# Questionable arguments for the correctness of perturbation theory in non-Abelian models

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We analyze the arguments put forward recently by Niedermayer *et al.* in favor of the correctness of conventional perturbation theory in non-Abelian models and supposedly showing that our superinstanton counterexample was faulty. We point out that for the $O(N)$ models their arguments amount to a reformulation of the problem in terms of yet other unproved assumptions, while for the gauge theories they are clearly inapplicable. We reply also to the claim that the $S$-matrix bootstrap approach of Balog *et al.* supports the existence of asymptotic freedom in the $O(3)$ model. [S0556-2821(98)03903-4]

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A recent paper by Niedermayer, Niedermaier, and Weisz (NNW) [1] purports to show that our criticism of the standard dogma regarding the alleged difference between Abelian and non-Abelian models is exaggerated and that there are good reasons to believe the orthodoxy. It is a positive development that members of the high energy physics community are now beginning to pay attention to the fact that this central issue for particle physics remains mathematically unresolved and that at least some arguments are needed in support of the conventional scenario. On the other hand, we think that the arguments presented in [1], while not sufficient, may be misleading some readers into believing that the issue has been settled. Therefore we feel compelled to once again attempt to clarify where the troubles lie. In spite of the fact that our view of these matters differs from theirs, we appreciate their efforts to elucidate these important issues and deplore the lack of interest manifested by most particle and condensed matter physicists.

We begin by recalling that it is generally claimed, and repeated in the opening paragraph of [1], that the reason for the necessity of a nonperturbative definition of QCD is to study its nonperturbative properties, such as its spectrum. As we have been stating repeatedly [2], there is a much more fundamental reason: One needs a nonperturbative definition of quantum field theory, because perturbation theory (PT) produces answers in the form of divergent (nonconvergent) formal power series. To interpret such series, and associate a numerical value with them, one needs a nonperturbative definition of the theory.

For theories such as QCD and the two dimensional (2D) nonlinear $\sigma$ models the lattice version provides the needed nonperturbative framework. Many interesting questions, such as the spectrum, the relevance of PT, etc., can be asked and have well-defined, albeit sometimes unknown, answers. In particular, it has been assumed for years that if a PT computation is free of infrared (IR) divergences, then it must be ''right.'' In the nonperturbative lattice framework, ''right'' can be given a precise mathematical meaning: The difference between the nonperturbative (true) answer and the PT answer truncated at a given order must be appropriately bounded:

$$\left| C(x,y;\beta,L) - \sum_{i=1}^{k} c_i(x,y;L)\beta^{-k} \right| = R_k(x,y;\beta,L) = o(\beta^{-k}). \tag{1}$$

Here $C$ is some Green's function, say, $\langle s(x)\cdot s(y)\rangle$, $x$ and $y$ lattice coordinates, $c_i(x,y;L)$ the PT coefficients for $C$, $\beta$ the inverse (bare) coupling, $L$ the linear size of the lattice, and $R$ the remainder. The mathematical statement that PT is providing the correct asymptotic expansion of $C$ in powers of $1/\beta$ is nothing but a shorthand for the inequality (1). In many articles, conference presentations, etc., one encounters the following meaningless statement: ''The PT series is asymptotic.'' What is meant is that the series is divergent. To say that the series represents an asymptotic expansion makes sense *only* if a nonperturbative definition exists and Eq. (1) can be verified.

For $L$ fixed it is straightforward to prove Eq. (1). The subtle question is what happens when $L$ goes to $\infty$? In particular, in order to prove that taking the termwise limit $L\to\infty$ in Eq. (1) produces the correct asymptotic expansion of $C$ one must control the remainders $R_k$, rather than merely prove that the limit $L\to\infty$ of $c_i(x,L)$ exists, as has been assumed for years in particle and condensed matter physics. In spite of vigorous attempts by mathematical physicists, this feat has been achieved so far only for Abelian cases [3], but Niedermayer *et al.* claim to have found a new line of attack, which supposedly, if not rigorous, makes it entirely plausible that the same is true for the non-Abelian cases. Unfortunately, as we will argue next, we find that their arguments are insufficient. Moreover, as we will indicate below, a crucial ingredient of their argument is a certain feature of the 2D nonlinear $\sigma$ models which is clearly not shared by the physically more interesting case of 4D Yang-Mills theories.

