Comment on "Renormalization-group picture of the Lifshitz critical behavior"

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We show that the recent renormalization-group analysis of Lifshitz critical behavior presented by Leite [Phys. Rev. B **67**, 104415 (2003)] suffers from a number of severe deficiencies. In particular, we show that his approach does not give an ultraviolet finite renormalized theory, is plagued by inconsistencies, misses the existence of a nontrivial anisotropy exponent $\theta \neq 1/2$, and therefore yields incorrect hyperscaling relations. His ϵ-expansion results to order ϵ^2 for the critical exponents of m-axial Lifshitz points are incorrect in both the anisotropic $(0<m<d)$ and isotropic $(m=d)$ cases. The inherent inconsistencies and the lack of a sound basis of the approach makes its results unacceptable even if they are interpreted in the sense of approximations.

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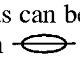
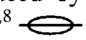
Leite[1] recently formulated a new renormalization-group (RG) picture of Lifshitz critical behavior. This work is built on his previous one[2] in which ϵ-expansion results to order ϵ^2 were reported for the critical exponents ν_{L2}, η_{L2}, and γ_L of an m-axial Lifshitz point. We have pointed out elsewhere[3] that these results, which are in conflict with ours,[4–6] are incorrect due to an erroneous evaluation of Feynman integrals. While the main points of our criticism in Ref. 3 apply equally well to Ref. 1, the latter makes mistakes on an even more basic level, as is discussed below.

(i) Leite's renormalization scheme *does not yield an ultraviolet (uv) finite (renormalized) theory*, and *the structure of the RG he formulates is incorrect.*

To see this, note that in dealing with the anisotropic case $(m \neq 0,\, d)$, he introduces a renormalization of the bare coupling constant u_0 and two renormalization factors Z_ϕ and Z_{ϕ^2} to renormalize the vertex functions $\Gamma^{(N,L)}$ with $N>0$ ϕ fields and L insertions of ϕ^2. Thus a *single* renormalization factor Z_ϕ is available to absorb the $\boldsymbol{q}$-dependent primitive uv divergences of $\Gamma^{(2,0)}(\boldsymbol{q})$, where $\boldsymbol{q}=(\boldsymbol{k},\boldsymbol{p}) \in \mathbb{R}^m \times \mathbb{R}^{d-m}$ is a d-dimensional momentum. Yet both $\partial\Gamma^{(2,0)}(\boldsymbol{k},\boldsymbol{p})/\partial k^4$ as well as $\partial\Gamma^{(2,0)}(\boldsymbol{k},\boldsymbol{p})/\partial p^2$ are primitively divergent. Using his normalization conditions (2a)–(2e), one can determine Z_ϕ such that the uv singularity $\sim p^2/\epsilon$ of $\Gamma^{(2,0)}$ gets absorbed by Z_ϕ, i.e., via the counterterm $(Z_\phi - 1)|\nabla_{(d-m)}\phi|^2/2$. But a pole $\sim k^4/\epsilon$ will then remain in his "renormalized" $\Gamma_R^{(2,0)}(\boldsymbol{k},\boldsymbol{p})$ because, with this choice of Z_ϕ, the counterterm $(Z_\phi - 1)|\nabla_m^2\phi|^2/2$ does *not* cancel this divergence, as can be seen from our result (B4) in Ref. 5 for the graph . Conversely, if Z_ϕ is determined so as to cancel the divergence $\sim k^4/\epsilon$, employing, e.g., his normalization conditions (4a)–(4c), then a term $\sim p^2/\epsilon$ will remain in $\Gamma_R^{(2,0)}(\boldsymbol{k},\boldsymbol{p})$. That not both primitive divergences can be absorbed by a single renormalization factor Z_ϕ is borne out by the fact that the renormalization factors associated with the above counterterms, called Z_ϕ and $Z_\phi Z_\sigma$ in Ref. 5 and explicitly given in its equations (40) and (41), differ. (Fixing them by an appropriate normalization condition rather than by minimal subtraction of poles would change their regular, but not their singular, parts.)

Hence Leite's renormalized function $\Gamma_R^{(2,0)}$ is ill defined, and since the renormalization parts of $\Gamma^{(2,0)}$ appear as divergent subgraphs of other vertex functions, his "renormalized" theory quite generally has this deficiency. The fact that the counterterms he employs are insufficient to subtract all primitive $\boldsymbol{q}$-dependent divergences of $\Gamma^{(2,0)}(\boldsymbol{q})$ implies that uv singular pieces of *nonlocal* form produced by higher-order graphs containing the subgraph[7,8] , such as , *will not get canceled by the subtractions provided by the counterterms to two-loop order*.

(ii) Leite's insufficient choice of counterterms *is biased towards giving the incorrect value $\theta = 1/2$ for the anisotropy exponent $\theta = \nu_{L4}/\nu_{L2}$.*

That is, if his renormalization scheme worked, rather than being plagued by the unacceptable inconsistencies (i), the ratio of the renormalization factors associated with the counterterms $\propto|\nabla_m^2\phi|^2$ and $\propto|\nabla_{(d-m)}\phi|^2$, i.e., the renormalization factor Z_σ of Ref. 5, would have to be uv finite. This in turn would imply the value $\theta=1/2$ for the anisotropy exponent to all orders in ϵ. Indeed, Leite finds the value 1/2 for both $\nu_{l4}/\nu_{l2}=\theta$ and η_{L2}/η_{L4}.[9] Yet this is wrong because Z_σ *must have poles in* ϵ as we saw above. As a consequence, the ϵ^2 term of θ is *nonzero* [cf. Eq. (84) of Ref. 4 and Sec. 4 of Ref. 5].

