Superconductivity at Westinghouse

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***Abstract* - We present personal reminiscences and a few highlights of activities in superconductivity at the Westinghouse Electric Corporation. These activities started in early 1950s and continued until the end of the company's involvement in manufacturing of electrical and electronic equipment. The initial fundamental studies gave way gradually to R&D into materials, conductors, electric machinery, fusion magnets and superconducting digital and analog electronics. Of many achievements we highlight here only some of the fundamental contributions, the synthesis of then highest critical temperature (T_c) material, Nb_3Ge, the R&D into electric power generators, the fusion large coil project and the successes in digital electronics, low- and high critical temperature, T_c. We acknowledge the guiding role and contributions of the late eminent Westinghouse scientist, John K. Hulm.**

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I. INTRODUCTION

During more than half a century after the World War II (WWII), and especially in the earlier decades of that period, many prominent industrial laboratories worldwide significantly contributed to progress in fundamental and applied superconductivity. In United States of America (USA), the most prominent achievements were certainly those of Bell Telephone Laboratories, and IBM Research, closely followed by those of General Electric (GE) and Westinghouse (**W**). Also other industrial laboratories made important contributions, for example the Ford Motor Company Scientific Laboratory, where quantum interference was discovered and SQUIDs invented in 1963-64.

Our intent in writing the present paper is to highlight some of the work done and contributions made at the Westinghouse R&D Center[1] often in collaboration with one or more

[1] Located until almost the end of last century at Churchill Borough of Pittsburgh, Pennsylvania, 15235, USA.

of the **W** manufacturing divisions. The anniversary of the discovery of superconductivity (100 years) prompts us to offer subjective and personal reminiscences of events which often led to results at the leading edge of science and technology.

Immediately after WWII and in the first decade after the launching of “Sputnik” by the Soviet Union (1957), the (then) **W** Research Laboratories enjoyed the climate of academic research associated with large freedom of pursuit of individual research interests. That “golden age” was presided over by Clarence Zener (1905-1993), a prominent scientist himself interested in superconductivity. Coming from the University of Chicago, he became the director of the Laboratories in 1951 and in the coming few years assembled a team of young and talented researchers, many of whom left a mark on our field. In 1954 he hired the late John K. Hulm, who was then an Assistant Professor at Chicago and with his doctoral student George Hardy just discovered a new class of superconducting intermetallic binary compounds, the A15 (1953). John promptly became the leader of **W** labs superconductivity effort. His direct or indirect leadership extended over nearly four decades and was always beneficial, in spite of varying fortunes of this effort and its changing profile.

The “golden age” did not extend much beyond mid-1960s. Zener left in 1965, and his successors, William Shoupp and later George Mechlin had to respond to the mounting **W** headquarters pressure to deliver new products and short-term developments offering direct tangible support to manufacturing divisions. Such work had to be directly ordered and eventually financed by one or more such divisions. Accordingly, the **W** Research Laboratories were soon to become the **W** R&D Center. More fundamental or exploratory work was still possible, but only after securing external funding by one or another U.S. Government or international agency. In superconductivity, the internal corporate support was directed predominantly towards large-scale applications, especially when cost-sharing with a government agency was possible. From the early 1980s on, emphasis shifted from large-scale applications to electronic technology and its applications. It remained so until the gradual dismembering of Westinghouse, which began with the sale of all electronic activities to the Northrop Grumman Corporation devoted essentially to military avionics. There, some of the superconducting electronics work continues until present.

In the following, we intersperse several individually authored personal reminiscences with sections highlighting the few *selected* major areas of work and achievement. These sections represent our joint interpretation of the past events. The whole paper is not a methodical record or chronicle of all past events, successes and occasional failures. Rather, our intention has been to reproduce the flavor of these times, the team spirit and joy at being part of the effort, including personal and somewhat autobiographical accounts that show the reader the multifaceted nature of the subject as well as the diversity of those who served it.

II. REMINISCENCE ON THE EARLY DAYS (1955 - 1963)

B. S. Chandrasekhar

A. Early Work on Materials

I joined the **W** labs in mid-1955. Zener also acted as Manager of one Department after another successively. I believe that the idea was that he could thus inject new life, with people as also with programmes, into the Department. He was in Metallurgy when I arrived, and so he put me in it. I wondered how I, a pure physicist, would fare in the midst of what I vaguely thought of as blacksmiths and ironmongers. All the low temperature people were in the Low Temperature Physics Department under the direction of Aaron Wexler[2]. To name just some of them: John Hulm, Mike Garfunkel, Cam Satterthwaite, Peter Chester, Bruce Goodman, John Rayne and Ray Bowers.

It took me just a few days, as I got to know my new Metallurgy colleagues and also the working atmosphere of the Labs, to realize that my concern about being isolated in Metallurgy was unfounded. I learnt about phase diagrams, solidification, magnetic materials, X-ray crystallography, single crystals, and hosts of other fascinating things, very quickly. My rapid change of field is seen from the following: in 1955 I published with K. Mendelssohn a paper on sub-critical flow in the helium II film, and in 1956 with Hiroshi Sato a paper, inspired by a theory proposed by Zener, on the second ferromagnetic anisotropy constant of iron.

John Hulm had a strong interest in seeking new superconductors and establishing empirical rules to help in the search. This was an interest that he shared with his friend Bernd Matthias, who was then at Bell Telephone Laboratories. John and I were in different departments, but we very soon got to know each other personally as well as scientifically. The possibility for such interactions was a splendid feature of Westinghouse that I enjoyed often, and I think that the credit goes to Clarence Zener for creating such an atmosphere. So a chance for collaborating with Hulm occurred soon, in1957, as follows.

The metallurgists working at Westinghouse's reactor division were developing binary cubic alloys of gamma-uranium with molybdenum and niobium. The point was that alpha-uranium, the room-temperature phase, is orthorhombic and when used in the reactor core undergoes severe deformation. The hope was that a cubic material might ameliorate the problem. Hulm got hold of samples from the reactor people, and suggested that we get together and measure them down to liquid helium temperatures. Why did he not go to one of the low temperature people in his own department? My guess is that they were more interested in looking at the basic properties of elemental superconductors like specific heat, thermal conductivity and so on, and might look on Hulm's proposal as "physics of dirt," to adapt Wolfgang Pauli's famous remark.

Anyway, Hulm and I did the measurements and published the results in 1958 [1]. The electrical resistivities were high and rose monotonically down to helium temperatures, and all the alloys became superconducting at temperatures between one and three kelvin, depending upon the composition. This represented the first report on superconductivity in cubic uranium. We did propose an explanation of the remarkable temperature dependence of the resistivity as due to a high and rapidly varying density of states at the Fermi surface, but a detailed theoretical explanation has yet to appear. John Hulm and Bruce Goodman some time later reported on the transition temperatures and critical fields of rhenium, ruthenium, and osmium [2].

Meanwhile, elsewhere in the same laboratory, some basic properties of the simple elemental superconductors: aluminium, indium, vanadium, and tin were being measured. These measurements produced some of the earliest convincing evidence for an energy gap in the electronic excitation spectrum of a superconductor, as required by the BCS theory. A comprehensive review of the work was published in 1958 by Biondi, Forrester, Garfunkel, and Satterthwaite [3]. This paper describes their work on microwave absorption in aluminium and tin, and also covers the work of Corak, Goodman, Satterthwaite and Wexler [4] and of Corak and Satterthwaite [5] respectively showing the exponential temperature dependence of

[2] It was Wexler who invented the liquid helium storage tank, an important early development that fostered developments in superconductivity.

the electronic specific heats of vanadium and tin. Bruce Goodman was the first to show from his measurement of the thermal conductivity of superconducting tin [6] that there was an energy gap. This was his doctoral work at the Cavendish Laboratory under David Shoenberg, before he came to Westinghouse. I note that John Hulm also got his doctorate from Cambridge under Shoenberg. So it was perhaps no accident that Bruce came to Westinghouse, and work on the energy gap took off there.

I digress here to make a couple of observations. There were not many low temperature labs in the fifties: the Collins liquefier had just appeared. The Westinghouse liquefier carried the serial number 2, and the Urbana machine was serial number 7. The second International Low Temperature Physics Conference (LT2) at Oxford in 1951 had fewer than 200 participants. The superconductivity community was a compact one. Kurt Mendelssohn (doctoral supervisor of Bowers and me) was a good friend of David Shoenberg (ditto of Hulm and Goodman) and Bernard Serin (ditto of Garfunkel). Everybody knew everybody, as it were. My other observation concerns research with special reference to physicists in industrial research labs in those days. A recurring topic for discussion was basic versus applied research. Was one superior to the other? Sides were taken, and positions defended and attacked vigorously. At Westinghouse, there were those who measured thermodynamic properties and others who searched empirically for new superconductors with higher transition temperatures. Though there was a gulf, if shallow, between the two groups, there were some like Goodman and me with a foot in each camp. I remember Zener saying, with his characteristic smile, that the distinction was not between pure and applied research, but rather between good and bad research. That is also my view.

