Superconductor Credits Bypass Alabama

The critical contributions of researchers at the University of Alabama in Huntsville have been largely ignored as attention has been focused on Paul Chu's team at the University of Houston

IT WAS THE MOST SOUGHT AFTER scientific prize of the decade—the world's first superconductor that worked at liquid-nitrogen temperatures—and Paul Chu got it. Working with his research team at the University of Houston plus collaborators from the University of Alabama in Huntsville, Chu landed the magic material just weeks ahead of his competition. That, at least, is the popular perception. Chu has since become one of the country's best known scientists and has been awarded the National Medal of Science, and the University of Houston is now a premier superconductivity research center.

The University of Alabama in Huntsville did not fare nearly so well. Yet it was the Alabama team, led by Maw-Kuen Wu, that first fabricated the superconductor and Wu only informed Chu about it after the fact. In much of what has been written about the discovery, the pivotal contribution of Wu's team has been glossed over or ignored completely.

Now the Alabama team is asking that it be given its due. The researchers say that not only have many accounts of the discovery failed to acknowledge their work but some even include factual errors that magnify Chu's role and minimize their own. A reading of the media accounts of the event confirms this. The real story of the discovery of the first liquid-nitrogen-temperature superconductor is both less tidy and more interesting than most accounts would indicate.

The events culminating in the fabrication of that first liquid-nitrogen-temperature superconductor began in January 1986. It was then that Georg Bednorz and Alex Muller of IBM's Zurich research laboratory found a lanthanum-barium-copper-oxygen compound that was superconducting—lost all resistance to the passage of an electric current—at 30 K. Although this is only 30 degrees above absolute zero, it is 7 K warmer than the previous best superconductor. The discovery hinted that superconductivity might be possible at much higher temperatures than previously thought.

Bednorz and Muller's result did not appear in print until September 1986. When it did, Chu at the University of Houston and Koichi Kitazawa at the University of Tokyo realized its implications and began to follow up on it. If materials could become superconducting at 30 K, which was warmer than many scientists thought would ever be achieved, why should they not lose resistance at 50 K or 100 K? And if materials could be found that were superconducting at 100 K, they could be cooled by liquid nitrogen—which boils at 77 K—instead of the much more expensive liquid helium. This would open up a whole world of potential superconductor applications.

Both Chu and Kitazawa were able to reproduce the original IBM results, and when they reported this at a 4 December 1986 meeting of the Materials Research Society (MRS), it set off a mad race to find superconductors that worked at even higher temperatures. Chu and Kitazawa had a head start, but it might not last.

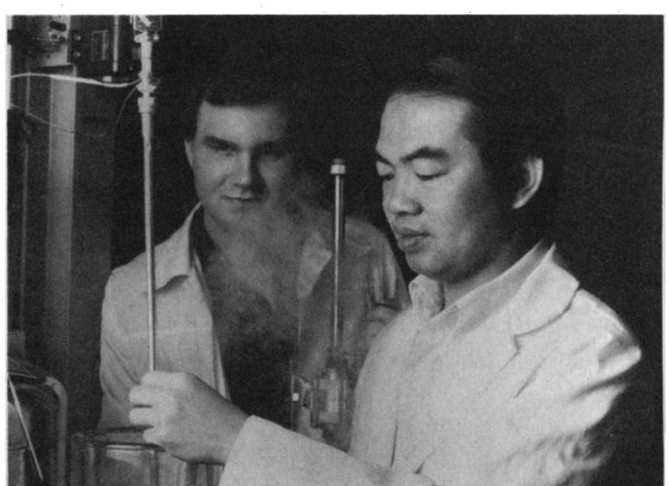
At that meeting, Chu ran into Wu, his former graduate student, and told him about the superconductivity results. The two agreed to collaborate in the search for other, higher temperature superconductors.

