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Comments on "The Density Matrix of Scattered Particles"

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This note, in rejoinder to a paper by Newton critical of our analysis of certain limitations of quantum scattering theory, seeks to acknowledge and to clarify the disparate interests of the two conflicting articles.

Newton[1] has given an illuminating account of the detailed argument required to make the standard collision theory work for certain mixed quantum states. If the incoming particle is prepared in a mixture confined to a parallel beam of finite section (impact parameter range), the outcoming particle emerges in a radiating pattern in all directions. The claim that this allows one to treat sequential collisions of the type contemplated in our investigation[2] is made on the basis of selecting a "partial density matrix" in any desired direction $\hat{\mathbf{p}}_1$, renormalizing this, and using it as the state of the incoming particle in the second collision.

A more explicit use of the controversial, and in our opinion discredited, notion of wave packet collapse could hardly be invented. There is nothing in quantum mechanical evolution that can possibly justify the substitution of such a partial density operator for the true one. The correct procedure would be something like the following.

Let the subsequent collision be between two particles which are both emerging from previous collisions each prepared in appropriate beam mixes. If one forms a combination of the two radially distributed density operators by direct product to form a two-body density operator and then applies the unitary evolution operator (S-matrix) to the result, none of the approximations required to validate the simple collision model given by Newton are satisfied in this case. If there were previous interactions between the

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two particles, the situation would be even worse, for the resulting two-body operator would not even be such a direct product. In either case the problem is quite different from the first collision with a directed beam preparation.

The only way to achieve equivalence between incoming and emerging states would be to prepare the particles initially in radially isotropic and energetically canonical distributions, but the result would then be merely a quantum analog of Maxwell's classical derivation of his equilibrium distribution function.

In our paper we were interested in situations far from thermodynamical equilibrium, and the use of collision mechanisms for approach to equilibrium. Newton's revelation of "fakery" in orthodox pure-state collision theory and admission of an analogy with the coarse-graining device used classically to suspend basic mechanical laws are welcome confirmations of our main contention, that, if collision theory is followed consistently with quantum mechanical unitary evolution, it is impossible to explain thereby the approach to equilibrium of a gas.

We would further assert again that collision theory between pairs is secure only in its intended realm of contrived scattering experiments, not in the larger domain of natural interactions among the constituents of complex assemblies.

REFERENCES

1. R. G. Newton, *Found. Phys.*, this issue, preceding paper.
2. W. Band and J. L. Park, *Found. Phys.* **8**, 677 (1978).