FOREIGN TRAVEL REPORT

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ITINERARY AND SUMMARY

This report presents information gained during my official travel to Europe from June 26, 1978 through July 17, 1978. A list of institutions visited and the people I contacted is given as follows:

June 28-30	Rutherford High Energy Laboratory Chilton-Didcot, Oxfordshire England Contacts: P.T.M. Clee, D.H. Thomas, R.L. Roberts, M.N. Wilson, J.H. Coupland, C.R. Walters, R.V. Stouveld, M. King, R. Newport, W. Toner, and P. Houzego
July 4-7	The 7th International Cryogenic Engineering Conference Imperial College London, England
July 10-11	CEC Nonmetallic and Composite Materials at Low Temperatures Hilton Hotel Munich, West Germany
July 12	CEN Saclay, DPH/PE STIPE 91190 Gif Sur Yvette France Contacts: H. Desportes and Lubar
July 13 and 14	Institute für Technischephysik Kernforschungszentrum Postfach 3640, 75 Karlsruhe West Germany Contacts: W. Heinz, P. Turowski, F. Arendt, K.P. Jüngst, G. Hartwig, Dustman, and W. Mauer

Introduction

The primary reasons for my trip to Europe were to attend the Seventh International Cryogenic Engineering Conference in London, England, and to attend the Munich Materials Conference. I presented two papers at the ICEC-7 conference in London: "A Large High Current Density Superconducting Solenoid for the Time Projection Chamber," and "Computer Design and Optimization of Cryogenic Refrigeration Systems." I also presented one paper at the Munich Materials Conference, "Vacuum Impregnation with Epoxy of Large Superconducting Magnet Structures." The London conference, run primarily by the Europeans on a biannual basis, gives one a chance to meet cryogenic experts from all over Europe. The Munich Materials Conference turned out to be an extremely good conference.

Since I was in England and on the Continent, I visited the three major GESSS superconducting laboratories: Rutherford in England, CEN Saclay in France, and ITP at the Kernforschungszentrum in Karlsruhe, West Germany. I had interesting and fruitful discussions with friends and former colleagues from the three laboratories. Things had changed a great deal at the Rutherford Lab in the two years since I was last there. My visit to Saclay was primarily to see the CELLO magnet, which is a competitor with the TPC magnet: the concept behind CELLO is currently being considered along with LBL and ANL techniques for use in a thin solenoid at Fermi Lab. The Saclay visit turned out to be very educational: I learned a technique for finding ground plane insulation shorts which could prove very useful if we are ever faced with this problem. I also visited Karlsruhe and as usual I was impressed with the size and vitality of the group there. When I spent two years at Karlsruhe, this group was the baby brother to all of the major groups in Europe. Now baby brother is growing up; Karlsruhe is on the verge of becoming the leading superconductivity group in Europe. My visit to Karlsruhe is potentially worth the whole cost of the trip. Some discussions with P. Turowski have led to some modifications in the design of the TPC magnet which saved us from some potentially bad strain problems.

Visit to Rutherford High Energy Laboratory, June 28-30 and July 3

Before discussing my laboratory visit, I'd like to give some of my impressions of England. I have been to England many times, and in particular have spent quite a number of days in the London area, including the area around Oxford. The most striking thing I noticed on this trip, compared to previous trips, is the rate of inflation: London has caught up with the rest of Europe. Since I was last in London in 1976, the prices for many things have doubled: hotel rooms which used to charge £4.00 for bed and English breakfast (eggs, bacon, marmalade, and toast) now charge £7 to £8, with only a continental breakfast included. Beer in the pubs has gone up from £0.19 per pint (20 oz.) to £0.37 per pint. The beer is still excellent, however, and good Guinness costs scarcely a penny more now. Public transit on the London underground has gone up 50 to 70 percent; by contrast, public transit in the U.S. is a bargain (at least in the Bay Area) compared to most of Europe. One good thing I can say is that one can get a ticket at Convent Garden for only £4.50. Also, British rail service has improved with the advent of the new 125 mph trains.

The price inflation I mentioned is caused in part by competition with more expensive continental Europe, but also is caused by government policy (so my British friends tell me). Wages have not kept pace with inflation; this is particularly true of professional salaries. The British tax system, which is highly burdensome and progressive already, extracts money from a man's pay at a rate which is double the rate of his pay increase. As a result, the so-called U.S. "taxpayer's revolt" and Proposition 13 were of particular interest to the British: I was asked more questions about Proposition 13 than anything else. People found it difficult to believe that Americans could actually vote to reduce taxes. One chap I knew remarked, "You colonials have always been revolting about something."

