Woodstock of physics revisited

Ten years have passed since the now famous American Physical Society meeting that heard the first breathless accounts of high-temperature superconductivity. Now, in calmer times, practical applications are emerging.

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Rising stars: Müller and Chu with Shoji Tanaka (right), whose Tokyo laboratory provided one of the first confirmations of Bednorz and Müller's discovery.

Snap quiz: who can tell me the winner of the 1987 Super Bowl? Not most physicists, I suspect, for whom it was certainly eclipsed by two events of far greater consequence that shared the early months of that year. One, the discovery of Supernova 1987A, perhaps portended the other: the announcement of superconductivity above liquid-nitrogen temperature on planet Earth — a dream fulfilled for many condensed-matter physicists like myself, whose careers had orbited around this elusive star.

The successful sighting[1] fell to W. K. Wu and C. W. (Paul) Chu and their teams of students and postdocs at the Universities of Alabama and Houston, following only five months after the publication in autumn 1986 by Georg Bednorz and Alex Müller[2] at IBM Zürich of their discovery of superconductivity in a previously unexplored class of compounds, the layered copper-oxide perovskites.

The 'inside' story of the hectic interval between the first week in January 1987 — when an announcement of the confirmation of Bednorz and Müller's discovery first brought 'high-temperature superconductivity' to wide public attention — and the week of the American Physical Society's March meeting, remains to be told. Suffice it to say that this period, and the last three months of 1986, were replete with incredulity, credulity, excitement, secrecy and a sense of immediacy in competition with one's peers, all of which resulted in, frankly, a substantial amount of intrigue and suspicion. All who participated surely came to understand, if they had not done so before, that physics is not only a science but, perhaps more significantly, an intensely human pursuit — something they do not teach you in graduate school.

The programme of the March meeting, held each year in a different US city, is 'cast in concrete' early the preceding December; thereafter, an absolute policy of no alterations prevails. By the deadline of 5 December 1986, for the 1987 meeting at the Hilton hotel in New York City, only one abstract had been accepted on the new materials: "Specific heat of Ba–La–Cu–O superconductors" by Rick Greene and his collaborators at IBM Yorktown. But the explosion of results that appeared in the new year prompted the meeting's organizers to take an unprecedented step. Brian Maple of the University of California, San Diego, was asked to put together a special post-deadline evening session devoted entirely to the discovery.

All those wishing to report results would be granted five minutes each, in order of the arrival of their request to take part — and did the requests rain in, reaching a downpour in the two weeks before the meeting, as confirmations of the Wu–Chu measurements were made. All in all, 51 presentations were to be given throughout the evening and early morning of Wednesday and Thursday, 18 and 19 March. That memorable and riotous session was to become our "Woodstock of physics", so named in honour of the village only 50 miles north where, in an obscure farmer's muddy field in 1969, the rock concert occurred that defined a generation of youth the world over.

Fever pitch: the room filled to overflowing with physicists eager for news of superconductivity.

Opening act

A few personal observations and anecdotes may help to convey the colour of that week in midtown Manhattan. Excitement was running high even before Wednesday night. On Monday, the opening day, the press were already beginning to catch some of us to be interviewed. That noon my colleague Ed Engler and I went to lunch at a nearby Brew 'n' Burger and found Alex Müller sitting by himself in a corner booth, attempting to escape the turmoil at the Hilton. At the time he was not yet widely recognizable to those attending the meeting or to the press — a situation that would soon change.

Main attraction: Müller's appearance at the meeting was greeted with enthusiasm by the crowd.

On Tuesday morning, the 17th, as the Saint Patrick's Day parade began only a block away, Greene gave, to an overflow audience, the only talk on high-temperature superconductivity printed in the meeting bulletin. He began by introducing Müller to the crowd, who responded with enthusiastic applause. The bulk of Greene's talk was pretty much a summary of the work to date at the three IBM labs. But at the end of his talk, as a tease following the many reports of 'unidentified superconducting objects' (USOs) that were even then beginning to fly about, he jokingly reported that a minor insulating copper-oxide phase, called '211' (after its Y/Ba/Cu cation ratio), which precipitates during the synthesis of the 91-K superconductor $YBa_2Cu_3O_{7-\delta}$, or '123', had a transition temperature of 300 K — but as a ferroelectric, not a superconductor! This was a complete fabrication, but was swallowed by many. To this day, I'm occasionally informed by one of the unsuspecting, "Did you know that 211 is ferroelectric? Probably that's why it's such an effective pinning impurity in 123."