The next 2 months witnessed a flurry of activity in the Houston and Alabama labs, as well as other labs across the world. Among other accomplishments, the University of Alabama in Huntsville team found superconductivity at 42 K in a lanthanum-strontium-copper-oxygen compound, and the Houston group detected fleeting signs of superconducting behavior in other materials at temperatures as high as 100 K. Other labs were seeing similar results.

Convinced by these glimpses of superconductivity at temperatures over 77 K that new superconducting materials existed related to the original La-Ba-Cu-O material, Chu filed for a patent on 12 January. Although the details of the patent application have not been made public, Chu supposedly used a shotgun approach, claiming rights to a wide range of materials, none of which had shown at that time reproducible superconductivity at temperatures over 77 K. Among the various compositions listed on the patent application, Chu says, was the particular Y-Ba-Cu-O composition that 17 days later would produce the first liquid-nitrogen-temperature superconductor. The patent is still being reviewed.

On 29 January, Wu and graduate students Jim Ashburn and Chuan-Jue Torng fabricated a sample of an Y-Ba-Cu-O material that showed reproducible superconductivity at 93 K. Wu immediately called Chu to let him know, and Wu and Ashburn flew to Houston the next day to perform more sophisticated tests on the material. The results were announced at a press conference on 16 February and appeared in the 2 March issue of *Physical Review Letters*.

Chu was the senior researcher and the spokesman for the group, and when the discovery was announced he was the natural focus of attention. Chu was careful to in-



U. of Alabama in Huntsville

M.-K. Wu and Jim Ashburn *in a University of Alabama in Huntsville laboratory. (Wu is in the foreground.) Wu and Ashburn fabricated the first material to become superconducting at a temperature higher than 77 K—the temperature of liquid nitrogen—although their contribution in the discovery has often been overlooked.*

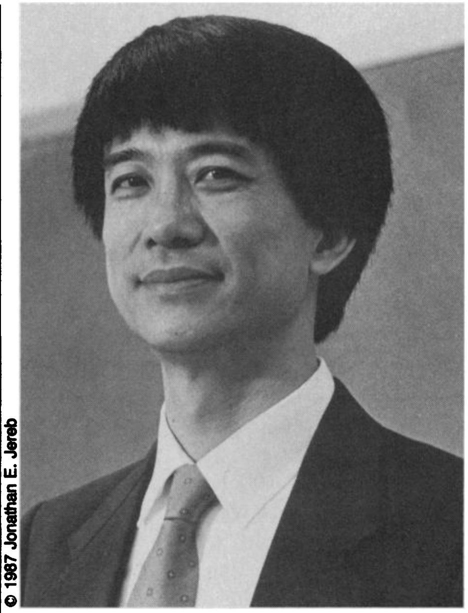
clude his collaborators whenever he spoke of the results, and Wu's name appeared first on the paper, but still Chu received the lion's share of the credit. Although the earliest press reports of the breakthrough generally included the Alabama team, after several months the account often was abbreviated to the point where Chu was referred to as the discoverer of the material and none others were mentioned.

Many superconductivity researchers seem to agree with this assignment of credit. Although they are aware that the material was first fabricated at the Alabama lab, they give Chu the major share of credit for the discovery in the belief that he was the driving force and the direction behind the efforts of both teams. Bob Hazen, the Carnegie Institution of Washington scientist who unlocked the crystalline structure of the Y-Ba-Cu-O compound for Chu, reflects this attitude when he describes Wu's team as "acting as an extension of the Houston lab."*

But Wu and his colleagues resent this interpretation and insist that their contribution was greater than just providing a few more bodies for Chu's army. "We're more than capable of doing our own research," says graduate student Ashburn, who is outspokenly irritated with what he sees as a general dismissal of his team's role. Wu, who is more restrained, says, "The contribution from both sides was equal, in my view." According to Wu's and Ashburn's accounts of the Alabama team's work, its efforts were essentially independent of Chu's direction and advice.

