Comment on "Reexamination of the $N=90$ transitional nuclei ^{150}Nd and ^{152}Sm"

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We show that, contrary to a recent paper, the new, analytic, critical point symmetry X(5), which is parameter-free (except for scale), reproduces the data in ^{152}Sm and ^{150}Nd quite well, and better than multiparameter models based on bandmixing of pure rotor states.

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The concept of shape/phase transitions in nuclei has recently taken on heightened interest. This has led to a new class of analytically solvable models, called critical point symmetries, describing nuclei close to a phase transitional point. In a spherical-axial rotor transition region, the relevant symmetry is X(5) [1], which is analytic and parameter-free (except for scale). X(5) is an idealized paradigm, intermediate between a vibrator and a rotor, and its predictions [e.g., sets of energies and $B(E2)$ values] are absolutely fixed. For example, the characteristic structural signature, which reflects the shape and the collectivity, $R_{4/2} \equiv E(4_1^+)/E(2_1^+)=2.91$.

References [2,3] showed that X(5) gives a good description of ^{152}Sm and ^{150}Nd. Reference [4] enlivens this discussion by arguing that ^{152}Sm and ^{150}Nd can be better described with weak $\Delta K=0$ mixing of pure rotational bands than with X(5), and cites Kumar's pairing plus quadrupole (PPQ) model as further justification. We differ, but appreciate their contributions since it has led us and others to a deeper understanding of X(5) itself.

In this Comment, we make three main points. First, X(5) is conceptually equivalent to models such as the pure $I(I+1)$ rotor or the harmonic vibrator. Its purpose, like those, is to provide a benchmark. Consider the rotor as an example of such a benchmark. One would have had no idea what the yrast energies ($0^+, 2^+, 4^+$, and 6^+) 0, 100, 329, and 692 keV meant before Ref. [5]. Afterwards, the structure is instantly recognizable, and one even sees previously undetectable perturbations. Comparison of X(5) with multiparameter perturbation schemes is not apt since X(5) is a paradigm and not a fit.

Second, X(5) *nevertheless* acquits itself very well in such comparisons and outperforms the multiparameter perturbed rotor. In showing this, we will also note that Ref. [4] presents two mixing interpretations—using $B(E2)$ values and level energies—which lead to very different predictions. Third, the PPQ results, cited in Ref. [4] as exemplifying a rotor model, are, in fact, far from a rotor.

The empirical $R_{4/2}$ value of 3.01 for ^{152}Sm immediately indicates a transitional nucleus. It is therefore hard to see how a small $\Delta K=0$ mixing [4] of $K=0$ rotor states with $R_{4/2}=3.33$ could give $R_{4/2}=3.01$ [and $R_{4/2}(0_2^+)=2.69$]. In fact, it cannot (see below). One approach in Ref. [4] to look for perturbations to the rotor is to fit the energies with an expansion in $I(I+1)$ [A and B coefficients]. The B coefficient extracted in Ref. [4] is small ($B=-15$ eV) as is the implied mixing in the 2^+ and 4^+ states, and $R_{4/2}$ changes only from 3.33 to 3.29, which is far from the data. The fits in Fig. 3 of Ref. [4] extend to 16_g^+ and $14_{0_2^+}^+$ and *look* excellent since the ordinate goes to 4 MeV: their incompatibility with ^{152}Sm (e.g., $R_{4/2}$) is not visible on that scale. However, it is easily seen in Fig. 1(a) where X(5) is clearly by far the best description.

The more explicit bandmixing interpretation in Ref. [4] uses interband $B(E2)$ values. However, the *apparent* linearity of the Mikhailov plots in Figs. 1 and 3 of Ref. [4], required for such an analysis, stems from the compressed scale and an ordinate going to negative values for an inherently positive quantity $[M \equiv \sqrt{B(E2)/\langle I_i 0 \Delta I 0 / I_f 0\rangle^2}]$. Replotting with a scale starting at zero [see Fig. 1(c)] shows a clear nonlinearity, casting doubt on a Mikhailov analysis in the first place.

Moreover, as noted in Ref. [4] itself, the $\Delta K=0$ mixing between the ground and 0_2^+ sequences must imply an *expansion* in energies of the latter (an *upward*, not downward, parabola in Fig. 2 of Ref. [4]). As a consequence, taking the B value given in Ref. [4] from their Mikhailov plot yields a discrepancy for the ($14_{0_2^+}^+$) state of ~1.5 MeV. Thus, the mixing and level energies deduced from the Mikhailov analysis are very different from those implied by the expansion in $I(I+1)$.

The absolute energy spacings in the 0_2^+ sequence are overpredicted [2,3] in X(5). While this is admittedly [2] a problem, it is common to the interacting boson model [7], geometrical collective model [8], boson expansion [9], and other microscopic models [10]. The bandmixing approach does not give this scale at all, except by parametrization, and does worse than X(5) on relative energies [see Fig. 1(b)].

