Referee report: SciPost by Mertens and Wu

Minimal Factorization of Chern-Simons Theory

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Summary

In their submission, the authors address the question of "cutting and gluing" Chern-Simons theory on an annulus. They work in the "edge mode" framework, with a twist: they seek a *minimal* set of edge modes made possible by the topological invariance of the theory (contrary to most edge mode literature they also work in the Poisson rather than symplectic framework). This minimal set of edge dofs is described by a finite, rather than infinite, dimensional phase space. This phase space is found to be a Drinfel'd double, which is recognized to be the classical limit phase space of a "particle on a quantum group". Although their abstract also mentions entropy in 3d gravity, the authors only very briefly comment on this point.

I find the manuscript features an interesting construction worth publishing. Unfortunately, I find myself unable to recommend publication of the manuscript in its present form.

General comments

Review v. Original material A first issue is the lack of clarity between what is original and what is not in the content of the paper (a lot of material certainly isn't). To the best of my understanding the main novelties presented in this article according to the authors are:

1. The idea of appealing to topological invariance of CS theory to access a *minimal* set of edge modes that allow for cutting and gluing.
2. The fact that these minimal edge modes are described by the classical analogue of a particle on a quantum group, i.e. some sort of "double" of the structure group of the CS theory.

However, the first idea is not new; and as for the second, there is a quite some literature on the Drinfel'd double appearing at point-like defects (punctures) in the closely related BF theory. (I would expect that there is literature on point-like defects and the associated algebraic structures also in CS theory, but I am not familiar with it – it would be interesting for the authors to compare with it.) Therefore, I would like to ask the authors to better explain what is new in their work and to better explain its relationship to what is known in the literature.

In fact, although their bibliography is extensive, most works are cited in unspecific ways that do not help clarify what is just a review/rephrasing of known material (and, I would claim, a large amount of the content of the paper is of this sort) and what is genuinely new.

(In this regard, I find it more informative when the relevant literature is cited at the relevant turns while explaining a concept, or even developing (reviewing?) a calculation, rather than briefly mentioning at the end of a long argument that a certain formula "was written down in the past in [XX]". As a reader, this would help me better contextualize what I am going to read with what I know from the literature, instead of slowly discovering that I am reading something I already knew. It is, of course, also a matter of giving credit where credit is due.)

It seems to me that the main novelty in the manuscript is (1) the derivation in the Poisson formalism, rather than symplectic (App. B-C, refs [38,41]), of how the r-matrix and therefore of the quantum group structure at the marked point (sections 2.5 and 3.1/4), as well as (2) possibly the specific quantum group structure that are found (this, however, would require a comparison to the literature that I find is currently lacking). I leave it to the authors to clarify what the novelty of their manuscript is and put the emphasis where necessary.

Informal exposition v. Mathematical rigor Another general concern I have regarding this manuscript is its widespread lack of rigor. This is not to say that the main results are wrong (I have no reason to suspect this, in fact), but I do find that the mismatch between the informal exposition and the technical nature of the material has lead to imprecise (and possible wrong) statements. Also, certain key formulas, on which the rest of the manuscript relies, are not justified at all. I will provide specific examples, and questions, below.

Use of the word "physical" In line with the vast majority of the literature on the topic of "edge modes", the authors use an undefined notion of what is "physical" to justify much of their setup – in particular the distinction between the two kind of boundaries and dofs.

I never understood those arguments, which to me seem circular or anyway rather empty. I personally prefer either (A) a more pragmatic mathematician's approach (like in: from these definitions/choices, no questions asked where they come from, these consequences follow), or (B) a more concrete physical approach centered on more precise model-building (what *is* the boundary? The literal edge of a 2d superconductor? A perfectly conducting plate? A fictitious surface where no boundary conditions are imposed? How do I model it?). Solution (A) seems much more convenient in this context.

For ease of writing this review, I will proceed in "chronological" order, i.e. section by section, instead of by importance. I apologize if this makes my review less practical to navigate for the authors, and the editors.