The happening

I was not present at 7 p.m. on Wednesday, when the doors of the Hilton's Grand Ballroom swung open to accept the surge of humanity — well, physicists — that then ensued. I was still having dinner with Müller, discussing some concerns he had over the unit-cell model for 123 that Robbie Beyers and Grace Lim in our group at IBM Almaden had determined from transmission electron microscopy and X-ray diffraction. Fortunately, the hotel had arranged reserved seating for the speakers, so on arriving at the ballroom we were able to make ourselves comfortable after manoeuvring through the swarm encamped on the floor and in the aisles. The talks started late, because of the difficulty of clearing this fractious mob — an issue that was finally resolved by the announcement that the hotel would provide television monitors in various lobbies that would carry the proceedings live. In the end, 1,800 people squeezed into a room meant for 1,100, with 2,000 more outside watching the monitors.

Things began with a 'plenary' session of 15-minute talks given by Müller, Chu, Shoji Tanaka (University of Tokyo), Bertram Batlogg (AT&T Bell Laboratories) and Z. X. Zhao (Institute of Physics, Beijing). Much to my satisfaction, Müller showed as his last viewgraph our Almaden 123 unit cell, so dropping the first bombshell of the night. Another came shortly after from Batlogg, who waved, to tumultuous acclamation, a piece of '123 green sheet' (a flexible substrate with as-yet unreacted precursor oxides). Batlogg's showmanship implied that applications were not far away, and set the scene for much of the press attention that we were to receive in the coming months.

Around 3 a.m., even the faithful were beginning to tire, and many drifted into the lobbies for individual discussion and gossip, always with one eye on the video monitors in case some of the remaining speakers might reveal one more item not previously covered. I joined Ted Geballe of Stanford University, who was pouring champagne for all gathered around him. Geballe could well claim to have been the mentor of the generation that had brought about the post-Bednorz–Müller discoveries, and he was clearly in his element. Around daybreak, Engler and I wandered a few blocks over to a rather rundown saloon on 7th Avenue for a final nightcap and wind-down of our eight-hour adrenalin rush. Our visit was reported several days later in the *Wall Street Journal* as, "Grant and Engler staggered into a midtown bar... ," much to the consternation, we later learned, of an IBM corporate vice-president when he read about it.

All of a sudden we physicists had become darlings of the media and the public. On Thursday night one of the most fashionable of New York discos, The Limelight, housed in a turn-of-the-century former Episcopal church on 20th Street, held a "physicists' night out". All one had to do was show up with a meeting badge, to go to the front of the line, and in for free. The scene of several hundred physicists bopping to the strains of 'heavy metal' music was too much, and certainly not in accord with our previous public image. In the months to come, there would be many interviews and television appearances. In May, the three main US news magazines all had cover stories on high-temperature superconductivity. Wow, what a time we perceived antisocial PhD nerds were having. But the central question would soon become, "Is this all there is? Will advancement of science and beneficial social application emerge, despite the hype?"

Sobering up

The years following the original "Woodstock" had dissolved into the disillusionment of the Vietnam War. The plaintive lyrics of Pete Seeger's classic, "Where have all the flowers gone?", revived in the 1970s, well reflected the frustration of the era that followed. Was a similar fate to be the legacy of our Woodstock? Would the expectations sown by us in New York in the spring of 1987 ever blossom? The answer is, yes, some shoots are appearing today, but it has taken longer than we naively believed at the time.

The large number of scientists from almost all materials-related disciplines who poured into high-temperature superconductivity following the New York meeting was testimony to the enormous excitement that a discovery both mysterious and with promise of great application can promote. Of course, the theoreticians are the first to jump on anything, so their presence was no surprise. The extraordinary number of others who entered the field, many of them physicists and engineers who had previously had nothing to do with superconductivity, I believe has a lot to do with the ease with which one could fabricate $YBa_2Cu_3O_{7-\delta}$. One of my most cited 'papers'[3] concerned the success of a high-school chemistry class in California who had made and tested their own sample of 123 that May, only four months after its discovery, and five months before the award of the Nobel prize to Bednorz and Müller for starting it all. For a while you could attract almost any audience, including the President of the United States and other heads of state, with parlour-trick-like demonstrations of magnetic levitation.