Rural England, with its rolling hills and pretty cottages, is as beautiful as ever. However, Dutch Elm disease has taken its toll; whole forests have died. The dead trees standing remind one that man has little control over the situation. The English look at the Dutch Elm disease as a natural phenomenon, pointing out that it has struck before, some 350 years ago.

Metrication has been set back: road signs and speed signs are still given in miles. Beer is sold in pints, and most jams, jellies, and marmalades are sold in one-pound jars. The Metrication Board has almost stopped trying to force the British people into going metric.

The Royal Mail has gotten worse: the British claim it is the worst in the world. I disagree with them--our postal service is worse. At least British postal clerks are polite as they take your money. Their postal rates are comparable to ours (among the lowest in the world).

Briton is still British; they have not fully joined the Common Market any more than the French have.

A. The General Situation at the Rutherford Laboratory

The Rutherford Laboratory, which is administered by the Science Research Council, is changing rapidly. Nimrod, the Rutherford 7 GeV weak focusing synchrotron, was closed June 6, 1978. Unlike the Bevatron in Berkeley, Nimrod was not converted to heavy ions. High energy physics in England is dead. Instead the English now support the SPS and ISR at CERN, and PETRA at DESY. The announcement of circulating beam at PETRA, along with the closing party for Nimrod (which was held the evening before I arrived at the Rutherford Lab), were like nails being driven in the coffin of English high energy physics. As a result, the morale at Rutherford Lab is rather low. Many good high energy and accelerator physicists will probably leave, attracted by physics prospects in Germany and Switzerland. They are also attracted by rates of pay which are two to three times that of the SRC, and in addition the taxes paid are a much lower percentage of the salary.

The change of direction of the Rutherford Lab is bemoaned by some and cheered by others. The Laboratory is by necessity becoming more diversified. However, it appears to have become a vassal to the British universities: for the most part the Rutherford Lab must supply support and technical expertise to British university groups. Several people I know at the Rutherford Lab complained bitterly that the Lab can no longer propose and carry out experimental work; this is very demoralizing to good innovative research people. Some of the people I talked to felt they were becoming second-class citizens. I think there is an important administrative lesson to be learned here.

The change of direction at the Rutherford Laboratory has affected morale, but it has its positive aspects as well. There is plenty of work, particularly for engineers. In fact, there is a danger of spreading the engineering talent too thin. The real bright spots are the laser test facility (which will be discussed later) and the new neutron facility. Rutherford's good people are very adaptable, so in that respect the Lab's future looks promising. Rutherford does have a militant but slow-moving group of bureaucrats who go on strike at the drop of a hat. There was one group that refused to deliver mail internally because of some policy that came out of London.

B. Superconductivity at the Rutherford Laboratory

Since the Rutherford Laboratory has changed its direction, less work is being done in the field of superconductivity. This is a shame because the Rutherford group has made many of the basic discoveries in the field: it was the best superconducting group in Europe, if not the world, but is now a ghost of its former self. The group has been broken up, and is now about one third to one half of its former size. Most of the basic materials work has stopped and much of the basic engineering development effort has also ceased. I talked at great length with P.T.M. Clee, R. Roberts, and D. Thomas about this situation. The Lab's new role as the valet for British university groups does not permit the kind of innovative research that was done in the past. Unfortunately, preeminence in superconductivity in Europe will pass on to the Saclay and Karlsruhe groups. The GESSS collaboration, for all practical purposes, is dead (at least for the Rutherford Lab).

The PT-55 high accuracy solenoid has been built. The first tests in a pool cryostat demonstrated stability, ability to quench, and field uniformity. The magnet achieved its desired goal of 1 part in 10^4 field uniformity over a length of 1 m and a diameter of 5 cm. The PT-55 magnet uses a conductive copper bore tube to protect the magnet during a quench. The Rutherford magnet group has modeled the bore tube system on their QUENCH computer code. "Quench back" was observed, but it occurred much faster than their code predicted: the QUENCH computer code could model "thermal quench back" but not "magnetic quench back." A check of the measured dB/dt within the superconducting winding showed that the superconductor should be driven normal by the coupled a.c. loss in the superconductor. Hence it is thought that "magnetic (or a.c. loss) quench back" was observed in PT-55.

I saw the Rutherford cable maker, which came from a plant in the north of England. This cabler was slow, as it was built around the turn of the century. Rutherford made their own cable for the AC-5 superconducting dipole because no firm in England could cable IMI wire in a satisfactory manner. Rutherford has both a 6 strand and an 18 strand cable maker. There was also a machine which braids glass into superconductor. Those who showed me the equipment were of the opinion that it is obsolete, since a good monolithic conductor is available which can do the same job in a dipole or quadrupole. I am inclined to agree, despite the fact that there is a lot of cabled and braided conductor being used these days.

