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Reply to "Comments on alternate formulations for preequilibrium decay"

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Blann's criticisms of the exciton model formulation are found to originate mainly in misunderstandings. An attempt is made to support the contention that the exciton model uses a correct quantum statistical approach to the problem of preequilibrium decay.

[NUCLEAR STRUCTURE Exciton model, precompound decay]

Criticism of the exciton model formulation for precompound decay, with specific reference to some papers published by workers of the Milan laboratory,[1] has been expressed not only in a recent Comment (Ref. 2) but also in previous publications by Blann and others, latest among which is a recent article by Blann, Mignerey, and Scobel.[3] In this reply we concentrate on the issues that appear to be at the basis of Blann's recurrent criticism.

Although our formulation of the preequilibrium exciton model (EM) is available[1,4] it may be useful to recall here some of its basic features. In EM, the energy distribution of the particles emitted in preequilibrium processes is expressed by means of a sum of terms of the type

$$\sigma_c\left(\frac{W_c^n(E,\epsilon)d\epsilon}{W_c^n(E)+W_{eq}^n(E)}\right)D_n\,, \qquad (1)$$

where σ_c is the cross section for composite nucleus formation, the bracketed quantity is the ratio between the decay rate for a given final channel and the *total* decay rate of the system from a state characterized by n excitons and excitation energy E,[5] and D_n is a depletion factor accounting for emissions in the continuum in previous stages of the equilibration cascade. Expression (1) does not imply, in our opinion, a quasi-equilibrium assumption, which we are aware would conflict with the notion that preequilibrium particle emission cannot be supposed to proceed through quasi-equilibrium states, since the process is too swift (e.g., at sufficiently high energies the decay from a 3-exciton state takes place in a time of the order of nuclear traversal times). It does imply, however, a statistical approach, which is a consequence of the hypothesis (essentially similar to that discussed by Blann *et al.*,[3] and definable as the equiprobability of all possible dynamical paths in a certain process) that the interactions between incident particle and target nucleons, and between nucleons in the intranuclear cascade, can occasion with equal probability any of the states available for a given type of configuration. In this connection we point out that, owing to momentum conservation, at least in the very first stages of the equilibration cascade, not all the states counted with the usual state density expressions (like Ericson's[6]) are reached; but in the framework of the Fermi gas model the probability of finding, after the first interaction, a particle with energy between ϵ and $\epsilon+d\epsilon$ in nuclear matter is well reproduced (also allowing for momentum conservation and Pauli principle) by the ratio[7]

$$\frac{\rho_{p-1,h}(U)g\,d\epsilon}{\rho_{p,h}(E)}\,,$$

where $U+\epsilon=E$, and the state densities $\rho_{p-1,h}(U)$ and $\rho_{p,h}(E)$ take into account all the possible states corresponding to a given energy. Therefore, since in the explicit expressions of W_c^n and W_{eq}^n (reported, e.g., in Ref. 4) the state densities appear only as ratios, we think that the decay rates we use should be adequate also when dealing with the first stages of the cascade.

The statistical hypothesis mentioned above makes it essential, in our opinion, that the probability of occurrence of a particular process be calculated as the ratio between the number of events favorable to the process and the number of all possible events. For this reason, in EM we introduce *total* decay rates which are intended to represent all the possible decay modes of the composite nucleus, once its total energy E and exciton number n have been fixed. It is our understanding that a different kind of statistical procedure is applied in the hybrid model. It seems to us that there, once the total energy E and the exciton number n are fixed, only one particular class of decay modes is selected: namely, the class containing the modes that involve "those neutrons or protons which have energies in the continuum between ϵ and $\epsilon+d\epsilon$." We believe this to be an important difference and will return to this point later on; but first we want to continue our reply to the suggestion that EM relies on quasi-equilibrium physics.

Expression (1) contains two types of decay rates: that for exciton-exciton interaction $W_{eq}^n(E)$, and that for particle emission $W_c^n(E,\epsilon)d\epsilon$ [as for $W_c^n(E)$, it is equal to $\sum_\nu\int_0^{\epsilon_\nu^{\max}}W_c^n(E,\epsilon)d\epsilon$, where ν labels the different kinds of particles that can be emitted]. The first, obtained by a calculation based on two-body collisions in nuclear matter, does not imply any assumption of quasi-equilibrium. The second, although of the same form as that usually employed for long-lived states, can also be justified outside the limits of such restriction. Using the notation of Ref. 2, and considering only the most frequent decay modes, $W_c^n(E,\epsilon)d\epsilon$ reads

$$\left[\frac{\rho_{\mathrm{p}-1,\mathrm{h}}(U_R)g(\epsilon')}{\rho_{\mathrm{p},\mathrm{h}}(E)}\right]\lambda_c(\epsilon)d\epsilon \,. \tag{2}$$