1 Section 1

1.1 Edge states

The authors write:

> A crucial role is played by local degrees of freedom that live at the entangling surface, called edge states, that facilitate a factorization of the model. These degrees of freedom are fictitious according to an observer who has access to the whole space ("two-sided observer"), but are an intrinsic part of the state space of an observer who only has access to half of the state space ("one-sided observer").

I find very hard to make sense of this kind of paragraph, where none of the relevant concepts is defined or explained a priori. In particular, what makes a dof fictitious vs. physical? Is there a mathematical difference? An experimental one? How do I decide if a dof is of either kind? In particular, how can I draw the difference between fictitious vs. physical dof in relation to a minimal vs. non-minimal edge extension of my phase space? As I said above, I think the best solution to this issue is to use the pragmatic approach (A).

Also, the above considerations, seem somewhat in contradiction with something the authors wrote just above (italic mine), which I think further strengthens my point:

> From this perspective, quantities calculated in the extended state space formalism are *not* necessarily intrinsic or physical.

The authors use many different words (seemingly?) interchangeably: edge modes vs. edge states vs. edge degrees of freedom, or phase space vs. state space. Since none of these is defined anywhere, I can't help but get confused, worrying that I am missing something. E.g. in a sentence like:

> Since the state space is a subspace of the space of functions on the phase space.

I am not sure what the "state space" actually is and even why it should be a subspace of the *space of functions* on the phase space.

Action: Please clarify and streamline the choice of language throughout the paper. Also, I would appreciate better clarity on the notion of edge modes, especially in relation to the fictitious vs. physical distinction.

1.2 Gauge v. Topological symmetry

This is a minor point compared to other criticisms, but it raises questions that I find interesting. Italic mine:

> However, Chern-Simons theory does not just have gauge invariance, but also has topological invariance, which *further* reduces degrees of freedom.

Although I can see, upon further reading, what the authors mean with this comment, I cannot help but notice that gauge symmetry *is* the reason behind the topological invariance of CS theory: the two are not separate, i.e., it is precisely the reduction of the gauge degrees of freedom that leads to topological invariance of the theory. Therefore, it *is* technically wrong to state that topological invariance "further" reduces the number of dofs wrt gauge invariance.

Action: Please fix the previous sentence.

Action (optional): I think it would also be quite interesting to try to make this point on gauge v. topological invariance more precise, since it would help clarify the question I asked at the previous point, which I here rephrase: what technical aspect of the CS gauge symmetry allows one to consider a more minimal set of edge modes, and under which circumstances? I understand that the answer is "topological invariance" but what I am trying to suggest is a more technical criterion, that I can verify and *compute* with (rather than just argue for), in this case allowing me to "reduce" the KM edge modes to the quantum group ones.

1.3 Formal derivation

a) I cannot quite follow the formal path-integral derivation going from (1.6)-(1.7) (which I understand) to (1.8)–(1.14).

Equation (1.8) seems a definition, but this is unclear. If I formally integrate over J, as suggested, I would find something like $\delta(A_L)$ – which however seems an unwanted result. How (1.8) "leads directly to" (1.9) is even less clear to me.

Moreover, there seems to be some typos in (1.8): I think it should be $A \rightsquigarrow A_L$ both in the measure and in the action; also, an integral $\oint_{\partial M}$ seems to be missing too.

I am also not sure how one goes from a J^μ defined only on ∂M to one defined for all $\vec{x}$ and $\vec{y}$, here I am assuming that $M = B \times \mathbb{R}$ with B some spacelike manifold to be split into two regions $B = B_L \sqcup B_R$ and $\mathbb{R}$ the time axis. Since (1.9) involves Poisson brackets, I assume that these are equal time brackets on a Cauchy surface, or a subregion thereof, i.e., that $\vec{x} \in B$ or maybe $B_{L/R}$. However, if J is defined only at the boundary[1] $\partial M = \partial B_L \times \mathbb{R} = \partial B_R \times \mathbb{R}$ (as suggested by (1.6)) I don't see how (1.9) can make sense.

