

Referee Report

Modular quantization and black holes

Summary

The manuscript develops a framework called modular quantization for two-dimensional CFTs with an $SL(2, \mathbb{R})$ -deformed Hamiltonian in the heating phase, motivated by Witten’s observer-based algebraic formulation of quantum gravity and by black-hole information questions in AdS_3/CFT_2 . The author argues that the fixed points of the Hamiltonian flow require conformal cutoffs. With these cutoffs, the stress-tensor eigenmodes generate a single copy of a “modular Virasoro algebra” on each one-sided interval, and the corresponding highest-weight construction gives a GNS Hilbert space on which the one-sided local algebra is a type-I von Neumann factor. Removing the cutoff is then argued to produce a type-III₁ limit, while scalar primaries localized at the fixed points define an emergent center after taking an appropriate closure. This center is used to define an edge Hilbert space, related by open-closed channel duality to an “interior” Hilbert space of fixed-point primaries.

The second main part applies this construction to eternal BTZ black holes. The manuscript identifies the modular Hamiltonian with the boundary Hamiltonian associated with the BTZ ADM clock, reviews an annulus/open-closed duality derivation of a Cardy-like density of states, and relates the cutoff parameter $\Lambda\sqrt{d}$ to the BTZ horizon radius. It then uses a bottom-up stretched-horizon model with Dirichlet boundary conditions for a massless scalar field, following earlier work, to argue that microstate correlators approach the Hartle-Hawking-Israel correlator in the strict semiclassical limit. Finally, after adding the proposed center, heavy fixed-point states are argued to produce quasi-periodic behavior of correlators in the large- c limit, suggesting a mechanism by which nonperturbative gravity replaces the smooth horizon by stretched-horizon microstructure.

The paper is ambitious and contains several interesting ideas. The attempt to put dipolar/angular/modular quantization, algebraic type changes, open-closed BCFT duality, and BTZ stretched-horizon physics into a single framework is potentially valuable. If made precise, it could provide a useful language for black-hole Hilbert-space sectors and for the relation between semiclassical horizons and finite- G_N degrees of freedom. However, in its current form the manuscript does not yet give a sufficiently controlled derivation of several claims on which the conclusion rests. Many steps are plausible, but the distinction between theorem, motivated construction, and conjectural holographic interpretation is often blurred.

Recommendation

I recommend **major revision**. The manuscript addresses timely questions and appears suitable in scope for a high-energy theory journal, but the central claims about the operator algebra, the emergent center, and the BTZ/horizon interpretation require substantial clarification before publication. I do not think the paper should be rejected outright: the core proposal is original enough, and many ingredients are connected to established CFT and AdS_3 technology. The revision should focus on making the algebraic statements mathematically and physically precise, narrowing the strongest claims to what is actually shown, and improving the presentation enough that readers can check the argument without reconstructing missing steps.

Evaluation of the Main Argument

The most convincing part of the paper is the starting observation that the $SL(2, \mathbb{R})$ -deformed Hamiltonian in the heating phase has fixed points, and that quantization along the corresponding modular contours is singular without a prescription near those fixed points. The construction of cutoff contours and the emergence of a discrete set of modular modes follows a recognizable line from Tada's dipolar quantization and related angular quantization work. The physical interpretation of the conformal cutoff as a one-sided BCFT regulator is also natural.

The paper is less convincing when it upgrades this regulated construction into precise statements about C^* -algebras, GNS representations, type-I/type-III transitions, and nontrivial centers. These are delicate notions. In several places the manuscript uses local CFT fields as if they were bounded operators, treats unbounded fixed-point limits as elements of a center, and infers type-III₁ behavior primarily from the continuity of the modular spectrum. These statements may be physically well motivated, but the current derivation is not at the level expected for claims about von Neumann algebra type and centers. The paper should either provide a more careful algebraic formulation, using appropriately smeared or exponentiated bounded operators and specifying domains and closures, or explicitly present these claims as a conjectural algebraic model supported by CFT evidence.