I saw the Rutherford Nb_3Sn sextipole, which was wound with multifilamentary Nb in a tin bronze matrix (this is multifilament Nb_3Sn in the "green" or unreacted state). The "green" conductor was insulated with glass braid impregnated with a vaporizable resin. After winding, the coil is potted in "perspect" (I believe this is lucite); it is then fired under vacuum. It is heated slowly at first so that the perspect and vaporizable resin are boiled away without breakdown (one does not want carbon particles in the insulation). Apparently the glass is very brittle; hence everything is potted before reaction. Eventually a reaction temperature of 800 to 900°C is reached and Nb_3Sn is formed by diffusion between the bronze and the niobium. After reaction the coil was vacuum-impregnated in epoxy resin. The Nb_3Sn sextipole apparently trained very little. Good performance was achieved (about 5 T at the inside of a 25 mm bore). The peak field of the magnet was about 7 T somewhere deep inside the winding. The justification for using Nb_3Sn was high current density at fields as high as 7 or 8 T. A Nb-Ti sextupole has also been built.

There were discussions between M.N. Wilson, P.T.M. Clee, R. Roberts, and J. Coupland on training and quench protection in dipoles and quadrupoles. There were strong differences of opinion on what causes training: some subscribe to the epoxy-is-the-cause theory; others blame the superconductor and the winding technique. M. Wilson continues to develop his theories on training in pressure potted coils. He feels that the stainless steel potting cans could make a difference; perhaps berylium copper cans would eliminate this problem. R. Roberts feels that the epoxy is the culprit, but pressure potting won't cure training. C. Scott feels that the superconductor is the villain: he cites both his measurements of slipping in Nb-Ti and the Karlsruhe experience. Others disagree, pointing out that the stress is not high enough. J. Coupland used the Nb-Ti sextupole as a training experiment. The six poles were wound from and potted in the same epoxy, but the training history for each pole was radically different. The pole that the technician thought had the best workmanship trained the worst and the converse was true. Needless to say, the argument goes on. The Rutherford people were interested in ESCAR and its problems, as well as the Brookhaven cold diode scheme. Their experience with our shorted secondary scheme has made believers out of the skeptics at Rutherford.

I had discussions with C. Walter and M.N. Wilson concerning the use of flat Nb_3Sn tape to wind large tokamack type coils. This tape is proposed in lieu of the multifilamentary type conductor now being proposed by MIT and others. The MIT scheme, which calls for very fine stranded force flow cooled cable in a tube, has fairly high a.c. losses. (The monolithic type conductors being proposed for TESPE and LCP are Nb-Ti, not Nb_3Sn.) Tokamack coils are supposed to have rather small radial components of flux (due to the poloidal coils, not the toroidal coils). Hence a flat tape type of conductor is attractive. M.N. Wilson proposed a tape 60 cm wide. The a.c. flux penetrates from the edge, but the penetration depth is rather small. The a.c. losses are rather high in the region where the flux penetrates the conductor, but there is no a.c. loss in other parts of the conductor. The expected a.c. loss in a flat tape wound coil is expected to be an order of magnitude lower than the fine stranded Nb_3Sn cable being proposed for the U.S. TESPE coil.

R. Stoville showed me a two pole superconducting motor rotor being built for South Hampton University. Tests on the rotor are expected to begin in September. This superconducting rotor will be used to measure motor a.c. losses, hunting, and other motor characteristics. I was also shown drawings of the Rutherford wiggler magnet.

C. Other Programs at the Rutherford Laboratory

As I have said earlier, the Rutherford Lab is moving away from high energy physics, except in the support of experiments at CERN and DESY. In fact, the only piece of hardware I saw which was high energy physics related besides PT-55 was the rapid cycling bubbling chamber. (Rutherford is building the chamber and Saclay is building the superconducting magnet.) R. Newport showed me bits and pieces of the bubble chamber. Of particular interest were the fiber composite pieces for moving portions of the chamber. For example, a series of fiberglass epoxy bellows were built and tested, and a final bellows design has emerged. The bellows are capable of being cycled 10^7-10^8 cycles without fatigue failure, yet they are entirely nonmetallic and nonconductive electrically.

I was shown the new laser beam facility at Rutherford by W. Toner (who is a former high energy physicist). I was really impressed with the facility (I am told that the Livermore Shiva facility is much bigger, but it is shrouded in secrecy). The Rutherford facility was built to study implosions of spheres filled with gas. In the laboratory, one can create various fully-stripped elements which can be spectrally studied. British astrophysicists have found this very useful for calibration of of their equipment for studying plasmas in stars. I saw results of studies which have been done at the facility on interial confinement of plasma (so-called laser fusion). The laser facility, which is basic research oriented, is one of the bright new areas which has generated positive morale at Rutherford.

