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Peer Review

Review of: "Towards Quantum Simulation of Lower-Dimensional Supersymmetric Lattice Models"

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The authors consider a lower-dimensional supersymmetric lattice model and how to formulate it for quantum simulations. They furthermore test the resulting formulation on quantum simulators, which highlights multiple strengths and weaknesses of the approach, thus giving valuable insight into worthwhile future research directions. Overall, I like the manuscript. It is well-written and easy to follow. The relevance and construction of the model are well-portrayed. The content is novel, timely, and interesting.

My main points for improvement are generally related to the presentation of certain decisions made and conclusions drawn. This may very well be a product of my ignorance regarding the explicit model studied, so I would appreciate it if the authors could expand or clarify these aspects.

The authors describe the model very nicely in section 2, but the following choice of encoding and circuit then seems to make use of additional theoretical knowledge that doesn't seem to be covered in the manuscript. This is most clearly seen in the choice of quantum circuit. The choice of Realamplitude makes perfect sense if it is known that the ground state has only real amplitudes. However, this doesn't actually seem to be stated in the manuscript, and in the conclusion, the authors state

“[...] the statevector results reveal that the general purpose Realamplitude ansatz yields reasonable results for systems with a modest number of bosonic modes. However, it does not appear to be sufficient to extract the untruncated behaviour of the theory.”

This seems to indicate that no such theoretical knowledge of the ground state is available, which begs the question of why the circuit was chosen. I understand if the answer to that is simply because it is easy to implement, but then the “general purpose” comment becomes confusing, as the Realamplitude circuit has a non-trivial symmetry that I would not expect to be satisfied without further theoretical knowledge of the ground state.

Similarly, it is unclear how many layers were used in the circuit. Was it just the two rotation layers and a single entangling layer for all cases, or did the number of layers vary with Λ? Again, if the circuit was left at the exact circuit shown in Fig 2 (device initialization is missing there, although |0> initialization would be my expectation), then that has implications which I would expect to be theoretically motivated (low entanglement to be first and foremost, but there are more subtle aspects related to the dimensional expressivity of the circuit and thus performance expectations of the VQE).

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Without having these answers, it is practically impossible for me to say whether the observed limitations are (a) model limitations requiring further work in terms of how we can map this to a computable problem, (b) algorithmic limitations requiring more work on how to solve the problem on the quantum device, or (c) self-imposed limitations because of poor choices made that render a perfectly solvable problem into an unsolvable problem.

The other main point of confusion I have is related to the digitization and efficiency claim. The authors justify the use of quantum computing because "it requires in principle a polynomial amount of resources". However, the number of Pauli strings shown in Table 1 is 2^{N-1} for the Harmonic Oscillator and significantly worse for the other two cases. This naively requires exponential resources. While there are certain optimizations that can be done, it is unclear to me whether these optimizations can reduce the total effort into the polynomial scaling regime. If that is indeed possible, then this would warrant an explicit discussion from my point of view, given the repeated reference to polynomial quantum resource requirements. This is partially picked up in the conclusion, with the authors saying that "efforts will also focus on more efficient digitization techniques to minimize the number of Pauli strings," but there is a massive difference between "this is known to be solvable with polynomial resources, we just didn't implement them or cannot implement them on current hardware," "there is good reason to expect polynomial resource scaling, we just need to figure out how to get there," and "although nobody has any idea how to attempt to implement this with polynomial scaling, there remains a chance that it could be."

All in all, I don't think these are massive issues with the work itself. Most of these points may very well be self-evident to someone familiar with the SQM model, but I believe clarifying these points would be greatly beneficial for someone who is not (like me). Even if the answers are "we don't know and we made decisions based on what can be implemented with today's resources," that is perfectly fine and does not take anything away from the value of the presented work, but I do think that it is important to be open about these kinds of practical decisions.

Declarations

Potential competing interests: No potential competing interests to declare.