In short, there is not much that I understand of the derivation of (1.8) and (1.9).

My confusion continues with the next equation, which now involves a "conjugate variable" whose origin in the action/path integral is completely unclear to me. Maybe what the authors mean is that this new dof is added by hand?

I also have troubles with the definition of $h = \int d\vec{x}\, g(\vec{x})$: the integral is essentially a sum, but how do I sum over group elements belonging to a non-linear space? (E.g. if $g(\vec{x}) \in SU(2) \simeq S_3$) To me this expression does not make sense.

Finally, what are the authors calling a "charge algebra"? They use this word before eqs (1.12)-(1.14) making me think that it is the 3 equations that form it, but right below they use the word in relation to eq (1.12) only.

When talking about its quantization a series of undefined symbols is used (probably relating to Hilbert spaces and irreps of G).

As a last note, I also have troubles with eq (1.16): not only it misses a left-hand side, but J' sits outside the integral where it is integrated. This can very well be a typo, but I think it speaks to my confusion about the definition of Z_L as including the integral over J. It would be more sensible to me to define a $Z_L(J)$ and a $Z_R(J)$ so that (1.16) becomes $< Z_L, Z_R >= \int DJ\; Z_L(J) Z_R(J)$, which might also help (at least in my view) make sense of (1.9)-(1.10).

[1] ∂M seems also an unfortunate notation, since it is not the boundary of M but, as I understand the text, that of $M_{L/R} = B_{L/R} \times \mathbb{R}$.

Action: I would like to ask the authors to clarify the derivations (1.6)-(1.16). Please correct me if some of my confusions are unfounded and/or correct the remaining issues highlighted above.

b) This is more a subpoint to the previous questions on edge states and the notion of "physical": under (1.8) the authors refer to J as "the edge degrees of freedom", to say below eq (1.14) that

> edge degrees of freedom are a set of charges (or currents) that satisfy a Poisson algebra, conjugate to the boundary large gauge transformations that have become physical at the boundary, and that Poisson commute among themselves.

Therefore, I have the impression that the authors have here already defined what is meant by edge modes in two different ways (currents vs charges). Moreover, they suddenly state that g are gauge transformation that "have become physical": once again, I cannot make sense of such statements.

Action: if possible, try to better clarify the concepts in use.

2 Section 2

2.1 Subsection 2.1

Personally, I would argue that the idea of minimal factorization described in this section, and key to the manuscript, is essentially identical to what was proposed in the cited article [26] – see e.g. the discussion above eq 5.1 or around 5.7. Although the construction of [26] relies on a lattice formulation (on a magnetic vacuum, rather than the customary electric choice), in the case of topological invariant theories like BF or Chern-Simons, the lattice can be freely refined or coarse-grained (compatibly with the topology of the underlying surface) in such a way that the choice of a specific graph/lattice becomes effectively irrelevant, and the results become in fact exact.

The work [26] differs from the present one in two important, related, respects: the state space is quantum and uses gauge theories for *finite* (as opposed to Lie) groups. Finite groups are needed for all expressions to be well-defined, and not be merely formal. This allows to avoid ill-defined distributional expressions (or worse, their product) such as (2.54) of the present work.

There is a third difference, between [26] and [DDR] and the present manuscript: the present article treats the "external" boundaries of the annulus, differently from the internal ones, while [26] can be argued to treat all boundaries like the "internal" ones of the present manuscript (alternatively, one could say that [26] and [DDR] only concern themselves with the description of boundary-less surfaces, maybe only 2-spheres, with punctures).

What is presented at the end of section 2.1 as "an alternative perspective" is nothing else than the reason why [26] provides exact results in the topological case, rather than discretized approximations.

Of course, this is not to say that [26] exhausts what is done in the present manuscript.

Action: what do the authors think of the above remarks? Do they agree with them? In what is their minimal proposal equal/different to that of [26]? I would like the authors to provide in the manuscript a more detailed comparison with the literature.