The neutron beam facility has started construction. I got mixed reactions about this project: some of the engineers I know are very concerned about it. They expect that the machine will be built, but they also expect a lot of mistakes to be made, resulting in delays. At the time I was there, Nimrod was being dismantled to make room for the new facility. Since there was no hardware to be seen, I didn't pursue this subject any further.

I was taken on a tour of the electron beam lithograph facility by P. Houzego. RHEL will be making masks to produce integrated circuit chips for various university groups. It was an impressive facility with its clean rooms. (For example, one section has very clean air with less than 100 0.5 μm or larger particles per cubic foot of air. I wonder if the employees must go through a daily dandruff check. There is a whole raft of possibilities for levity here.) Unfortunately, the mask facility will probably be obsolete before it is finished: it will produce silicon chip masks with 2-3 μm line widths, but people I talked to from IBM tell me that sub-one micron line widths will be the norm within a few years.

Still, the mask facility will do a good job of performing a service for the British universities. The basic question remains: should a major research facility like RHEL be the maid and valet to a bunch of university groups? This feeling has really hurt morale all over the Rutherford Lab.

D. High Energy Physics Discussion

I had a number of discussions with accelerator physics people I know at the Rutherford Lab. For example, G. Rees, M.N. King, and M. Harold are working on the neutron beam machine, CERN $\bar{p}$-p studies, and heavy ion fusion. I think these people will eventually get out of accelerator physics or they will go to other non-British labs where the pay is higher. Over coffee some of the Rutherford people were discussing Fermi Labs and the various possibilities for a new director. There were a couple of CERN people present from the SPS, and the usual CERN attitude about Fermi emerged in the discussions. It was felt that the SPS will be a better experimental facility in the 200-400 GeV energy range, because Fermi Lab was supposedly designed to be cheap in order to get the first experiments in the energy range; CERN would then do the bulk of good physics in the energy range. (I am sure that Fermi Lab people will disagree with this view--at least I hope they do.) CERN people say Fermi Lab either has to upgrade their experimental facilities at a cost of millions of dollars or they must quickly get into a new energy range. I had the feeling that CERN was not in a hurry to build an energy doubler.

I was told that PETRA had beam. People in Europe are very excited by this because it means that Europe has the jump on the U.S. in a new energy range. Whether or not PETRA can press the advantage is another thing: the only experiment which will be fully ready at beam turn on is PLUTO. Still, the Germans have done very well thus far. The continuous delay problems on PEP, including lack of funding, may well give the advantage to the Europeans in e^+-e^- physics in the PETRA-PEP energy range.

On July 3, 1978 I gave a 1½ hour seminar on the Lawrence Berkeley Laboratory thin coil superconducting magnet program and the ESCAR program. There is considerable interest at the Rutherford Laboratory since P.T.M. Clee and R. Roberts spent one week at Berkeley in 1976 studying various magnet alternatives for a CERN experiment which was not accepted. P. Clee and R. Roberts have studied various winding techniques for thin magnets, and the Rutherford program has verified that the LBL bore tube technique works. Their PT-55 magnet shows considerable evidence of quench back. Hence, the Rutherford group is interested in our work even though they may find no direct use for it in the near future.

The ICEC-7 Conference, Imperial College, London, England, July 4-7, 1978

I attended the Seventh International Cryogenic Engineering Conference and Exhibition from July 4 through July 7, 1978. During this conference, I gave two papers. The first was an oral presentation during the first superconducting magnet session on the morning of July 4, 1978; the title of the paper was, "A Large High Current Density Superconducting Solenoid for the Time Projection Chamber." The second paper was a poster session paper given on the afternoon of July 5; it was entitled, "Computer Design and Optimization of Cryogenic Refrigeration Systems." Both papers were well received. The second paper developed into a discussion of alternative energy sources, and I had many requests for pre-prints.

There was a general plenary session paper on cryogenics in space. Examples of the uses for cryogenics included: (1) masers which operate between 0.2 and 4.5°K; (2) relativity measurement with superconducting gyroscopes; (3) magnetic field measurements using squids; (4) time measurement at 2°K with a resolution of 3 parts in 10^{16}; and (5) data processing with superconducting Josephson junctions. The conditions in space (i.e., zero g, high accelerations and vibration, and a complex thermal environment) will challenge the cryogenic designer. One interesting concept which is being considered for the shuttle is intermediate temperature shields using solid methane (65-80°K) and solid ammonia (130-150°K). There were four other papers which dealt with space applications of helium (mostly superfluid helium).

