these networks by various heat treatments (*5–7*) but it must be pointed out that they are comparatively coarse networks. As quenched from the γ-phase, the particles in the network are virtually undetectable except by transmission electron microscopy and are no more than 25 to 50 Å in diameter, but are still of the order of 0.1 μ apart (*6*). Under conditions of slow-cooling and after α-phase annealing the particles will be larger than this, about 0.1 to 1.0 μ, but are much further apart, about 1.0 μ (*8*).

A reasonable estimate for the coherence length for these particles of U(CNO), if they become superconducting below 1°K, is about 100 Å (*9*). If this is the case, no superconductivity should be observed in the quenched condition, and in that the particles are so far apart in the slow-cooled or annealed conditions, no bulk superconductivity effect should be observed there either. The latter remark also applies to the isolated particles of U(CNO), U_6Fe, UAl_2, and UO_2 not associated with the networks. Of these only U_6Fe has been observed to become superconducting above 1°K (*10*). In addition it should be pointed out that UN becomes antiferromagnetic below 45°K (*11*), so it is most unlikely to become a superconductor.

No investigation by normal metallographic means (*5*), by x-rays, or by transmission electron microscopy (*6*, *8*, *12*) of any nominally pure uranium has ever shown the presence of retained β-phase at room temperature. Phases based on γ-uranium have never been retained in alloys containing less than about 9 atoms percent solute (*13*).

The situation is thus very different from that existing in the rhodium-lanthanum system (*14*) where the addition of 0.5 atoms percent lanthanum is sufficient to develop a grain boundary network of $LaRh_5$ whose thickness varies from 0.1 to 1 μ.

Hence the apparent superconductivity of α-uranium does not seem to be explicable on the basis of networks of U(CNO) or of retained β- or γ-uranium. However the phase U(CNO) should be investigated for superconductivity down to a lower temperature than 1.2°K (*10*), the lowest temperature to which UC and UN have been investigated.

B. W. HOWLETT
Metallurgy Division, Atomic Energy Research Establishment, Harwell, England

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Howlett's discussion appears to verify our hypothesis. In our paper we stated the reason for our belief that the superconductivity observed in α-uranium above 0.3°K is not a bulk effect and explained the apparent superconductivity observed as being due to filaments. Howlett says of our hypothesis, "If this is the case, no superconductivity should be observed in the quenched condition, and, in that the particles are so far apart in the slow-cooled or annealed conditions, no bulk superconductivity effect should be observed there either." This is just what we had tried to explain, namely why there is no bulk superconductivity. All caloric measurements, after all, do show the absence of bulk superconductivity (*1*), but Howlett nevertheless assumes bulk superconductivity.

The remaining metallurgy mentioned by Howlett is valid only for thermodynamic equilibrium, which is hard to obtain in uranium. It is not valid to assume the total impurity concentration to be below 100 ppm, the minimum required for our hypothesis (*2*).

In the meantime the two different systems of filaments we suspected have been seen with the help of an electron microscope.

B. T. MATTHIAS
Bell Telephone Laboratories, Murray Hill, New Jersey

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29 August 1966

Dimethyl Sulfoxide and Dogs

In view of the current situation regarding the use of dimethyl sulfoxide (DMSO) for experimental use on humans, I submit that the report by L. F. Rubin and P. A. Mattis [*Science* **153**, 83 (1966)] on oral administration of DMSO to dogs has such broad implications that a request for clarification of certain experimental conditions and of the results is in order.

Having experienced considerable difficulty in obtaining reproducible physical measurements even with recrystallized DMSO, I am concerned that no mention was made of the source of the material used for their experiments, of its purification, or of its analysis for impurities. The doses seem massive; even small percentages of impurities might of themselves account for the effects noted.

A more general question concerns the dosage range and the accuracy of the report. Assuming that the work is intended to have bearing on the use of DMSO as a medicinal agent, I wonder why data were not collected under conditions more closely resembling those usually obtaining in such usage—especially external application in small amounts—or at least more fully in low-dosage regions; even 2.5 g per kilogram of dog seems an unusually high dosage. But, even for the adverse situation created, the reported results of the tests are ambiguous. What does "could (would) not tolerate such dosage" mean?

I presume from "seven survivors" (of the original 12 or of the ten receiving DMSO?) that it means that the others died from the DMSO; but Table 1 says, of results of actual administration, that the dogs were "reported to have vomited drug after dosage." It would have been also relevant to report the cause of death, the general condition of the survivors, and whether other effects were sought or found.

One may argue that this report should be considered apart from the current controversy concerning use of DMSO, but both the timing and nature of the report oppose this argument. Assuming the validity of my earlier premise, I suggest that more relevant and thorough study and a more careful report were warranted.

L. J. SACKS
Department of Chemistry, State University College, Buffalo, New York