2.2 Section 2.2

a) Equation 2.7 for the Hodge star operator is wrong. The correct one is

$$**=(-1)^{k(n-k)}s$$

where n is the dimension of the manifold, k is the degree of the form $*$ acts on, and s is the signature of the metric used to define $*$. On the 2d cylinder that the authors consider, this reduces to $**=(-1)^{k(2-k)}s=(-1)^k s$. If $k=1$, as relevant in (2.8), then $**=1$ only if the metric is chosen Lorentzian, a condition not specified in the text. From this it follows that the eigenvector equation (2.8) can only hold in Lorentzian signature.

Similarly, definition (2.11) makes fully sense only for $k=0$, which is the case used in (2.9-10).

Action: please fix this small, largely inconsequential, imprecision.

b) Below eq 2.8 it is stated that those boundary conditions reduce the dynamics to a boundary WZNW dynamics. I think it would be valuable for the reader if the authors elaborated on this a bit more, giving some details on how to go from the boundary condition to a specific boundary dynamics.

Action: I leave it to the authors to decide how much to say about this. They could however at least cite a relevant article where the details are carried out.

c) Technically, equations (2.9)-(2.10) are incorrect, since W is not a single-valued function on the cylinder—unless one had restricted to the trivial monodromy sector. Also, later, W is correctly presented as a quasi-periodic function on the real line, rather than the circle.

Action: please clarify the definition of W from the onset, to avoid confusions.

d) Below equation (2.11), the authors introduce (I believe for the first time?) the phase space P_{annulus} – however, they do not explain how this is defined (and why). An explicit definition is given later (2.17), but this definition does not address why this is the right space to consider starting from CS theory.

Moreover, I find the definition in (2.17) difficult to read, especially if I am trying to understand what the elements of P_{annulus} are (a particular class of functions $\mathbb{R}^2 \to G$?) and even more so if I want to try and picture them, as stated, "as manifolds". I would find a more explicit description of (2.17) in the following terms easier to follow:

$$P_{\text{annulus}} = P'_{\text{annulus}}/G$$

where the manifold P'_{annulus} is a fibration

$$\pi : P'_{\text{annulus}} \to G$$

of fibre (are the fibres all isomorphic?)

$$\pi^{-1}(m) = L^m G \times L^{m^{-1}} G \ni (W(x^+), W(x^-))$$

and the quotient of P' by G is wrt the action (2.21).

Reading further I noticed that the projection $p : P' \to P$ onto the quotient by $\cdot/G$ seems closely related the authors' gluing map (cf eq 2.55–57). To avoid *defining* P_{annulus} in terms of a gluing, one would need (as I stated above) a different, a priori, definition of this manifold.

(Note that this gluing presentation of P_{annulus} is given later in 2.55, but was already implicit in the expression 2.17).

Action: please provide an *a priori* definition of what is meant by the phase space $P_{annulus}$ so that (2.17), or an improved version thereof such as the "gluing" (2.55), can be presented as the result of a computation.

2.3 Section 2.3

a) I don't quite understand the very first sentence of Section 2.3. What topology is it referring to? What does "identified" mean? And how does (2.17) "identify a topology"?

b) Is eq (2.13) a definition? Or is j defined a priori in a different way? What is the rationale behind this renaming of variables? Does it have annything to do with J from section 1?

c) Starting from this section, I am often confused with by the notation jumping back and forth between $x^{\pm}, y$ as elements of S^1 or of its universal cover $\mathbb{R}$. I find this confusing.

d) Where do the crucial equations (2.24)-(2.25) come from? How are they justified? That is: Where does the Poisson bracket come from? Why does it have this form?

e) In (2.27) and (2.28), what does $y \notin S^1_{L/R}$ mean? Since $W(x^+, x^-)$ is defined to take two arguments one in S^1_L and the other in S^1_R, one must have that in an expression involving $W(x^+, y)$ the variable y *must* be in S^1_R. So why not just writing this, instead of $y \notin S^1_{L/R}$?

f) Maybe worth mentioning K^{ab} is the inverse of K_{ab}: do you need any hypothesis for this inverse to exist?