F. Brown of Thor Cryogenics in Oxford, England talked about Thor Cryogenics' Nb_3Sn Nb-Ti hybrid magnets. These magnets have the Nb-Ti coil on the outside, and the peak induction in the Nb-Ti magnet is about 8.5 T. Both the Nb-Ti and Nb_3Sn conductors are round. The Nb_3Sn conductor is wound unreacted, and is reacted at 700°C for several days. The Nb_3Sn and Nb-Ti coils are potted in epoxy. There is just bronze--no copper--in the multifilament Nb_3Sn. There is no evidence of training in the Nb_3Sn part of the coil. The coil charging time is about 1 hr to 10 T. The first magnet built went to 11.0 T at 4.2°K and over 13.0 T at 1.8°K; Thor's hybrid magnet goes to 14.5 T at 4.2°K. All of the Thor magnets are small.

F. Kirchner of Saclay reported on the Saclay SUPER SOLO solenoid. This magnet is intrinsically stable. It operates at a current density of 3×10^8 $A{\cdot}m^{-2}$ with a stored energy of 425 kJ. The magnet has a persistent switch which closes in less than 1 ms. The joints are 50 cm long, soldered with 50-50 solder, and have a resistance of $10^{-8}\Omega$. This results in a decay time constant greater than 2×10^8 s (6 years). The SUPER SOLO solenoid system is to be used in a heavy ion switch for the SATURN acceleration system.

A group from Dubna in the Soviet Union is building a superconducting ring which is about the size of the LBL ESCAR ring. The magnets are 2.5 T window frame magnets. The ring, which will be located outside the present Dubna ring, will have a repetition rate of 0.1 to 0.5 Hz. Several model magnets have been tested which are 0.4 m long with apertures between 5.5 and 7.6 cm. The conductor used is a Rutherford type cable which is 0.95 mm by 2.8 mm; it has 12 strands which start out at 0.5 mm diameter. These strands have a transposition pitch of 27 mm and contain about 460 25 μm diameter filaments of Nb-Ti. There is sextupole correction in the magnet above 1.8 T control induction (just like G. Danby's magnets at Brookhaven). At full field (2.5 T) the field uniformity is 4×10^{-3} without correction current and 0.5×10^{-3} with correction current. The measured a.c. losses have been as low as 6 $W \cdot m^{-1}$ when the flux change rate is 2 $T \cdot s^{-1}$. The magnets have trained to currents between 80% and 96% of the magnet critical current.

The Wisconsin group reported on their two-phase flow experiments with helium 1 (helium at a temperature above the λ point, which is 2.17°K). The Wisconsin data was reported in terms of a two-phase flow multiplier, the pressure drop in two-phase flow over the pressure drop for flow of the liquid phase alone. This multiplier decreases with pressure and mass flux density, and it increases with quality. There is no significant size effect in the experiments: a homogeneous model (such as Martinelli-Nelson) predicts a phase flow reasonably well. The Wisconsin experiment is quite detailed. The results agree with what has been measured at LBL within superconducting coils. In short, two-phase flow in horizontal tubes is nothing to be afraid of.

I talked to Eric Adam and Eric Gregory of AIRCO on Nb_3Sn multifilamentary superconductor. AIRCO quoted me the price of conductor which has a cross-sectional area (the matrix) of 3 mm^2. The matrix contains 55 bundles of 900 filaments (about 50,000) which are 3 μm in diameter. The initial bronze-to-niobium ratio is 2.8 to 1. The bronze starts out at 13% tin before reacting and is 2% tin after reacting. There is copper outside. The (depleted) bronze resistance at 4.2°K is 3×10^{-8} ohm•m. We can get any twist pitch we want: the conductor will carry 1500 A at 10 T and 4.2°K. The estimated price for 400 kg of this conductor is between 200 and 250 thousand dollars if we buy the material fully reacted. Forvar insulation is not available, but Eric Adam suggests epoxy coating or a mylar wrapped conductor.

Dennis Herrel of the IBM Research Center in Yorktown Heights, New York, talked about IBM advances in superconducting computers. The memory circuits are based on Josephson junctions and SQUID's. He showed pictures of microcircuitry (about 2 μm line size) which were incredible. A computer module with 2×10^5 circuits would be smaller and faster than silicon systems. For example, superconductor circuit speeds would be 60 ps, compared to 1.5 ns for silicon circuits. Cycle times are reduced from 60 ns to 5 ns. Total heat dissipation is reduced, and hence the volume is reduced from 1 m^3 to about 10^{-4} m^3. I found this paper fascinating.