Those efforts began with Chu and Wu's conversation at the MRS meeting. Wu agreed to try substituting strontium for barium in the original Bednorz-Muller material, and by mid-December his team had fabricated a La-Sr-Cu-O material that was superconducting at 39 K. (Researchers at AT&T Bell Laboratories had also found superconductivity in a La-Sr-Cu-O system in December.) The Alabama team continued experimenting with the strontium system, trying different compositions and preparation techniques, through the rest of December and on into January.

During that time, Wu's group attempted a couple of other substitutions for the barium without much success. With calcium, they did get a superconductor but it had a lower critical temperature than with barium, not higher. Magnesium did not work at all. Believing that the crystalline structure of the La-Sr-Cu-O compound was the key to its superconductivity, Wu and his two graduate students discussed which substitutions



Paul Chu *won the National Medal of Science for his work on superconductors.*

might leave that structure unchanged but somehow increase the critical temperature.

These discussions spurred Ashburn to take what would be the key step in the discovery of the 92 K superconductor, a step that has been completely overlooked in most accounts of that discovery. On 17 January, Ashburn did some calculations on the effects of substituting for both lanthanum and strontium in the La-Sr-Cu-O system. Making certain assumptions about what effect different elements would have on the crystalline structure and the critical temperature, he produced a chart predicting which substitutions would produce the best superconductor. Scribbled on the back of a homework problem, that chart pointed directly to Y-Ba-Cu-O as the most likely candidate. Ashburn also calculated that the starting mixture should have composition $Y_{1.2}Ba_{0.8}CuO_4$. Wu and Ashburn decided to try it.

The Alabama group had no yttrium available, however, and Wu had to borrow some from colleagues at the Marshall Space Flight Center in Huntsville. On 28 January, the team mixed up the Y-Ba-Cu-O mixture just as Ashburn had calculated and put it in the oven to bake overnight. The next day, they pulled the mixture out of the oven and tested it. Ashburn says he was hoping the material would be superconducting at a few degrees higher than the best of the earlier compounds, which had critical temperatures just over 40 K. Instead, the mixture was magic.

"The very first sample we made, we got 90 K [critical temperature]," Wu remembers. Ashburn called a friend into the room as they were taking the resistivity measurements. "Watch," he said, "you are looking at history." After the measurements, Wu says, his hands were trembling so much he could not do anything for 30 minutes. A bit later, Wu called Chu to tell him they had made history. "We've hit the jackpot," he said.

Many of the written accounts of this discovery contain a major misconception—the idea that the Alabama group zeroed in on the magic compound through a series of experiments stretching over weeks and closely supervised by Chu. A report by *Time* in May 1987, for instance, had the first trials of the yttrium compound taking place before Chu's patent application was filed on 12 January, and spoke of Wu's team as "subjecting their creation to a series of heat and chemical treatments." Hazen's book on the superconductor discovery describes the Alabama group as spending January trying various substitutions and proportions, gradually raising the critical temperature until it "narrowed its search to a promising composition in the yttrium-barium-copper oxide system."

Yet Wu and Ashburn say the month was mostly spent looking at the strontium substitution and the yttrium success came on the first try. They were not seeing tantalizing hints of superconductivity at temperatures near 100 K, such as Chu was, and they were not getting day-to-day direction from Chu. Ashburn recalls they did have frequent phone calls with Chu, but these were mostly just sharing rumors of what other groups were doing. The widespread perception that Chu was actively overseeing the Alabama group's effort is wrong, Ashburn says.

Although Wu and Chu remain friendly, their accounts of the discovery differ somewhat in emphasis—if not in facts—and the version that has made it into the media is basically Chu's version. All along, Chu has said it was his studies of the effects of high pressures on the superconductors that led the team to substitute yttrium for the lanthanum. Since putting a superconductor in a high-pressure atmosphere often increased its critical temperature, he says, he wanted to mimic the effect of the pressure by using smaller ions in the crystalline lattice. Since yttrium is very similar to lanthanum but with a smaller atomic radius, it was the natural substitution to make, Chu says, and his lab ordered yttrium in early January.