Action: please clarify the above questions, and especially address point d) above. Equations (2.24–25) are *the* starting point for the entire manuscript, so their derivation cannot be left unclarified.

2.4 Section 2.4

a) Italic mine:

> As explained in Section 2.1, the physics is only sensitive to the homotopy type of the entangling boundary, *so we can simply set the topology of the Cauchy slice to be a disc with a puncture in the bulk.*

In fact I think it would be useful for the reader to be told what a puncture is, in this context. It is not just a marked point: it is in fact a marked point with a celium. The cilium at the puncture ensures that we are able to distinguish between two different "tadpole" Wilson loops ("small" loops starting and ending at the puncture): those that do not encircle the cilium are contractible while those that do encircle it are not. In other words, the cilium is a different way to represent the fact that the puncture was originally a circular boundary with a marked point on it (one can see the cilium as a "shrinked" version of the circular hole, and the puncture as the marked point on its boundary).

Action: clarify notion of puncture (optional, but imo welcome).

b) Eq (2.15) might once again be more suggestively written in terms of a fibration $\pi : P_\odot \to G$, $\pi^{-1}(m) = L^m G$.

Action: optional.

c) In relation to [26] and especially its companion paper by the same authors on the fusion basis [DDR], it is interesting to compare (2.54) to Ocneanu's tube algebra and its relation to the Drinfeld double of a finite group. There the fact of using a finite group allows one precisely to avoid ill-defined distributional expressions such as (2.54).

Action: please see if it makes sense to engage with the existing literature more constructively on this point. Also, it might be worth commenting on the distributional nature of eq 2.54: is the image of the map *actually* an element of the target space?

d) I find footnote 10 quite mistifying (italic mine):

> A priori, the monodromy m could be dynamical. But the puncture *plays the role of the horizon* for a *boundary observer*, so it is natural to impose the following boundary condition on the puncture to remove dynamics *invisible* to the boundary observer,
>
> $$\partial_u W(x, u) = 0$$
>
> An alternative motivation to introduce Eq.(2.50) is that the topological invariance implies the bulk Hamiltonian is zero. *If we don't impose any boundary condition on the puncture, the corresponding phase space will not give rise to a minimal extension due to the redundant dynamics on the puncture.*

I don't see how in CS theory, with no metric, we can talk about horizons. I also don't know what is understood to be an observer, and even less, a boundary observers, and as a consequence what could be or not "visibile" to it (what is the meaning of visibile?). The last sentence is similarly obscure to me.

Action: I would suggest the authors to either clarify or avoid such vague language, since the resulting text is (imo) impossible to evaluate in the present form.

e) The fact that the G-action 2.56 is a gauge symmetry in P_{annulus} I suppose could have been explained earlier on and a priori, so that the fact that we need to quotient it out comes as a consequence (and not as a "definition" of gauge symmetry). In fact, one could argue that this G action is the residual gauge symmetry at the marked point. I emphasize this, even though I am sure the authors have it clear, as an example of where I find the exposition too informal and too little structured, at the detriment of clarity.

Action: I leave it to the authors to decide whether to imrpove on this or not.

2.5 Section 2.5

a) There is a sudden change of notation here, whereby the $^{\pm}$ indices have been dropped. It might be useful to the reader to be warned that now one is focusing on only one side and therefore the index is dropped.

Action: I leave it to the author to decide.

b) Deducing Eq 2.58 from equations 2.27-28 seems possible only if y can be taken to be the position of the "entangling puncture". But this contradicts the idea stated above that y must belong to either one of $S^1_{L/R}$... In the same way that I thought eq 2.24-25 required a more detailed justification, so I think the same is true here. (Note that, as defined, W has two arguments which belong to the two distict boundary components respectively.)