B. High Field Superconductors

In the next couple of years, I worked alone and sometimes with John Rayne or James Bardeen, doing experiments on thermoelectric effects (Westinghouse was interested in thermoelectric generators), elastic constants of single crystals, paramagnetism, cyclotron resonance. In 1961 I was invited by Bryan Coles to come to Imperial College, London, for six months as a visiting fellow. Westinghouse said OK, and said that they would make up the difference in salary. Ah, the golden age of industrial research! I went to London. I was there when the Bell Labs paper by Gene Kunzler and colleagues on Nb_3Sn that remained superconducting at over 8 tesla while carrying a significant current appeared [7]. The prospect for practical superconducting magnets was no longer a dream and life was not to be the same after that. It is interesting that there followed increased activity in the area of alloy superconductors, theory as well as experiment, though nothing on the scale of what followed, 25 years later, after the announcement of high-temperature superconductors. Within a year or two after the Bell Labs paper, superconducting magnets were commercially available. It has taken somewhat longer for practical applications of high-temperature superconductors to materialise. I returned from Imperial College to Westinghouse late in 1961. Hulm had set up a cryophysics section in the physics department and was heading it and staffing it from within and without. Its mission was to do basic research on high field superconductors and development work on superconducting magnets, and interact with a manufacturing division which would produce such magnets. Among the people in it then or some time later were Hank Riemersma, Adolphus Patterson, Stefan Wipf, George Cunningham, Tony Venturino, Paul Steve, Dick Blaugher, Martin Lubell, Mike Walker. One day soon after my return from London, Hulm called me into his office and asked me to take over from him the management of the cryophysics group. I said yes, and did it till I left two years later to join Western Reserve University, which is now Case Western Reserve University.

C. High Field Magnets and Expected Field Limits

The years 1961-1963 were a time of intense and exciting activity for me at Westinghouse. My research covered a broad spectrum from fundamental superconductivity, critical currents and fields in wires of alloy superconductors, flux jumps as a cause of degradation (which was what we called the fact that the critical current in a magnet was less than in a short sample of wire), all the way to winding a magnet with niobium-zirconium wire of two compositions to reach 68 kilogauss which was a record at the time. We found, as did other workers in the field, that copper-clad wire performed better in magnets than simply insulated wire. Our publications on these subjects not cited in the text are listed in the Addendum.

I mention a couple of episodes as illustrative of how things were then. Soon after I became section manager, Hulm asked me to write up a justification of why we wanted to pursue superconductivity research; I think he wanted to present it to the corporate top brass. I wrote up a page or so about how much we needed to know about superconducting alloys, critical currents, and critical fields, in order to end up with magnets of hitherto undreamt-of strengths. One question was how the critical field went with decreasing temperatures. There was no theory at that time, and experiments showed the critical field rising linearly with decreasing temperature, quickly reaching about 5 tesla. I recall one optimistic extrapolation suggesting 70 tesla at 0 K for V_3Ga. James Bardeen, a summer intern, and I had recently been looking at the paramagnetism of some uranium alloys, and so I had thought a bit about the response of electron spins to magnetic fields. I must have had an "Aha!" moment: that a strong enough magnetic field should break the pairing of two electrons with opposite spin, flipping one of them and thus destroying the superconductivity. I recall quickly looking up the numbers and concluding that the pair-breaking field numerically in tesla would be about twice the transition temperature in kelvin. This result, if correct, would be a bit of a damper on some of the more exuberant guesses about critical fields that were then current. I immediately wrote up "A Note on the Maximum Critical Field of High Field Superconductors" and gave it to Hulm for his review, because he was the expert in the subject and I only a beginner. He seemed not to be persuaded by my reasoning. Anyway, he was about to leave for a visit to Ted Geballe at Stanford, and said he would get back to me on his return. And he did, looking rather excited. Geballe had mentioned to him something that Al Clogston at Bell Labs told him about there being a limiting field due to electron spins! So Hulm asked me to send the paper out at once. I had recently received a notification about a new journal called *Applied Physics Letters* inviting contributions. Since my paper, I thought, was of interest to people making wires for superconducting magnets, I sent it there. It appeared a month later [8], but not before my friend Ted Berlincourt had phoned me to say that he was the referee, congratulated me and expressed his chagrin that he had not thought of it! A month later appeared Clogston's version of the same idea in *Physical Review Letters*. In today's atmosphere both papers would have been submitted to *Nature* or *Science*, of course. Different times, different drummers.

Soon after I became section manager, I heard that Cliff Jones, who had joined another group in the Labs, might be interested in a switch. I had known him as a research student at Imperial College, and thought that he would be a great addition to our group. We arranged the switch, and I was right. One of the first things he did was to set up a pulsed field magnet which took us up to 18 tesla, and measured the critical fields of the alloys to the lowest temperatures [9,10]. He continued to be a dynamic member of the group.

D. What Made Westinghouse Special

Some final reflections: the atmosphere at Westinghouse Research Labs during my time there, 1955-1963, was most conducive to both learning and research. This was because of the personalities of the leaders like Zener and Hulm, and the calibre of the colleagues that Zener had assembled around him, and the wonderful technical and library staff. Departmental boundaries were not allowed to inhibit collaborations, and the sense of community was excellent. A special feature of the Labs was the broad spectrum of expertise in experimental techniques, theory, materials, and other fields that were available and easily accessible. This was the strength of the great industrial research laboratories of that period that exist no more.

III. THIN FILMS AND THE Nb_3Ge SYNTHESIS

J.R. Gavaler

A. How Did We Come to Thin Films

John Hulm hired me into his superconductivity group in 1964. My previous experience at Westinghouse had been in the Semiconductor Department where I was primarily involved in the preparation and evaluation of thin films. John's main interests were searching for new superconductors and developing high-field superconducting magnets. In 1961 workers at the Bell Labs built a magnet using Nb_3Sn - the first magnet made with a high-T_c compound [7]. At that time "high-T_c" meant ~15 K and above. They solved the problem of brittleness by winding their magnet with wire whose core was filled with Nb and Sn powders. The magnet was then heated to form the compound. My first project was collaborative with Stan Autler and Pat Patterson to determine whether a thin film of Nb_3Sn might be flexible enough to be wound into a magnet without degrading its superconducting properties. Because the 3/1 A15 phase in the Nb-Sn system is the only stable phase at high temperature, it was possible to use a simple preparation method. Niobium ribbon was passed through a molten tin bath and the coated ribbon was then sent through a 900^{o}C furnace to form a ~5 micron thick film of Nb_3Sn. We wound and tested (Figure 1) small test magnet that achieved a field very close to that predicted from short sample measurements [11].

In 1960, following his discovery of Nb_3Sn, Matthias moved his record for T_c another couple of degrees higher with the discovery of alloys of Nb-Al and Nb-Al-Ge that became superconducting at ~20 K. In the late sixties, news came out that an organic compound had been found that showed a sharp decrease in resistance at about 40K. The decrease was attributed to superconductivity, even though the resistance never went to zero. Brookhaven Laboratory sponsored a conference to discuss this result and other current research directed toward finding new higher temperature materials. I went along with Hulm to the conference and there, because of their close friendship, I had the opportunity to meet Bernd Matthias, the discoverer of Nb_3Sn. Matthias was a fascinating fellow. In addition to being an outstanding scientist, he was charismatic, an entertaining speaker, highly opinionated, outspoken, often right, sometimes wrong, but never boring. Over breakfast he was in top form, claiming that the upcoming

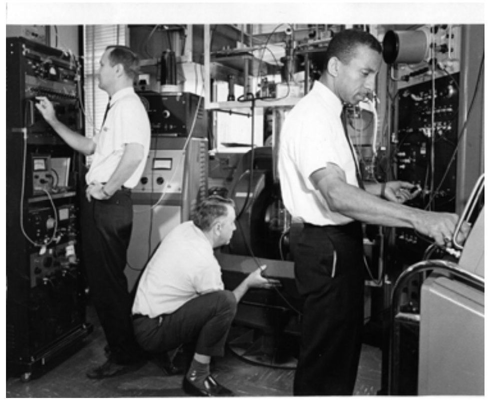

Fig. 1. Testing of the Nb_3Sn tape magnet. From left to right, John Gavaler, Gus Foley, Pat Patterson (the only photo I saved from these days).

conference on organic "superconductivity" would be the first time that a conference was ever held on a non-existent subject. In his talk later that morning he indulged in what was one of his favorite pastimes – ragging his theorist friends about their uselessness as far as providing any guidance in the search for new superconductors. He specifically singled out the Nobel Prize winning BCS Theory in that regard, all but suggesting that the prize should have gone to an experimenalist. When Bob Schrieffer (the "S" of BCS) arose to speak, people were eager to hear his response. It consisted of two words. But those two words came only after what seemed to be a pointless story about a girl and a horseman. His punchline was, "Nobody knew'er but the…" he paused and then finished in a louder voice, "horseman knew'er". It took awhile but it finally dawned that "horse manure" and "horseman knew'er" have exactly the same sound. Nobel Prize winning physicists sometime have very interesting senses of humor.

We never got involved with organic superconductivity at Westinghouse but Hulm was intrigued by, what the authors dubbed, the "pairing across a barrier mechanism". Workers at Brookhaven reported that a sandwich structure consisting of a dielectric between two Al films had a T_c of ~5 K, four times higher than that of bulk Al. They speculated that a similar structure using Nb might have a T_c as high as 35 K. Our intention was to first reproduce the Al results with Re (T_c ~2 K) and then with Nb (T_c ~9 K). To set a baseline, films of Re and Nb were deposited in what was then a standard evaporation system, which had a background pressure of ~10^{-6} torr[3]. The resulting Re film had a T_c of ~7 K and the Nb film ~5 K. I was convinced that background gaseous impurities had contaminated the films and changed their properties. To prepare high purity films they would have to be deposited in an ultra-high vacuum environment. Instead of evaporation, I thought sputtering would be a better deposition method. With sputtering, high-melting-temperature materials could be deposited without the high temperatures needed for evaporation. In addition, sputtering has the advantage of top down rather than bottom up deposition. At that time sputtering had a reputation of being a "dirty" deposition method and was not often used. I believed the reputation was undeserved and was due to sputtering typically being done in systems having high levels of gaseous impurities.

B. Thin Film Sputtering In Ultra-high Vacuum; Niobium Nitride Films

[3] As this is a historical account, we use units then most common.

