Referee report for: *The QCD θ-parameter in canonical quantization*

This paper aims at investigating the role of the $\theta-$ term in QCD in particular in view of the recent claims regarding the posibility that this topological term has no effect in physical observables. This work aims at reconciling this claim, based on the statement that the infinite volume limit has to be taken at fixed topological sector, with a detailed study of what happens in finite volume using canonical quantization.

Unfortunately the text is full of misconceptions or plainly wrong statements. Problems start very soon

1. Eq.(1) is not correct. A theta term in the Lagrangian is irrelevant (as it is a total derivative). The theta term only appears in the path integral (where it is not irrelevant), and is a consequence of inequivalent representations of the operator algebra or of the unbounded nature of the momentum/position operators when constructing the path integral formulation of a theory.

2. In Eq.(2) the authors write the partition function of a theory in infinite volume. The authors argue that since topological quantization is only true in infinite volume, one should take the infinite volume limit at fixed topological sector. First, it is difficult to understand how one will take the infinte volume limit at fixed topological sectors if one argues that these sectors are **only** defined in infinte volume. Second, there is a miriad of two dimensional models that have been exactly solved directly in infinite volume (i.e. the expressoin for Z in Eq.(2) is known). The results of these models do not agree with results if one takes the infinite volume at fixed topological sector. Third, we understand how to compute infinite volume quantities in a QFT since a long time. The procedure is not arbitrary, but comes from the basic relation

   $$\langle O(x_1)\cdots O(x_n)\rangle_L = \langle O(x_1)\cdots O(x_n)\rangle_{L=\infty} + e^{-mL} + \dots \qquad (1)$$

   i.e. any local correlation function in a finite volume L^4 reproduces the infinite volume result up to exponentially small corrections. This result is generic (i.e for any QFT with a mass gap), and correct no matter if there is topology quantization or not. This basic relation immediately tells us how the infinite volume limit should be taken, and for the cases where the boundary conditions in finite volume allow for topology quantization, one has to take the infinte volume limit of the sum over all topological sectors. This relation is quite straightforward

3. "Furthermore, fixing the winding number Δn on T^4 leads to a well-defined Euclidean quantum field theory for each n so that there is a priori no necessity for summing over Δn.". This is just wrong. A theory at fixed topological sector is **not** a well defined Euclidean field theory. Fixing the topological charge breaks clustering. Formally there is not transfer matrix (Hamiltonian).

4. The use of Hilbert space along the text is incorrect. Sentences like "...the Hilbert space is too large..." just makes no sense. The Hilbert space is always very big because by definition a Hilbert space includes all functions whose inner product is finite. There is not even the requirement of continuity on a Hilbert space (i.e. the sentence the Hilbert space of continuos functions makes no sense). When doing canonical quantization one have to focus on the operators acting on the Hilbert

space, and in which cases the symmetric operators define QM observables. This is how the theta dependence appears, unfortunately this discussion is not found in the paper.

5. The correct general constructions on a Torus leadsto the topological charge to be $\Delta n \in \mathbb{Z} + 1/N$. In other words, particular choices of the boundary conditions (matrices t) can lead to the charge in $SU(2)$ to be fractional: only $\pm n + 1/2$ are possible values, and in particular there is not configuration with zero topological charge.

6. Although chapter 2 is basically a review of well known facts, it lacks rigor. The condition of the temporal gauge Eq.(12) is **not** a gauge fixing condition on the Torus (the zero momentum mode of A_μ is physical, and determines the valaue of the gauge invariant Polyakov loops). Imposing this condition, again, breaks locality and formally there is no Hamiltonian. In other words, the **crucial** condition for the cancellation of the phases in the following chapters, is in fact not a gauge choice, but has physical consequences and the theory with such condition does not have a Hamiltonian. It is difficult to follow the rest of the paper after this problem.

Finally, let me end this report with some general comments. Although the paper is presented as a reconciliation between a proposed order of limits and the finite volume formalism, one has to note that the different order of limits affects not only the theta dependence of quantities, but many other observables. Should we take the other order of limits now?, or we can just drop theta and do the usual ordering of limits?. If the other order of limits is still required (and theta was not visible), why is this work needed?

I do not want to use any argument of authority and I hope that I have given compelling evidence that the manuscript has fundamental problems. Nevertheless the authors should be aware of the extense literature in topological field theories (starting with Witten seminal papers), and the vast literature in critical phenomena where two dimensional systems have been exactly solved (in particular all $U(N)/SU(N)$ pure gauge theories in two dimensions). Some of these exact solutions are in direct contradiction with the order of limits advocated by the authors and we find no explanation on what is the mistake that has been done in more than 40 years of scientific literature.

It is well known that time boundary conditions can be chosen at will without affecting the Hamiltonian. The authors choose periodic so that topological sectors are well defined in finite volume. But obviously the same spectrum of the Hamiltonian can be obtained using different time boundary conditions (i.e. open), where no topology quantization is present. Their argument(s) seem to break when topology is not well defined in finite volume.

Although I value the attempts of the authors of solving difficult problems, their solution(s) are in direct contradiction with how we understand locality in quantum field theories, have fundamental flaws, and leave too many questions unanswered.