Fifty Years of Superconductivity —and The Next Fifty Too

David C. Larbalestier

Abstract—**John Hulm made major contributions to the field of superconductivity as an academic scientist, applied scientist, later as manager at Westinghouse Central Research Laboratory of one of the largest industrial research programs in the field, and finally as a senior statesman of superconductivity. A brief memoir of my interactions as a younger generation scientist is presented. I was particularly impressed by his belief that development of new and better superconductors in practical forms underpins the development of superconducting technology.**

Index Terms—**Superconducting materials and applications.**

ALTHOUGH I was neither a personal friend nor a direct scientific collaborator of John Hulm, occasional but always significant interactions with him greatly helped my career over the period from about 1975 to about 1995. We both worked on materials which he had pioneered, the A15 compounds and the BCC alloys of Nb-Ti. Meeting or talking with John was always positive, especially valuable for a young scientist trying to define his own path amidst the deep tracks made by its leaders. John was clearly one such leader—a most enjoyable aspect of this invitation to speak at the John Hulm symposium at the 2004 Applied Superconductivity Conference (ASC) is how warming it has been to remember him.

I got to know John Hulm first when he was the Scientific Attaché at the London Embassy of the United States in the period 1975–1976. At this stage Roger Boom was trying to persuade me to leave the Rutherford Laboratory and to become an Assistant Professor at the University of Wisconsin. A green card was an essential component of the move and travel documents. John's first vital role in my career was to ensure that our family paperwork came together in one place at the US embassy. With our son Nik born in Switzerland and our daughter Laura born in Oxford, our paperwork floated between a Swiss desk and the British desk. John's vital role was to unite the files! We got our green cards in about 4 months! John's wonderful memoir, "Superconductivity Research in the Good Old Days" at the 1982 Applied Superconductivity Conference briefly touches on this "vital" work, as well as many other much more significant experiences with Brian Pippard and Bernd Matthias [1]. Not touched on except in some after-hours stories are other of his "vital" London experiences—for example as a bagman taking out the cash for frequent visiting congressional parties. But he also took the pulse of British science by visiting many laboratories and talking with directly with many younger scientists like myself, encouraging them to come to or to visit the USA. Both my wife Karen and I are very grateful for his friendly support in helping us come to the USA, an adventure that has been personally and professionally rewarding for almost 30 years.

Manuscript received October 5, 2004.

The author is with the Applied Superconductivity Center, the Department of Materials Science and Engineering and the Department of Physics at the University of Wisconsin, Madison, WI 53706 USA (e-mail: larbalestier@engr.wisc.edu).

Digital Object Identifier 10.1109/TASC.2005.849688

My first Applied Superconductivity Conference was the 1974 meeting at Oakbrook in the Chicago suburbs. Charlie Laverick, one of Argonne National Laboratory's north-country Englishmen, Bruce Strauss and Paul Reardon, then at Fermilab, were the organizers. Bob Rose, Bruce's former thesis adviser, confided to me that ASC was rapidly going down hill—holding an ASC in a shopping center was apparently a big let down after Annapolis in 1972. But Charlie and Paul and Bruce provided a serious quantity of receptions and superfluids (fine wine, beer and spirits) to assuage these concerns. Ted Geballe and John Hulm had an invited talk at the meeting that I reread for this symposium. Many of the key points made in that paper remain valid today. Their paper, "Superconducting Materials up to Now and into the Future" [2], addresses the science of raising T_c and H_{c2} in the A15 compounds, and then transitions into discussion of the properties of composite conductors, paying particular attention to the evolution of Nb_3Sn conductors from Kunzler's Nb tube with internal Nb and Sn powder, through the RCA and GE tape conductors to the filamentary possibilities presented by the use of the bronze process due to Tachikawa and to Howlett. I was particularly excited by this view because I had come to Oakbrook with results on real Nb_3Sn magnet conductors, that is diffusion-barrier-protected copper containing multifilaments, made in our Harwell-Rutherford Laboratory collaboration [3] with which we had been able to generate more than 12 tesla fields in some small but still useful solenoid magnets [4]. I was convinced that filamentary Nb_3Sn was going to make big inroads into high field magnet technology and very encouraged that Geballe and Hulm agreed. Today I am even more pleased that filamentary Nb_3Sn indeed succeeded!

When I went to Madison in 1976, I was preoccupied with filamentary Nb_3Sn. Fortunately Roger Boom encouraged me to work on Nb-Ti too. I was aware of Hulm and Blaugher's classic paper on transition-metal superconductivity [5], but unaware that the particular design of the Union Jack was responsible for study of Nb-Ti [6]! Although its T_c and H_{c2} are only half those of Nb_3Sn, Nb-Ti's uses have remained enormously larger simply because it is ductile, easy to fabricate in high quality, and economical. The superconducting magnetic energy storage (SMES) project that drove much work in Madison was constrained economically in much the same way that HTS applications are constrained today—namely that the conductor should be of maximum performance and lowest cost. Roger