(iii) Leite obtained *incorrect hyperscaling relations* because he missed the fact that θ is an independent exponent, not identical to 1/2 for all $\epsilon>0$.

For example, his results (54a)–(54d) for δ_{L2}, β_{L2}, δ_{L4}, and β_{L4} do not hold. These relations violate standard scaling laws such as

$$\beta_{L2} = \frac{\nu_{L2}}{2}[d-2+\eta_{L2}+m(\theta-1)] \tag{1}$$

whenever $\theta \neq 1/2$.[10]

(iv) The author apparently *misunderstands* the *role played by the variable* σ, as his remarks in the last paragraph of Sec. III and in Sec. VI.A indicate.

Since the classical scaling dimensions of the momentum components p and k differ, a dimensionful parameter σ is indeed needed to relate them: $\sigma^{1/2}k^2$ and p both have dimension $(\text{length})^{-1}$. It is true that σ could be set to unity. However, the important point the author misses is that an initial value $\sigma=1$ gets mapped under RG transformations $\mu\to\mu\ell$ onto a scale-dependent one[4,5] $\bar{\sigma}(\ell)$ *different from unity*.

(v) The author's $O(\epsilon^2)$ results for the critical exponents of the *isotropic Lifshitz point* ($d=m$) *are also false*; the discrepancies with known results[6,11] are again *due to his incorrect calculation of Feynman integrals*.

In his treatment of these integrals, he constrains internal momenta over which one must integrate to be orthogonal to other, external momenta. As a consequence he gets even the simple one-loop integral $I_2(K)$ defined in Eq. (150) wrong. The error occurs already in the transition to Eq. (151). Similar ''approximations'' (mistakes) are made in his calculation of two-loop integrals. He asserts that our results in Ref. 6 could not be trusted because we absorbed a convenient factor F_d in the coupling constant. He is quite mistaken: The choice of such a factor corresponds to a uv finite reparametrization of the theory which does not affect universal quantities.

(vi) We *fail to see* that Leite's (incorrect) ϵ-expansion results *qualify as acceptable approximations*.

Being unaware of the fundamental problems of his approach mentioned above, he obviously thinks that his ϵ-expansion results are correct despite the approximations he made in his computation of Feynman integrals. Evidently, this is *not* the case.

We are convinced that the property of the dimensionality expansion to yield *asymptotically exact* series expansions is an extremely valuable one which *should not be sacrificed* except for compelling reasons. Nevertheless, one may ask whether Leite's results (or small modifications thereof) might be acceptable when interpreted in the sense of approximations, even though we see no need for approximate ϵ-expansion results. We believe that any such approximation scheme ought to meet two important criteria: (a) It should be justifiable by convincing physical and/or mathematical reasons and (b) it should be consistent and yield a well-defined approximate theory.

From our above critique it is clear that neither (a) nor (b) is fulfilled by Leite's analysis. Note that the goal (b) is not at all trivial to achieve when following the rationale of defining an approximate renormalized theory. If one determines counterterms such that they absorb the uv singularities of approximate Feynman integrals of the corresponding renormalization parts—rather than those of the true ones[12,13]—one inevitably runs into the problem that these renormalization parts also appear as subgraphs of higher-order graphs of the same and other vertex functions. Since the approximately determined lower-order counterterms do not cancel the true uv singularities of these subgraphs, nonlocal uv singularities generally will remain unless one succeeds in designing an approximation scheme that produces approximate expressions for Feynman integrals of, in principle, *arbitrary order* which comply with the local structure of their primitive uv singularities, so that a well-defined uv finite approximate renormalized theory results.

As long as one works with the correct, unapproximated Feynman integrals, the uv finiteness of the theory can be proved with the aid of the forest formula[12] by explicitly giving the subtractions that a general Feynman integral requires to render it uv finite and to relate the final subtractions of the primitively divergent graphs to the theory's counterterms. In order to be sure that the approximation scheme yields a well-defined renormalized theory, one would have to extend such proofs to the approximated theory or at least present convincing evidence for its renormalizability. Depending on the choice of approximation scheme, a mathematically rigorous proof may well turn out to be more involved than familiar renormalizability proofs of the proper, unapproximated theory.

In summary, Leite's analysis has no sound basis, is plagued by inconsistencies and uv problems, and his results are incorrect, failing even to qualify as acceptable approximations.

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[7] The bulk counterterm $\propto(Z_\phi Z_\sigma-1)$ is also needed to cancel the nonlocal singularities one encounters at two-loop order in semifinite extensions of the model considered by Leite; see Ref. 8 for details.

[8] H.W. Diehl, S. Rutkevich, and A. Gerwinski, J. Phys. A **36**, L243 (2003).

[9] In general, one has $(2-\eta_{L2})/(4-\eta_{L4})=\theta$. If θ were 1/2, this would imply $\eta_{L2}/\eta_{L4}=1/2$.

[10] The correct hyperscaling relations [e.g., Eq. (1)] can, of course, easily be derived also by phenomenological scaling arguments.

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