

Reply to Comment on "Experimental test of the Anderson-Mott transition model for excitation transport"^{a)}

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Klafter and Jortner¹ (KJ) claim that the experimental evidence given by us² "does not exclude the concept of localization in a low-temperature inhomogeneously-broadened isotopic impurity band." While it is obvious that no experiment could exclude such a generally stated concept, we still believe that our experiment was indeed a critical test on the adequacy of an Anderson-Mott mobility edge interpretation³ for the observation⁴ of interest: A "critical concentration" behaviour of the electronic energy transport (EET) in the given system (naphthalene) under the given conditions (temperature, sample preparation, etc.). We also recommended² that the same test be applied to other systems exhibiting a critical EET concentration.

The objection of KJ to the validity of our test pertains to the effect of the concentration of the sensor ("energy sink") C_S on the critical concentration of the donor ("impurity") C_c (\bar{C} in KJ notation). KJ claim that this effect can be explained by the localization length effect on the expected Anderson-Mott type transition (resulting in a premonitory effect because of high C_S , i. e., $\bar{C} < \bar{C}$, where \bar{C} is the Anderson-Mott critical impurity concentration). Our objections to this recent, modified model^{1,5} suggested by KJ for our EET experiments are listed hereunder.

(1) There seems to be total agreement between our paper² and KJ^{1,5} that, for $C_S < 10^4$, \bar{C} should practically coincide with \bar{C} . Our new data in Ref. 2 (in contradistinction to the data of Ref. 4) were specifically taken for such low sensor concentrations, where C_S (SC in our notation²) was about 10^4 for one EET curve and 10^5 for the other. We showed that the measured donor (impurity) "critical concentration" C_c revealed by the second of these curves was about twice that of the first curve. This is hardly a negligible effect; rather it is about two orders of magnitude larger than the effect expected from the KJ localization model.⁵ This one point should suffice to support our conclusion, i. e., a negative result in our test for an Anderson-Mott type transition (for our specified experimental system). We list a few auxiliary observations supporting our conclusion.

(2) The premonitory transition effect due to excitation localization^{1,5} should show a *sublinear*, asymptotically decreasing dependence of C_c on C_S . However, the observed effect is at least linear and actually appears to be superlinear. The KJ localization model⁵ should actually show an EET curve "convergence" with lower C_S (at about 10^4). This is *not* observed over the wide range^{2,4} between $C_S = 10^2$ and 10^5 .

(3) The threshold (C_c) behavior of the curve is sharper at high C_S and much less so at low C_S . We believe that the KJ model⁵ gives the opposite behavior.

(4) We have by now experimental data on the effect of temperature on the EET. A glance at Fig. 1 (which includes essentially the original curves of Ref. 2, taken at 1.8 K, with the same samples again measured at 4.2 K) reveals two effects: (a) A reduction of C_c with temperature. This is consistent with a dynamic model^{4,6-8} but is not predicted from the KJ Anderson localization model. (b) While KJ predict⁵ an erosion with temperature of their transition the observed EET transitions actually sharpen up with increased temperature (Fig. 1). (c) The temperature effect is similar both above and below C_c , a behavior *not* expected if C_c marks a transition from localized to extended states.

In conclusion, we believe that the critical EET concentrations observed in naphthalene^{2,4} are not accounted for by the KJ models^{1,3,5} of an Anderson transition. Here, as previously,² our observations do conflict with KJ's original model,³ in which the *sensor concentration* played *no* role in determining the critical donor concentration. Even though the more recent version of their model^{1,5} does allow for *some* effects due to the sensor concentration, the observed effects seem to be quite different in the systems where this test has been applied. Also, the temperature effect is consistent with a dynamic model, i. e. thermally assisted hopping throughout the full C range. Furthermore, we mention the very recent evidence in favor of a dynamic (kinetic) model obtained from time-resolved EET measurements⁹ and note that a simple minded diffusion model is offered¹ by KJ as an alternative to their own Anderson transition model. Conceptually, this alternative does agree with our dynamic in-

