

HIROSHIMA-NAGASAKI GLASS: A LENS ON NEUTRON DOSES

I am attempting to locate documented glass from Hiroshima and Nagasaki that could provide neutron dosimetry using induced fission tracks caused by the nuclear weapons in 1945. I solicit help from anyone who knows of appropriate material.

Oddly, even 48 years after the events, neutron dosimetry remains uncertain by factors of 1.6 at ground zero and of 8 at a distance of 1.5 km from the epicenter at Hiroshima.¹ And reliable numbers are important, since permissible neutron exposures to human beings are based on the experience at Hiroshima—where the bomb allowed more neutrons to propagate to significant distances than was the case at Nagasaki.

Because of its usual structural homogeneity, glass records neutron-induced fission particularly well as radiation-damage tracks. As I pointed out² in 1987, etched tracks in glass of conventional uranium content (approximately 0.3 parts per million) would allow thermal neutron fluences to be measured up to approximately 1 km from the epicenter at Hiroshima, with the sensitivity being proportional to the trace uranium concentration within the glass. In short, higher uranium contents would allow dosimetry to greater radial distances.

Although many other materials, including plastics and dielectric crystals, also record tracks, glass is so ubiquitous in modern society and so simple to study that it is the clear first choice. Examples of appropriate substances are windows, lamps, decorative glass, glass electrical insulators, instrument covers, bottles, drinking glasses, auto windshields and headlights, glass display racks, refrigerator shelves, eyeglass lenses and the surface layers of porcelains.

While the possibilities are almost

countless, the immense obstacle now is the limited quantity of material whose position just prior to the detonations is established. Had etched fission tracks in solids been known in 1945, relevant dosimetry materials surely would have been sought, found and used. Nevertheless the opportunity still exists in principle, and I am attempting to prod people's memories in the hope of improving our knowledge of the health effects of neutrons.

The glazed overlayers of tiles are also candidate materials. Samples were kindly sent to me by Tsuneto Nagatomo and Masaharu Hoshi, but unfortunately my attempt to reveal the stored tracks failed because the combination of low uranium content and considerable structural nonuniformity conspired to make track scanning slow, laborious and consequently not very reliable (because of low counting statistics).

Material that was melted by radiation from the explosion is also an unlikely storer of neutron-induced tracks, unless only a thin enough layer was melted that it cooled rapidly between the moment of electromagnetic burst and the slightly delayed arrival of the neutrons.

In principle the high uranium contents that characterize many of the minor accessory minerals in certain rocks make them sensitive detectors of neutrons. Examples of such minerals are zircon, apatite and sphene, which are common constituents of granites. However, in old detector material that contains uranium, spontaneous fission of ²³⁸U stores tracks that are useful in determining ages of minerals but impedes dosimetry by preloading samples with tracks. Only if it is known that natural minerals were sufficiently heated prior to the neutron exposure that tracks were annealed out can

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these minerals serve as convenient neutron dosimeters for the present purposes.

In short, uniform, well-located glass is the material of choice. I would welcome communication from anyone who knows of such material, or who can direct me to someone who might, at the Department of Earth and Environmental Sciences, Rensselaer Polytechnic Institute, 110 8th Street, West Hall G-17, Troy NY 12180-3590; phone 518-276-8523; fax 518-276-8627.

References

1. K. Shizuma, K. Iwatani, H. Hasai, M. Hoshi, T. Oka, H. Morishima, *Health Phys.* **65**, 272 (1993).
2. R. L. Fleischer, *Health Phys.* **52**, 219 (1987).

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In Explaining High T_c , Is d-Wave a Washout?

The Search and Discovery story "In High- T_c Superconductors, Is d-Wave the New Wave?" (May 1993, page 17) did not adequately reflect the nature and severity of the negative comments that I expressed to its author, Barbara Goss Levi. I believe my misgivings are widely shared in the theoretical community and I think they should have been less casually treated.

Levi's treatment of the experiments, which occupied most of the article, was careful and pretty much evenhanded. One not entirely minor point is that the photoemission spectrum of Zhi-xun Shen given in the striking illustration on page 19 is also a striking illustration of how lines drawn to "guide the eye" often deceive it instead. The blue and green points (obtained above and below T_c , respectively) on curve B differ by more than experimental error and clearly signal an energy gap in the Fermi surface of some magnitude at point B in wavevector space. (I estimate approximately 0.3 of the gap at point A.) The line as drawn is not a good representation of either set of points. Thus while the evidence for anisotropic electron pairing is strong, that for a node in the gap, as expected for $d_{x^2-y^2}$ pairing, is not.

I also feel that the remark quoted from Malcolm Beasley seriously misstates the situation: Many of us feel that experiments have *already* told us a great deal.

While the theoretical section of the story is brief, the prominence given

the experiments is clearly motivated by theory, so the following points should be taken into consideration:

▷ The "spin fluctuation" theories of T. Moriya, Douglas Scalapino and David Pines, especially that of Pines, are in a real sense not true theories but rather heuristic models with many unexamined assumptions. Some of those assumptions are very questionable, particularly the assumption that antiferromagnetic spin fluctuations can result from a perturbative "Fermi liquid" model. Antiferromagnetism as normally observed is a consequence of the Mott-Hubbard gap, which cannot be treated perturbatively. (The exchange coupling J is proportional to $1/U$, which cannot arise perturbatively in U , the interaction coupling constant.) Another, related assumption is the neglect of vertex corrections, as pointed out by J. Robert Schrieffer in his talk at the Santa Fe meeting where the problem of spin-fluctuation theory was extensively discussed.

▷ The spin-fluctuation model relies heavily on detailed computer calculations that are not subject to independent check and that have unknown sensitivity to the choice of parameters. Not only are a considerable array of arbitrary parameters adjusted to fit the data, but the spin fluctuations enter into the calculations via an arbitrary function that is only weakly determined by experiment. These computer fits are carried out for normal-state transport and magnetic properties as well as superconducting properties, and their workings are not available for detailed examination. In contrast, the most seriously competing theory, my own, relies on no calculations that cannot be verified by anyone on the proverbial back of an envelope (except for the recent calculations of gap anisotropy by Sudip Chakravarty, Asle Sudbø, Steven Strong and me). The few parameters used are not sensitive and are mostly commonsensical. Even the gap anisotropy (which was not produced in response to the experiments, contrary to Levi's statement) follows from a few-parameter tight-binding model that fits the calculated band structure. Normal-state properties are all simple power laws with the power determined from first principles. The fact that my approach is theoretically deep leads to computational simplicity.

▷ The normal states of all of the high- T_c materials at optimal doping differ very little; for instance, as shown by Bertram Batlogg, the resistivity per plane at optimal doping is

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