Cliff Jones was my manager at the time. I told him I'd like to try depositing superconducting films using a high-purity sputtering method. I can still remember his great response. "That's a wonderful idea, John. By all means, go ahead and do it", and then after a half beat pause he added, "What the hell is sputtering?" Since nothing resembling the deposition system I had in mind was commercially available, we had to build one. To my great good fortune, Bill Lang and Jack Singleton were doing their pioneering work on the science and technology of ultra-high vacuums on the floor above us. Leaning heavily on their expertise and with a significant contribution from Mike Janocko, a system was built that could reproducibly achieve vacuums of $5x10^{-10}$ torr or less [12]. Sputtering Re and Nb produced films that from both superconductivity and x-ray measurements had the same properties as the pure bulk materials. As one might expect, I felt both pleased and relieved. Hulm gave the new system his tacit seal of approval by making it one of his stops when giving visitors a tour of the department. Another satisfying aspect: the results demonstrated sputtering was, in fact, an excellent method for depositing high-purity films. In years following, ultra-high vacuum sputtering became and continues to be a prime technique used to deposit films in the semiconductor industry. By the time our new system became operative, it had been generally concluded that the enhanced T_c's of the Al sandwich structures were due to the formation of amorphous Al and not to any new mechanism. So work in that area was discontinued. However, now we had a unique tool capable of depositing high-purity films of all the high-T_c superconductors – something that had not yet been done. NbN films were the first deposited. They had T_c's of ~15K - the same as that of the bulk material. There had been theoretical speculation that an enhancement in T_c could be obtained by reducing the grain size of a superconductor to ~100 angstroms. We prepared NbN films with thicknesses less than 100 angstroms and saw no enhancement. Although there was no dimensional effect on T_c there was a strong one on critical current density $J_c(B)$ values and on upper critical fields. Films less than 10 nm thick had current densities at 4.2K of $J_c \approx 2x10^6$ amps/cm^2. One of these films, formed into a microbridge, had a record high value of $3x10^7$ amps/cm^2. At the low deposition temperatures used (~500 ^oC) there was no lateral grain growth. This produced films with vertical columns separated by voids**.** Films less than 10 nm thick had extremely high upper critical fields, in some case as high as 45 tesla (extrapolated to $T = 0$), which was then another new record [13].

C. Record-high T_c in Nb_3Ge Films

Among all the A15 structure compounds surveyed, the most intriguing was Nb_3Ge. Bulk Nb_3Ge was reported to have a T_c of only ~7 K. However, x-ray analysis indicated that its composition was actually $Nb_{3.3}Ge$. Matthias *et. al*., using a very fast quenching technique, prepared bulk samples whose composition was closer to the ideal 3/1 stoichiometry. The onset of superconductivity in these samples was ~17 K [14]. Nb-Ge films were sputtered in our system using a composite target, half Nb and half Ge. By positioning a series of substrates beneath this target, films with a wide range of compositions could be deposited. The highest T_c obtained in initial experiments was ~17 K. However, when the sputtering gas pressure was increased to 0.3 Torr, an order of magnitude greater than that typically used, films with T_c's of ~23 K were obtained. X-ray analysis of the films showed a lattice parameter of 5.15 angstroms, the value predicted for stoichiometric Nb_3Ge. The critical importance of a high sputtering gas pressure is explainable by the fact that, at low pressures, particles ejected from the target fly off in straight lines and impact the substrate with high energy. These particles apparently prevented the formation of the relatively unstable stoichiometric Nb_3Ge compound. When a much higher pressure was used, the ejected particles experienced multiple collisions with gas atoms and thus moved diffusively to the substrate with energy insufficient to inhibit the growth of the desired 3/1 A-15 phase.

I wrote a paper covering the Nb_3Ge results and submitted it to Applied Physics Letters [15]. I also sent an abstract to a conference being held in Gatlinburg, TN, later that summer. Coincidently, the APL paper was scheduled to come out the week of the conference. A few days before leaving for Gatlinburg, I mailed preprints to some friends and, as a courtesy, I also sent one to Lou Testardi at the Bell Labs, whom I didn't know well, but who was chairing my session. After presenting my results, I sat down ready to listen to the remainder of the program.

At this point things took a wildly unexpected turn. Instead of introducing the next speaker, Testardi began giving a talk of his own. He announced that he had also successfully sputtered Nb_3Ge films and that they had record high T_c's, similar to mine. I was dumbfounded. After receiving my reprint, he had, (as I later learned) worked day and night and had reproduced my results. Then using his role as chairman he had inserted himself into the program right after my talk. As things turned out, although that little incident in Gatlinburg was a bit disquieting at the time, there were actually no significant or lasting consequences. I still got to have my fifteen minutes of fame along with all the little perks that went with it.

After the successful preparation of Nb_3Ge films, there was an excellent empirical reason to believe that stoichiometric A15 structure Nb_3Si, if it could be prepared, would have an even higher T_c. This set off a race among several laboratories to be the first to do this. It never happened. In one laboratory, the desire for a positive result was apparently so intense that the workers succumbed to the temptation of manufacturing data - claiming they had, in fact, deposited Nb_3Si films with T_c's of ~25 K. When they presented their results at a conference, Alex Braginski of our group took on the uncomfortable but necessary chore of pointing out that these results were not credible and explaining why. The only response was embarrassed silence. After this and several other false alarms, when I heard the rumor that two Swiss scientists had discovered a metallic oxide having a T_c of 35 K my immediate reaction was total disbelief. But this time, as I soon learned, the "holy grail" had indeed been found. Later, when YBCO was reported to have a T_c of over 90 K, the frenzy was fully on. We, along with many other laboratories, were soon successful in preparing thin films of YBCO thus permitting the start of a multi-pronged investigation of their potential use in a variety of electronic applications. This part of the story will be picked up in later sections.

IV. APPLICATIONS OF SUPERCONDUCTIVITY– AN OVERVIEW

The first Westinghouse foray into applications of superconductivity, in the early 1960s, followed immediately the events and work evoked in Section II: **W** developed small superconducting magnets for NMR spectrometers. These magnets, initially wound with NbZr wire, were marketed for some years using NbTi wire under the trade name of HI120 (a reference to the H_{c2} of NbTi at 4.2 K) and, together with their power supplies, were probably first products on the US market. One of us (AIB) recalls seeing one of these magnets still in operation at an NMR lab in the past decade. The speed with which **W** developed the first superconducting product had its root in the origins of the Corporation in East Pittsburgh, with the creation of a lab for the quality control of incoming materials, which grew greatly with **W** involvement in the Manhattan Project in purifying uranium. When the transition metal alloys were systematically explored by, among others, John Hulm and Dick Blaugher, who were first to report on these [16][4], **W** was well prepared to exploit the situation.

This short-lived attempt to serve the scientific instrument market was ill-conceived, perhaps because such a small business was totally alien to a large multinational corporation accustomed to working with big customers like national governments and electric utilities. The magnet venture was unprofitable and was soon wound down. None of the big companies, Westinghouse, RCA[5], *etc*, made a small superconductor-related business work at the time, with the exception of GE who spun off IGC[6].

About a decade later, Westinghouse got involved in technically successful developments of large superconducting (SC) apparatus for central station generators, transformers and homopolar motors for ship propulsion. By the mid-1970s, involvement in DOE programs on controlled thermonuclear fusion and especially on superconducting magnets for magnetic fusion reactors followed. Still later, in the early 1980s, **W** got involved in superconducting electronics, just about the time IBM terminated their Josephson Computer project. The following sections contain technical highlights and some reminiscences on a few of these developments.

V. SUPERCONDUCTING ROTATING MACHINES

A. The Beginnings

The first report on a study of superconducting alternators was by Woodson, Stekly *et al.* in the mid 1960s [17]. It stimulated the work by J. Smith *et al*. at the Massachusetts Institute of Technology (MIT) in the late 1960s. They successfully demonstrated a small vertical shaft alternator with a superconducting field winding and room-temperature armature [18]. John Mole from the **W** Electro-Mechanical Division (EMD) spent some sabbatical time at MIT and became familiar with Joe Smith's group work. As the EMD primary role was to manufacture medium-size generators and motors (50MVA machines typically), they became interested and so the first Westinghouse program was instigated by the **W** Power Systems Group, the original core of the Westinghouse Corporation. A published concise summary of the **W** generator work is included in [19]. We briefly summarize here the work on superconducting generators, both utility-type and airborne, which was conducted primarily at the R&D Center. The work on motors for ship propulsion is included here only by reference [20,21,22].

B. The 5 MVA Utility Generator

This first program's objective was to demonstrate a prototype superconducting generator that would be representative of large "power system" utility generators. It was performed in

[4] Early investigations on these alloys were also reported soon thereafter by Ted Berlincourt and Dick Hake of Atomics International.

[5] The Radio Corporation of America (RCA) was first to develop Nb_3Sn tape conductors deposited by CVD - for winding magnets.

[6] InterMagnetics General (IGC), produced various research magnets and in 1980s grew under the leadership of Carl Rosner to become an extremely successful MRI magnet company, recently sold to Philips for well over US$ 1 billion; see RN22.

collaboration between EMD and the R&D Center. A two-pole, 3600 rpm, 5 MVA superconducting synchronous generator was designed, built and successfully tested in 1972 [23]. The generator just after the test is shown, with part of the design and test team, in Figure 2. This 10-day continuous operation test provided the first proof of feasibility for SC generators in application to power systems.

