Review of "Topological holography: Towards a unification of Landau and beyond-Landau physics‘’ by Heidar Moradi, Seyed Faroogh Moosavian, and Apoorv Tiwari

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.

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. 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.

   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.

   - "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.

   - "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.

- "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.

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.

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 non-trivial 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.

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.

5. Page 14, " can symmetry-related aspects of a $1 + 1$d 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 $1 + 1$d are given by a collection of line-like operators, therefore which $2 + 1$d 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.

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

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.

8. Page 18, I wonder what the relation between SOA[G] defined in (3.17) and ribbon operators in Kitaev quantum double models is? I have trouble to identify 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.

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.

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.

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.

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.

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.

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 A_L in the modular tensor category $\mathcal{C}$ associated to the bulk of finite gauge theory. According to the general anyon condensation theory (arXiv:1307.8244), the category $\mathcal{C}_{A_L}$ of right A_L-modules in $\mathcal{C}$ is the fusion category of the gapped boundary. The fusion product in $\mathcal{C}_{A_L}$ 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.

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 $\mathbb{Z}_n$ 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?

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

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

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

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

I think that the main weak point of this paper is its ignorance with the relevant references. Since I am very fascinated by this mysterious holographic duality, I tried my best to go through some of the references and online talks in the last month. I will share some of my understanding of the situation.

1. One of the early paper on this subject is arXiv:1801.05959, which is based on the idea of strange correlators.

2. I think that [118] has a close relation to this paper under review, especially in established the holographic map. Another closed related work is [127], which also focus on finite abelian groups.

3. References [107,108] mainly discuss topological phases in a higher dimension and its boundaries. I did not see the idea of holography there. I happened to talk to one of the author in [107] and got the confirmation of my impression. The dualities studied in their works are dualities in the same dimension not holographic ones. Please double check, I could be wrong.

4. A vast generalization of the topological holography in all dimensions appeared in two somewhat related but also slightly different ideas:
   (a) Categorical Symmetry [114,92,116,117]: One can hear many online talks by Wen on this topics. [114] contains some discussion on Ising model and the usual KW duality that has some overlap with this work. Some study of the critical points and phase diagrams appeared in [117]. But they studied a very different topics (i.e. phase diagrams of the boundaries of 2+1d topological orders).
   (b) Topological Wick Rotation [123-127]: an idea both interesting and mysterious. These authors claim to have a rigorous proof of this mysterious idea in [124] for the 3d-TQFT-and-2d-CFT cases. Unfortunately, the proof is too mathematically technical for me to gain an intuitive understanding. I do not see the study of phase diagrams or critical points there. They illustrated their idea in Ising chain in [126], which is the simplest paper to read in this series. [126,127] has some overlap with this paper.
5. Some studies on 1+1d TQFTs with fusion category symmetries might also be related to this work (see for example 2110.12882) because the data for the construction are from one dimensional higher theories.
6. For the relation between topological holography and AdS/CFT, this paper arXiv:2203.09537 simply treats them as the same kind dualities with some inspiration from the Jackiw-Teitelboim dilaton gravity theory or the SYK model.