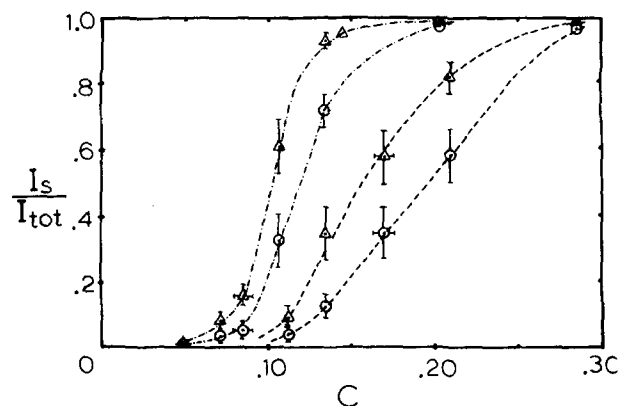


FIG. 1. Temperature dependence of critical concentration.¹⁰ The threshold for energy transport, C_c , decreases with increasing temperature from 1.7 K (circles) to 4.2 K (triangles). The dot-dashed curves are samples with $S = 10^3$ while the dashed curves are samples with $S = 10^4$. Note that $S \equiv C_S/C$ and that $I_S/I_{tot} \equiv I_S/(I_S + I_d)$, where I_S is betamethylnaphthalene phosphorescence (0-0) and I_d is that of $C_{10}H_8$.

terpretation. It appears, however, that the details of their diffusion model [Eqs. (1) and (2)]¹ are grossly oversimplified, but this topic is best addressed elsewhere.¹¹ We believe that the nature of the dynamics (i. e., diffusion vs. percolation) is indeed the interesting question, as the weight of the experimental evidence seems to be against the static Anderson transition models.

Note added in proof: An interesting analogous case for quasi two-dimensional systems is that of "non-metallic conduction in electron inversion layers at low temperatures" [D. J. Bishop, D. C. Tsui, and R. C. Dynes, *Phys. Rev. Lett.* **44**, 1153 (1980)] where there is a lack of true metallic conduction and where the authors state: "There is no evidence of any abrupt behavior and our data qualitatively support the intuitive arguments of AALR [E. Abrahams, P. W. Anderson, D. C. Licciardello, and R. V. Ramakrishnan, *Phys. Rev. Lett.* **42**, 673 (1979)] who predict that there should exist no sharp mobility edge in two dimensions..."

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⁴R. Kopelman, E. M. Monberg, and F. W. Ochs, *Chem. Phys.* **19**, 413 (1977).

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⁸E. M. Monberg and R. Kopelman, *Chem. Phys. Lett.* **58**, 497 (1978).

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¹⁰D. C. Ahlgren, Ph.D. thesis, University of Michigan (1979).

¹¹R. Kopelman and P. Argyrakis, *J. Chem. Phys.* **72**, 3053 (1980). See also Refs. 7 and 9 of the preceding comment.

Erratum: The vibrational infrared spectrum of the Group IV transition metal nitrides [J. Chem. Phys. 70, 3497 (1979)]

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Recent evidence indicates that the ground state vibrational frequency of TiN is 1039.63 cm^{-1} .^{1,2} This indicates that the 969.6 cm^{-1} band does not arise from ground state TiN molecules as reported. The correct assignment for this band is not presently known.

In view of this error, the reported frequencies for ZrN and HfN should also be regarded as very tentative until they can be verified by an independent measurement.

The authors are grateful to Dr. A. E. Douglas and Dr. F. W. Froben for providing information of their work prior to publication and seriously regret any inconvenience that this error may have caused.

¹A. E. Douglas and P. M. Veillette, *J. Chem. Phys.* (to be published).

²F. W. Froben, *J. Mol. Structure* (submitted for publication).