Dear Dr. Wen,

The above manuscript has been reviewed by two of our referees. Comments from the reports are enclosed.

These comments suggest that the present manuscript is not suitable for publication in the Physical Review.

Yours sincerely,

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Report of the First Referee -- BH11160/Gu

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The paper discusses a local lattice gravity model based on so-called qbit variables, which are basically compact angle variables defined on the vertices and links of a 4d hypercubic lattice. It is claimed that in the naive continuum limit (lattice spacing going to zero) the model reproduces some of the features of general relativity (GR), such as a helicity two massless mode, with possibly the correct couplings to matter.

An effort to find a new lattice model for gravity would be commendable, but I find there are a number of problematic aspects with the paper that prevent me from recommending publication. These include:

1) The author's lattice action is constructed by introducing a number of lattice fields in a suitable way, and then requiring that in the weak field limit the action reproduces GR. Unfortunately the model so
obtained only reproduces GR in this limit, and strong field effects will generally be different from GR. Thus it is not a theory of gravity yet.

2) It is argued that the action is locally gauge invariant, in the same way that compact lattice QED is locally U(1) invariant. Unfortunately GR is not the local gauge theory of the SO(4) group, it contains diffeomorphisms as well, as a consequence of general coordinate invariance. That is why the lattice formulation of gravity is generally regarded as a lot more subtle.

3) The authors argue (page 27) that the unphysical spin zero and spin one modes are massive, or in their language "gapped". Unfortunately this should not necessarily be regarded as a desirable feature, since in the continuum these modes are not massive: instead they decouple and can thus be removed by a suitable choice of gauge, as explained in standard GR textbooks (eg Weinberg 1972, page 256). Thus the existence of massive spin one modes would seem to point instead to a breaking of general coordinate invariance in the lattice model.

4) More generally the authors seem to gloss over the key issue of general coordinate invariance, which is crucial in obtaining the right degrees of freedom both on the lattice and in the continuum. They appear to constantly confuse local gauge invariance with diffeomorphism invariance. I suspect this confusion is in part the root of their problem in obtaining the right excitation spectrum, and in interpreting their results.

5) One basic issue I have is the complete neglect of the vast literature on lattice gravity, starting with the lattice theory of gravity (Regge 1961), and various later attempts at formulating gravity on a hypercubic lattice, and which eventually failed due to the graviton doubling phenomenon. The authors are strongly advised to educate themselves on this subject, and to consult appropriate references, including the recent monograph "Quantum Gravitation" by H.W.Hamber (Springer 2009), roughly half of which is seemingly devoted to the lattice formulation of gravity.

In light of the above comments I cannot recommend publication.

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Report of the Second Referee -- BH11160/Gu
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I have always admired the work of the senior author, who has done deep and serious work in the past, and so I am disappointed by this paper.

Unfortunately, the manuscript appears to be misguidedly pretentious. The paper is seriously flawed and it should not be published. Even if it were correct, PRB would not be the natural venue for its publication. It should be judged against the serious body of work that has been done in the subject, usually published in PRD, Classical and Quantum Gravity, JHEP and Nuclear Physics B.

The first few sections contain a compendium of incorrect statements not only on gravity (classical or quantum) but on quantum mechanics and gauge theory. I will not address this issue here as it will be
take me as long as writing a separate paper on quantum mechanics and quantum field theory.

The authors also do not appear to be aware of the huge body of very serious work that has been done on the assumed subject of the paper since at least the 1950's, in which many of the supposed new ideas discussed here had been debunked a very long time ago. The so-called "theory of light" is hardly new as it has been known since the mid 1970's as lattice gauge theory, by Wilson who developed the path integral version, and by Kogut and Susskind who developed the Hamiltonian version which is closer in spirit to the models the author likes. These authors had the good judgment (and deep insight) not to be fooled by formalism and to confused the model with the physics as the author of this paper does. On the gravity side there is a large body of work on "lattice gravity" which goes back at least to the work of Regge in the 1960's and which is nowadays known as simplicial quantum gravity. This approach does not have the shortcomings of Wen's work as it automatically has Einstein's GR as the naive effective low energy theory (and it is hence "emergent"). The problem all these theories have is that at the full quantum level their actual vacuum does not look at alike like that GR as their effective space-time geometry is not only widely fluctuating but it has a "filamentary" structure akin to that of a theory of scalar particles. The same problem is known to affect bosonic perturbative string theory away from it critical dimension (26). Contemporary string theory solves this problem non perturbatively (although whatever else it describes is a separate issue). Wen does not appear to be aware of that work and of the serious obstacles they had (and have) to face and only seems know the much less serious "loop quantum gravity" work.

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