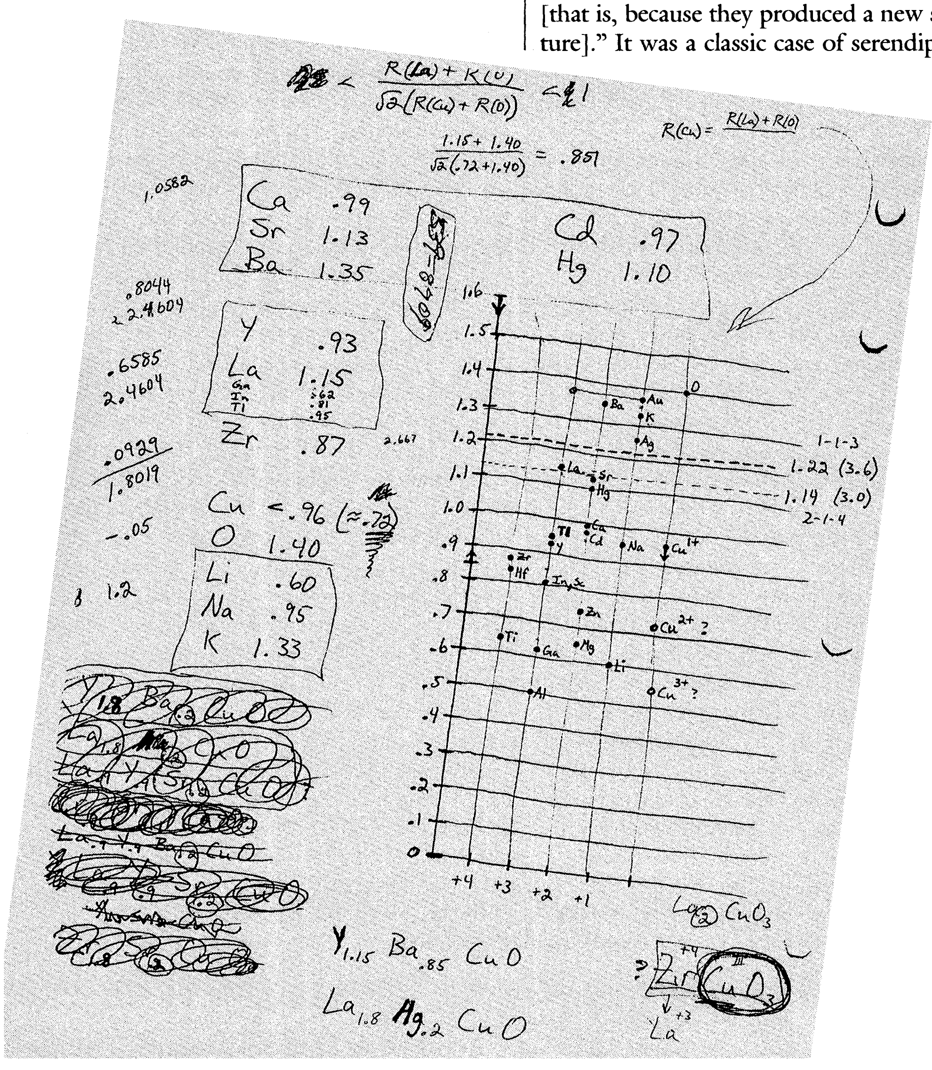
On the other hand, Wu and Ashburn say Chu's high-pressure studies had nothing to do with their own decision to try yttrium—it was due completely to Ashburn's back-of-homework calculations—and note they did not even try to get yttrium until late in January.

A scribbled chart *pointed the way to a new high-temperature superconductor.* ⟶

**The Breakthrough: The Race for the Superconductor*, Bob Hazen, Summit Books, New York. 1988.

In response, Chu says that his and Wu's teams have different "perspectives" on the decision to try yttrium. He notes that he and Wu spoke of yttrium as a potential substitution as early as late December (a point on which Wu agrees), and Chu says he showed Wu the patent application filed on 12 January which included the yttrium mixture. Chu also says Wu and Ashburn took part in a conversation with him and other Houston researchers in early January where the yttrium substitution was talked about.

