Referee Report on "Testing the Mechanism of Lepton Compositeness" by Afferrante, Maas, Sondenheimer, and Törek

This manuscript continues the series of studies that A. Maas and coauthors have made on implications of the basic requirement that physical states should be gauge-invariant. The paper maintains a rigorous approach in the tradition of axiomatic quantum field theory. Early work that emphasized the importance of gauge invariance for physical states was published by Fröhlich, Morchio, and Strocchi (FMS), as cited in refs. [2] and [3]. A novel result in the manuscript is evidence from lattice simulations confirming the key role of a gauge-invariant operator product of a fermion field and Higgs field via lattice gauge simulations. Because of the problems with trying to implement chiral gauge theories on the lattice, the authors use vectorial fermions for their study. They candidly admit the limitations of their work, saying "...we have yet investigated a quenched, vectorial system" where a fermion bare mass is allowed as a gauge-invariant term in the Lagrangian, in contrast to the true Standard Model, where bare fermion masses in the Lagrangian are forbidden by the chiral gauge symmetry. Nevertheless, they affirm that they have obtained evidence that the FMS mechanism is working in the theory. I believe that this work is a useful contribution to the literature and recommend its acceptance by SciPost. I have some comments and suggestions for the authors before the paper is accepted:

1. In Section II, the authors write down some relevant terms from the Standard Model Lagrangian, with a single generation of leptons, in Eq. (1). Because there is only a single generation, it appears that the chiral lepton part of the theory has a global SU(2) anomaly, as was discussed in E. Witten, An SU(2) Anomaly, Phys. Lett. B **117**, 324 (1982). Since the authors do not actually use this Lagrangian for their study, but instead one with vectorial fermions, this does not invalidate their numerical results. But they should either note the presence of the global SU(2) anomaly in the theory of Eq. (1) or use an even number generations of chiral fermions, which removes the global anomaly.

2. A major reason that the lattice approach to gauge theories is so powerful is that it can maintain exact gauge invariance, in contrast to continuum perturbative approaches. An important consequence of this, which is central to the analysis, is that gauge-variant quantities vanish under integration over the Haar measure. This was pointed out in the paper S. Elitzur, Impossibility of Spontaneously Breaking Local Symmetries, Phys. Rev. D **12**, 3978 (1975). It would be appropriate for the authors to cite this paper.

3. On citations, there is also another relevant early paper that the authors might consider citing, namely J. Bricmont and J. Fröhlich, An Order Parameter Distinguishing Between Different Phases of Lattice Gauge Theories With Matter Fields, Phys. Lett. B **122**, 73 (1983).

4. On p. 5, in Eq. (13), the authors should give expressions for the quantities c_Y and c_W. This is also important for Eqs. (14) and (15), which contain dependence on these quantities.

5. There is a confused citation on p. 5, where the authors seem to imply that Lee and Shrock did not carry out studies with Yukawa interactions; the footnote reads, "Note that gauge-Higgs-fermion systems without Yukawa interactions have also been investigated on the lattice [31,32]." This seems to imply that Lee and Shrock did not carry out

lattice investigations of gauge-Higgs-fermion systems with Yukawa interactions, but in fact they did, in the papers I-H. Lee and R. E. Shrock, Study of the Chiral Transition in a U(1) Gauge-Higgs-Fermion Theory with Yukawa Couplings", Phys. Lett. B **199**, 541 (1987) and I-H. Lee and R. E. Shrock, The Chiral Transition in an SU(2) Lattice Gauge-Higgs-Fermion Theory with Yukawa Couplings, Nucl. Phys. B **305**, 305 (1988).

6. On p. 6 the authors say "To obtain a lattice version, the standard discretization of the bosonic sector is used [30,33]." But this is unclear, because there are actually two standard discretizations of the bosonic sector, namely with variable-length and fixed-length Higgs fields. The authors should make clear which type of discretization they use. If they use fixed-length Higgs fields, they should comment on the effects of this approximation.

7. As is well known, Wilson fermions involve explicit breaking of (global) chiral symmetry, which must then be restored by tuning of the hopping parameter. The authors comment on this problm in the Appendix A, saying that "the use of Wilson fermions, which break chiral symmetry explicitly [34], could be problematic." As the authors should be aware, many lattice gauge theory simulations use staggered fermions, which have the advantage of retaining a continuous remnant of the original global chiral symmetry. It would be valuable for the authors to redo their analysis with staggered fermions to check their conclusions. I realize that this could involve a lot of work, unless the authors already have computer codes that implement staggered fermion simulations, so this suggestion is optional. Domain-wall fermions would also be useful, although they require extensive computer resources.

8. On p. 6, the authors write that "We do not expect a substantial influence of the fermions on the FMS mechanism regarding the mass spectrum of the theory." They should justify this claim or weaken it.

9. On p. 8, there is the statement that "For the spectroscopic results, we have in principle clear predictions from the analytical results in sections III B 2 and IIIC." But in order for this to be true, the authors should have given explicit expressions for the quantities c_Y and c_W. I could see any place where they gave such expressions. If one does not know what c_Y and $c)W$ are, then I do not see how it is true that one has clear predictions for spectroscopic results.

10. On p. 14, in Appendix A, the authors state that "due to the fact that Wilson fermions require mass renormalization [34], it would be necessary to change the values of the fermion parameters to keep the physical masses fixed when moving along such lines of constant physics", but also say that they do not do this in the present study. They say that "When in a next step a continuum extrapolation will be attempted, this will change". This seems to undercut the authors' claims somewhat, since it implies that they are not attempting in the present work to perform a continuum extrapolation. This is confusing, since elsewhere in the paper they seem to imply that their lattice spacing is sufficiently small to obtain physical results. They need to clarify this.

I hope that the authors will find these comments and suggestions to be useful. After the authors respond to these, the manuscript could be reconsidered for publication.