In their paper, Niedermayer *et al.* now make clear that they are considering the asymptotics obtained by letting $L$ go to infinity as a certain function (actually a power) of $\beta$, and from this they draw, using additional assumptions, certain conclusions about the asymptotics one would obtain by first letting $L$ go $\infty$ at fixed $\beta$ and then sending $\beta$ to $\infty$. It remains an open question if the assumptions made by them, (i) the equality of the PT coefficients with Dirichlet and free boundary conditions (BC) in the limit $L\to\infty$, and, (ii) $|R_k^\alpha(\beta,L)| \leqslant B_k(\ln L)^{p(k)}\beta^{k+1}$, are actually correct. Point (ii) above is actually quite plausible because, as we have shown in our superinstanton paper [4], at large $\beta$ the distance needed for the spins to rotate by an average angle of $O(\beta^0)$ grows exponentially with $\beta$ [the action of one superinstanton being $O(1/\log(L))$] and thus in a box of size $L$ the spins should be pretty well aligned and PT probably correct. This is an interesting observation made by NNW because it tends to suggest that in the $O(N)$ models the large fluctuations may be associated with terms of $O(\exp(-\beta))$. However, this feature is special to 2D $O(N)$ models and as we showed in our other superinstanton paper [5] in gauge theories it takes only a box of size $\beta$ to obtain fluctuations of $O(\beta^0)$. On the other hand, point (i) above is highly nonintuitive, hard to prove or even to verify at, say, $O(\beta^{-3})$, $N$ arbitrary. In fact let us emphasize that even the IR finiteness of PT with Dirichlet or free BC is far from obvious and does not follow from David's proof [6]. The latter was given in the continuum, using a magnetic field regulator and dimensional regularization. Thus, while (i) above may actually be true, it is far from obvious and NNW have not even verified it for $O(\beta^{-3})$, $N$ arbitrary.

If one, however, accepts assumptions (i) and (ii), together with the unproved but eminently reasonable correlation inequalities, one can indeed conclude that in the $2D$ $O(N)$ nonlinear $\sigma$ models PT produces the correct asymptotic expansion at a fixed lattice distance. But a fixed lattice distance is not the case relevant for the continuum limit (hence our disagreement [5] with David's criticism) [6]. For taking the latter limit, one must also let $|x-y|$ diverge as a given function of $L$ or $\beta$ (see [7]). How PT would fare then is a completely open question. What is clear though is that NNW's argument would not apply even for the continuum limit of 2D $O(N)$ models because the lattices relevant for this limit are also $O(\exp(\beta))$ and according to naive tree level PT even larger than the distance over which the spins undergo fluctuations of $O(\beta^0)$ (see [7] for details). Therefore, even if NNW's arguments were correct for PT at a fixed lattice distance in the 2D $O(N)$ models, there is still good reason not to believe that, for instance, the standard prediction regarding the Callan-Symanzik $\beta$ function (and hence asymptotic freedom) is correct.

Next let us discuss their claims regarding superinstantons (SI's). First, while they are correct in stating [after Eq. (2.32)] that the limits $\beta\to\infty$ and $L\to\infty$ cannot be interchanged for SIBCs, so far they have not proved that those limits can be interchanged for *any* BC, and so their observation does not justify their calling SIBCs ''sick.'' It is also important that the difference between periodic and SIBCs found by us occurs *only* for non-Abelian models. The latter point, regarding this manifest difference between Abelian and non-Abelian models, which we both verified and explained in our paper [4], is totally ignored by Niedermayer *et al.* In fact, if SIBCs are ''sick,'' as they and David [6] would like to argue, how come they are alright for the $O(2)$ model? Or are Niedermayer *et al.* claiming that even for $O(2)$, the IR divergence they claim to have found at $O(1/\beta^3)$ is present? This is an important point which they (and David) should address. It was similarly ignored by Brezin, David, and Zinn-Justin (Ref. 20 in the Niedermayer *et al.* paper) when they tried to argue that in 1D the IR divergences occur ''for dimensional reasons'': This is clearly false, since dimensional analysis works the same way for $O(2)$ and for $O(N)$ $N>2$ models. Our explanation (see [4]) for this difference is that only for $O(2)$ is the Gibbs measure a function of gradients, hence IR finite [to see this, parametrize the spin as $(\cos[\phi(x)],\sin[\phi(x)])$].

Second, assuming that indeed PT with SIBCs does become IR divergent at sufficiently large order, while with, say, periodic BCs not, does it mean that taking the termwise limit $L\to\infty$ of the latter produces the correct infinite volume expansion? The mathematical answer is clearly *no*, since what is important for asymptoticity is control of the remainder, not merely finiteness of the terms. Of course, if one could prove the stronger assumption (i) above [and also (ii)], then that would control the remainder and prove asymptoticity. It should, however, be remarked that it is even a stronger failure of the perturbative method if different BC's not only give different results, but some give finite and others infinite answers. Since it is *a priori* not clear that the true infinite volume expectations actually have asymptotic expansions in inverse powers of $\beta$ at all, it is conceivable that an infinite answer is correct in the sense that it shows the failure of such an expansion (the true expansion may contain for instance logarithms).