The BTZ application is also interesting but overstates the degree of derivation. The entropy matching and correlator matching rely on several identifications: the annulus modular parameter with the torus Cardy limit, the choice of microcanonical energy, the relation $\Lambda\sqrt{d} = \pi r_h/l_{\text{AdS}}$, the tuning of stretched-horizon parameters to reproduce the Bekenstein-Hawking entropy, and the use of prior stretched-horizon scalar correlator results. These ingredients form a coherent picture, but at present they are not independent checks of the proposal. The paper should more clearly distinguish which results are derived within modular quantization and which are imported from earlier stretched-horizon models or fixed by matching semiclassical thermodynamics.

Major Comments

1. **The derivation of the modular Virasoro algebra needs to be made more transparent.**

The discussion of the central term, the cutoff prescription, and the quantization condition for $k(n)$ is central to the whole paper. The manuscript argues that equal-length cutoffs at the two fixed points are needed in order to obtain a real central term and a Kronecker delta, and then rescales the modes so that the final algebra takes the usual Virasoro form. This is plausible, but a reader needs a more explicit derivation of the limiting distribution and of the normalization of the central term. In particular, the transition from the sine factor in the regulated commutator to a discrete delta, and then to a Dirac delta in the $\epsilon \rightarrow 0$ limit, should be written in a way that fixes all factors of π , Λ , and \sqrt{d} .

The final one-copy algebra after imposing conformal boundary conditions has central term proportional to $c/6$, as expected if holomorphic and antiholomorphic sectors are combined, but the absorption of the cutoff-dependent zero-mode shift should be spelled out more carefully. The manuscript should also correct the apparent typo $\delta_{n+n',o}$ and explain whether n runs over all integers or whether the earlier half-integer possibility has any residual role. Since all later claims about the Hilbert space rely on this algebra, this section should be self-contained and checkable.

2. **The argument excluding the factorized boundary condition B.C-I is not correct as stated.**

In the discussion of boundary conditions at the cutoffs, the manuscript claims that imposing the condition corresponding to $T(z) = 0 = \bar{T}(\bar{z})$ at the cutoff points would force the analytic functions T and \bar{T} to vanish identically. Vanishing at one or two points does not imply that an analytic function and all of its derivatives vanish, unless the condition is imposed on an open set or unless further constraints are supplied. If the intended statement is that the condition must hold for a continuous family of cutoff phases or for all local boundary points on a small cutoff circle, this should be stated explicitly and derived. If instead the condition is only imposed at the endpoints of a contour, the conclusion does not follow.

This point matters because the manuscript uses it to motivate conformal boundary conditions as the only physically meaningful local choice and hence to justify the single-copy modular Virasoro algebra. The conclusion may still be right, but the present proof is insufficient.

3. The GNS and von Neumann algebra discussion uses local fields too freely as bounded operators.

The construction of a GNS Hilbert space at finite cutoff is one of the main advertised results. However, local CFT primaries and stress tensors are unbounded operator-valued distributions; they are not elements of a C^* -algebra of bounded operators simply because a particular conjugation avoids coincident singularities. The manuscript should specify the algebra being represented. For example, is it generated by smeared bounded functions of fields, by exponentials of smeared fields, by a net of local von Neumann algebras associated with intervals away from the cutoff, or by some formal operator algebra of modes?

The statement that finite norms imply bounded local operators is too strong. Finite matrix elements in a chosen dense domain do not imply boundedness. Similarly, the claim that local operators in any other state “locally look like the vacuum” does not by itself establish boundedness. The paper should revise this discussion using standard algebraic language: define a local net or a regulated mode algebra, construct the cyclic representation, identify the null ideal, and only then discuss the generated von Neumann algebra by taking a bicommutant. Without this cleanup, the type-I factor statement remains suggestive rather than demonstrated.

4. The type-III₁ and center claims need a more rigorous criterion.

The manuscript argues that the $\epsilon \rightarrow 0$ limit has type-III₁ behavior because the modular spectrum becomes continuous and hence the Connes spectrum is \mathbb{R}_+ . Continuity of the spectrum of one modular Hamiltonian is not, by itself, a proof that the resulting algebra is a type-III₁ factor. One must specify the algebra, the faithful normal states considered, and the relevant intersection of modular spectra. The cyclic and separating property of the limiting vacuum should also be demonstrated or assumed explicitly.

