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Foundations of Physics

Dear Editor

Here is my answer to the reports of the referees:

Reviewer 2:

> This paper offers a novel approach to understanding the field content and gauge interactions of the Standard Model. It embeds the usual electroweak doublet in a three dimensional Kaehler-Dirac field. These Kaehler-Dirac fields carry additional indices corresponding to generation number and a combined color/lepton number (rather similar at first sight to Patti-Salam models) and can be mapped into the parameters of a affine transformation. The author argues that the natural action of translations and rotations associated with this affine structure restricts the possible gauge groups to essentially SU(3)xU(1). This part seems clear to the referee.

This restriction to $SU(3) \times U(1)$ holds only for Wilson lattice gauge fields.

> The author then argues that electroweak gauge invariance is then an emergent symmetry associated with deformations of the affine parameters.

Not exactly. I argue that there appears, in the large distance limit, an effective field, which interacts with fermions in a way similar to gauge fields. No claims are made that fields of this type have gauge symmetry. Last not least, in the SM (after symmetry breaking) weak fields are massive, and the fermionic mass terms violate gauge invariance as well.

> I don't think this section is very clear. It seems that this requires a commuting interpretation of fermions which seems a very drastic departure from conventional wisdom. I realize the author tries to address this in the later sections of his paper but I am personally unconvinced.

Our approach is, indeed, a very drastic departure from conventional wisdom. Unfortunately, it is hard, if not close to impossible for me, to meet the objection "but I am personally unconvinced", which does not contain even a single hint about what is missed to convince the reviewer.

The approach requires a quantization of fermions based on canonical quantization. That means, the fundamental fields, indeed, commute. But the fermion operators, which we construct in the section about fermion quantization, anticommute as usual. Thus, we obtain (in the continous limit) the same anticommuting fermion operators as used in the standard approach.

Let's also note that the related section on fermion quantization contains several strong theorems. Especially, the most critical part – the isomorphism of the algebras of (commuting) spin field operators and (anticommuting) fermion operators – is an exact theorem in all dimensions. Moreover, an approximation for the Hamilton operator is required only for spatial dimension > 1. For spatial dimension 1 we have also an exact transformation of the Hamilton operator. While we need, of course, the case $dim = 3$, which requires some approximation, our departure from conventional fermion quantization remains drastic for $dim = 1$ as well. Thus, the fact that for $dim = 1$ we have an exact transformation in all parts, including the Hamilton operator, does not seem to leave much room for an unspecified rejection because the reviewer is "personally unconvinced".

I have made some modifications in the fermion quantization section of the paper to clarify that this section contains some mathematically strong equivalence results.

> In addition there seems to be no strong argument for the appearance of an SU(2) gauge field in the long wavelength limit.

I have rewritten most parts of the section about weak gauge fields. Theorem 2 and the formulas (28) to (36) allow now to compute effective gauge-like fields $A_\mu(x)$ from the coefficients used to describe lattice deformations. I hope this gives a sufficiently strong argument for the appearance of a gauge-like field in the long wavelength limit. Note that exact gauge symmetry for weak fields is not claimed to appear, even in the long wavelength limit.

> Overall this paper outlines a quite radical approach to understanding the Standard Model and most probably will not turn out to be correct

I could not resist here to comment that the same can be said about the mainstream competitors — string theory as well as loop quantum gravity: Most probably they will not turn out to be correct.

> (the author would need to show how Lorentz invariance is recovered, how mass terms arise and how the electroweak breaking occurs for it to be taken very seriously).

I would object that these criteria for being "taken very seriously" are not really fair. It would follow that, for an alternative approach, it is not sufficient to obtain the fermion content of the SM exactly, and the SM gauge group almost exactly — without the computation of the mass matrices it should not be taken very seriously. I would like to know which of the existing approaches, following similar requirements, should be taken seriously. As far as I know, string theory does not make, even after over twenty years of very intense research, definite predictions, nor about the particle content of the SM, nor about the mass matrices.

> Nevertheless off-the-beaten track approaches like this one can be important and the paper brings up a number of nice ideas. If the author can strengthen and clarify the sections of the paper concerned with the generation of the electroweak SU(2) I think it would be suitable for publication.

I would object that this criterion for suitability for publication is too strong. Accepting the request for clarification, I think, to require a strengthening of the results about electroweak fields is not really fair. It seems to me, that the results about fermions as well as strong gauge fields, taken alone, should be already sufficient for a publication, even without a single result about weak fields.

Despite this objection, I have heavily rewritten the relevant section, and, at least in my opinion, not only clarified, but also strengthened the part about the generation of electroweak fields. I argue now not only (as in the first submission) that the effective interaction acts on fermions via the generators of $U(2)_L \times U(2)_R$ (this is now theorem 3), but obtain, in the large distance limit of the correction terms, effective fields $A_\mu(x)$, which interact with fermions like usual gauge fields (theorem 2).

> That said there seem to be a couple of references missing concerned with models using Kaehler-Dirac fields to provide models for beyond standard models physics – I am thinking of the the paper by Banks et al Phys.Lett.B117:413,1982 and also concerning the absence of fermion doubling in lattice transcriptions of the Kaehler-Dirac equation (J. Rabin, Nucl.Phys.B201:315,1982). These should be added.

Done.

Reviewer 3:

> The author discusses an attempt to reformulate the standard model (SM) in terms of condensed matter physics.
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> It starts from the observation that the number of SM fermion doublets is equal to the dimension of the three-dimensional affine transformation group Aff(3). Here, right-handed neutrinos are assumed to exist so that neutrinos form usual Dirac particles.
>
> The three-dimensional space is latticized, and the fermions are expressed as staggered fermions on $\mathbf{Z}^3$. A $U(3)$ gauge symmetry, which contains the $SU(3)$ strong interaction, is incorporated by introducing corresponding Wilson's lattice gauge field. He explains that phonon-like d.o.f. corresponding to lattice deformations play a role of $U(2)_L \times U(1)$ gauge fields. The $U(2)_L$ contains the $SU(2)_L$ weak interaction, and the $U(1)$, linearly combined with the central $U(1)$ of $U(3)$, is regarded as electromagnetic $U(1)$.
>
> As he wrote, the derivation of $U(2)_L \times U(1)$ gauge fields from lattice deformations should be clarified, and the Higgs sector is not considered here.

Correct. I hope the modifications in the section about weak gauge fields have, at least partially, clarified the derivation of the $U(2)_L \times U(1)$ gauge fields.

> In appendix A, he discusses that general relativity has a condensed matter interpretation. Four-dimensional metric is expressed in terms of fields of density, velocity and stress tensor, and the continuity and Euler equations correspond to the harmonic gauge condition for the metric.
>
> Although the idea seems somewhat interesting, I think that a number of arguments need to be refined before the publication. In particular, it is not clear why the chiral structure of $U(2)_L$ arises under the assumption that right-handed neutrinos exist. In order to realize SM, the right-handed neutrinos have to be eventually decoupled. There should be some argument how to realize the decoupling.

What is applied here to the gauge-like correction terms for lattice deformations is the general postulate of translational invariance, which requires, that some direction in the leptonic sector remains invariant. It remains open, which direction. But, whatever the choice of this direction, there have to be some particles – the particles which contain the direction – which have to be left invariant by all gauge fields.

Because this “decoupling” is based on a fundamental symmetry, no further “decoupling mechanism” is necessary.

Anyway, I hope the modifications in the relevant section of the paper clarify this point.

Sincerely yours

I. Schmelzer