There was also some progress of a substantial nature. By the time of the following March meeting, a number of new layered copper-oxide perovskites had been discovered (some might say rediscovered). One of them, containing thallium oxide sheets, set a world-record transition temperature (T_c) of 125 K (ref. 4), which stood until 1993. (A related structure, with HgO essentially supplanting the TlO portion[5], now holds the ambient-pressure world record of 133 K.)

Nevertheless, a certain introspection was beginning to surface. The 1988 March meeting hosted a symposium entitled "Physics and the press". A panel of journalists and physicists (of whom I was one) confessed a collective *mea culpa* over the excesses that each had committed: there were no levitated trains, no electricity too cheap to meter, no dime-sized supercomputers remotely on the horizon. I believe that the experience helped to moderate and make significantly more critical (with a few notable exceptions) the press response to the 1989 announcement of 'cold fusion'. In 1995 and 1996, high-T_c superconductivity began to make news again as power applications came closer. By and large, the coverage has not been sensationalized — I guess we learned our lesson.

Reality set in with a vengeance in late 1988 and throughout 1989–90. The high-T_c materials are type-II superconductors; that is, they allow an applied magnetic field to penetrate in a lattice of discrete 'vortex lines', the presence of which dominates almost all of the applied physics of superconductors. A great richness has arisen from the study of flux lattice dynamics in these very anisotropic systems — the resulting phase diagram has even been termed 'vortex matter'. One can arguably claim that the layered copper-oxide perovskites represent the very essence of type-II superconductivity. The art of useful applications lies in devising means to 'pin' the vortex lattice against dissipative flow arising from the Lorentz force imposed by a current or an external magnetic field. Working against the pinning potential are thermal fluctuations, which free the vortices to move — a phenomenon dubbed 'flux creep'.

Early showmanship: Bob Cava, of AT&T Bell Labs, displays for the press the 'green sheet' that his colleague Batlogg had waved in the evening session, along with a disk of the 123 superconductor. Now the superconductor itself can be made in flexible form.

In 1988, Michael Tinkham of Harvard University speculated that, because high-T_c superconductors would indeed be operated at relatively high temperatures, the vortex lattice might never remain completely pinned against flux flow, and therefore would not transport current without loss, particularly in high magnetic fields^6. By this reasoning, if room-temperature superconductors were ever discovered, matters might only get worse.

On top of this, it was becoming clear that something beyond "shake 'n' bake" synthesis, so popular with us physicists, would be needed to turn these brittle and complex oxides, cousins to a teacup, into wires and electronic devices. Five, often six, elements were required to make some of the most promising layered copper oxides. No one had yet attempted to bring such complex materials into practical form. Many secondary phases would precipitate during the synthesis, resulting in serious degradation of the current-carrying capacity; in addition, difficulties associated with crystalline anisotropy seemed to be insurmountable. A news story in *Science* entitled "Superconductivity: is the party over?"^{7}, which dwelt on the original promises of March 1987, and the likelihood of their remaining unfulfilled, was widely noted in press, industrial and government circles. Fortunately, the party was not yet over — the band was just out on a break.

Growing up

As the 1990s began, many of the less committed workers and institutions left the high-T_c field. But others, mainly materials scientists and new companies intent on commercialization, replaced the departed comrades and started to focus on critical problems, both fundamental and applied. In the early 1990s, the band returned from its break, and the party began to swing again, albeit to a more mature beat. It was as if Nature, having chastized us for our undue haste and optimism, finally lifted her penance, bestowing in addition some remarkable gifts.

Excellent and patient efforts by materials scientists worldwide led to better understanding of phase equilibria, especially in the 'BSCCO' family, comprising $Bi_2Sr_2CaCu_2O_8$ (Bi-2212) and $Bi_2Sr_2Ca_2Cu_3O_{10}$ (Bi-2223). The substitution of small amounts of lead for bismuth was found to stabilize the 2223 phase, resulting in a bulk superconductor with a transition temperature of 110 K. Moreover, owing to the micaceous character of the BiO layers, both materials were far less brittle than might have been expected, which helped enormously in the drawing and rolling operations employed in wire manufacturing.