Action: Please clarify this point since it has important repercussions in a later section.

c) Footnote 12: why these two situations are similar? In which sense?

Action: I leave to the authors what to do about this.

d) With reference to the crucial assumption:

> Since we are seeking a minimal extension that allows a proper factorization of the phase space, it is natural to assume $r_{12}(x_{12})$ is independent of the monodromy m.

I wonder: how reasonable is it that this is possible to achieve? Maybe it is obvious! But what seems nontrivial to me here is that r is not the integration constant ($\tilde{r}$ is, cf. also second sentence of section 5.2) and relationship between r and $\tilde{r}$ is rather complicated. But maybe it is enough to fine tune $\tilde{r}$ to "eat" the non-trivial dependence on m hat r seems to inherit via eq 2.64.

Action: please comment on this central point, i.e. the viability of their assumption. It might be that the answer to my question is rather simple, but given the importance of this point in the later construction, I believe it should be addressed explicitly.

e) With reference to Footnote 18: what is the a priori definition of a r-matrix? From the text one can see that a *classical* r-matrix is defined as a solution to the MCYBE, but what about the general r-matrix?

Action: please clarify, add a reference, or remove the footnote, since as it is now the footnote is not informative.

f) where does eq 2.73 come from?

Action: please clarify, this seems an important identity and to me (I might be wrong) it doesn't seem to be trivial. If it can be simply derived from some other equation, it is enough to state so, of course.

3 Section 3

3.1 Section 3.1

a) I find this section very confusing. $P_\odot = \bigcup_m L^m G$ is a Poisson manifold. We can use its Poisson structure to compute the bracket between any function of the variables $W(x)$ in $P_\odot$. Superficially, this is seems to be what is being done here, to compute the brackets of $g(x)$ and h. However, this is not the case. In fact, neither g nor h is uniquely defined by $W(x)$, in fact the map $(g(x), h) \mapsto W(x) = g(x)h$ is many-to-one, and therefore not invertible. Therefore, I wonder: what is going on here?

In fact, as it turns out, the goal of the section does not seem so much to split W into g and h, but to arrive at the DG phase space. So, how does one go from $P_\odot$ to the bracket (2.74-3.4-3.5) on DG?

The locality arguments seem to me not reliable. If anything all the W's are non-local functions of the original CS dof (the connection A). (In fact, going on-shell of the constraints, here the flatness constraint or in YM the Gauss constraint, destroys locality (in a controlled way). One could even argue that gauge redundancy is what is needed to restore a local description of the theory.)

To me a more convincing derivation seems to go along the following lines (but I have not tried to make it precise, so it might not be fully convincing either): first one observes that eq. 2.70 says that that m generates right translations of $W(x) \in P_\odot$ by G. Second, one uses the argument given in section 4, to derive a Poisson structure on G, the group with the right action on $P_\odot$, from the requirement that this action is Poisson. Finally, one puts together these two observations, with eq 2.74, to derive the brackets $\{h, h\}$, $\{m, m\}$, $\{m, h\}$.

This has the advantage of making clear the mixed nature of this algebra, mixing the CS dof m with the "(residual) gauge dof" h.

I have the impression that this (possibly slightly revised) argument is one of the core original results of the paper. If so, it should be given more emphasis – especially in relation to what is new about it and how it compares to other derivations of the Drinfeld double in the context of 3d gravity: cf. eg. [MN], [Dup+]. Note that these articles refer to a BF theory, which is a CS theory on a *double* gauge group (e.g. $\Lambda = 0$ 3d gravity is a SO(3) (or SU(2)) BF theory, or a $ISO(3) \simeq SO(3) \times so(3)$ (or $T^*SU(2) = SU(2) \times \mathbb{R}^3$) CS theory. See the point below.