The **W** design followed the earlier MIT designs in using a rotating SC field winding cooled with liquid helium supplied by a bayonet and rotating face seal assembly. Among the key design features we mention here the use of: (a) a "flooded" SC winding that allowed close thermal contact of the liquid helium with the SC winding and (b) thin wall torque tubes to isolate the low-temperature region from the ambient temperature. The drive end torque tube, radiation shields, and field excitation leads were cooled by counterflowing the helium exhaust gas to reduce the heat leak into the winding containment structure. A radial spoke arrangement was used on the non-driven end to support the winding structure and radiation shields and provide the ability to accommodate the relative thermal contraction from the cryogenic inner structure to the ambient outer shells. More detailed descriptions are given mostly in difficult to retrieve IEEE papers [24].

Fig. 2. The 5MVA Superconducting Generator after successful test with some of the EMD and laboratory design and test team alongside, left to right: front row - Jim Parker, Don Litz, Adolphus Patterson, Cliff Jones and Tom Fagan; standing - John Mole, Henry Haller and Mike Walker.

C. High-Speed Four-Pole USAF Generator

Nearly a year before the completion of the 5 MVA tests, **W** was funded by the Wright-Patterson Aero Propulsion Laboratory of the United States Air Force (USAF) to develop a high-speed, light-weight, SC generator for military application. This program was divided into three phases with the first phase providing the construction and test of a complete SC rotor typical of the advanced generator final design. The design approach for the USAF rotor proposed a four-pole winding configuration operating at 12,000 rpm providing an effective power of 5 MVA at 400 Hz. The overall mechanical and cryogenic design was similar to the 2-pole Westinghouse 5 MVA machine: (a) the use of radial spokes to accommodate the thermal contraction, (b) cold electromagnetic shields, (c) high thermal contact for the SC winding by forced convection helium and the use of the exhaust helium gas to cool the thin wall torque tube, (d) radiation shields, and (e) concentric power leads. A prototype SC rotor following this design was built and successfully tested in Jan 1974. Details of the rotor design, construction, and testing can be found in [25,26].

Following completion of the USAF prototype 5 MVA rotor tests, a detailed review of the rotor design and cold-running tests has shown that some major design changes were needed for the final generator. The changes were implemented, the generator construction completed in early-1977, and following warm spin-tests, cool down tests were initiated. Due to a problem in maintaining an acceptable vacuum within the rotor below 10^{-3} torr, the rotational cold tests were aborted. A subsequent static cool-down of the rotor in liquid helium showed the SC winding and all internal leads and interconnects were satisfactory and the field winding was excited to currents consistent with design objectives. Further attempts to resolve the vacuum issue were not explored due to funding constraints for both the Air Force and Westinghouse. The completed generator, shown in Figure 3 mounted on the test stand, achieved a total weight of 1066 pounds, a significant "light-weight" accomplishment of below 1.0 lbs/kVA if the rotational tests could have been completed.

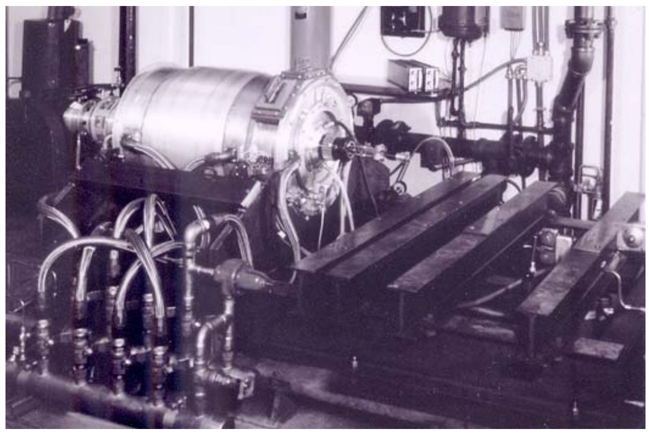

Fig. 3. The Westinghouse/Air Force "10 MVA" Generator installed on the test stand.

D. Westinghouse-EPRI 300 MVA Program

The early success of the MIT machine and subsequent construction and test of the **W** 5 MVA generator immediately caught the attention of the entire world and precipitated major research programs at a number of the large industrial laboratories in the United States, Europe, Japan, and the former USSR. Within the next few years, major development programs on SC synchronous machines were under way at General Electric, Siemens, Brown-Boveri, Alsthom-Atlantique, Mitsubishi, Fuji, Hitachi, Toshiba, and the former Soviet-Union principally at the Glebov Institute and Kharkov Institute. The review papers by Smith *et al*.,[27] Smith,[28] and Edmonds [29] provide excellent discussions of this period, which covers about 10 years. During this decade, a number of machines were built and successfully tested. These tests demonstrated that ac superconducting machines could be built at large sizes suitable for electric utility installation. Our 12,000-rpm SC four-pole rotor test for USAF demonstrated through the high centrifugal loads, that larger diameter machines with ratings near 1000 MVA, could be constructed and operated with liquid helium [25].

The prospect for building much larger synchronous generators offered by the superconducting technology appeared as an immediate opportunity for the electric power industry. As a result, in 1975, the Electric Power Research Institute (EPRI) contracted Westinghouse and General Electric to carry out conceptual design studies on 300 MVA and 1200 MVA superconducting generators. These design studies focused on using a "conventional" two-pole, 3600 rpm approach with the primary objective of a detailed preliminary design for a 300 MVA SC generator that could be scaled to 1200 MVA.

Following these design studies, EPRI, in 1979, awarded Westinghouse a jointly funded program to design and construct a 300 MVA generator incorporating a SC field winding. The key objectives for the 300 MVA program were the demonstration of a SC generator with a reduced size and weight, improved efficiency, and reliability and cost comparable with conventional generators. The key design features for the 300 MVA generator followed earlier designs with the use of cold and warm electromagnetic shields; the use of copper stabilized Nb-Ti conductor for the field winding with the conductor wedged into a slotted non-magnetic rotor forging consistent with conventional generator winding practice; and a helium management system that permitted the helium to be in close contact with the Nb-Ti conductor. A cross-section of the 300 MVA rotor shown in Figure 5 illustrates these design approaches.

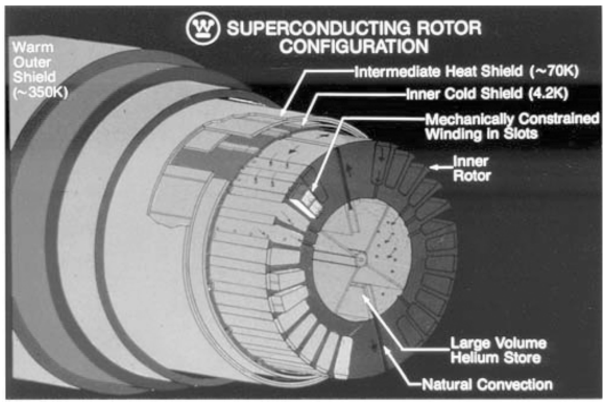

Fig. 4. Cross-section of the 300 MVA rotor showing the electromagnetic shields and helium cooling system.

A major departure from earlier SC machine designs occurred with respect to the stator design. Westinghouse planned to use a novel spiral pancake design that offered the capability for much higher stator voltages approaching transmission levels. This ambitious program continued successfully up to 1983, at which point the program was terminated by joint agreement between EPRI and Westinghouse. The major factor for stopping the effort was based on the world market conditions for power equipment that reduced the ability of both partners to continue financing this program.

At the conclusion of the program significant progress had been made on resolving key issues for the construction and operation of the complete generator. Construction of many of the long lead items was well under way with no technical barriers or problems identified at that time. A summary of the analytical and modeling studies, construction efforts and experiments carried out up to 1983 are reviewed in references [30,31,32]. The SC generator R&D at Westinghouse essentially ended with the conclusion of the 300 MVA EPRI program.

VI. REMINISCENCES ON THE MIDDLE YEARS (1961 – 1975)

M. S. Walker

A. The Work Climate, and the First "Boom" Period

Essentially all of my fourteen years at **W** were spent in collaborative efforts. I remember fondly the personalities involved and the synergy of our interactions.

When I joined the labs in the fall of 1961 an effort was already underway to make long lengths of NbZr wire towards the construction of the first 100 kilogauss (10 T) magnet, in competition with the GE people who were pursuing Nb_3Sn as their preferred material. I was assigned to Mac Fraser in Metallurgy (under Fred Werner who worked for Jim Bechtold, Hulm's counterpart under Zener) and was doing experiments on flux pinning and the anisotropy in critical current, J_c, produced by rolling to flatten the conductor [33, 34]. I also spent time in the physics group (that included Cliff Jones, Howard Coffey, Stefan Wipf and Marty Lubell) under the tutelage initially of Adolphus (Pat) Patterson, to set up efficient wire testing in support of production.

Pat was a mainstay; quietly competent, some half-dozen years earlier the first black engineering graduate from Carnegie Tech. With his degree in electrical engineering, he had begun applying for work at Westinghouse, and finally after several years made his way in as an electrician, probably the first black engineer to join the research labs. He was a welcoming colleague. We rafted the Youghiogheny River together, and it was Pat who later for many years hosted a summer picnic at his farm out in the country for those of us involved in superconductor project, their families, and other friends and family.

I was a "wet-behind-the-ears" metallurgist, just out of MIT. I remember the atmosphere: Clarence Zener, ***the*** Clarence Zener, would sit down at my table for lunch with six or eight of us (it seemed he chose tables at random) and expound on the advantages of multiple layers in the design of battleship armor or, later, on the prospects of power generation from temperature differences between the surface and somewhat lower regions in the sea. Or, instead, up in the labs over lunch Marty Lubell would play separate games of chess against several of us, all at the same time.

