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Reply to "Study of low-energy physics with high-energy muon interactions in nuclear emulsion"*

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None of the discrepancies which led us originally to conclude that the method proposed by Jain and Stern was not reliable have been resolved in the preceding paper. We restrict our comments here only to those points raised by Jain that have not already been discussed adequately in our earlier article.

In our laboratory we have been involved in a long-range study of a variety of muon-induced interactions in nuclear emulsions. Recently[1] we described a number of discrepancies between our results[2] and those of Jain and Stern.[3] These discrepancies led us to conclude that the method proposed by Jain and Stern for distinguishing between interactions involving light nuclei and those involving heavy nuclei was not reliable. Arguments based in part on these discrepancies were presented in Ref. 1 to show that when nuclear emulsion was used in the manner proposed in Ref. 3 it falls far short of being the "ideal target-detector system" for studying nuclear structure.[3] In the preceding paper,[4] hereafter referred to as A, Jain discusses some of these arguments and reports on a second analysis of the data using the "shortest-range-prong" technique. We feel that most of the objections raised in A have already been answered and we refer the reader to Ref. 1. We restrict ourselves in what follows to comments on those remaining objections which concern points that we have not previously discussed.

(1) Although our data on $(1+1)$ events[1,2] are consistent with dominance by giant-dipole-resonance (GDR) events, we have neither assumed this in our analysis nor presented our data as proof of such a hypothesis, as is suggested in A. Moreover, we are not convinced that the arguments enumerated in A are sufficient to show that 90% of their $(1+1)$ events are GDR events. Their arguments involve the angular distributions and the kinetic energy spectra of the ejected protons. The angular distributions given in Fig. 2 of Ref. 3 do not appear, as claimed in A, to be greatly different from the $\sin\theta$ distribution predicted for an isotropic distribution. Certainly Fig. 2 of Ref. 3 does not show 90% of the data to be in disagreement with the assumption of isotropy. Similarly, the energy spectrum presented in Figs. 1(a) and 1(b) of Ref. 3 is substantially the same as the well-known energy spectrum[5] of evaporation prongs. The difference in the tail of the two distributions would involve only a few events. The shift in peaks and resonance energy between the distributions labeled "light" and "heavy" do not have the required statistical weight to be significant. There were only 50 and 36 heavy events in the 10.1- and 15.8-GeV/c distributions, respectively.[3]

(2) In A, Jain claims that by applying the independent "shortest-range-prong" technique he obtains a separation into light and heavy events which is not far different from that obtained by the method we objected to.[1] What is important is whether the two techniques make the same identification for individual events. The total separations obtained with the two techniques differ by at least 10%. Yet reliable separation is essential to the

use of nuclear emulsion as both target and detector in the manner outlined in Ref. 3. Our objections to the method of separation proposed in Ref. 3 were not based on the relative numbers of light and heavy events obtained. There were large discrepancies with our data as well as a lack of theoretical justification.

(3) In our experiment (2 + 1) and (1 + 1) events were obtained on separate scans. Our analysis of (2 + 1) events was based on 104 (2 + 1) events,[2] not ~20 as stated in A. Forty-six of these events were used for the energy spectrum.

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