Comment on the Walliser-Weigel approach to exotic baryons in chiral soliton models

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This comment discusses a recent paper by Walliser and Weigel on the quantization of chiral soliton models in the context of exotic baryons. Claims made in that work are misleading due to unfortunate nomenclature. Moreover, attempts in that paper to go beyond the leading order calculations of the phase shifts are *ad hoc* and never justified. This comment also addresses a technical issue in that paper: the identification of the excitation energy of the pentaquark obtained via conventional rigid rotor quantization with a frequency obtained in the context of small amplitude fluctuations. The identification is erroneous: the small amplitude fluctuation result is based on a first-order perturbation computation of the frequency around a zero mode solution at a frequency far from zero and well away from the perturbative regime.

The comment seeks to clarify the recent paper by Walliser and Weigel[1] on the quantization of chiral soliton models in the context of exotic baryons. A principal purpose of this comment is to demystify several statements in ref. [1] which are extremely misleading.

Ref. [1] uses the phrases “rigid rotor quantization” and “collective” differently than is used in much of the literature. This may create the false impression that basic fabric of the approach is in conflict with earlier analyses [2, 3, 4, 5, 6, 7]. These earlier analyses showed that the rigid-rotor approach as employed by Praszalowicz[8] and by Diakonov, Petrov and Polyakov[9] is not justified at large N_c for baryons with exotic quantum numbers and that the correct way at large N_c to compute the relevant physical observable, namely, the phase shift, is via small amplitude fluctuations (*i.e.*, the method of Callan and Klebanov[10]. In fact, the explicit calculations by Walliser and Weigel show precisely that the Callan and Klebanov approach gives the exact phase shifts at large N_c and that the rigid-rotor approach used by Praszalowicz and by Diakonov, Petrov and Polyakov cannot be used validly without substantial modification. Walliser and Weigel refer to this modification as the rotation-vibration approach (RVA). It is shown in sec. V of ref. [1] that at large N_c the RVA is indistinguishable from the Callan-Klebanov approach. Thus, the substance of ref. [1] is in accord with the analyses of refs. [2, 3, 4, 5, 6, 7].

However, despite this fundamental agreement of refs. [2, 3, 4, 5, 6, 7] with the approach of Walliser and Weigel for the S-matrirx at large N_c, multiple statements in ref. [1] appear to be in conflict with refs. [2, 3, 4, 5, 6, 7]. As an example, consider the claim in the abstract of ref. [1] that “*We thoroughly compare the bound state* [that is the Callan-Klebanov approach] *and rigid rotor approaches to three-flavored chiral solitons. We establish that these two approaches yield identical results for the baryon spectrum and the kaon-nucleon S-matrix in the limit that the number of colors tends to infinity.*” This appears to be in flat contradiction that the rigid rotor approach gives incorrect results for exotic states and the correct approach is to compute phase shifts according to the Callan-Klebanov approach as was originally done in ref. [4].

A central reason for this apparent discrepancy is linguistic. When refs. [2, 3, 4, 5, 6, 7] refer to the “rigid rotor approach” they mean the approach used by Praszalowicz[8] and by Diakonov, Petrov and Polyakov[9] in the early predictions of pentaquarks from chiral soliton models. That is, they mean using the collective Hamiltonian introduced by Guadagnini [11] to directly calculate properties of discrete states which are then equated with the pentaquark. In the language of ref. [1] this corresponds to simply using the Lagrangian of Eq. (4.1), converting it to a collective Hamiltonian, quantizing the Hamiltonian and using the discrete states so obtained to calculate directly the physical of baryons. The central purpose of refs. [2, 3, 5, 6, 7] was to show that this procedure while legitimate for the two flavor Skyrmion and for non-exotic states in the three flavor models, is inadequate to compute the physical properties of exotic baryons in the large N_c limit of the theory. It is clear that the authors of ref. [1] agree that it is not. They write in sec. I B that “*...it is necessary to include small amplitude fluctuations in the RRA. We call this approach the rotation-vibration approach (RVA).*” The authors then go on to show, correctly, that the S-matrix computed in the RVA is identical to that computed Callan-Klebanov approach at large N_c. Thus, when claims are made in ref. [1] that the “rigid-rotor approach” agrees with the Callan-Klebanov approach at large N_c, it is often referring to the RVA and not to the unadulterated rigid rotor approach used by Praszalowicz[8] and by Diakonov, Petrov and Polyakov[9]. Thus, despite the apparent conflict, there is no underlying disagreement between the results of these previous analyses and the large N_c results of Walliser and Weigel for the physical observables.