But the more relaxed overall ambiance conducive to science and creativity belied a fierce competition to exploit a potential technological breakthrough in superconductivity. The first goal was to reach 100kG and create a position for a high-field research magnet and instrumentation business, and whatever might follow. Mac Fraser was in charge of NbZr wire development and manufacture, with John Hulm personally expediting the effort; seeing that billets were flown to extruders and back to our wire drawing operation in the nuclear materials group in Blairsville, east of Pittsburgh.

NbZr was very difficult to work with; hard and tough, tearing up the drawing dies and often breaking in the drawing process. Fortunately, Bill Reynolds, also in Metallurgy, was working on NbTi wire. Down in metallurgy it was known as "Reynolds Wrap". It was marketed then as HI120, and later in multifilamentary form came to be the workhorse of the industry. The latter is still today essentially the wire used in almost all large scale superconductor applications from the high energy physics particle accelerators to the magnet windings in MRI medical diagnostic imaging systems.

However, back then there was a problem. Magnets made of monofilament wire would go "normal" (lose superconductivity) well short of their anticipated fields and currents as a result of "flux jumps", sudden releases of small amounts of energy as persistent currents induced within the wires by field changes would suddenly die away. At low temperatures these energies were sufficient to raise the wire temperature enough to significantly lower the J_c. The better the wire, the worse the flux jump.

It was on the flux jumps that John Hulm and I began to work closely together. I had begun to study this problem. By then I was located in the Magnetics department under George Wiener. John would come down into the lab when he could during the day and occasionally on an evening. I was single at the time and often worked at night, impatient about getting results. John called it "Experimental Therapy", a chance for him to get away from the desk. There was a joy and camaraderie in shared discovery. We would sit together watching the scope, voltages from pickup coils wrapped around or near superconducting wires on which we changed a magnetic field, a series of evenly-spaced blips on the screen. We tried to figure out what they were and eventually related them to the sequential periodic collapse of the induced currents [35,36].

John loved to tell jokes; some of them off-color. For the latter he would get a sort of naughty twinkle in his eye as he told them. Some jokes he told many times. Even bad ones: "Tantalum; Tant tell anyone," was one of those. Part of the entertainment was watching John tell the joke.

B. The First "Bust" Period, and More Basic Work on Materials

The "bust" came with a change of economic circumstances within the company and the failure of the thrust to create a superconducting magnet and instrumentation business. There was a drastic downsizing in the overall superconductivity effort, with the departure of many of our colleagues for greener fields. And there was a brief return to more fundamental research among others on semiconducting superconductors.

Fortunately I was hidden during the downsizing, at the time embarked on Westinghouse's generous Solid State Physics Fellowship work/study Ph.D. program at Carnegie Tech (now Carnegie Mellon University). As part of the work, Hulm sent me to Bell Labs to copy and re-create a pulsed calorimeter that had been developed by Morin and Maita, this for the measurement of the critical temperature and specific heat properties of Ti, Zr, and V nitrides and carbides, in search of a better understanding of the application of the BCS theory and in search of superconductors that would operate at higher temperatures than Nb_3Sn. Neal Pessall (in Metallurgy) made the alloys, I made the measurements, and the three of us collaborated to analyze, report and project next steps. Together we managed to create and examine some of the higher T_c materials of that time [37].

C. Start of the Second "Boom" Period: Design and Test of the 5MVA Generator

By the time that I returned to active duty at Westinghouse (after finishing my doctoral dissertation) in 1970, a team had been assembled from various divisions to build and test the world's largest superconducting generator until that time, the 5MVA generator described in Section V. The various efforts involving superconductivity from the R&D Center had been consolidated into the Cryogenics and Superconductivity group under the management of Jim Parker, with Cliff Jones leading parts of the effort and otherwise spearheading much of our marketing to secure government funding, a role that Cliff took on with his typical zeal. It was a good place to be. Jim was always careful in his management of people, concerned for their welfare as the group went through its various expansions and contractions, and he was also involved technically in various aspects of machine design.

The overall 5-MVA machine was designed mainly by John Mole's group from the Electro-Mechanical Division, now located on-site at the labs, including Don Litz and Henry Haller, also with help from Tom Fagan of R & D, with input from the rest of us in the Cryogenics and Superconductivity group. I specified and contracted for the manufacture of the conductor under Bruce Zeitlin's direction at Airco. Pat Patterson wound the conductor into the rotor with the help of Paul Steve. It was the most advanced superconductor of its time, essentially a prototype for conductors used in magnets for the Fermilab Tevatron and, to this day, MRI medical diagnostic imaging systems.

There were a few anxious moments during component testing and assembly of the 5MVA machine. We waited a bit nervously at the last stages of rotor construction. The tubular structural stainless steel outer shell would be heated for expansion and then dropped red-hot to shrink tightly into place over the cool rotor winding (which stood on-end, with all of its intricate instrumentation leads and plumbing properly tucked away). If the rotor missed its mark or contacted, cooled and shrunk too early, or stopped short of fully seating over mating interior structural parts (and the superconductor winding), the only recourse would be to cut the assembly apart and salvage.

Cliff was in charge of the testing of the machine, and he drew on many of the experienced experimentalists from Parker's group to staff the test. Patterson set up the drive motor, installed the test bed and generally put the wiring and plumbing into place. Mike Janocko was placed in charge of the external liquid helium supply system. I was assigned to handle the instrumentation and eventually served as foreman for the test, running three shifts around the clock. We achieved rated current at low voltage and rated voltage while drawing low current, all that we could muster with the generator drive power available at the labs, all that had been planned, and a clear demonstration of success. Figure 2, presented in Section V, shows some members of the EMD and laboratory design and test team alongside the 5MVA generator just after this test.

D. AC Losses

With the generator, another aspect of conductor design had come into play; "ac losses", heat generated because of alternating magnetic fields from the stator that under certain operating conditions would be experienced on the superconducting field winding of the rotor. This was particularly important, because heat at the approximately 4.2K temperature typical of operation would require for removal a refrigeration power of as much as 1,000 times the actual ac loss rate; a major factor affecting net electrical power delivered, the design of damper shields to reduce the ac magnetic field, and the design of cooling systems to remove the losses.

By the time of the design for the 5-MVA generator, good ac loss theory had been developed by groups at the Rutherford Laboratory in England and at the Brookhaven National Laboratory in the U.S. But compact winding required that the conductor be somewhat flattened to a rectangular cross section. Other projects (to be described later) would require conductors with highly resistive material in the "matrix" around the filaments, in addition to the copper. Theory would need to be modified and extended to accommodate new geometries, matrix arrangements, and operating regimes. And we would have to build apparatus to test the theory and our designs.

About the time of the 5-MVA, Jim Carr had begun working independently on a new theory of ac losses (with funding that John Mole managed to acquire from the Navy; most of our work on losses was through this or otherwise supported by the Navy). Starting from first principles (as it was his way), Jim put together, through an elegant derivation involving a transformation from the spiral path of the filaments to Cartesian coordinates for the overall wire, a mathematical description of ac losses in superconductors that not only accurately described ac losses in conductors of circular cross section, but was readily capable of expansion and adaptation to other more complicated composite mixed-matrix wire geometries. Jim, John Murpy and I worked to better define the parameters involved and the two of them, Jim mentoring John, extended the theory to various conductor geometries and operating regimes. Eventually Jim summarized his work on this "Anisotropic Continuum Model" in the book on ac losses published in 1983 [38].

I was more of a practitioner. I led the experimental work and application of the theory to the design of conductor for various applications. Most of the loss measurements over the years were made with the help of Jim Uphoff using a helium-boil-off calorimeter designed and built by Yu Wen in the early days of the 5-MVA project [39]. Murphy also helped with the measurements. Dan Deis and Mahendra Mather sometimes got involved on losses, mainly through complementary magnetic measurements using a Foner vibrating magnetometer. George Wagner took over my role when I left Westinghouse in 1976. Altogether, we examined a very wide range of conductor types under all sorts of practical alternating or changing field conditions for a range of applications and collectively published nearly two dozen papers, only a few of which are cited here [40, 41, 42, 43, 44].

E. Test of the Rotor for the Air Force High Speed

The same test bed and much of the same test team subsequently was used to test the rotor of the high-speed 5 MVA generator. The complete machine was designed mainly by Jack McCabria of the Aerospace Electrical Division with the management at R&D of Dick Blaugher. Dick had been a researcher in the early days (with Hulm) of transition metal alloys, now returned after serving for many years at the electronics division in Baltimore. (It is still interesting to me that Dick raced his Ford Mustang in his spare time, and that Mike Janocko, always intrigued with precision mechanical devices - as was apparent from his lab - served on occasion as Dick's pit crew.)

A technically-based summary of the high speed generator project has already been provided in Section V. What I remember is the somewhat relieved and approving nods when the rotor was first taken up to speed, safely and stably passing through those rpm's at which unfavorable resonances might be expected, with their attendant vibrations that shook the test bed. At its rated speed of 12,000 rpm the rotor screamed like a banshee. We had heavily sandbagged the static cover over the rotor, lest it breaks loose, but the test, although a bit frightening, went smoothly. However, Dick recalls being deserted by the rest of us in the test crew when it came time to ramp up for the 13,500 rpm overspeed test. He and Dan Deis, who happened to be passing by at the time, completed that test alone. Figure 3 shows the completed generator at Wright-Patterson Air Force Base.

F. Nuclear Fusion

Soon after testing of the 5 MVA rotor, we began a further collaboration with the Nuclear Division people under the direction of Zalmon Shapiro. Our first project in the fusion arena was to develop a superconducting 300 kJ energy storage coil for the Department of Energy, to evaluate superconducting coils for the accumulation and then rapid-discharge of energy. This was needed for the "Theta Pinch" fusion concept being explored at Los Alamos National Laboratory. The project was once again an R&D and EMD joint effort, and John Mole led the project with help from Ed Mullan.

