

We also extend the notion of fusion structure to critical theories, which gives information about which conformal field theories can be dual to each other. This notion plays an important role when we use dualities to compute full conformal spectrum of highly non-trivial phase transitions.

Note that in section 5, we make heavy use of fusion structures to solve phase-diagrams of non-trivial quantum spin chains. As explained in the introduction of section 5, our methodology is particularly powerful for global symmetries G with many dualities and few fusion structures. And weak when there are few dualities, and many fusion structures. Note that this just depends on the global symmetry G.

Therefore our notion of fusion structure is **an inherent feature of a $1+1$d gapped phase**, and it plays an important role in our construction. Since it is related to fusion rules of anyons that are condensed, it is essentially also related to the underlying category theory descriptions. But we take the referees point that it is a confusing terminology for mathematical readers with knowledge of fusion categories. We have therefore added some sentences to clarify this for such readers.

15. Section 4.3, I found the appearance of (4.15) very mysterious. I do not see how one can determines which CFT can appear at a critical point without studying it via concrete lattice models. But the sentences before (4.15) gives me an impression that this can be done very generally, maybe for Zn or all G? If so, can authors provide more details? From Section 5, I can see that lattice models are indeed introduced for concrete computation. So maybe a couple sentences should be add here to avoid the confusion. By the way, I think that the discussion of the critical points and phase diagrams might be new in literature. I hope that the authors should emphasize it in the introduction. I also wonder what the relation between this work and the work 2008.08598?

    We are a bit puzzled by why the referee finds equation (4.15) mysterious. This is just the standard definition of the partition function, as is well-known in quantum statistical physics. In our case, it is a *generalized twisted partition function*, where we have inserted symmetry operators along time (twisted boundary conditions) and space. This is a well-known notion, see for example the cited Petkova-Zuber paper [88]. In fact, this notion goes back to the literature on orbifolding and conformal field theories [174,175], as well as standard gauging in lattice spin models (see for example the standard references by Wegner [24] and Kogut [26]).

    The section is about lifting our dualities, to their action on partition functions. Here the partition functions are general and for any theory. The main result of the section are in equations (4.25) and (4.26). These show how the partition functions of two dual theories are related, and these hold for any theories.

This is later used for critical theories to compute the conformal spectrum of their underlying CFT. But this is only possible if the partition function on one side of the duality is already known. This is a far more modest claim than the claim of constructing all possible CFTs in the phase diagram of models with a given symmetry. Again, we stress that the construction works when G is any finite Abelian group.

We give many concrete and highly non-trivial examples of this in section 5.

Perhaps, the confusion stems from the following sentence

For example, we will see that it can be used to compute the conformal spectrum of many non-trivial phase transitions relatively easily.

We have modified this sentence as follows

For example, we will see that it can be used to compute the conformal spectrum of many non-trivial phase transitions relatively easily ***if we know the conformal spectrum on one side of the duality****.*

16. Section 5.1 and 5.2, this case has been studied by many people. I suggest to add some relevant references here.

    We chose to start with these well-studied examples so as to exemplify the framework of the paper in a relatively-known setting. Concretely, the aim was two-fold

    (1) To clarify the point that why the idea of studying phase diagrams using dualities (that we introduced earlier) is not very powerful in the case of $\mathbb{Z}_N$-symmetric spin chains. The arguments are based on the notion of **fusion structure** we introduced earlier.

    (2) Furthermore, we used these simple examples to test our **new formula** (equations 4.25 and 4.26) for duality-related partition functions. We confirmed the results by matching them with the existing literature. In particular, we have re-derived the self-duality relation of $\mathbb{Z}_N$ parafermion CFT partition function, first derived in [185].

    We also added the references [37, 38] in this new version.

17. I think that section 5 contain main new results of this work.

    As we have detailed in previous responses, sections 4 and 5 are more or less entirely new results. Section 3 has a combination of new results and some known results, that are connected and derived from a new unified point of view. Section 5 makes heavy use of the results from sections 3 and 4.

18. Section 6, I think that the generalization to fusion category cases has already appeared in arXiv:2112.09091 and perhaps in arXiv:2110.12882 as well.

    We agree that in the references cited, models with fusion category symmetry were constructed. Using certain notions of module categories over modular tensor categories, they were able to construct dual models with dual symmetry. These works do not develop tools to study phase-diagrams of more general systems of such symmetry structures, which is what we had in mind in section 6.

19. I found many discussions in Appendix more interesting than the main text.

    We would like to thank the referee who found these appendices interesting. The appendices were meant for experts of the field, like the referee, however, we hope that a more general audience will find the main text more interesting.

List of Changes

Here, we collect the list of changes in the new revised version. For the sake of readability, all the newly-added sentences in the submitted version have appeared in the brown color. All reference and page numbering in the following refers to the newly-submitted version.

(1) We have added a few paragraphs in the Introduction, starting on page 4 and ending on page 6 right above *Summary of Results*, explaining (1) similar ideas in different contexts that appeared in the past, (2) the similarities and differences of our approach to the problem of unification of phases of matter with other approaches, and contextualizing the work, and finally (3) emphasizing on the practical aspect and concreteness of our approach to building up phase diagrams of various models from the ground up.

(2) Added a sentence in the introduction clarifying the relation between the string operator algebra developed in our work and the patch operators discussed in arXiv:1912.13492 (Wen-Ji) and arXiv:2203.03596 (Wen-Chatterjee).

(3) We added a few sentences at the beginning of Section 3.1 to describe the content of the section more concretely.

(4) We have added a paragraph at the end of Section 3.7 to emphasize that the construction in that section is new and generalizes the one that appeared in [39, 40].

(5) In Section 4.1, we have added a few sentences (as a footnote) after the sentence

    We will call the group structure of a Lagrangian subgroup $\mathcal{L}$ the fusion structure of the corresponding gapped phase.

    to clarify the confusion regarding the term "fusion structure".

(6) We added a sentence in the very first paragraph of Section 4.3 to clarify further the content of the section and to explain better when we can compute the full twisted partition functions of a transition.

(7) We have added a reference to the classic paper of Kramers and Wannier [41, 42] in Section 3.5, where we are discussing the generalized version thereof applicable to any finite Abelian group G.

(8) List of added references scattered throughout of the newly-submitted manuscript: [1, 4, 5, 6, 2, 3, 43, 37, 38, 44, 36, 30, 34, 40, 41, 42].

(9) All the other points/questions raised by the referee have been addressed in the above replies.

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