Dear Dr. Garisto:

We would like to start the appeal process for this paper. The reason for the appeal is that we feel this paper has not been properly and fairly reviewed.

The referee of this paper and one of the referees of PRB paper BH11160 made a simple and basic mistake at very beginning: they misunderstood our model as a model on 3+1D space-time lattice, while our model is actually a model on 3D space lattice.

Referee of PRL: it is one more model of hypecubic lattice gravity
Referee A of PRB: which are basically compact angle variables defined on the vertices and links of a 4d hypercubic lattice.

Other negative comments from the referees are related to such a basic misunderstanding.

The second referee of PRB has a narrower definition of quantum gravity, which requires diffeomorphism gauge symmetry at lattice level. In this paper, we used a broader definition. We require diffeomorphism gauge symmetry only at low energies. So diffeomorphism gauge symmetry can be emergent.

We like to stress that the emergence of conventional gauge symmetries have been well established. So we feel that it is unfair to rule out the possibility of the emergence of diffeomorphism gauge symmetry at the very start.

(see the exchange below)

We hope our paper can be reviewed by referees who do not misunderstand our model as a 3+1D space-time lattice model. We also hope our paper can be reviewed by referees who have an open mind to accept emergent diffeomorphism gauge symmetry as a possible approach to quantum gravity.

Please let us know what do we need to do to start the appeal process?

Thank you very much

Xiao-Gang

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The second referee of PRB seems do not allow "emergent gauge symmetry" and has a narrower definition of quantum gravity.

Second Report of the Second Referee of PRB:
The response of the authors to the comments of your two referees show that they are wrong at a very basic level. Contrary to what the authors state General Relativity is a gauge theory as it is based on the notion of general covariance which is a local symmetry. Any hamiltonian description (whatever this may mean in this case) must necessarily involve local constraints whose solution...
is highly non trivial. From this point of view, one of the differences between GR and standard gauge theory is that in the latter the gauge group is compact while in GR the gauge group is non-compact. 

Our reply:
It appears that the referee thinks that quantum gravity, by definition, is a theory with some gauge symmetry described by non-compact gauge group. The referee believe that "Any hamiltonian description (whatever this may mean in this case) must necessarily involve local constraints". We strongly disagree with such definition of quantum gravity. It is well known (at least in the field of modern condensed matter theory) that gauge symmetry can be emergent from a system that has no gauge symmetry on lattice scale. In this paper, we try to show that diffeomorphism gauge symmetry can also emerge from a model that has no diffeomorphism gauge symmetry on lattice scale. We feel that it is unfair to say that our model "has nothing to do with gravity" because it has no diffeomorphism gauge symmetry at lattice scale. We have shown that our model has emergent diffeomorphism gauge symmetry at low energies, and thus it is a quantum theory of gravity.

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On Wed, 4 Nov 2009 17:02:47 -0500, prl wrote
> Re: LH12204
>     Lattice qubit model as a quantum theory of gravity
>     by Zheng-Cheng Gu and Xiao-Gang Wen
> > Dear Dr. Wen,
> > We have received your resubmission. Perhaps we should have been more explicit in our previous letter to you. When we said, "On the basis of all the reports on your work", we were taking into account the reports on your PRB paper BH11160. In fact, we consulted Referee 2 of that paper on this Letter LH12204, although we did not receive a separate report from that referee in the end.
> > The relevant question for PRL is not "is this right?" but "have the authors made a convincing enough case to be disseminated to the entire physics audience?" Here the answer is clearly "no". In fact, as the case BH11160 shows, your work is not even suited for dissemination to a specialized audience.
> > Yours sincerely,
> > Robert Garisto
> > Associate Editor
> > Physical Review Letters
> > Email: prl@ridge.aps.org
> > Fax: 631-591-4141
> > http://prl.aps.org/
> >
> > Physics - spotlighting exceptional research: http://physics.aps.org/

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