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Of course, as a logical matter, definitions when used in a self-consistent manner cannot be incorrect. In this sense, there is nothing wrong with ref. [1] use of "rigid rotor approach" to refer to a calculation including both vibrations and rotations. However, it would have been wise to avoid such a usage: it is seriously misleading about previous work. It is also a particularly perverse locution in that the vibrations, the non-rigid degrees of freedom, play an essential role.

The authors of ref. [1] use of the word "collective" can also be seriously misleading. Part of the origin of this potential confusion stems from the fact that "collective" has many usages in the literature. One common meaning in many-body physics is to refer to dynamics in which many particles are moving with significant coherence: *eg.*, the giant dipole resonance in low energy nuclear physics. A "collective coordinate" in such a context refers to a choice of coordinate which couples strongly to this coherent motion and weakly to the single particle motion. The definition of such a degree of freedom is somewhat arbitrary in such circumstances but a judicious choice can simplify one's description. More generally, one can introduce "collective coordinates" in a similar sense for problems in which some class of motion which is characteristically more coherent than typical motion.

There is also a second, more technical, usage of "collective" which is commonly used in semiclassical treatments of soliton models[12]. In this context, "collective" refers to degrees of freedom which are formally adiabatic in the sense that they become arbitrarily slow as the weak coupling (*i.e.*, semiclassical) limit is approached. Such modes are associated with the zero modes of the linearized classical equations of motion around static solitons. They have the property that they completely decouple from all non-collective degrees of freedom in the weak coupling limit. The large N_c limit is precisely such a weak coupling expansion. Thus, the formulation of a systematic $1/N_c$ expansion for chiral soliton models depends on correctly identifying the "collective modes" in this second more restricted sense. References [3, 6, 7] carefully distinguish between static and dynamic zero modes and show that not all static zero modes have associated dynamic zero modes. This is done precisely to show there are fewer collective degrees of freedom in this second more technical sense than are obtained by counting the number of flat directions assuming that there is one pair of collective modes for each. This, in turn is extremely useful in developing a simple systematic treatment of the theory at large N_c and zero SU(3) flavor breaking.

When Walliser and Weigel write, "*Generally we may always introduce collective coordinates to investigate specific excitations. This is irrespective of whether the corresponding excitation energies are suppressed in the large N_c limit or not. This is in contrast to the claims of ref. [33]* [refs. [2, 3] of this comment] *that a scale separation is necessary to validate the collective quantization coordinate quantization. Eventually the coupling terms and constraints ensure correct results.*" and "*...collective coordinates are* not *necessarily linked to zero modes of a system and any distinction between dynamical and static zeromodes [34]* [ref. [7] of this comment] *is obsolete.*", they *must* be referring to some variant of the first usage of "collective" and not the second more technical sense. The statement is manifestly wrong if used in the second sense. Moreover, in this context the phrase "collective coordinate quantization" used above clearly refers to the RVA (which as noted above is at large N_c identical to the Callan-Klebanov approach) and not the rigid rotor approach of Guadagnini [11]. This is unfortunate since for "collective quantization" may call to mind Guadagnini's rigid rotor approach and thus create an impression at odds with the facts.

Before turning to the substance of ref. [1], a final quibble about language. Reference [1] uses the unfortunate moniker "bound state approach" to describe the Callan-Klebanov method of small amplitude vibrations. While Callan and Klebanov did refer to this method as the "bound state approach" [10], that was in the context of non-exotic states which were, in fact, bound. For exotic states—which are unbound resonances—describing the method as the "bound state approach" is perverse. For this reason this method will be referred to as the "Callan-Klebanov" approach throughout this comment.

Apart from these linguistic issues, there are substantial problems in some of the calculations in ref. [1]: important conceptual difficulties arise in attempts to compute quantities associated with exotic baryons beyond the computation of the phase shifts at leading order in $1/N_c$—the regime in which it agrees with refs. [2, 3, 4, 5, 6, 7]. Two principal issues arise in this regard: i) the separation of the phase shift into a "background" and "resonant" part, and ii) the calculation of the width of the exotic baryon for $N_c = 3$.