There is a more basic difficulty in comparing a bandmixing interpretation with X(5). The pure rotor makes no predictions for most of the observables predicted automatically by X(5). To obtain just the yrast and yrare observables with the rotor requires at least six parameters: $E(2_1^+)$, $E(0_2^+)$, quadrupole moments of both bands, and unperturbed intrinsic and mixing matrix elements. (The fact that the latter two are extracted from a Mikhailov plot does not alter their status as parameters.)

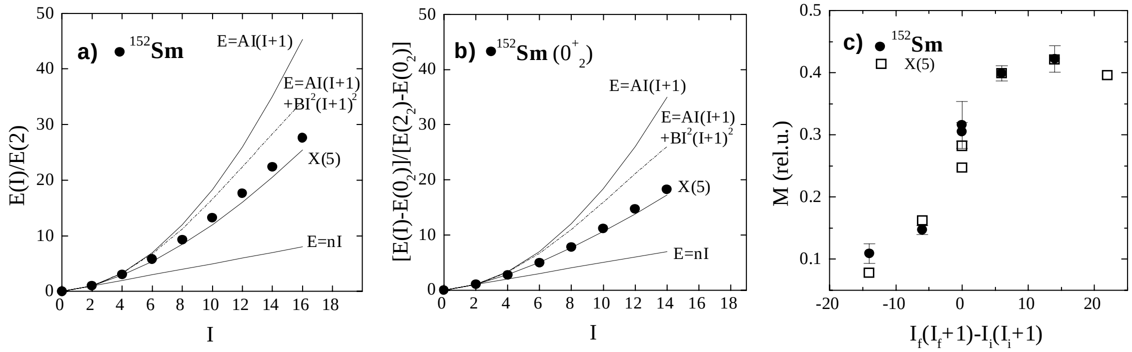

FIG. 1. Comparison of (a) yrast and (b) yrare energies of ^{152}Sm with X(5), the vibrator, the rotor, and with the fits in Ref. [4]. The form of these plots is the same as in Refs. [2,3,6]. Note that the mixed rotor results from Ref. [4], though less successful, require two parameters for each band. (c) Mikhailov plot for interband transitions in ^{152}Sm and for X(5) (normalized to the $0^+_2 \rightarrow 2^+_1$ transition).

X(5) predicts intersequence (0^+_2-based band to ground band) $B(E2)$ values to be about three times those observed in ^{152}Sm [2]. This is perhaps not surprising since these transitions, which are collective in the vibrator, become very small in the pure rotor and ^{152}Sm is slightly on the rotor side of X(5) ($R_{4/2}$[X(5)]=2.91). In any case, this discrepancy is a *single* problem in the intrinsic matrix element in X(5) that affects all the interband transitions shown in Figs. 3 and 4 and again in Table II of Ref. [4]. Since multiple instances of this add no new physics, the critique in Ref. [4] amounts only to the statement that the X(5) interband intrinsic matrix element is about 1.7 times higher than the data, as we have noted ourselves [2]. We show in Fig. 1(c) that, except for this factor, X(5) reproduces the data excellently. Of course, one cannot compare the rotor to X(5) here at all since the rotor model makes no predictions about this matrix element.

Finally Ref. [4] cites PPQ calculations [11] to support the perturbed rotor approach, stating "the PPQ model…provides a reasonable microscopic justification for the parameters we have extracted from the band-mixing analysis," and that the 0^+_2 state in Kumar … "is confined within the same deformed minimum as the ground state." However, Kumar's potential is shallow (3.1 MeV); there is considerable zero point energy (~1.5 MeV) and the 0^+_2 state is in fact barely within the deformed minimum. Kumar *himself* said [11] that the 0^+_2-band is "close to the oblate and spherical barriers and hence the assumption of small-amplitude, harmonic vibrations is not valid." Moreover, the $R_{4/2}$ value in Ref. [11] is only 2.75, far from 3.33, and $R_{4/2}(0^+_2)$=2.31 (vibrator regime). Finally, though Kumar's PPQ calculation [11] is superb, it actually has 8–10 parameters (albeit some are regionally fit), which have been enumerated by Kumar himself, rather than 2 as cited in Ref. [4].

In conclusion, X(5) should be compared with other invariant benchmarks such as the pure (unmixed) rotor. Nevertheless, even when challenged against several-parameter band-mixing calculations, X(5) provides a better account of the data. Moreover, X(5) provides parameter-free predictions of an array of quantities such as $E(0^+_2)$, relative 0^+_2-band energies, interband $B(E2)$ values, and so on, that cannot be predicted by the rotor at all without explicit parametrization. Finally, Kumar's PPQ calculations [11], though excellent, are far from the rotor and cannot be used as a microscopic proxy for it.

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