Reply to "Comment on 'Energy partition in near-barrier strongly damped reaction $^{58}Ni+^{208}Pb$' "

M. B. Chatterjee,* S. P. Baldwin, J. R. Huizenga,
D. Pade,† B. M. Quednau, W. U. Schröder,
B. M. Szabo, and J. Tõke

Department of Chemistry and Nuclear Structure Research Laboratory, University of Rochester, Rochester, New York 14627

(Received 5 November 1992)

It is pointed out that the Comment by Viola *et al.* does not contest the conclusions stated in our Rapid Communication [Phys. Rev. C **44**, R2249 (1991)]. Instead, the Comment offers and disputes an interpretation of our data, which is more extensive than warranted, given the actual uncertainties associated with our data and method of analysis.

PACS number(s): 25.70.Lm

In a recent paper [1] energy partition in strongly damped near-barrier collisions was studied for the very mass-asymmetric system $^{58}Ni + ^{208}Pb$. It was concluded that independently of the direction of the net mass flow, the heavy leadlike ejectile receives more than 50% of the total excitation energy generated in a collision. This conclusion was based on fits of neutron energy spectra with theoretical distributions obtained from simulation calculations using a set of assumptions regarding the primary masses and excitation energy division between the reaction fragments. Given the accuracy of the data and of the analytical technique employed, as assessed through series of simulation calculations, we felt that it is unjustified to attach an absolute significance to the fact that "arithmetical" best fits to the experimental neutron spectra were actually achieved when, in the simulation calculations, 90% of the total excitation energy was allocated to the leadlike fragments. Within the accuracy of the data, no dependence of the energy division on the direction of net mass transfer between projectile and target could be discerned.

The Comment by Viola *et al.* [2] points out that uncertainties in primary masses may influence the results of the simulation calculations. However, it fails to present results of any calculations or estimates showing that these uncertainties could possibly lead to conclusions different from those drawn in our paper and restated above. While pointing out the obvious sensitivity of the neutron multiplicity to the mass-to-charge ratios arising through the neutron binding energies, the authors of the Comment fail to appreciate the fact that in our experiment the slope parameters of the energy spectra of the emitted neutrons were determined along with the total neutron multiplicities. In the simulation calculations, it was found that for any given excitation energy division these slope parameters depend only weakly on the primary fragment masses, mostly through the level density parameter a. Therefore, the experimental slope parameters provide significant constraints on the values of the excitation energy division compatible with the data, almost independent of the neutron binding energies. Because of this insensitivity of the spectral slope parameters to the primary masses, we were able to state in our paper that the advantage of the method to study the excitation energy division through measurement of neutron emission patterns consists largely in a weak dependence of the results on assumptions made on the primary fragment masses. This insensitivity is not only intuitively obvious because of the rather straightforward character of the relationship between the slope parameter and the excitation energy, but is also supported by simulation calculations. The Comment presents no proof to the contrary and it does not even address this important point. Instead, it offers hand-waving qualitative arguments as to the inconclusiveness of our analysis, based on neutron binding energies alone and leaves to the reader the nearly impossible task to reconcile these arguments with those based on spectral shapes.

An important point of the Comment, also reflected in its abstract, is a speculation on the possibility of the negative correlations between the flow of mass and energy. The Comment argues against such correlations. In our assessment, the actual data and the method used in its analysis are not accurate enough to warrant such speculations. We fully agree with the Comment that any claims regarding the possible negative correlations would be inconclusive. But no claims regarding such correlations have been made in our paper, nor has it been claimed that the nucleon exchange model has been tested adequately. To answer these certainly valid questions was not the object of our paper or experiment.

In conclusion, we do not see any material disagreement between the conclusions actually reached in our paper [1] and views expressed and substantiated in the Comment. We agree that a better experiment is perhaps possible in which the fragment mass is measured with a resolution good enough to warrant new simulation calculations aiming at extracting more accurate quantitative information on the excitation energy division than that reported in our paper [1].

This work was sponsored by U.S. Department of Energy Grant No. DE-FG02-88ER40414.

*Present address: Saha University, Saha, India.

†Present address: Dornier GmbH, Friedrichshafen, Germany.

[1] M. B. Chatterjee *et al.*, Phys. Rev. C **44**, 2249 (1991).
[2] V. E. Viola *et al.*, preceding Comment, Phys. Rev. C **47**, 3001 (1993).