Consider issue i): the paper relies on a separation of the phase shift into a "background" and "resonant" part to conclude that there is a resonance in the exotic pentaquark channel. However, this separation is unnecessary, pointless, arbitrary and poorly motivated. It is unnecessary since the phase shifts (which are directly computed in the Callan-Klebanov approach) contain *all* of the physics about the scattering available at leading order. It is pointless since there is no possible experiment which measures the "resonant" piece separately. It is arbitrary, since *any* separation of a full amplitude into a background and resonant contribution is ultimately model dependent and hence arbitrary. Finally, it is poorly motivated: the "background" depends on a misidentified collective degree of freedom. It associates the background with those fluctuations orthogonal to the "collective" fluctuation of freedom. However, as will be discussed in detail below, this putative "collective" fluctuation plays no special role either physically or mathematically. It should be stressed, however, that regardless of whether one believes that there is some mathematical or physical significance to the "collective" mode identified for exotic baryons, the separation of the phase shift into "background" and "resonant" contributions remains unphysical.

Next focus on the motivation for separating the phase shift into a "background" and "resonant" part. The background is taken to be due to fluctuations constrained to be orthogonal to the "collective" mode identified by the authors. This "collective" mode is the zero mode associated with non-exotic oscillations, $z(r)$. As noted above, there are two commonly used meanings of "collective", but the mode taken as "collective" here appears to satisfy neither definition when used to describe the exotic degree of freedom. It is neither associated with a zero mode of the exotic channel as in the technical definition used in refs. [3, 6, 7] nor does it correspond approximately to a particularly coherent class of motion which dominates the behavior in some region. Moreover, it is not special mathematically except at zero frequency (where it satisfies the small amplitude equation of motion and is associated with the non-exotic fluctuation); at finite frequencies in the neighborhood of the putative resonance it does *not* satisfy the small amplitude equation of motion. This issue will be discussed in some detail at the end of this comment. Thus, there is no apparent special quality about this mode, neither physically nor mathematically. It seems to be a completely arbitrary choice and there is no apparent reason to identify the motion orthogonal to it as corresponding to "background".

Next consider issue ii): the claims the pentaquark width is computed at $N_c = 3$. However, this claim is unjustified on very basic grounds. In the first place, it is based on the "resonant" phase shift and as noted above the separation into "resonant" and "background" is unjustified. However, even were that not the case, the calculation at $N_c = 3$ would not be legitimate. Note that entire formalism for treating the chiral soliton model is based on a $1/N_c$ expansion and has only been computed consistently at the leading nontrivial order. For example, the form of the profile function $F(r)$ was computed using classical equations of motion which are valid at large N_c but which have subleading corrections. At $N_c = 3$ these induce corrections which are not included in this paper. Similarly, the computation of kaon-nucleon S matrix does not include dynamical effects in which the kaon-nucleon scattering goes into kaon-pion-nucleon states. Such effects alter the kaon-nucleon scattering amplitude when functioning as intermediate states and for energies above the pion production amplitude yields inelastic contributions. One can justify dropping this dynamics in the large N_c limit but not at $N_c = 3$. Thus, the authors make one set of $1/N_c$ corrections—those associated with the "collective" mode—to all orders to get an $N_c = 3$ result while simultaneously neglecting even the first-order correction to others. *A priori* there is no reason why such a result should be any more reliable at $N_c = 3$ than the leading order expression. Certainly, the $1/N_c$ expansion does not justify such a procedure and the authors give no other argument.

Thus, both the procedure to separate the "background" and "resonant" contributions and the procedure to compute the resonant width at $N_c = 3$ are *ad hoc*. Neither has been justified in a systematic $1/N_c$ expansion nor from any other systematic framework. Any conclusion based on these must viewed with skepticism.

The final issue addressed in this comment is an error in ref. [1] which can lead to significant confusion. The context is the small amplitude equation for the kaon vibrational modes around a Skyrmion imbedded in the u-d subspace in Eq. (3.5) of ref. [1]:

$$h^2\eta(r) + \omega\left[2\lambda(r) - \omega M_K(r)\right]\eta(r) \ , \tag{1}$$

where $h^2(r)$ is a differential operators, $\lambda(r)$ and $M_K(r)$ are functions; λ arises from the Witten-Wess-Zumino term. There is a zero frequency solution to this equation in the limit of zero SU(3) flavor breaking:

$$z(r) = \frac{\sqrt{4\pi} f_\pi \sin\left(F(r)/2\right)}{\sqrt{\Theta_K}} \tag{2}$$

where $F(r)$ is the Skyrmion profile function and Θ_K is the normalization constant computable from $F(r)$. Now suppose one wants to include SU(3) violations into this formalism: the SU(3) violating term is in $h^2(r)$ and equals $m_K^2 - m_\pi^2$. For small SU(3) violations one can use first-order perturbation theory to compute the shift of the frequency of the zero mode away from zero. This is done by simply taking Eq. (1) replacing $\eta(r)$ by $z(r)$, multiplying on the right by $z(r)$ and integrating. One obtains