Boom urged me to understand Nb-Ti and to remove "black magic" that controlled the processing of Nb-Ti at that time. Several years of wonderful experiments followed before we thoroughly discovered how to reliably get very large current densities [7]. In early 1983, mid-way through this excitement, I was invited to Poland and was delighted to find that John was invited too. This special symposium celebrated the 100th anniversary of the liquefaction of oxygen in Krakow by the Polish patriots, Wroblewski and Olszewski. They had taken their knowledge of gaseous liquefaction learned in Pictet's laboratory in Paris determined to first liquefy oxygen in Poland, so aso to strike a blow for Polish culture which was suffering one of its periodic occupations by Russia. In 1983 Polish nationalism was represented by *Solidarity*, which had been suppressed only a few months before and historical resonances of occupation were all around. Wroblewski and Olzsweski, having united briefly in 1883, however soon fought a duel, fortunately not fatal. We all walked over to the cemetery to see that their newly restored graves were far apart. We reflected on this careful respect to their historical relationships with appropriate appreciation of the irony of the situation. In this extraordinary environment I had many stimulating discussions, both scientific and general that cemented my high regard for John. And, consistent with his generous and broad-minded support for young scientists [1], John encouraged the visit of more than one young Pole to the US by contacts made at this time.

My next significant interactions were in the spring of 1987 when the fever of high temperature superconductivity (HTS) was sweeping the globe. Superconductivity had been on the front page of every conceivable publication and expectation was palpable that room temperature superconductivity was imminent. The Superconducting Super Collider (SSC), many miles of Nb-Ti magnets, was getting ready for project approval. Optimism was so high for HTS that a strong movement to build the SSC using HTS magnets developed. At this point the National Academy charged John with chairing a panel under the Committee on Science and Public Policy (COSEPUP) to prepare the "*Briefing on High Temperature Superconductivity*" [8]. The panel was charged both to consider the great surprise of the discovery and to make a preliminary assessment of its technological impact. The committee was well balanced between fundamental and applied scientists but of course this very balance led to intense debate. What emerged was a wise document whose balance and good sense is evident even today. Some key points were: *Short term problems and long term opportunities are both easily underestimated; Science and technology are strongly intertwined, requiring simultaneous progress in basic science, processing science, and applications: Take a long term, multidisciplinary view; The pre-commercial applications of HTS to new markets will extend for a decade or more*.

In reflecting on John Hulm's contributions to the science and technology of superconductivity, these wise words, much influenced by John, still echo strongly. What happens to our field in the next 50 years will come from the broad soup of Nb-base superconductors, HTS, perhaps MgB_2, and as yet unknown superconductors. Major HTS applications in the power industry are possible but only if conductors become very cost effective. I am struck today by the possibility that high field applications of Nb_3Sn may be superseded either by Bi-2212 given the recent 25 T superconducting magnet [9] or by MgB_2, which offers the possibility of $>$30 T operation [10]. In short superconductivity is still full of opportunity. Our symposium "*Fifty years of Superconductivity—and the next 50 too!*", emphasizes John Hulm's vision. John spans the period of superconductivity-only-as-science to super-conductivity-as-technology. He discovered high-T_c behavior at $\sim$17 K in the A15 compounds while teaching at Chicago. When high field superconductivity suddenly became real in 1961, John was at Westinghouse, ready to lead one of the premier industrial research labs toward applications. For about 25 years his lab at Westinghouse played an enormous role. Now a different generation and a very different mix of industry, universities and national laboratories must take up the challenge for the next 50 years. The words of John Hulm's COSEPUP panel are an excellent charge for this goal. Many thanks, John, for your cheerful, warm and insightful encouragement!

REFERENCES

[1] J. Hulm, "Superconductivity research in the good old days," *IEEE Trans. MAG-19*, pp. 161–166, 1983.

[2] T. Geballe and J. Hulm, "Superconducting materials up to now and into the future," *IEEE Trans MAG-11*, pp. 119–124, 1975.

[3] D. C. Larbalestier, P. E. Madsen, J. A. Lee, M. N. Wilson, and J. P. Charlesworth, "Multifilamentary niobium tin magnet conductors," *IEEE Trans. Magn.*, vol. 11, pp. 247–250, 1975.

[4] D. C. Larbalestier, V. W. Edwards, J. A. Lee, C. A. Scott, and M. N. Wilson, "Multifilamentary niobium tin solenoids," *IEEE Trans. Magn.*, vol. 11, pp. 555–558, 1975.

[5] J. Hulm and R. Blaugher, "Superconducting solid solution alloys of the transition elements," *Phys. Rev.*, vol. 123, pp. 1569–1581, 1961.

[6] R. Blaugher.. [Online]http://www.ascinc.org/asc04/johnHulm.htm

[7] D. C. Larbalestier *et al.*, "High critical current densities in industrial scale composites made from high homogeneity Nb 46.5 Ti," *IEEE Trans. Magn.*, vol. 21, pp. 269–272, 1985.

[8] J. Hulm *et al.*, "Report of the Research Briefing Panel on High Temperature Superconductivity," National Academy of Sciences Press, Washington DC, 1987.

[9] H. W. Weijers *et al.*, "The generation of 25.05 T using a 5.11 T $Bi_2Sr_2CaCu_2O_x$superconducting insert magnet," *Supercond. Sci. Technol.*, vol. 17, pp. 636–644, 2004.

[10] V. Braccini *et al.*, "High-field superconductivity in alloyed MgB_2thin films," *Phys. Rev. B*, vol. 71, 2005. 012504.