There were a number of materials papers I found interesting. K.A. Yuschenko of the Soviet Union and R.P. Reed of NBS Boulder talked about the joint US-USSR material test program for LNG vessels. The US materials tested were 5083 aluminum, 9% nickel steel, and 5% nickel steel. The USSR materials were an aluminum similar to 5083 and a steel which is 80.5% iron, 0.3% chromium, 19% manganese, and 0.2% nickel. D.J. Radcliffe of the University of Oxford talked about carbon fiber composites and their thermal properties. For example, the thermal conductivity of various composites at 4°C, 77°K, and 300°K is as follows: (1) epoxy alone: 0.05, 0.08, and 0.15 $W \cdot m^{-1}k^{-1}$, respectively; (2) carbon fiber along the fibers: 0.03, 1.0, and 20 $W \cdot m^{-1}k^{-1}$, respectively; and (3) carbon fibers perpendicular to the fiber: 0.015, 0.15, and 1.0 $W \cdot m^{-1}k^{-1}$, respectively. The thermal contraction of the composite with 90° ply is about 1 to 2×10^{-5} from 250°K to 4°K. The reason is that upon heating, carbon fiber contracts along the fiber and expands perpendicular to the fiber. The price of carbon fiber composites in the United Kingdom is 40-60 US dollars per kg for a composite which is half carbon. An Italian group from Genoa measures thermal conductivity of epoxy resins at helium temperature using a phase lag method. Some results are: (1) epoxy: 0.06 $W \cdot m^{-1}k^{-1}$ from 3.5 to 8°K; (2) STYCAST 2850 FT: 0.06 to 1.5 $W \cdot m^{-1}k^{-1}$ from 3.5 to 8°K; and (3) STYCAST 2850 GT: 0.06 to 2.5 $W \cdot m^{-1}k^{-1}$ from 3.5 to 8°K.

There were numerous interesting papers at ICEC-7. I will only mention some not described previously. H. Katheder from Karlsruhe talked about the cryogenics for the Hyperon beam quadrupoles. R. Roberts of the Rutherford Lab talked about PT-55. M. Hoenig of MIT spoke on the

US TESPE project, which has since been cancelled by DOE. S. Kadkany of Wisconsin talked about the Wisconsin energy storage magnet 1.8°K dewar. P. Lebrun of CERN discussed the ISR superconducting quadrupoles and their testing and operation. W. Herz of Karlsruhe talked about the cryogenic system for their superconducting proton linac. D. Braunschweig of Linde A.G. spoke on the industrial application of liquid nitrogen from food processing to tine shreading. B.J. Maddock of CERL Lab at Weatherhead, England, gave an excellent paper on the state of superconducting electrical power engineering. F. Vosswinkel talked about the Linde A.G. double-acting helium pump for electrical cable application. There were a number of papers which discussed the continued development of practical Nb_3Sn conductor. The authors of these papers came from IMI, Mitsubishi Electric, Krupp, AIRCO, and the Soviet Union. W. Lehman talked about his measurements of helium boiloff in helium vessels with ruptured vacuum vessels and other faults. R. Warren of LBL talked about his electrical leads for ESCAR. J. Dean talked about measurements on a supercritical refrigeration loop. A. Hostettler of Sulzer gave a paper on the refrigerator for the CERN SPS North experimental area. I found the conference to be valuable because of the many approaches which were discussed to solve many diverse cryogenic problems.

Conference on Nonmetallic Meterials and Composites at Low Temperatures, Munich, West Germany, July 10-11, 1978

Before presenting my impressions of the Munich Materials Conference, I would like to present some observations on travel on the Continent. First, Europe is expensive, and the continued decline of the dollar only makes things worse. There are almost no bargains left in Europe--if you find any, feel lucky because it won't last long. The high prices are due to a combination of factors, including high taxes. Most European goods have a value-added tax (a sales tax between 11 and 25 percent).

I traveled to the Continent on the Harwich-Hoek van Holland ferry. This boat trip is aboard clean, shipshape Dutch boats which are very well run. For £2.20 (about $4.40), one can get a comfortable berth for the night and be awakened by a cup of tea as the boat pulls into Holland. Train service from the Hoek is very good: I rode the Rheingold Express (named for Wagner's first Ring Opera) into Germany as far as Mainz. The train is excellent, the food is not bad, and it is reasonably priced. The beer is excellent, as usual. From Mainz I took an intercity train to Munich. I recommend the Eurail Pass; for travel of any distance, it is cheaper than flying by a factor of 2, and you can go anywhere in Western Europe on some of the best trains in the world.

Munich has changed a lot since I last spent time there. I can report that the Hotel Bundesbahn is still a great bargain. It is a solid middle-class hotel in the railway station, and is almost absolutely quiet. A single room runs about 33 DM without bath. One can still get "ein gross Bier" and get a full liter of Bavarian beer; the price is about 4.50 DM ($2.30 US).

Prices are high in Germany, yet the inflation rate is among the lowest in the world. The Germans are more afraid of inflation than anything else, and as a result their government seems to be more effective in curbing inflation than any other country except possibly Switzerland. In dollar terms German prices have increased greatly, but in terms of the deutschmark the inflation rate has been less than three percent (even a three percent inflation rate is alarming to a German). I have opinions about why there is a difference in inflation rates between the US and Germany, but I am no economist.