Action: For me, this is one of the most important point in the conceptual structure of the manuscript. It should be made fully clear. Therefore, I ask the authors to clarify my confusion above and, if they think it fit, possibly restructure the argument. In this regard mine is only a suggestion and I leave to the authors to decide whether it is good or not. A comparison with other appearances of the Drinfel'd double in closely related contexts should be spelled out, or at the very least acknowledged.

b) Something that confuses me in the result mentioned here is this: the Drinfeld double DG is locally isomorphic to $G \times G^*$, with G^* a dual group. For example for $G = SU(2)$, one could choose G^* to be either the Abelian group $\mathbb{R}^3$ or the non-Abelian group $AN(2)$, so that $DG = T^*SU(2)$ or $SL(2, C)$ respectively. In general, however, G^* will not be equal to G. Here, however, the authors say that the bracket (2.74), (3.4) and (3.6) define a Poisson structure on the Drinfeld double DG — with both h and m in the same group G (by construction). Is this consistent?

This might be related to the point the authors make elsewhere about G having to be either complexified or split-real.

(In the references about 3d gravity above, the structure group G of the associated CS theory is already a double $G = DH$, and the split in the subfuctors $H \bowtie H^*$ arises by considering a BF polarization for the 3d gravity theory, before discretizing. Note that the discretization can be seen, a cell at the time, as a restriction of the theory to a disk with a boundary with marked points, yielding a setup not too dissimilar from the one presented by the authors.)

Action: I would appreciate it if the authors could explain this point to me. They can then decide how much of this discussion to include in their manuscript.

3.2 Section 3.2

Before eq. 3.17, the authors write $m = e^{-2\pi p/k}$ and $h = e^q$. However, for m and h to be in $U(1)$ it seems that a factor of i is missing. I am mentioning this in particular with reference to the statement

> Note that whether k is finite or taken in the linear limit ($k \to \infty$), the structure of this edge algebra is the same.

Since it seems to me that taking the limit "de-compactifies" $U(1)$ to $\mathbb{R}$, I don't quite understand their comment above whereby the two edge algebras are the same, since they involve very different underlying spaces (one compact the other not).

Action: can you please make the quoted sentence clearer, and better explain what you meant by it? Is my comment any relevant?

3.3 Section 3.3

a) What kind of 3d gravity theory do they have in mind? In particular: what signature and what sign of the cosmological constant are they considering?

b) Above eq 3.23, the notation $\mathcal{F}(SL_q(2,R))$ has not been explained/introduced (neither the $_q$ nor the $\mathcal{F}$).

c) Above eq 3.33 the authors talk about the limit $q \to 1^-$, which I am fine with. However, in the previous page they had just criticized calling a limit of this kind a $q \to 1$ limit (right below eq. 3.24). Is there a difference in the way the two $q \to 1$ limits are understood?

Action: please clarify points a)-c).

3.4 Section 3.4

a) Regarding the opening statement: I wonder, what is the analogue (if any) of this ambiguity in the choice of a (integrating) r-matrix at the level of the WZW theory?

b) In equation 3.39, I am confused about the viability of the constant term r on the rhs. The reason is that this term does not seem to be skew under the exchange $x \leftrightarrow y$, a property all other terms enjoy.

c) Footnote 27 is not very clear to me.

Action: please clarify points a)-c).

4 Section 4

a) The authors write in the first paragraph (italic mine): "*This action itself forms a Poisson manifold as well*, which we now identify as [...] G_s [...]". I think this is a typo. But for clarity: the action is the *map* $\mu : G_s \times P \to P$ and therefore is not a manifold. Please rephrase consistently.

b) Below eq 4.6:

> This is the same Sklyanin bracket as earlier for the coordinate space Poisson algebra (e.g. (3.21) in the case of SL(2,R)), *except here living on a different manifold (the group G instead of the phase space of the model $P_\odot$).*

As I tried to explain above, I disagree with this point of view. I don't think h has every been shown to "live" in $P_\odot$, since it cannot be unambiguously defined from $W(x)$.

Action: please clarify points a)-b).

