JOHN BARDEEN'S HIGH-T_c ANTICIPATIONS

Much has been written and told about John Bardeen's accomplishments and idiosyncrasies. I would like to relate a few events that reveal some lesser known characteristics.

In the mid- to late 1960s, I started discussions with Bardeen on the work function of metals, particularly my simple classical model vs the many-body quantum mechanical description Bardeen had pioneered with Eugene Wigner. I was also intrigued about the possibility of high-temperature superconductivity, particularly the proposals of Bill Little *re* long-chain organic superconductors and the "excitonic mechanism." But Bardeen did *not* favor these ideas. Moreover, he was convinced that within the Bardeen–Cooper–Schrieffer phonon concepts, superconductivity was limited to temperatures below 30 K. My vague ramblings about new mechanisms and totally new phenomena were not convincing. After many arguments, I gave up possible pursuit and did not see Bardeen until August 1983. Upon seeing me again he immediately said, "I now believe that high-temperature superconductivity is possible." He did not say, "Hello"; he did not say, "I have not seen you for about 15 years." Rather, he resumed the discussion exactly where we had "paused," as if there were no gap in time or space. I responded in kind and asked, "What made you change your mind?" His argument was twofold:

▷ Vitaly Ginzburg and his collaborators in Moscow had extended the excitonic ideas of Little to a metal–semiconductor interface, and Bardeen liked these concepts. He had his last two graduate students, David Allender and James Bray, work on the problem. In fact, one of their graphs[1] shows superconductivity possibly occurring above 800 K! In view of Bardeen's conservatism, I was shocked. Bardeen defended the graph vigorously because he thought it was very important to show the theoretical possibilities, even though he felt it unlikely that any real material would exhibit these characteristics. Bardeen said he argued strongly with the referee to retain at least one graph with superconducting transition temperatures of this magnitude.

▷ There had been considerable confusing experimental work on "superconductivity" in CuCl under dynamic pressure. After many years and many attempts, this work was largely disregarded as "crazy." Bardeen felt there was something interesting lurking beneath the confusion. This view was also held by a few other brave souls whom I encountered, including Paul Chu, Gary Vezzoli, Ted Geballe, Allen Goldman, Peter Walsh and Mark Miller.

Strangely, Bardeen was *not* influenced by the fact that superconductivity had finally been discovered in organic materials (as was originally suggested by Little) and the transition temperatures were steadily increasing with further research. Bardeen said he never bothered to think about it much! That seemed a very strange attitude. After considerable harassment from me, Bardeen considered the organics and concluded that they did not fit the BCS phonon mechanism. (This was very important to me because I was searching for unusual phenomena.)

Bardeen encouraged me to start an experimental search for high-T_c superconductors. He was very disappointed about the status of experimental searches, for several reasons:

▷ Although the Russians had a very vigorous theoretical effort under Ginzburg for approximately ten years, the experimental effort was weak and sporadic. Interest had waned.

▷ The limited experimental efforts in the US after the theoretical work of Allender, Bray and Bardeen had largely disappeared.

▷ The Japanese were expanding a serious search for high-T_c superconductors, guided largely by the excitonic ideas of Bardeen. Although Bardeen was flattered, he worried that if the Japanese should discover high-T_c superconductors first and uncontested, the US would be in very

dire economic straits. He was already very disappointed that the Japanese, starting from zero, had eradicated the US's early dominance of the semiconductor industry.

Interestingly, in all his arguments to stimulate a search for high-T_c superconductors, Bardeen never once mentioned a theoretical, abstract or philosophical motivation. He only stressed the myriad practical applications and the economic viability of the US. I found this very surprising for a physicist with such an abstract and theoretical reputation.

With Bardeen's aid, I finally succeeded in getting support for an experimental search for high-T_c superconductors at 3M Corp in 1985. Although we did discover new oxide superconductors, they were not high T_c. Later, when Georg Bednorz and Alex Müller discovered the cuprate superconductors, Bardeen extolled the commercial potential of high T_c and encouraged me to form my own company to exploit the opportunities. It is not clear that that was good advice.

A priori none of us foresaw the possible connection between oxide superconductors and the interfacial excitonic mechanism proposed by Ginzburg and his colleagues and by Allender, Bray and Bardeen. The layered characteristics of the high-T_c superconductors suggested such a mechanism, and we continued to discuss this and many other options until Bardeen's death. Both he and Ginzburg felt that an excitonic mechanism was as likely an explanation of high T_c as any other, although certainly not conclusive. Bardeen, always philosophical, said the truth would eventually be known and he was not going to argue with anybody, since he had already published his thoughts on high T_c. If this interfacial excitonic theory eventually proves successful (in the oxides or otherwise), it should earn Bardeen a third Nobel Prize in Physics. (At the "Woodstock of physics," it was widely promulgated that high T_c was totally unanticipated. Apparently most physicists do not read the literature or do not take it very seriously.)

Bardeen was totally devoted to physics, a subject his wife described as his mistress and obsession. I was always amazed at his enormous reading capacity and immediate comprehension. He was always up to date. He was also an extremely attentive listener, although he usually appeared to be asleep, a characteristic many people found very disturbing! Many misinterpreted his somnolent appearance as reflecting a lack of interest, but he was just deep in thought. In our last conversation he asked, as always, about business, and I said, "We are just trying to survive!" "Me too" was his characteristically laconic reply. I will remember him with great admiration as a physicist and human being of highest integrity and exemplary behavior.

Reference

1. D. Allender, J. Bray, J. Bardeen, Phys. Rev. B **7**, 1020 (1973).

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Ferment over Beer Bubbles

Scientists have always been fascinated by macroscopic phenomena that provide insight into the underlying molecular mechanisms. The rising of a bubble in a glass of beer, as described by Neil E. Shafer and Richard N. Zare (October 1991, page 48) is a case in point. Shafer and Zare explain why a bubble grows as it rises, using phenomenological theory. At the same time, the rise of a bubble reveals underlying molecular motion.

Starting from an equation that describes the increase in the number N of CO_2 molecules in the bubble with time t,

$$\frac{dN}{dt} = \gamma 4\pi r^2 \qquad (1)$$

where γ is a phenomenological coefficient and r the radius of the bubble, Shafer and Zare find that

$$r = r_0 + v_r t \qquad (2)$$

where r_0 is the radius at $t = 0$ and $v_r = \gamma k_B T/P$ is the rate of increase of the bubble's radius, experimentally found to be 4×10^{-5} m/sec. In the derivation it was assumed that γ and P, the pressure in the bubble, are constants. Strictly speaking, this is not true. P depends weakly on r, since in addition to the atmospheric pressure it contains a contribution due to the curvature of the bubble and a hydrostatic term. The coefficient γ can be identified with the flux of CO_2 molecules toward the rising bubble and can be calculated from standard transport theory:[1]

$$\gamma = D\frac{\Delta n}{\delta} = 0.623\, D^{2/3}\frac{u^{1/3}}{r^{2/3}}\Delta n \qquad (3)$$

Here D is the diffusion constant of a CO_2 molecule; Δn, the difference in CO_2 concentration far from the bubble, n_∞, and at the bubble surface, n_0;

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