I've talked about the British postal system as compared with ours. The Germany postal system is better than either because it is subsidized by their telephone system, which charges rates which are double ours. German postal rates are high: a domestic letter is 20 cents and a foreign letter runs between 25 and 45 cents (more if it weighs over 5 gm). A typical two-page letter by air to the US is 45 to 55 cents.

The Munich Materials Conference was a jewel of a little conference. There were around 60 people attending, and all papers were given in a single session (there were no parallel sessions). The people at the conference all got to know each other, so ideas flowed freely. There were many good papers. One of the unusual aspects of the conference was the number of woman delegates present--over ten percent of the delegates were women. Unfortunately, there was only one woman representing a US laboratory (she is a recent Russian immigrant). The topics of the Cryogenic Materials Conference included: review of high polymers, electrical properties of polymers, physical and radiation properties of polymer materials, nonmetallic thermal insulators, composite materials, and applications of nonmetallic materials at low temperatures.

The conference started with papers by U.T. Kreibich of Ciba-Geigy AG, Basel Switzerland, and G. Hartwig of ITP Kernforschungszentrum, Karlsruhe. These papers dealt with the chemical and physical properties of polymers, and polymers such as nylon and polyethylene were compared. Nylon transforms to a glass-like brittle material, while polyethylene is ductile and capable of plastic deformation even at 4°K. The epoxies behave just like nylon. There was considerable discussion about what might be done to make epoxy behave like polyethylene at 4°K yet be castable and strong at 300°K. Both Ciba and Karlsruhe are working on this problem. For example, the role of flexibilizers was discussed. They tend to lower the elastic modulus at 300°K because they increase the thermal contraction coefficient; however, they have almost no effect on 4°K ductility. The loss of ductility is in general accompanied by less damping and dielectric loss in the epoxy.

Dielectric losses in polymers are related to the mechanical damping in the material, according to papers from the University of Ulm and the National Bureau of Standards at Boulder. Dielectric losses in polyethylene were discussed in papers by W. Meyer of the Institut für Hochfrequenztechnik at Braunschweig, Germany, and by J. leG. Gilchrist at the Low Temperature Research Center at Grenoble, France.

The insulation session was keynoted by L.L. Sparks of NBS Boulder. He talked about foam insulations which would have application in LNG tankage. In general, foams have a thermal conductivity ranging from 0.005 to 0.05 $W \cdot m^{-1} k^{-1}$, depending on temperature, density, and the degree to which the cells are closed. Foam is three or four orders of magnitude more conductive than the best evacuated multilayer insulations. The total thermal contraction at 50°K is from 0.8 to 1.6×10^{-3}, and the elastic modulus is as high as 8×10^{7} $N \cdot m^{-2}$. The compressive strength of typical foams is from 2×10^{5} to 2×10^{6} $N \cdot m^{-2}$ at 100°K and below. Tensile and shear strength is typically within a factor of two of the compressive strength. E.L. Sharpe of Bell Aerospace talked about the degradation of foam insulation with time when samples were tested in an environment similar to LH_2 tanks used in aircraft flight operations. G.E. Padawer talked about balsa wood insulating tank supports. He pointed out that wood is our oldest composite material; balsa wood is a particularly interesting material because its strength is 10 times that of foam. The properties of wood were measured before 1930, but since that time the work has been forgotten. In balsa wood the strength and modulus in the longitudinal, radial, and tangential directions vary in the same way. The longitudinal strength of balsa wood is 32 times the tangential strength. The radial strength is 4 times the tangential strength. (Note that longitudinal, radial, and tangential refer to directions in the tree where longitudinal is along the grain.) Thus balsa wood has to be laminated in order to be effectively used.

There were a series of papers given on carbon fiber composites. These included papers by W. Roland of the University of Marburg, Germany; D. Kullman of Siemens AG, Erlangen, Germany; H.M. Ledbetter, NBS Boulder; and W. Fritz, W. Hüttner, and G. Hartwig of the Kernforschungszentrum, Karlsruhe, Germany. The carbon fiber epoxy composites are characterized by high strength (up to 4×10^{9} $N \cdot m^{-2}$), high modulus (around 4.5×10^{11} $N \cdot m^{-2}$) and low contraction coefficients (near zero), and low density (between 1.4 and 2.0×10^{3} $kg \cdot m^{-3}$). Carbon fiber composites are highly anisotropic in many of their properties. The Wisconsin group talked about their glass and glass polyester composites. Other authors also talked about various types of compsites.