5 Section 5

A few lines above the table the authors write (italic mine): "allowing a regluing or a *surjective factorization map*". I think they wanted to write *surjective gluing map.* Since factorization is the "inverse" of gluing, which in turn involves a quotient (i.e. a projection), the factorization map is always ambiguous: an ambiguity one has to "gauge fix" somehow to define a factorization map at all. Therefore, the (gauge fixed) factorization map cannot be surjective (it should, however, be injective).

Action: please clarify.

5.1 Section 5.1

Footnote 31 seems not really relevant since as the authors say the choice is highly non-generic and cuts off whole sectors of the theory.

Action: minor comment, I leave it to the authors to chose what to do.

5.2 Section 5.2

a) At the beginning: r vs $\tilde{r}$ as the integration constant. Small subtlety that might be confusing for the reader.

b) "Higher rank groups have more solutions, and hence a priori physically distinct and consistent minimal edge algebra factorizations." Does this correspond to anything in the CS action? Maybe to different boundary conditions and therefore WZW theories?

c) Eq 5.5 is a distribution in $y^{\pm}$. It is unclear to me how the expression on the rhs belongs to P_{annulus}, whose elements are functions of $(x^+, x^-) \in S^1_L \times S^1_R$. One can maybe integrate (average) in $dy^+ dy^-$ or something along those lines? Topological invariance should say that the averaging is inconsequential?

d) above eq 5.6 the map 4.4 is called the KM *extension* – should it be "gluing"? Anyway, I am not sure what the meaning of that sentence is, so I might just be confused.

e) Reference to [19] below eq 5.7 is an example of a vague and un-informative citation.

Action: please clarify a)-e). Particular attention should be given to c).

6 Section 6

Like in my comment d) to section 2.4, I find the last paragraph of "Towards an action principle" quite obscure.

The rest of the section on entanglement and 3d gravity are virtually the only mentions of these topics, even though they feature in the abstract. I think this is a bit misleading.

Regarding 3d gravity and the Drinfeld double see also the reference cited above (end of comment a) to Section 3.1).

Regarding the uniqueness of the minimal factorization map see [26] and its companion paper on the fusion basis (in particular: the tube algebra).

Action: I leave it to the authors how/if to restructure this section in light of all my comments so far. The above are some points that came to mind while reading it.

7 Appendix B

a) Equation B.2 is confusing since the Cartan 3-form is not δ-exact and therefore B.2 cannot be satisfied for any ρ. This is commented in footnote 34, but I still find it unsatisfactory for this cannot yield a global notion of phase space, whereas the r-matrix construction presented in the text seems to achieve that. Why one seems to give global results and the other not? Is there a subtle catch in the Poisson/r-matrix derivation whereby those structures are also not global? One could note that in the r-matrix/Poisson presentation one never invokes the decomposition $G \to (G^+, G^-)$... This seems related to my confusion above.

Also it might be worth mentioning that Ω_L (without ρ) is non-closed because one is picking only some of the factors in the closed $\Omega = \Omega_L + \Omega_R$, while in fact throwing m into the mix even though the second and fourth terms of C.15 cancel each other (since $m_+ m_- = 1$).

b) At the very end of this appendix, the authors say "the idea of modifying the symplectic structure to facilitate edge states and factorization is well appreciated in the literature, starting with the work [43]." However, [43] has nothing to do as far as I can tell with adding a ρ of this kind by hand. Do the authors see this differently? Do they see a closer parallel?

Action: please clarify a)-b).

References

[DDR] Delcamp, Dittrich, Riello. On entanglement entropy in non-Abelian lattice gauge theory and 3D quantum gravity. JHEP 2016, 102 (2016).

[MN] Meusburger, Noui. The Hilbert space of 3d gravity: quantum group symmetries and observables. Adv. Theor. Math. Phys. Vol. 14, Issue 6 (2010), 1651-1716.

[Dup+] Dupuis, Freidel, Girelli, Osumanu, Rennert. Origin of the quantum group symmetry in 3d quantum gravity. Phys. Rev. D 112, 084071 (2025)