In this formula, the first term within the bracket is the product of the number of particles times the probability density that the composite nucleus, with excitation energy E, be in an n-exciton configuration containing one nucleon of excitation energy between ϵ' and $\epsilon'+d\epsilon'$. $\lambda_c(\epsilon)$, the decay rate corresponding to the emission of this nucleon in the continuum, is given by

$$\lambda_c(\epsilon)=\frac{\sigma_{\mathrm{inv}}(\epsilon)(2\epsilon/m)^{1/2}\rho_c(\epsilon)}{g(\epsilon')V}\,, \tag{3}$$

where $\epsilon'=\epsilon+B$, ϵ is the energy of the emitted nucleon, and B its binding energy. In our opinion it can be seen, by examining the structure of our formula, that no new hypotheses are implied by it in addition to those present also in the hybrid model or in the master equation approach of Harp, Miller, and Berne.[8] The expression of $\lambda_c(\epsilon)$ is commonly deduced using the detailed balance principle; however, its structure is that to be expected on the basis of the statistical hypotheses previously mentioned, even in the absence of quasi-equilibrium. Formula (2) can indeed be factored into three terms

$$\left(\frac{1}{\rho_{\mathrm{p},\mathrm{h}}(E)}\right)[\rho_{\mathrm{p}-1,\mathrm{h}}(U_R)\rho_c(\epsilon)d\epsilon]\left(\frac{\sigma_{\mathrm{inv}}(\epsilon)(2\epsilon/m)^{1/2}}{V}\right), \tag{4}$$

the first of which represents merely the probability of occurrence of a particular state in a configuration of p particles, h holes with a total energy in the interval E, $E+dE$; the second gives the total number of states available to a particle with energy between ϵ and $\epsilon+d\epsilon$ in the continuum and to the residual nucleus of energy U_R; the third represents the reduction in the number of final states due to the finite size of the nucleus and, if required, to the Coulomb barrier.

Another criticism in Blann's comment[2] is the suggestion that there are errors in the EM formulation "due to improper inclusion of spectator effects." To support this criticism, a numerical example is given concerning the relevant lifetimes which enter the two formulations. To this we reply by pointing out that our treatment concerning this matter is at variance with that of the hybrid model on account of the already mentioned difference in the statistical hypotheses used. We think that the hypotheses used in EM are consistent with the requirements of a quantum-statistical treatment. In our opinion, because the composite system is in a state that is a superposition of many shell model states, one cannot consider a particular configuration but must take into account the wholeness of the configurations corresponding to a given total energy and to a given exciton number. Besides, even when only a particular configuration is considered, it seems to us that the lifetime of the system cannot be characterized solely by its swiftest particle; and neither would we deem it consistent, as already remarked by Miller,[9] to assume (if only the unbound particle interactions are taken into account) that at the next stage of the cascade the state densities corresponding to given n and energy could be those foreseen, e.g., from Ericson's formula.[10] A further demonstration of our "spectator bookkeeping error" is said to result from comparison with the Harp-Miller-Berne master equation approach. We limit ourselves to state that also in EM the particle-particle collision probability per unit time depends only on the particle energy; and in fact (as will be discussed later) we do reach the same conclusion as Miller concerning the apparent mean free path in nuclear matter.

These arguments are summarized in the preceding comment in the statement that our EM formulation "uses an improper lifetime dependence" which "leads to unrealistic parameter values for (intranuclear) mean free paths." To support this contention that a mean free path longer than that deduced from Fermi gas model and free n-n cross sections is unrealistic, a number of inadmissible consequences are listed that it is suggested would follow. But such claims are not accompanied by quantitative evaluations. As for our position, we indicate that an account of new results reinforcing the basis on which our interpretation was founded has been presented in a recent letter,[11] and need not be repeated here. One comment worth recalling, however, is that the systematic disagreement between calculated and experimental cross sections that we find with the use of the short mean free path value corresponding to free n-n scattering can be noted also in treatments different from our EM.