But the reason to doubt PT is more serious than the mere absence of a mathematical proof. What we have stressed over the years [8–10], is that PT is a saddle point expansion, and for such a procedure to work, two conditions should be met: (1) the saddle should be ''sharp''; (2) the saddle should be far from the edge of the integration region.

In $O(N)$ models, on an infinite lattice, the Mermin-Wagner theorem guarantees that the saddle cannot be sharp. While this has been known for years, in our papers [4,10] we showed that in the infinite volume limit superinstanton configurations become degenerate with the trivial vacuum; consequently *any* correct PT expansion, irrespective of the BCs used, must include their contribution for $L\to\infty$.

A correct saddle point expansion should include expansions around configurations with many superinstantons. From the double well harmonic oscillator it has been learned long ago that it is crucial to include such configurations that are nearly degenerate with the ground state (in that case a gas of instantons and anti-instantons) in order to reproduce the correct asymptotics in the semiclassical limit (see, for instance, [11]). From the experience with that model one might expect that the effects of the superinstanton gas might be reproduced if one includes *all* saddle points, including the ones in the complexified spin space [12]. Niedermayer *et al.* fail to appreciate this point. But even more importantly, they do not seem to notice that their arguments for the ''sickness'' of SIBCs would equally well apply to the $O(2)$ model, where in fact there is no difference between Dirichlet and SIBCs.

Before concluding, let us make another point regarding finiteness versus correctness of PT: In 1D it is true that free BCs, which give a finite answer, give the correct answer. The reason is that in 1D one knows the highest eigenvector of the transfer matrix (''ground state''), which is just a constant on the sphere, and free BCs only project onto that eigenvector, making the expectation values independent of $L$. No such simple $L$ dependence occurs in 2D with any BC; hence there is no reason to make any analogy between finiteness and correctness with the 1D case.

In the final paragraphs of their paper Niedermayer *et al.* reiterate the standard nonperturbative arguments in favor of the standard dogma. We have answered many times these arguments, which we find wanting [9]. Let us briefly recap. In [13] we showed that in the $1/N$ expansion the limits $N\to\infty$ and $\beta\to\infty$ do not necessarily commute for $L=\infty$. For $O(3)$ the Bethe ansatz prediction for $m/\Lambda$ [14] is larger than its Monte Carlo (MC) value by about 15%. Of course, if our prediction that there is a transition to a massless phase at finite $\beta$ is correct, then at some $\beta$ the MC value for $m/\Lambda$ must cross the predicted value, however with a nonvanishing slope. The MC data (produced by us), testing the bootstrap $S$-matrix prediction, seem to support that prediction at least for low $p/m$. At large $p/m$ both the lattice artifacts and the $S$-matrix prediction are under much poorer control, and so to claim, as Niedermayer *et al.* do, that the results coincide with renormalized PT and show asymptotic freedom is a gross exaggeration. In fact the MC data suggest that while the $S$-matrix prediction seems to be right, it is still likely to *disagree with asymptotic freedom*: Indeed, we find [15] that MC data for the dodecahedron model are indistinguishable from those for $O(3)$, but the former is not likely to possess asymptotic freedom (since at sufficiently large $\beta$ it possesses long range order).

While we find these arguments in favor of the accepted dogma wanting, we believe that our percolation arguments in favor of the existence of a massless phase in *all* $O(N)$ models are much more compelling and under better theoretical control. Indeed in [16] and [17] we proved rigorously that for a different version of the $O(N)$ models, the so-called cut action in which the spin gradient is restricted, either a certain well-defined ''equatorial cluster'' percolates or the model must be massless. Although five years have passed, no mathematical physicist has provided us, either in print or in private, with any heuristic arguments of how this equatorial cluster could possibly percolate. Moreover, as we stated in those papers [16,17], if the equatorial cluster does not percolate, the typical configuration must be such that the inverse image of any sufficiently large piece of the sphere forms clusters of arbitrarily large size. As we emphasized in [4], such scale invariant configurations, which we believe should be the typical configurations at low temperatures, are very much like a gas of superinstantons, an independent observation, which came three years after the percolation arguments were written. Niedermayer *et al.* should not ignore these facts. If they find anything wrong with our percolation arguments, they should explain it; if not, they should worry that the standard picture may after all be wrong.


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