Our approach relied heavily on an open, helium-saturated winding of multiple twisted insulated mixed-matrix multifilament NbTi wires (though eventually all-copper matrix wires were used for this first test). Don Litz was responsible for the mechanical design, I led the conductor design and Phil Eckles did the cryogenic design, which included analysis of heat transfer and bubble clearing from the cables based on my calculation of the expected field-change-induced losses. Mike Janocko set up for and performed all of the cabling of the conductor and winding of the coil. Of the three competing designs, ours was the only one that successfully tested well beyond rating to an impressive 540kJ of energy storage [45].

This was only the beginning. We had soon embarked on the design of tokomak reactors and were beginning to design the large "D"-shaped coils for what would become the Large Coil Program at Oak Ridge National Laboratories described in Section VII.

G. My Last Year at Westinghouse and Work by Colleagues on Transformers

During my last year literally big things towards power generation and transmission were in the works, as described in Subsection F above and in Sections V.C and VII. Dan Deis was working with Jim Carr on the design, construction and test of many small superconducting transformer coils that would lead to a major Westinghouse design project (that including Jim Carr and Hank Riemersma) of a 1,000 MVA superconducting transformer. This design was never published outside of the company.

I, meanwhile, at Cliff Jones' direction was travelling every two to three weeks to the Oak Ridge National Laboratory to lend a hand on the fusion effort underway down there (described in Section VII) with plans in place to move down and work there for a year. I was also providing expertise and measurement support on losses for Westinghouse as a subcontractor to Intermagnetics General Corporation (IGC) in Albany, NY, on an Air Force Manufacturing Technology program towards the design and development of an ultra-fine filament Nb_3Sn conductor for the low-loss pulsed energization of a field winding for a high speed airborne generator. I left Westinghouse in January 1976 to lead this project at IGC, at that time the largest conductor development project underway in the United States.

VII. NUCLEAR FUSION: THE LARGE COIL TASK

Following earlier fusion-related work evoked in Sections VI.F and VI.G, we embarked upon the Large Coil Task (LCT, also known as Large Coil Program, LCP)[7]. It was an International Energy Agency (IEA) collaboration of the US, EURATOM, Japan and Switzerland to develop and test large superconducting magnet coils for Tokamak-type fusion reactors [46]. In 1977, three of the six coils were contracted by DOE to General Dynamics, GE and **W**; the other three came from overseas. Each D-shape coil had a specified core bore 2.5 x 3.5 m and was to achieve the 8T peak field by following one of several alternative design approaches. The assembly of all six coils was successfully tested at the Oak Ridge National Laboratory (ONRL) in a special facility known as IFSMTF[8] [47].

The Westinghouse coil was one of the three using supercritical forced LHe flow through the conduit containing the cabled conductor[9], but the only one using Nb_3Sn rather than NbTi superconductor. While all six coils reached and exceeded the design goals, the W coil distinguished itself by its excellent stability acknowledged by the ORNL test team [48].

The **W** program was managed by the Advanced Energy Systems Division, AESD, with the R&D Center support on SC magnet and conductor development. The decision to employ

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[7] We gratefully acknowledge input to this section by former **W** colleagues; Carl Heyne and Sharad Singh.

[8] The International Fusion Superconducting Magnet Test Facility.

[9] The forced cooling system thermal design drew on the expertise **W** acquired in the NERVA-ROVER program for a nuclear powered propulsion system for interplanetary travel (eventually cancelled by the US Nixon Administration).

Nb_3Sn as the conductor material required that the brittle nature of the A15 superconductor be considered, which resulted in the use of small wires with the superconductor centered in the middle of the conductor to reduce strain. Airco teamed with Westinghouse to develop and supply the conductor, under the leadership of Phil Sanger of Airco. The conductor strand comprised nearly three thousand 3.5 micron niobium filaments imbedded in a bronze core. The tantalum sheathed bronze core was centered in a copper conductor to form a 0.7 mm diameter strand. The strands were cabled in successive triplets and subcables until a 486 strand, fully transposed cable was formed.

The cable was encased in a stainless steel sheath using tube mill technology to form the final conductor. Its conceptual design is described in [49]. This conductor design required a compaction ratio that would allow super critical helium flow (at 15 atm) while providing sufficient strength to avoid additional compaction caused by Lorenz forces. The conductor ac losses were tested as were the pressure drop characteristics. The conductor final design employed a 40% void fraction. Tests were conducted to determine the degradation in conductor characteristics as a function of strain, which determined the minimum bend radii in the coil winding. The need to provide sufficient void fraction in the conductor to improve conductor stability required that the structural design of the magnet reduce compressive stress on the conductor. This was accomplished by employing a plate type structure with machined slots. The number of conductors in a single slot varied from two in the high field region to six in the lower field regions of the coil. The conductor sections were joined using a specifically developed flash welding procedure that was accomplished in the header region of the coil. The conductor joint was prepared before reaction to form the Nb_3Sn superconductor by achieving a 100% compaction of the cabled conductor, which was inserted into a copper tube. Following reaction, the flash welding formed an electrical joint with surprisingly good resistance values. The helium was introduced into the conductors using an insulated helium feed tube that employed an electrically insulating glass micarta tube with cryogenic fittings to connect the individual conductor headers to the helium supply plenum. The coil was built at the Large Rotating Apparatus Division (LRAD) and then sent to ORNL for testing. The Westinghouse coil proved to be the best performing coil of the test array. Consequently, the **W** design philosophy was adopted for advanced coils for follow-on tokamak reactors and specifically for ITER.

VIII. MORE ON MATERIALS - THIN FILMS BY OTHER METHODS

A.I. Braginski

A. Chemical Vapor Deposition of Nb_3Ge

John Gavaler's success in attaining the world record in critical temperature at to 23 K had quite a resonance in the R&D Center, and even the popular press at large, and so for the first time I became interested in superconductivity. Soon thereafter, Cliff Jones suggested I might want to synthesize Nb_3Ge by chemical vapor deposition (CVD) more suitable for manufacturing of practical conductors. In the magnetics group, which I was leading at the time, we worked among others on the CVD of ferromagnetic ferrite and garnet films. I gladly accepted Cliff's suggestion and initially started helping a talented chemist, George Roland of the Crystal Growth Department, with whom I already collaborated on another topic and who now obtained the task of synthesizing Nb_3Ge using CVD. On a rather short notice our work was successful [50]. Eventually, I transferred to the Superconductivity Department then lead by Jim Parker and became responsible for the materials R&D Section.

To make a long story short, by 1976 we had the Nb_3Ge conductor on thin Hastelloy tape substrate, with the Nb_3Ge layer deposited by a reel-to-reel process from chloride precursors. The critical temperature onsets were also up to 23 K, but obtaining a homogeneous A15 phase by CVD was rather difficult and the upper critical field was significantly lower than in sputtered samples. With the continuing support or AFOSR[10] we conducted a systematic comparative investigation of Nb_3Ge prepared by sputtering and by CVD on different substrates. Also many other laboratories reproduced John's results by yet different methods such as electron beam evaporation, for example. The body of results published on Nb_3Ge films was growing fast. By 1976, we reached some level of understanding of phase relationships and electromagnetic properties and I could present an invited talk at the Applied Superconductivity Conference [51]. From then on the work became two-pronged. On the scientific side we investigated the stabilization of the metastable Nb_3Ge phase by controlled oxygen doping first observed by John [52,53] and also by epitaxy. In the latter we followed Dayem *et al.*, who stabilized Nb_3Ge by depositing it on Nb_3Ir single crystals [54].

In the second prong, to pursue the technology of practical Nb_3Ge conductors, I secured a Department of Energy (DOE) project on developing Nb_3Ge tapes conductors for power transmission lines. This was carried out for a while, but soon the DOE interest in power transmission lines declined. We thus stopped this work without regrets.

B. The "MBE" Growth of Thin Films and Tunneling Trilayers

While the study of Nb_3Ge stabilization brought us to epitaxy of thin superconductor films, by the end of 1970s we also became increasingly interested in Josephson tunnel junctions made of refractory, higher-T_c materials, especially nitride and A15 tunneling structures. My intense lobbying of our upper management was surprisingly effective and in early 1980s we got a new laboratory with a brand-new and quite expensive toy: the probably first ever "MBE"[11] ultra-high vacuum deposition system for refractive superconductors with diverse material sources such as multi-beam electron gun sources, effusion cells and magnetron sputtering heads, plus surface cleaning guns, and structure and composition analysis tools such as RHEED, LEED, XPS and AES[12] [55], see Figure 5. Equally important, a new member of our group, John Talvacchio, joined us after graduating from Stanford. He soon became a leading contributor to our new effort.

The new toy kept us busy and rather productive until the news of the discovery of true high-T_c materials, the cuprates, spread in the late 1986. Until then we were publishing quite a bit on epitaxial films and junctions made of of Nb, NbN and A15 trilayers, especially Nb_3Sn and Nb_3Ge with artificial and epitaxial barriers (see references in [55]). While I see this work as the precursor of today's efforts, for example to fabricate junctions with barriers with suppressed two-level defect concentration for qubits, I have to admit that in terms of any practical technological advance our return on the investment made by Westinghouse was next to none: our epitaxial trilayer junctions have been invariably of lower quality than the polycrystalline devices. During the following high-T_c frenzy (1987/1988), we also attempted to harness our "MBE" for the task of synthesizing REBCO films[13]. Now, almost a quarter of

[10] Acronym of the Air Force Office of Scientific Research. John's discovery was made under AFOSR sponsorship; our program director was the unforgettable Max Swerdlow.