There is also tension between calling the limiting algebra a type-III₁ factor and then adjoining an emergent nontrivial center. A factor has trivial center. The manuscript sometimes says the original limiting algebra is type-III₁, while the enlarged algebra after adjoining fixed-point scalars is no longer a factor and may be closer to type-I on a new Hilbert space. This distinction is important and should be made precise with notation. I suggest separating the discussion into: (i) the limiting one-sided local algebra without fixed-point affiliated operators; (ii) the proposed enlarged algebra including bounded functions of fixed-point scalar limits; and (iii) the representation of this enlarged algebra on the proposed edge Hilbert space.

5. The construction of the emergent center from fixed-point scalars is currently too formal.

The fixed-point scalar operators are first described as unbounded and not part of the original one-sided algebra, then as affiliated operators, then as bounded after smearing, and finally as elements of a center. These are distinct operations. The paper should specify what is being smeared, over what support, and how the smearing can both make the operator bounded and leave it localized at the fixed point strongly enough to commute with the one-sided algebra.

The Lorentzian commutator argument also needs more care. It assumes that the fixed-point operator commutes with the two-sided modular Hamiltonian, that H_L commutes with all one-sided R -operators in the limiting sense, and that the equal-time commutator with a fixed-point operator follows from spacelike separation. These statements are plausible in a regulated theory, but they become subtle exactly in the limit where the fixed point is a horizon/boundary point and the operator is not in the original local algebra. The paper should either provide a regulated proof before the limit is taken, or soften the statement to an algebraic proposal motivated by the Euclidean channel.

6. The open-closed shrinking limit should be clarified.

The open-closed string duality discussion is one of the most promising parts of the paper. Still, the treatment of Cardy boundary states and Ishibashi descendants in the shrinking limit needs clarification. The text states that descendant contributions vanish as $\Lambda \rightarrow \infty$, but then writes a formula suggesting that the limit of $L_{-n}^{\zeta'}|h\rangle$ is $|h\rangle$. Presumably the intended statement is that all positive-level descendants are suppressed and only the primary component remains. This should be corrected and normalized carefully.

The coefficients a_h, b_h , boundary entropies, normalization of the limiting boundary states, and possible dominance of the identity sector should be treated consistently. If the limit of a boundary state is a sum over fixed-point scalar primaries, while the partition function is dominated by the identity, the manuscript should clearly distinguish the state-level shrinking limit from the entropic saddle. This distinction is important for the later claim that the full edge Hilbert space exists even though the strict thermodynamic entropy is vacuum dominated.

7. The entropy matching to BTZ is not yet an independent derivation.

The annulus partition function in modular quantization is argued to reproduce a Cardy density with entropy $S_{\text{micro}} = c\Lambda\sqrt{d}/3$ and logarithmic correction $-3/2 \log S$. The paper then identifies $\Lambda\sqrt{d}$ with $\pi r_h/l_{\text{AdS}}$ in order to recover the BTZ entropy. This is an interesting observation, but the identification appears to be imposed by matching rather than derived from the bulk geometry or boundary modular parameter. The manuscript should explain why this is the unique or natural identification, especially because the text also notes subtleties in compactifying the θ coordinate and in comparing the annulus and torus descriptions.

The logarithmic correction also deserves more careful treatment. The one-sided modular result gives a coefficient $-3/2$, while the later two-sided/factorized discussion gives -3 , in agreement with the radial quantization result. The paper should present this as a consistency condition and explain exactly which ensemble and which Hilbert space each logarithmic correction refers to. Otherwise readers may be left unsure whether the difference is a prediction, a regulator artifact, or a matter of comparing one-sided and two-sided partition functions.

8. The stretched-horizon bulk model is useful but should not be presented as fully derived from modular quantization.

The bulk analysis relies on a Dirichlet stretched-horizon scalar model and fixes the parameters ϵ_b and J_{cut} by imposing the Bekenstein-Hawking entropy. This is a legitimate

phenomenological strategy, especially given the earlier literature cited, but it is not a first-principles derivation of the stretched horizon from the boundary algebra. The manuscript should state more explicitly that the Dirichlet model is a bottom-up realization consistent with the modular quantization picture, not a unique consequence of it.