Perhaps even more important was the role of silver in inducing texturization in BSCCO, a process still incompletely understood. Most of the current carried by silver-sheathed BSCCO is transported by a layer a few tens of micrometres thick, adjacent to the interface. Even so, as I write, the critical current density in metre-scale lengths exceeds 55,000 A cm^{-2} at 77 K in self-field, and its sustainability at longer lengths is getting better and better.

Today, oxide-powder-in-silver-tube Bi-2223 and silver ribbon dip-coated with Bi-2212 are the mainstay products of 'Generation I' high-temperature superconducting wire in multi-kilometre lengths manufactured in the United States and Japan, and, after a late start, in Europe as well.

This wire is eminently suitable for electric power cables at 77 K, and for electromagnets, both stand-alone and in transformers and rotating machinery, at 30 K. Last year saw the public announcement of prototypes for both types of application^8, and I can predict with confidence that the next two years will see their use in industry under engineering test conditions.

Meanwhile, flux creep has not turned out to be the problem it was once thought to be. Although Generation I wire does exhibit flux creep at 77 K, at the currents and magnetic fields typical of cable applications the resultant losses are much less than in conventional metals. The same is true at 30 K in magnetic fields of several tesla. At 4.2 K (liquid-helium temperature), high-T_c superconductors can operate in persistent-current (lossless) mode up to magnetic field strengths much higher than can niobium–titanium, the present mainstay technology of superconducting magnets in high-energy particle colliders and tokomaks.

And there is better news coming. New methods of processing good-old YBCO 123 in wire form are under development^{8,9}, taking advantage of its superior performance in high magnetic field and high temperature compared to BSCCO. It is reasonable to expect this 'Generation II' technology to mature as the millennium approaches. As for electronic applications, steady progress has been made in the development of both SQUID magnetometers and radio-frequency filters, and several companies are on the verge of introducing both passive and active thin-film devices as communications and sensor products^9.

On the theoretical front, it did not long escape notice that high-temperature superconductivity in these systems invariably occurred in an insulating host crystal composed of two-dimensional sheets of antiferromagnetically coupled copper ions, and that the onset of superconductivity on doping to metallic levels of conductivity was probably not just an unrelated accident. A common feature of the 'magnetically mediated' models was the suspicion that the pairing symmetry was '*d*-wave' in nature, as opposed to the '*s*-wave' character found in phonon-coupled low-T_c materials. (It is important to note that 'plain vanilla' BCS theory can accommodate both of these symmetries, and others as well.) So, around 1992, the attention of most experimentalists wishing to concentrate on 'mechanism-determining' measurements shifted to techniques designed to uncover the symmetry of the condensate wavefunction — less difficult, it was hoped, than a frontal attack on the pairing physics itself.

Improvements in the materials science of the layered copper-oxide perovskites helped provide the high-quality thin films and single crystals that were needed to probe the symmetry of the wavefunction. Most of these experiments suggest that the condensate state has *d*-wave symmetry, but data from direct tunnelling into both crystals and films of 123 require some *s*-wave component as well. To add to the confusion, measurements of penetration depth and thermomagnetotransport on the electron-doped compounds based on Nd_2CuO_4 imply *s*-wave symmetry. As yet, phase-sensitive experiments such as those done on 123 have not been possible in this latter system.

Sometimes I am asked by my applications-oriented colleagues whether it is really important to know what the pairing physics is in detail. The answer is, "Of course! We physicists are driven to know the how of things, and we would draw immense intellectual satisfaction from solving this puzzle." But would the answer aid in applications or serve as a guide to the discovery of new materials? Probably not.

Ten years young

Well, there you have it. Ten years later, over 50,000 papers published, more than a hundred compounds discovered, some elegant science advanced, but with the mechanism of high-T_c superconductivity as yet unresolved, and applications due in the next two years — the excitement of Woodstock continues. Scientific interest remains high, attested to by the suprisingly constant number of abstracts received by the newsletter *High-T_c Update* since its beginnings in 1987, and the frequent publication of papers in scientific journals of broad readership such as *Physical Review Letters*, *Nature* and *Science*.