[11] MBE – Molecular beam epitaxy, in this case somewhat a misnomer.

[12] RHEED – reflection high-energy electron diffraction, LEED – low energy electron diffraction, XPS – x-ray photoelectron spectroscopy, AES – Auger electron spectroscopy.

[13] REBCO – rare earth barium cuprate.

century later, some researchers still pin their hopes to synthesize new high-T_c materials and multilayers by atomic layer-by-layer MBE [56].

Fig. 5. The "MBE" system in use (ca. 1984). From left to right, John Gavaler, Alex Braginski, John Talvacchio and Mike Janocko [55]. Reproduced with permission by Elsevier.

IX. THE ADVENT OF HIGH-T_c CUPRATES

A.I. Braginski

As most of the superconductivity researchers worldwide, I was unaware of the Bednorz and Müller discovery of superconductivity above 30 K in lanthanum barium cuprate [57] until the news came out at the 1986 MRS Fall Meeting in Boston. There, Jože Bevk of Bell Labs and I organized a high-T_c symposium, among others to expose various false claims on high-T_c observations, which appeared in print in the preceding year or so. Have we been aware of the Bednorz and Müller work, we would have invited them to present their results, even if not believing these. Unfortunately, those of our colleagues who rather accidentally stumbled on this work dismissed it outright and did not mention it to either Jože or me.

Sometime in the middle of our meeting, Paul Chu[14], who for some years already pursued the study of high-pressure effects on superconducting properties of various compounds, presented a contributed talk on the effect of pressure on the T_c of a strange sintered mixture of lanthanum, barium and copper oxides. At some rather low temperature, his samples showed a sudden drop of resistance, which could have been interpreted as percolative superconductivity. Interestingly, under increasing pressure this resistance drop was shifting to much higher temperatures.

After Paul's presentation, one of our invited speakers, Koichi Kitazawa of Tokyo University[15], stood up and showed a viewgraph saying more or less this: "in such an oxide mixture two researchers from the IBM Switzerland lab observed what could be superconductivity above 30 K and we set out to verify that. My graph of diamagnetic susceptibility *vs* temperature shows about 15% of Meissner effect thus proving the presence of a superconducting phase". This was an electrifying shock for the whole audience. Jože, Ted Geballe (Koichi's upcoming session's chair) and I immediately asked Koichi to say more

[14] Then at Cleveland State University, now at the University of Houston.

[15] Now President of Japan Science and Technology Agency (JST).

about in his next day invited talk. He called Tokyo requesting permission from the group leader, Shoji Tanaka (see RN18), who agreed and sent the latest result. Overnight, the Meissner effect reached 20%.

After Koichi's talk, our meeting started to disintegrate. Many attendees were leaving for their laboratories to immediately start working on this new and most exciting lead. Less than 10 days later, AFOSR called a meeting at the Monterey Naval Postgraduate School where we discussed the U.S. strategic response to this new opportunity. On the second Christmas day, the first page of "New York Times" carried a major title "Bell Laboratories Discover High Temperature Superconductivity". This was the exaggerated announcement of Bell Labs immediate success by partly substituting Sr for Ba: this raised T_c by a few degrees above the result of Bednorz and Müller. I evoke this just to illustrate the atmosphere of these unforgettable days and months.

Once the discovery was accepted, it became immediately obvious to most of us that Paul Chu's work on the pressure effect was of seminal importance: one needed to substitute atoms having smaller ionic radii; natural candidates were the rare earth elements. It is no surprise that Paul had an advance on this: on February 6th, M.K. Wu *et al*. of Alabama State University, who worked under Paul's guidance, submitted to *Phys. Rev. Letters* the announcement of critical temperature at 93 K, well above the liquid nitrogen temperature [58]. Many others, including our group, had similar results by the time that letter was published on March 2nd. A new era in superconductivity was launched. The annual APS March meeting in New York turned into the "Woodstock of Physics".

Unfortunately, the new discovery and the resulting euphoria raised unwarranted expectations of immediate technological consequences. Some prominent but less than responsible colleagues were promising immediate applications in all possible fields, including levitated trains, *etc*., *etc*., and the purse-holders believed them for a while. At Westinghouse, John Hulm, then our Chief Scientist, kept warning the **W** Headquarters that many years of hard work lay ahead before any applications could follow; he was branded a "defeatist"[16]. The same happened to me when briefing lower management echelons. Soon enough, disenchantment followed and, by my retirement in 1989, all or most of corporate support for high-T_c superconductivity disappeared. Nevertheless, in these two years some ground was laid for the future successful **W** work in high-T_c electronics, as described next.

X. LATE YEARS: ACTIVE JOSEPHSON JUNCTION ELECTRONICS

John X. Przybysz

In the early 1980s, the superconductivity group at Westinghouse R&D in Pittsburgh, PA, began the transition from large scale to electronic applications, led by Dick Blaugher and Alex Braginski. An ultra-high vacuum deposition and analysis chamber system was purchased from Riber for the deposition of Josephson junctions (JJs). Dick spent time at the National Bureau of standards lab in Boulder, Colorado, where he learned the art of integrated circuit fabrication from Clark Hamilton. I joined the team in November, 1985, less than a year before Bednorz and Muller discovered high-temperature superconductors (HTS).

[16] This euphoric and uncritical attitude was a rather general phenomenon in the Western world. Only the Japanese retained cool heads and started methodically planning the future. A Sumitomo division manager I visited in 1988 told me they don't expect any results on their investment earlier than in 30 years. Now, 25 years later, even this sounds optimistic.

The first JJ digital circuits at Westinghouse were shift registers designed with the JSPICE program from Ted Van Duzer at Berkeley. A 4-stage latching logic circuit set the world's record for the fastest shift register in any technology, at 3.2 GHz in 1989 [59].

A year after the discovery of HTS, Marty Nisenoff[17] and Harold Weinstock[18] supported an effort to make infrared focal planes from HTS materials. The Josephson digital electronics group expanded, with Joonhee Kang making good niobium JJs and Don L. Miller designing and demonstrating the world's first lobe counting analog to digital converter [60].

Good fortune smiled upon the superconductivity researchers at the annual stockholders' meeting, when an elderly woman asked the CEO, "What is Westinghouse doing to take advantage of the breakthroughs in superconductivity?" He said we were doing plenty, though he could not recite the details off the top of his head, and promised to get back to her. Chief Scientist John Hulm seized the opportunity, explaining to the Chairman that he was about to fund a multi-year, multi-million dollar initiative in superconductivity, "All it needs is your signature, boss."

The corporate initiative brought a new clean room and new equipment, including the first Lesker System III deposition system. New researchers joined from within the labs - chemists, ceramicists, and electrical engineers who were "born again" as superconductivity researchers. Among them was Dan Meier, who was determined to bring statistical process control to JJ circuit fabrication.

Konstantin Likharev toured the USA in the summer of 1990, promoting RSFQ and marketing the PSCAN circuit simulator. In December 1990, NIST hosted a US-only workshop to evaluate the potential of RSFQ. Van Duzer, Hamilton, and I favored RSFQ. The Old Guard was committed to latching logic, ridiculing the promise of RSFQ. Fortunately for the Berkeley/NIST/Westinghouse team, Sam Benz had working RSFQ circuits on display in his NIST laboratory. When Van Duzer hosted the follow up workshop in March 1991, everyone loved RSFQ. One former foe even recalled that, actually, he had originally proposed the fundamentals of RSFQ.

I have never forgotten an early encounter with the reality of single flux quantum data. A 4-bit RSFQ shift register was giving erratic results, because there was a lot of interference coupling into the bias lines. Every SFQ datum that went into the shift register would eventually come out, though not in exactly 4 clock cycles. At the end of a frustrating day, I turned off the dc bias lines and went home, leaving the chip cold with one last SFQ bit somewhere inside its four registers. The next morning, I turned on the biases and fired in a few clock pulses. Out came the SFQ datum! The smallest possible loop of persistent current, a single flux quantum, had passed the night unaffected, undiminished, nobly unchanged.

From the Westinghouse Space Division, Hal Ball soon brought money to develop an HTS analog to digital converter (ADC). That program produced the first SFQ circuit in HTS [61] and a 4-bit lobe counting ADC, using a Rapid Single Flux Quantum (RSFQ) binary counter [62].

Those were exciting times. When we were testing the first HTS shift register, I had a small flu that would normally have kept me home sick. Not now. My upset stomach could not tolerate caffeine, so I worked on adrenaline. Arriving at 7 am, Martin Forrester and I worked around the clock to 1 am, until we had captured the data showing correct operation of that first SFQ circuit in HTS.

An internal Westinghouse study, driven by Cliff Jones, produced a roadmap for the development of RSFQ electronics in HTS. RSFQ uses non-hysteretic JJs, making it possible

[17] Naval Research Laboratory, NRL, and Office of Naval Research, ONR.

[18] Air Force Office of Scientific Research, AFOSR.

to build digital circuits in HTS. For mobile applications, HTS is preferable, since it works with small, light weight coolers. The roadmap for HTS JJ electronics emphasized ADCs and digital signal processors (DSPs), since none of these requires much memory, the bane of superconductive electronics. Delta-sigma ADCs figured prominently in this plan, including second-order modulators for high signal to noise ratio and bandpass modulators to simplify the receiver [63]. Flux quanta provide an absolutely accurate, miniature Josephson voltage standard on the chip to measure incoming signals with high precision.

One of the most widely referenced results of the internal study was a calculation which showed that HTS materials could yield chips with thousands of JJs [64]. The key was to reduce the standard deviation of critical current to 5%. Brian Hunt, Martin Forrester, John Talvacchio, and Jim McCambridge led the Westinghouse efforts to make uniform HTS JJs. Steady progress brought the number down from 30% to 8% [65].