In fact, it was pointed out in Ref. 11 that for the reactions analyzed there the available Monte Carlo calculations[12] systematically underestimated the emission of high energy particles and overestimated the emission of low energy particles, just as was found to be the case with the EM predictions when a short mean free path was used. This conclusion is not invalidated by Blann's recent letter[13] which is cited in Ref. 2. Our answer in this connection is given below.[14] Even the improvement in data reproduction reported in EM when adopting a long mean free path is matched by equivalent findings in other approaches. For instance, referring to results obtained with the Harp-Miller-Berne master equation, Miller himself wrote: "It is seen that a reduction in the internal transition probabilities by a factor of four brings about considerably better agreement with the experimental results"[15] (cf. Ref. 9 and Fig. 10 therein). Also, some authors who employed the hybrid model (with results that Blann, however, intimates to be erroneous) were willing to allow for sizeable reductions of the collision rates. Thus in their study of $(n,2n)$ and $(n,3n)$ reactions induced by neutrons of $E_n \leqslant 30$ MeV in nuclei with $25 \leqslant A \leqslant 205$, Bayhurst *et al.*[16] express the opinion that the hybrid model estimate of $\lambda_+(\epsilon)$ should be reduced by a factor of 4. Several other statements to the same effect can be found in papers published in a recent technical volume,[17] whence we quote the following passage: "Really the collision rate $\lambda_+(\epsilon)$ must be multiplied with an adjustable parameter $1/K$, where $K \approx 5\ldots 10$. Only in this case the hybrid model gives satisfactory absolute preequilibrium cross section values at excitation energy about 20 MeV."[18] We understand that such concurrences with our long mean free path suggestion are simply a consequence of the fact that disagreements between exciton and hybrid model results tend to lessen with decreasing composite nucleus excitation energy. But, as a general conclusion of the above discussion, we feel that since the inadequacy of the short mean free path value appears to be not model dependent, the suggestion that our position is brought about by the use of an improper lifetime dependence is far from proved.

Once it is recognized that the notion of a long intranuclear mean free path is not a mere peculiarity of our EM formulation, the question of whether its indicated value[11] (~17 fm) is unacceptable is certainly foremost. We have already mentioned that this issue should be settled by quantitative evidence. Were we to follow the vein of the preceding comment, we might reply to point (1) concerning the difficulty with angular distributions, that as a matter of fact the experimental distributions appear quite less forward peaked than those calculated by Hayakawa, Kawai, and Kikuchi[19] within nuclear matter. Qualitatively, we would expect that long mean free paths might remedy this disagreement because with this hypothesis the calculated number of forward particles should be comparatively reduced, since on the main only particles coming directly from first collisions might have had short enough paths in the nucleus to retain a memory of the direction of incidence. We might similarly counter the other points. For the time being, there is no denying that the notion of a long mean free path is generally unpopular, so that some other way to bring together calculations and experimental results would be more appealing to many physicists. Thus far, the only suggested alternative is that of assuming (to quote Miller again[9]) "that an important part of precompound emission occurs when the excitation is concentrated in the outer diffuse region of the nucleus." We agree on the expectation that neglect of peripheral interactions would bring about an apparent mean free path longer than the possibly correct one. Our difficulty in accepting the idea of a preponderance of peripheral interactions stems from the fact that we know of no decisive experimental evidence for the intervention of such processes with the required rate of occurrence, nor did computational attempts produce support for the idea: Monte Carlo calculations with the VEGAS code, taking into account the geometry of the process and the correct variation of nuclear density with radius, failed to give better accord with the experiments than the barely fair agreement obtained with the exciton model, when in both calculations the mean free path predicted by Fermi gas model and free n-n cross sections was used.[11,12] Blann claims to have found the solution of the mean free path problem in his hybrid model, particularly in the "geometry dependent" variation (GDHM). We think that the foundations of his claim need some strengthening yet. As for results, Blann himself has summarized the general outcome of the more effective version—the GDHM—by stating[20,21] an ability to reproduce the experimental data within a factor of 2, which is not a decisive agreement. As for physical consistency, he introduces in the GDHM further assumptions which we question in addition to those already present in the hybrid model: his use of a density dependent single particle level density $g_x(R)$[22] and of density dependent mean free paths and decay rates (even if said to be averaged along the particle trajectory) may be expedient but is not convincing. Furthermore, his recourse to peripheral interactions results in assumptions we find contradictory, such as:

(a) The projectile-target interactions and the first chance n-n cascade interactions occur with maximum probability in the shallow outer region of the nuclear volume where the density ρ is smaller by an order of magnitude than the central density ρ_0.[22]