"If that conversation took place, it was irrelevant to what took place later," Ashburn says. The Alabama team decided to look at yttrium for totally independent reasons, and Chu was not even aware that Wu's team was working with yttrium until the good-news phone call on 29 January, Wu and Ashburn say. All the earlier discussions of yttrium were basically irrelevant to the Alabama team, the two say, because yttrium was just one in a range of elements that were natural candidates for substitution into the existing superconductor formula and yttrium was never singled out from other possibilities. Wu does credit Chu's high-pressure studies for suggesting the idea of making substitutions in the superconductor recipe, particularly the successful strontium substitution, but not for the yttrium substitution.



Chu, on the other hand, says his high-pressure results did point toward yttrium (as well as other elements), and he says his lab actually tried yttrium in January as part of a carefully planned substitution program. In one of those twists of fate that can make all the difference in history, Chu had used inexperienced undergraduates to mix up the yttrium-containing compounds, and those batches showed no superconductivity. Since the undergraduates' batches of La-Ba-Cu-O—the mixture used in the original Bednorz-Muller experiments—did not work either, Chu had decided they were doing something wrong and had planned to assign a research associate to redo the substitutions. If the University of Alabama in Huntsville group had not found the yttrium material, Chu says, the Houston group probably would have soon.

One irony of the discovery is that although both Chu and Wu had separate reasons for substituting yttrium, both reasons were in fact wrong. Chu and Wu were both trying to reproduce the structure of the original La-Ba-Cu-O compound with slightly different components, but the material they stumbled upon had a completely unexpected structure. As Ashburn puts it, "Everything happened just as it was planned, but for a totally unforeseen reason [that is, because they produced a new structure]." It was a classic case of serendipity.

How should one apportion credit in a discovery such as this one? Paul Grant, a superconductivity researcher at the IBM Almaden Research Center, suggests that one should apply the management exercise of asking: Would this result have happened if X had not been involved? "It never would have happened if Paul Chu had not been the central figure controlling it," Grant says, although he admits most of his information about the details surrounding the discovery comes from Chu. Since several other labs were close to finding the same superconductor, it is likely the discovery would have happened somewhere else if it were not for Chu, he says.

But whatever portion of credit Chu or Wu may deserve professionally, Chu certainly received most of the media attention, and Wu got very little. There are several possible reasons for this. One is simply that the press finds it easier to focus on one scientist, and Chu is the obvious choice—he is senior to Wu, he is more comfortable speaking in front of an audience, and he is slightly more accessible simply because it is easier to get to Houston than to Huntsville. Wu also seems to have been reluctant to push himself in front of the spotlight, perhaps because he was hesitant to compete with his old teacher. Graeme Duthie, head of the physics department at the University of Alabama, says, "To a certain extent, the university did not pursue the publicity due to Dr. Wu's concern about his relationship with Dr. Chu." The University of Houston press office had no such concerns, on the other hand, and did a good job publicizing Chu's role. "As a result," Duthie says, "Houston got everything and we got very little."

A year and a half after the discovery, Chu dislikes the controversy over who deserves what credit. He and Wu had agreed to work together, he says, and no matter which lab actually found the material, both would have shared the discovery equally. "Even if we got it, they would get the credit," he says.

Wu has reaped some rewards from his part in the work, for he has landed a tenured full professor position at Columbia University—a nice step up from being an untenured assistant professor at the University of Alabama in Huntsville, the position he held before the discovery. (The University of Alabama in Huntsville promoted Wu to tenured full professor after the discovery.) Still it rankles some that when most people recall the discovery of the first truly high-temperature superconductor, the names that come to mind are Paul Chu and the University of Houston, and not M.-K. Wu and the University of Alabama in Huntsville.

■ **Robert Pool**