In 1992 it seemed that HTS technology for Josephson digital electronics could be developed over 5 years for a cost of about $50 million. When I gave this estimate to a US government workshop, officials said that the world had changed with the collapse of the Soviet Union. In the days of the Iron Curtain, America developed every emerging technology, in case it turned out to be important for defense. Now, new technologies were optional. One wag remarked, “It is the ultimate failure of the Soviet Union. They could not provide a reliable threat.”

From 1995 to 1998, the “Big 3” collaboration of TRW, Westinghouse, and Conductus in HTS junction development conducted joint program reviews, established uniform measurements standards and exchange of samples, which showed that everybody was getting the same results. Researchers saw clarity and steady progress. However, Government officials lost interest in HTS JJ technology when it seemed that the 1993 Conductus claims of 5% uniformity could not be repeated.

In 1992, Stu Wolf went to the Defense Advanced Research Projects Agency (DARPA) to start a program in digital data switches for the Global Grid, an early name for the internet. Our Hodge Worsham built a low power, high speed, 2x2 cross-bar switch that passed up to 30 gigabits per second [66].

Indeed, we excelled at GHz testing and demonstration of Josephson digital circuits. Nobody got bits from room temperature to cold circuits and back again faster. Dave Petersen’s American Cryoprobe chip holders eliminated ground bounce. DC bias lines were heavily filtered. And DC-blocks (10 MHz high pass filters) were used to eliminate “The Mother of All Ground Loops” between JJ chips and 50-ohm test gear [67].

Westinghouse teamed with Lincoln Lab to develop a spread spectrum modem. We built spread spectrum code generators, digital data modulators and demodulators, all of them working at GHz clock speeds in the early 1990s. When Paul Dresselhaus joined us in 1996, he brought design software from SUNY Stony Brook that made larger circuits possible. Eric Dean used two chips in separate dewars to demonstrate spread spectrum coding, transmission, and decoding at 2 GHz [68].

The desire for a better radar transmitter led indirectly to Westinghouse participation in the development of the Josephson AC voltage standard. Doppler radars detect moving targets as small differences between the transmit signal and the return echo. High precision signal sources are needed for the best radars. When Worsham, Ball, and I visited NIST in 1995, Sam Benz and Clark Hamilton shared their latest development, large arrays of JJs on the signal line of coplanar waveguide. I suggested delta-sigma modulation as a means to produce AC outputs. Our ADCs were already using small Josephson voltage standards on-chip to measure received signals. The NIST structure gave us enough JJs in series to make a transmit signal with significant output voltage. The resulting invention is now the basis for the US standard AC volt [69]. At Westinghouse, Andy Miklich designed microwave baluns and filters to extract the 2 GHz version of quantum accurate signals.

Delta-sigma analog to digital converters finally found their natural sponsor in the Office of Naval Research (ONR), for high dynamic range radars. In the first meeting, ONR made it clear that they wanted the HTS version. Memories of the IBM computer project convinced them that LTS would not work. "HTS is new," they told us, "it has a chance to work." The Westinghouse approach minimized Josephson junction count, to enable the earliest possible realization in HTS. Only the modulator would be done with JJs. All digital filtering would be done with room temperature semiconductors. At the 1997 International Superconductive Electronics Conference, Don Miller showed the first digital output of a superconductive delta-sigma modulator, a slide full of data ONEs and ZEROs collected at 1.28 GHz [70]. The quantization noise spectrum of superconductive delta-sigma ADCs demonstrated 15 octaves of noise suppression, something that no semiconductor ADC has ever done.

Westinghouse hosted the 1996 Applied Superconductivity Conference. It was not supposed to be that way, but the collapse of the US Superconducting Super Collider project sent ASC President Phil Sanger to Westinghouse as a refugee. Good-bye, Houston. Hello, Pittsburgh! He found a willing accomplice in Art Davidson, who worked tirelessly toward a successful conference. At the opening session of ASC96, the attendees were welcomed by Northrop Grumman Corporation, which had purchased the superconductivity research along with the Westinghouse defense business.

For a time, our researchers went about placing Northrop Grumman stickers over top of the old Circle W on posters and plaques describing 50 years of accomplishments in superconductivity. Eventually, they gave up. There were too many.

XI. CONCLUDING REMARKS

The era of applied superconductivity dawned half a century ago with the demonstration by Gene Kunzler at Bell Labs of the first high-field Nb_3Sn superconducting magnet [7]. In the 1950s, the leading US industrial corporations mentioned in our Introduction, including **W**, successfully pursued fundamental research in superconductivity hoping for its eventual practical use in the future, however distant. Kunzler's seminal discovery and demonstration resulted immediately in focused industrial efforts to make applications of superconductivity a reality. At even larger scale that was repeated a quarter of century later, with the discovery of HTS. Unfortunately, in most major corporations these efforts have been usually short lived: they used to fade away once the management realized that the development and commercialization efforts will be long-lasting and expensive. The willingness to invest in such endeavors depended thus largely upon external, *i.e.*, taxpayers' money. The support by US government energy agencies, primarily DOE for power-related applications, have been following cycles related not only to major discoveries, but also to ups and downs in the world economy and major energy crises. That support was mostly only temporary – until the next recession or another new funding priority.

Westinghouse was one of the very few industries that maintained the continuity of efforts since their beginning until the company's dismemberment in 1996, even if periods of boom and bust alternated as illustrated in Section VI and mentioned elsewhere. This relative stability has to be credited largely to John Hulm and his ability to both steer the evolving directions of R&D and to convince the **W** upper management that such efforts are worthy of company's at least partial support. Indeed, John Hulm's stewardship and vision were exemplary. Upon his untimely death many tributes to his role and contributions were presented[19] and published; we refer the readers to two of these [71,72]. Those who would like

[19] John Hulm Memorial Session: 50 years of high field superconductivity and the next 50 years! The ASC 2004,

to gain some insight into his personality and sense of humor should also read his speech given at the 1982 Applied Superconductivity Conference (ASC) [73]. John understood the importance of and supported international collaborations as exemplified by his stint (1974-1976, on temporary leave from **W**) at the United States Embassy in London as the US Science Attaché for Europe [74], and also by the **W** participation in LCT.

Special credit is also due to one government agency, the AFOSR[20]. Since late 1960s it maintained continuity of support for the relatively fundamental **W** work in materials – until the group was sold to Northrop Grumman and even beyond. This was due to the long-term vision of the late Program Director Max Swerdlow (1915 - 1989) and his successor Harold Weinstock.

Table I. Westinghouse Superconductivity Achievements.

YEAR	**EVENT**
1956	Fundamental studies on the electronic specific heat of superconductors – Corak, Satterthwaite, and Goodman.
1957	Fundamental studies on the superconducting energy gap – Biondi, Forrester, Garfunkel, and Satterthwaite.
1961	Transition Alloy Studies – Hulm and Blaugher Study of BCS behavior for T_c – Hulm, Blaugher, Geballe, and Matthias First Nb-Zr wire developed. First high-field magnet from Nb-Zr constructed.
1962	First theoretical prediction of upper critical field - Chandrasekhar
1962	First commercial Nb-Ti wire developed.
1965	First high-field (10 T) magnet from Nb-Ti.
1966	Studies on SC Carbonitrides – Matthias, Hulm and Pessall
1970	Studies on SC Semiconductors - Hulm, Deis, Jones, and Ashkin
1971	First SC utility power generator prototype at 5MVA.
1973	Synthesis of stoichiometric Nb_3Ge with record T_c of 23 K - Gavaler
1975	Proto-type multi-pole high-speed SC generators for airborne use developed.
1983	Comprehensive theory of AC losses in superconductors – Carr.
1986	Westinghouse Nb_3Sn fusion magnet (LCP) successfully tested.
1989	Electronics: the fastest digital logic at 3.2 GHz – Przybysz *et al*.
1990	First lobe-counting A/D converter – Miller *et al.*
1995	Delta-sigma modulator for AC volt standard – Przybysz
1997	LTS delta-sigma A/D converter with 15 octaves noise suppression - Miller *et al*.

The continuity of W activity since early 1950s until almost the end of XX century made possible significant contributions in diverse areas of superconductivity research and applications. Table I provides our list, which includes also work not highlighted in this article. Which of these had the strongest and lasting impact is a matter of subjective judgment. In our opinion these were the energy gap and specific heat investigations [3-5], the synthesis of metastable Nb_3Ge, the pioneering work on superconducting machines, especially generators [21-26,30], the theory of ac losses in composite conductors [39], the LCP **W** coil [46-48] and the conceptual contribution to the accurate ac waveform synthesis [69], the basis for ac volt standards recently developed at NIST.

In closing, we like to offer a comment on the Westinghouse Electric Corporation as such. As many may know, during more than a century it had a leading role in the creation of

Evening of Tuesday October 5th, 2004; organized by R.D. Blaugher. Most presentations published in *IEEE Trans. Appl. Supercond*. **15** (2005).

[20] US Air Force Office of Scientific Research

electrical and electronic technology worldwide. Founded by a talented inventor, George Westinghouse, it was run during its existence by technology savvy executives, who usually rose through the ranks. Technology was appreciated and generally understood by them as exemplified by the support given superconductivity. The engineering staff was appreciated and benevolently treated, as some of us could directly experience by obtaining support for graduate studies or in taxing moments of life. However, the management's business acumen was old-style, not commensurate with the current aggressive short-term profit strategies. The first CEO hired from outside of the company promptly sold out the technology-oriented manufacturing divisions and part of the R&D team with them. The proceeds were invested in broadcasting media. Overnight, Westinghouse became CBS. The venerable name and logo were sold to Japanese nuclear industry.

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