(b) A sizeable fraction of the precompound spectrum [30% in the case of the ^{54}Fe(p,p') reaction] comes from the nuclear region where $\rho/\rho_0 < 0.1$.[22]

(c) The decay rates in the same region are those corresponding, for nucleons of, e.g., 60 MeV, to mean free paths of several tens of fm (cf. in particular Fig. 1 of Ref. 22). It should be noticed that such mean free paths are those corresponding to the maximum density along the particle trajectory. The author has modified, partly at least, his procedure in later papers[20,21] by introducing the refinement of averaging along the particle trajectory. An indication of the eventual effect on mean free paths of this change can be derived from Figs. 4–7 of Ref. 21. It appears that the emission of preequilibrium particles, calculated with the averaging procedure, is always greater than that obtainable for the maximum density along the incident particle trajectory, so that the result corresponds to a substantial rise of the effective mean free path.

To close this reply, we may add a few words concerning Sec. 3 of Ref. 2. What can be considered the basic issue there should now be clear from the last part of the preceding discussion. Blann *et al.* had stated, in their paper[21], their belief in "the failure of the (hybrid model) calculations when surface interactions are not included explicitly," and also claimed that calculations of their own had shown a similar failure of the EM. We were disturbed by the implication that a sizeable amount of surface interaction could be thought to be the *only* assumption capable of giving a reasonable reproduction of the data, and therefore showed[23] that a different calculation based on the EM and our phenomenological decay rates gave as good accord with the same data as could be expected. Blann's present attempt does not, in our opinion, get at the root of our differences, and we are not prepared to accept his suggestion that his reinterpretation of our analysis confirms "the necessity of the geometry dependent approach."[24]

The GDHM and the EM approaches rest on different statistical assumptions: we maintain that those underlying the present formulation of the GDHM are objectionable.

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[1] See Refs. 9–13 of Ref. 2. The criticism is aimed at the work of our group, but the basic arguments would be directed also against the position of many other research workers who have been using the exciton model, and whose relevant papers are quoted. e.g., in our article [Riv. Nuovo Cimento 6, 1 (1976)].

[2] M. Blann, Phys. Rev. C 17, 1871 (1978).

[3] M. Blann, A. Mignerey, and W. Scobel, Nukleonika 21, 335 (1976).

[4] E. Gadioli, E. Gadioli Erba, L. Sajo Bohus, and G. Tagliaferri, Riv. Nuovo Cimento 6, 1 (1976); E. Gadioli, Nukleonika 21, 385 (1976).

[5] Hence we do not agree that in EM "the λ_c (ϵ) and λ_+ (ϵ) of the denominator [of Eq. (1) of Ref. 2] are replaced by *average values over all particles and holes* of the *n* exciton states."

[6] T. Ericson, Advan. Phys. 9, 423 (1960).

[7] The somewhat lengthy calculation supporting this statement can be supplied on request.

[8] G. D. Harp, J. M. Miller, and B. J. Berne, Phys. Rev. 165, 1166 (1968); G. D. Harp and J. M. Miller, Phys. Rev. C 3, 1847 (1971).

[9] J. M. Miller, in *Proceedings of the International Conference on Nuclear Physics, Munich, 1973,* edited by J. de Boer and H. J. Mang (North-Holland, Amsterdam/American Elsevier, New York, 1973), Vol. II.

[10] Although, after the above explanations, discussing the numerical example on lifetimes of Ref. 2 is irrelevant, we remark that the choice does not seem appropriate. If in fact the purpose is to emphasize that the two formulations do produce different results, the single particle lifetime with the prescription used in Blann's previous works, and the exciton configuration lifetime as in our previous works might be calculated. Results nearly coincident numerically, namely, 0.182×10^{-22} s for the single particle lifetime and 0.170×10^{-22} s for the exciton configuration lifetime, would then be found.

[11] E. Gadioli, E. Gadioli Erba, G. Tagliaferri, and J. J. Hogan, Phys. Lett. 65B, 311 (1976).

[12] G. B. Saha, N. T. Porile, and L. Yaffe, Phys. Rev. 144, 962 (1966); D. R. Sachdev, N. T. Porile, and L. Yaffe, Can. J. Chem. 45, 1149 (1967); M. V. Kantelo, Ph.D. thesis, McGill University, Montreal, Canada, 1975 (unpublished).

[13] M. Blann, Phys. Lett. 67B, 145 (1977).