On the other hand, several important corporations, such as IBM, AT&T and Bellcore — vital participants in the discovery years — are no longer major players. Their place has been taken by companies, both small and large, whose core businesses are much closer to the power and communications arenas, the historically natural targets for superconductivity. Interestingly, the best bulk and thin-film samples available for fundamental investigations are now made by these companies.

Government support for superconductivity remains high, hovering at about $200 million per year in Japan, heavily oriented towards power, and $150 million per year in the United States, targeted mostly to electronics. The US national laboratories and Japan's ISTEC, which has recently been refunded for another ten years, have been crucial partners with private industry, especially in power-related developments.

President Bill Clinton's 1998 budget submission includes a 60 per cent increase in the Department of Energy's superconductivity power programme, and, arguments about 'corporate welfare' aside, I think there is a good chance that Congress will approve it. Superconductivity has enjoyed bipartisan support in the past, and the advances wrought under the department's Superconductivity Partnership Initiative with industry are now vindicating that faith. Another very encouraging development has been the rapid growth in the past two years of involvement by European industry, utilities and government in power areas of superconductivity, now estimated at $100 million per year.

One fallout from the large human and financial resources expended in the past ten years has been a level of worldwide patent activity in high-temperature superconductors rivalled only by semiconductor technology. Yet some fundamental patents have yet to be issued, primarily delayed by claims adjudication and interference proceedings. Who 'owns' high-T_c superconductivity remains to be determined. But one thing is certain: as applications emerge, there will be a lot of corporate lawyers knocking on each other's doors. You may want to follow my strategy in this regard. My wife Maria and I have two quite argumentative teenagers in our household (sound familiar?), whom we are encouraging to take their talents to law school and beyond, specializing in patent litigation. If high-temperature superconductivity ever produces substantial revenue returns, their parents' senior years will be secure. As a current Americanism for certainty puts it, "You can double-click on that icon."

Legacy for the next generation

What might our situation be ten years from now? Although another breakthrough such as we had in 1986–87 would be lovely, it is unlikely. Most probably, the layered copper-oxide perovskites are as good as we are going to get for some time. The current record transition temperature — held, quite appropriately, by Chu's institute in Texas — sits at 164 K in Hg-1223 at high pressure[10], establishing an existence proof that at least this level might be possible in some unknown ambient-pressure phase. But my own instincts are that the copper oxides have already been played out. No other material system, not even semiconductors, has received such intense scrutiny. If there were more gold to be mined, it would have been struck by now, given the talent and tools dedicated to the search over the past ten years.

Since Woodstock, three other material systems that might have qualified in the 'old days' for the appellation "high-Tc" have been found. These are the cubic perovskite $Ba_{1-x}K_xBiO_3$ (ref. 11), the alkali-metal-doped fullerenes[12] and the Pd and Ni borocarbides[13], with transition temperatures ranging from 15 to 30 K. None holds strong promise for application because of their relatively low (by present standards!) values of T_c.

The next breakthrough in superconducting materials will come, as it always has in the past, from an unexpected source, maybe in organic or biological compounds. Perhaps the discovery will not even involve superconductivity, but rather 'perfect conductivity' of an exotic nature, mediated by a soliton or charge density wave mechanism, as has been proposed from time to time. In the meantime, one important legacy of the copper-oxide perovskites, beyond giving the world high-temperature superconductivity, is the renaissance now occurring in the transition-metal oxide family, typified by the detection of new effects such as giant and colossal magnetoresistance.

It might surprise some that many of the veterans of Woodstock are still actively involved in high-T_c superconductivity. It shouldn't — most of us were working on superconductivity well before 1986, and the phenomenon is incurably addictive. Significant numbers of people, myself included, are no longer with our former institutions, having moved on to universities or to government and private agencies with a focus on superconductivity, or into new commercial ventures to capitalize on the Bednorz–Müller discovery. Over the years, despite our past and present competitive battles, we have come to form, like Henry with his nobles and archers at Agincourt, a "band of brothers", whom others envy because we were there.

Sadly, there will be no official recognition of the ten years since Woodstock at this year's March meeting in Kansas City. Perhaps this is as it should be; New York was the quintessential stage on which to play out that drama, and to celebrate anywhere else would be inappropriate. Nevertheless, a few of us old soldiers will most surely gather in some midwest waterhole to raise a cup in remembrance of 'the way it was'. Cheers. ☐

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