$$\omega^2 = \frac{3\Gamma}{8\Theta_K} + \omega_0\omega \ , \tag{3}$$

$$\text{with } \omega_0 \equiv \int dr\, r^2 z^2(r) 2\lambda(r) = \frac{N_c}{4\Theta_K} \ , \tag{4}$$

$$\text{and } \Gamma \equiv \frac{8\Theta_K(m_K^2 - m_\pi^2)}{3}\int dr\, r^2 z^2(r) \ , \tag{5}$$

which corresponds to Eqs. (3.7)-(3.9) of ref. [1]. Since Eq. (3) was obtained from the zero mode via first-order perturbation theory in SU(3) breaking it is only valid at linear order in Γ and has the unique solution at this order:

$$\omega \equiv \omega_\Lambda = \frac{3\Gamma}{8\Theta_K\omega_0} + \mathcal{O}(\Gamma^2). \tag{6}$$

The frequency is denoted by ω_Λ as it corresponds to the excitation energy of the Λ above the nucleon.

While ref. [1] has Eq. (3) (it is Eq. (3.9) of that reference), it neglects to mention that the equation obtained from the zero mode via first-order perturbation theory in SU(3) breaking and thus is only valid near zero and only to first order in Γ. This is unfortunate since ref. [1] uses Eq. (3.9) to all orders in Γ to obtain—incorrectly—a general expression for ω_Λ. Even more troubling, ref. [1] identifies the second solution of the quadratic in Eq. (3) with the θ^+ pentaquark:

$$\omega_\theta = \left(\sqrt{\omega_0^2 + \frac{3\Gamma}{2\Theta_K}} + \omega_0 \right) /2 \, , \tag{7}$$

which is Eq. (3.11) of ref. [1]. However, the calculation of ω_θ is totally without justification. It is based on perturbation theory around the zero mode and is only valid near $\omega = 0$. However, the solution is parametrically far from $\omega = 0$ (it is of order N_c^0 regardless of the value Γ) and hence outside the regime of validity of the perturbative treatment. One can see that the claimed result is wrong simply by returning to the SU(3) limit in which case $\omega_\theta = \omega_0$. If one focuses on the original small amplitude equation—Eq. (1) above—one sees explicitly that $z(r)$ is *not* an eigenfunction corresponding to an eigenfrequency of ω_0. In summary, the quoted expression for θ_θ is unjustified in the context of the Callan-Klebanov approach. Moreover, it is clear in this analysis that $z(r)$, while a collective degree of freedom associated with a zero mode for the non-collective state, plays no special collective role for the exotic channel—it merely represents a mode for which was chosen *arbitrarily* as the mode in which the expectation value of Eq. (1) was taken.

The erroneous calculation of ω_θ leads to an important misstatement in ref. [1]. In the introduction, it is stated that "*In sections III and IV we compare the two approaches* [the rigid rotor approach and the Callan-Klebanov approach] *and show how they yield identical spectra at large N_c...*". Note in the context of sec. IV of ref. [1] the rigid rotor approach refers to the original approach of Guadagnini[11] and not the RVA. Thus, if this statement were correct, the analysis of refs. [2, 3, 4, 5, 6, 7] would be wrong. However, the evidence presented for this in the context of exotic states at zero SU(3) violation is given in Eq. (14) where it is shown that at large N_c the excitation of the pentaquark is identical with ω_θ as calculated above. From this the authors write, "*Thus we conclude the BSA* [i.e., Callan-Klebanov approach] *and the RRA are consistent when flavor symmetry breaking is omitted.*" This conclusion is unjustified. It is based on an invalid calculation of ω_θ in the Callan-Klebanov approach. Indeed, based on the calculation, the authors ought to have concluded that the two methods are, in fact, distinct.

In summary, the approach of Walliser and Weigel in ref. [1], where justified by the $1/N_c$ expansion, agrees with the Callan-Klebanov approach and is consistent with the analyses of ref. [2, 3, 4, 5, 6, 7]. However, this basic fact is generally obscured by a very unfortunate use of language in refs. [1]. It is obscured further by the erroneous of computation ω_θ which is then used to argue spuriously that the rigid rotor approach and the Callan-Klebanov approach are equivalent. Finally, the principal new elements of this approach, the separation of the phase shifts into a "background" and "resonant" part, and the scheme to compute the phase shifts at $N_c = 3$ are not justified.

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