Several approximations in this section should also be made sharper: neglecting the J -dependence of the normal-mode spectrum, replacing sums by integrals in the combined $\epsilon_b \rightarrow 0$, $J_{\text{cut}} \rightarrow \infty$, $G_N \rightarrow 0$ limit, and using mean occupation numbers to convert the microstate correlator to a thermal Hartle-Hawking correlator. The statement that the exact HHI correlator is reproduced should be qualified by the assumptions under which variance terms and discreteness effects vanish. This would make the result stronger, not weaker, because it would identify precisely where finite- G_N corrections enter.

9. The paper should separate established results from conjectural interpretation.

The manuscript contains several strong interpretive statements: that adding the center implements nonperturbative gravity, that unitarity restoration requires stretched-horizon microstructures, that heavy edge states are horizonless geometries, and that the framework provides a single-CFT alternative to gravitational path-integral wormholes. These are interesting claims, but the paper’s derivations do not yet establish them at the same level as the algebraic mode calculations. I recommend explicitly labeling such points as conjectures, proposals, or evidence. A short theorem/proposal/interpretation structure would help the reader understand which conclusions follow directly from the calculations.

Minor Comments and Presentation Issues

1. The paper would benefit from a shorter, sharper introduction that lists the genuinely new results relative to Tada’s modular/dipolar quantization, angular quantization, the author’s previous modular quantization papers, and the Burman-Das-Krishnan stretched-horizon correlator work.
2. There are several typographical and language issues that should be corrected throughout. Examples include “Von-Neuman” instead of “von Neumann”, “it’s” where “its” is meant, “complimentarity” instead of “complementarity”, “degenrate”, “correltor”, “straightword”, and several subject-verb agreement problems.
3. Some text appears to be left from drafting rather than intended for publication. For example, the paragraph beginning “Here is a smoother and more formal rephrasing” should be removed or integrated.
4. Several citation placeholders or incomplete citations appear to remain, including empty citation braces and places where a reference is requested but not supplied. These should be fixed before publication.
5. The notation uses several different small parameters: the boundary cutoff ϵ , the stretched-horizon regulator ϵ_b , inverse temperatures, and sometimes β in the original Hamiltonian. A notation table would be very helpful.
6. The paper should consistently distinguish Euclidean modular time s , Lorentzian time t , the coordinate θ , and the compactified angular coordinate used in the BTZ image sum. Some of the claims about thermal periodicity and image sums depend sensitively on these identifications.

7. The formula for the closed-contour partition function in Appendix A is written schematically as an exponential rather than as a trace. If this appendix is retained, it should be made precise enough to justify the conclusion that the Cardy entropy contribution vanishes.
8. Appendix B is useful, but the role of the image sum and the precise analytic continuation should be stated more explicitly. Since the conclusion about quasi-periodicity of heavy states is used in the main text as evidence for unitarity restoration, the appendix should be self-contained.
9. The manuscript should avoid repeatedly calling results “exact” unless the assumptions and limits are specified in the same sentence. In several places “exact” means exact only after a strict semiclassical or thermodynamic limit and after discarding exponentially suppressed variance terms.
10. The acknowledgment of AI tools may be subject to journal policy. The author should check the target journal’s requirements and adjust the acknowledgment if needed.

Conclusion

The manuscript proposes a creative and potentially important framework for thinking about BTZ black-hole Hilbert spaces through modular quantization. The finite-cutoff construction and its relation to open-closed BCFT duality are promising, and the connection to stretched-horizon correlators is suggestive. However, the current version relies on several algebraic and holographic steps that are not yet established with sufficient precision. A revised version should focus on making the modular Virasoro construction fully transparent, formulating the GNS/type/classification statements with appropriate operator-algebra care, clarifying the status of the emergent center, and presenting the BTZ/stretched-horizon part as a controlled set of assumptions and checks. With these revisions, the paper could become a valuable contribution to the literature on modular quantization, algebraic AdS/CFT, and black-hole microstructure.