Reply to "Comment on 'Quark-meson coupling model for baryon wave functions and properties'"

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We respond to the Comment by Thomas and Miller, and reassert our view that the quark-meson coupling model can be used to explain the existence of a second nucleon resonance near 1440 MeV and the Δ resonance at 1600 MeV.

In a recent Comment, Thomas and Miller[1] (TM) outline numerous improvements to a calculation done previously by us.[2] We can find nothing objectionable with the suggestions made by TM. However, there are numerous comments and claims to which we would like to respond.

TM correctly point out that we did not use a Yukawa distribution for the meson wave functions. Instead, we used a Gaussian distribution for calculational simplicity. Given the roughness of the calculation, we feel this approximation is justified. Also for simplicity, we used the same length parameter (β) for all three-quark–one-meson basis states within the physical wave function of each ground-state baryon, even though a shorter length parameter would have been more accurate in some cases. TM notice that using the same value of β in all basis states leads to an underestimate of it. They are correct, but in fact there is another, more important effect. Contributions from spatial excitations of quarks will change β even more. However, the main conclusions are not affected by improved values of β. Our calculation of the spectrum of states with nucleon quantum numbers will still predict an excited state with an energy close to the Roper resonance that couples to the πN scattering channel.

TM claim that the energy shift of the nucleon due to pion coupling is "fairly model independent at about $(-300, -400)$ MeV"[1]. We are not the first to estimate the energy shift to be about -100 MeV. In the original article,[2] numerous estimates by other authors were included, all showing that our estimates were in rough agreement with previous calculations.

TM examine the strong decay width and conclude that the first-order correction (as shown in Fig. 1 of Ref. 1) has not been included in our calculation. We have not done a field-theoretic calculation of the width, nor have we used Feynman diagrams, but by using physical states (instead of bare states) in our calculation, we have, in effect, included corrections to order g^3 (where g is the quark-meson coupling constant), and have therefore included contributions from processes shown by TM. (There is the possibility that TM are also using physical wave functions, making the indicated process a higher-order correction. If so, we retort that corrections on the order of 10–20% do not change the fundamental conclusions of the article.)

We would like to restate our view that there are two interesting questions in low-energy πN scattering, one each involving the $N(1440)$ and $\Delta(1600)$. In the case of $N(1440)$, there is speculation that two states lie within a single scattering peak,[3] while standard quark models predict only one. In the case of $\Delta(1600)$, there exists a weak resonance in the data, but standard quark models consistently predict energies that are between 100 and 300 MeV too high, and usually about 200 MeV too high. There are therefore potentially two observed states in the data that cannot be accounted for by standard quark models. In our calculation, there are two predicted states in the $S=0$ sector that couple (relatively) strongly to the πN continuum and that correspond to the two states not accounted for by other models.

TM assert that the excited states predicted by our model are not resonances, but rather, are part of the πN continuum. TM's interpretation may in the end be correct, but we believe that our shell-model approach is a good first approximation. To the extent that pseudoscalar mesons are viewed as $\bar{q}q$ pairs, our basis states are equivalent to "exotic" $(q^4\bar{q})$ states sometimes used in other calculations. It is not unusual, therefore, to interpret physical states dominated by three-quark–one-meson states as resonances.

In general, we appreciate the intentions of TM's Comment but we feel that the main conclusions are not compromised by the calculational simplicity used in the original investigation.

[1]A. W. Thomas and G. A. Miller, preceding Comment, Phys. Rev. D **43**, 288 (1991).
[2]W. J. Leonard and W. J. Gerace, Phys. Rev. D **41**, 924 (1990), and references therein.
[3]See, for example, the following articles from a group predominantly from UCLA and another at the Leningrad Nuclear Physics Institute (LNPI) and references therein. The most recent articles by the UCLA group are A. Mokhtari, A. D. Eichon, G. J. Kim, B. M. K. Nefkens, J. A. Wightman, D. H. Fitzgerald, W. J. Briscoe, and M. E. Sadler, Phys. Rev. D **35**, 810 (1987); M. E. Sadler, W. J. Briscoe, D. H. Fitzgerald, B. M. K. Nefkens, and C. J. Seftor, *ibid.* **35**, 2718 (1987); D. B. Barlow *et al.*, Phys. Rev. Lett. **62**, 1009 (1989); G. J. Kim *et al.*, Phys. Lett. B **219**, 62 (1989); C. J. Seftor, S. D. Adrian,

W. J. Briscoe, A. Mokhtari, M. Taragin, M. E. Sadler, D. B. Barlow, B. M. K. Nefkens, and C. Pillai, Phys. Rev. D **39**, 2457 (1989); and G. J. Kim, J. Arends, W. J. Briscoe, J. Engelage, B. M. K. Nefkens, M. E. Sadler, M. Taragin, and H. J. Ziock, *ibid.* **41**, 733 (1990). Articles from LNPI include V. A. Gordeev, V. P. Koptev, S. P. Kruglov, L. A. Kuz'min, A. A. Kulbardis, Yu.A. Malov, S. M. Mikirtich'yants, I. I. Strakovsky, and G. V. Scherbakov, Nucl. Phys. **A364**, 408 (1981); and V. V. Abaev *et al.*, Z. Phys. A **311**, 217 (1983).