[14] In this footnote we reply to objections raised in Ref. 2 to our appraisal of published results of calculations based on intranuclear cascade models (ICM). (i) We do not find these objections evidence for changing our belief that ICM calculations have always shown a definite tendency to underestimate the emission of high energy particles, and *ipso facto* to overestimate the emission of low energy particles. This is the explicit conclusion of Miller (Ref. 9), confirmed by a later report of H. W. Bertini, G. D. Harp, and F. E. Bertrand [Phys. Rev. C 10, 2472 (1974), Fig. 4], and again by works cited in our letter (Ref. 11). It is also known that the above mentioned effects are even more pronounced in results obtained with the VEGAS code than in Bertini's calculations. Still, VEGAS is the most sophisticated type of calculation available for ICM, and does take into account effects (refractions and reflections) which Bertini choses to neglect. (ii) We have stated that the disagreement found between experiments and EM calculations using a $\lambda = 4.2$ fm mean free path (mfp) is comparable to that reported in several publications of ICM results, but we

did not claim or imply that the use of a "4 times normal" mfp value would improve the ICM results. As a matter of fact, since the ICM calculations referred to are essentially classical, we expect that they should fail when the mfp value approaches the nuclear dimensions. (iii) The problem of the reaction cross section value corresponding to a long mfp has been considered in a previous publication [E. Gadioli, E. Gadioli Erba, and P. G. Sona, Lett. Nuovo Cimento 10, 373 (1974)]. It was shown there that the nucleus turns out to be too transparent only when the low density nuclear region, where the mfp does not vary appreciably [G. W. Greenlees, G. J. Pyle, and Y. C. Tang, Phys. Rev. 171, 1115 (1968)], is not included (as it should be) in the effective interaction volume. The question has been taken up [E. Gadioli, E. Gadioli Erba, and G. Tagliaferri, Phys. Rev. C 17, 1294 (1978)] in a work in which we obtain the result that our long mfp is not in contradiction with the phenomenological values of the absorptive (imaginary) optical potential parameters.

[15] We hasten to add that Miller's very next sentence reads: "Needless to say, an arbitrary reduction in the quantity by a factor of four is hardly acceptable." We want it understood that by quoting, as we do, this statement of a fact, we make no claim about Miller's opinion at the time of his writing. The remark in Ref. 2 that "the comparison made by Gadioli to a result of Miller's (in which the mfp values were increased fourfold, but without recalculating the reaction cross section in a consistent fashion) is at best inconclusive" does not seem fitting, since in the HMB approach (Ref. 8) what is calculated is not the reaction cross section, but rather the probability of emission in a given channel.

[16] B. P. Bayhurst, J. S. Gilmore, R. J. Prestwood, J. B. Wilhelmi, N. Jarmie, B. H. Erkkila, and R. A. Hardekopf, Phys. Rev. C 12, 451 (1975).

[17] *Nuclear Theory in Neutron Nuclear Data Evaluation* (IAEA, Vienna, 1976), Technical Document No. IAEA-190, Vol. II.

[18] D. Hermsdorf, G. Kiessig, and D. Seeliger, in *Nuclear Theory in Neutron Nuclear Data Evaluation* (see Ref. 17), paper No. 14. Blann's correspondence with the Dresden group does not demonstrate that the code they used was defective, nor that they have acknowledged any faults in their calculations.

[19] S. Hayakawa, M. Kawai, and K. Kikuchi, Prog. Theor. Phys. (Kyoto) 13, 415 (1955).

[20] M. Blann, Nucl. Phys. A213, 570 (1973).

[21] M. Blann, R. R. Doering, A. Galonsky, and D. M. Patterson, Nucl. Phys. A257, 15 (1976). The pertinent sentence reads: "It is felt that the models (HM and GDHM) have an inherent uncertainty in predicted absolute cross sections of the order of a factor of two."

[22] M. Blann, Phys. Rev. Lett. 28, 757 (1972).

[23] E. Gadioli, E. Gadioli Erba, and G. Tagliaferri, Phys. Rev. C 14, 573 (1976).

[24] We should clarify the statement (of Ref. 2) that in our paper (Ref. 23) the "use (of) a partial state density expression with a limit to the depth of hole excitations of 20 MeV (was) a change from earlier calculations...." Actually, this change was not introduced as a new feature in that paper, because the limitations on state densities brought about from the finite depth of the potential well have been taken into account in all our papers since 1973, when the matter was examined [E. Gadioli, E. Gadioli Erba, and P. G. Sona, Nucl. Phys. A217, 589 (1973)].