J.W. Ekin talked about training of small superconducting magnets which are highly stressed. He and A.F. Clark did a number of experiments with various epoxies. Three magnets were reported. They were potted with (1) a wet layup with flexible epoxy; (2) a wet layup with stycast; and (3) vacuum impregnation with a hard rigid epoxy. The magnet with the flexible epoxy trained the worst (the thermal contraction coefficient was high), and almost as bad was the stycast magnet (the thermal contraction coefficient was low). The magnet potted with the rigid epoxy showed much less training than either of the other two. Rigid epoxy is considered by many to be unacceptable, but this experiment didn't support this theory. I think the answer is that wet layups will train more than vacuum impregnated samples will.

Applications of composite materials included a paper by O. Evans *et al.* of the Rutherford Lab on the successful testing of a glass fabric epoxy bubble chamber bellows. The component must seal the liquid hydrogen chamber, but it must also accommodate a stroke of ±3.5 mm innumerable times over the life of the bubble chamber. The Wisconsin group talked about how they propose to use composites in their 36 TJ energy storage magnet. This project presents many interesting stress problems. Westinghouse talked about nonmetallic materials selection for use in their 600 kJ pulsed energy storage coil built for Los Alamos. L.D. Michelone talked about how Itek Corporation plans to use the low thermal contraction coefficient of carbon fiber epoxy composites in a space telescope to be sent up in the space shuttle. E.W. Johnson of the University of Calgary in Canada talked about the design of a 2300 km cryogenic pipeline which is under study for piping gas from the Mackenzie River delta to Southern Canada. This paper was interesting but it is not clear that the technology is really ready yet.

The last paper of the conference was the LBL paper which I presented. The title was, "Vacuum Impregnation with Epoxy of Large Superconducting Magnet Structures," by P.B. Miller, J.D. Taylor, W.F. Wenzel, and myself. The paper was quite different from other papers dealing with epoxy because it dealt with technique, not material. I pointed out that proper epoxy technique is extremely important with the brittle types of material now available, and that the liquid properties and cure properties are also important. The paper was different enough to evoke some discussion among those who advocated the development of new materials. This conference was a very valuable one in many ways. I learned a great deal about materials which I will probably be working with in the years to come.

<u>Visit to CEN Saclay, Gif sur Yvette, near Paris, France, July 12, 1978</u>

I took the Orient Express from Munich, entering France on July 12 at about 2:00 a.m., French time. The train had traveled smoothly through the night from Strassbourg to Paris. At each station stop, there was a monotone announcement of the passage of the Orient Express. The train arrived in Paris at 7:30 in the morning. Gare du Est has changed: there are now modern baths, showers, and shaving facilities within the station. The squat type toilets are gone (thank God).

I arrived at Saclay at 9:30 in the morning, and was escorted about the site by Dr. H. Desportes, the director of the Saclay superconducting magnet group. My primary reason for visiting Saclay was to see the CELLO superconducting solenoid which will be used as part of an experiment for the PETRA machine in Hamburg. I was introduced to Mdm. Lubar, the chief engineer on the CELLO magnet. She developed the CELLO magnet conductor and did a lot of the basic engineering work on the magnet.

The first person, outside the Lawrence Berkeley Laboratory or the Oakland San office, who reads this report and calls me within six months of the date of this report, will receive a two dollar bill.

I was impressed with the CELLO magnet, which was just being put in its cryostat vacuum vessel. The magnet is about 1.5 m in diameter and 3.0 m long. The conductor is wound in a single layer on a thin aluminum alloy bore tube, and is made from two parts: a copper based superconductor (1.6 mm by 2.2 mm) is soldered to an ultra pure aluminum (2.2 mm by 9.0 mm). The copper based superconductor is wound on the outside so that it can help support the magnetic forces; a hard aluminum band is wound outside the layer of conductor for additional support. The CELLO magnet tubular cooling system operates parallel to the axis of the solenoid rather than around the axis as in the LBL TPC magnet.

The CELLO magnet had a number of shorts between the coil conductor and the bore tube, apparently caused by thin ground plane insulation or by faulty wet layup epoxy technique. The shorts were found by putting a small current in the coil; deviation in the pattern of the magnetic field indicated the presence of a short. The technique apparently is quite accurate because the locations of the shorts seemed to be known to about 5 or 6 mm. They were removed by cutting a small plug out of the thin bore tube; the plug was then filled with an alumina filled epoxy. Five or six shorts were plugged and filled.

The CELLO magnet handles cryogenic and electrical lead services in a different manner than the TPC magnet. The CELLO magnet has a special vent stack which contains the bayonet connections to the refrigerator, a liquid reservoir, and the gas cooled electrical leads. The electrical leads terminate in the liquid helium pot; they are vertical with the room temperature end on top, as they are in conventional liquid pot systems. The CELLO magnet support system is not self- centering like the TPC solenoid magnet support system.

The total cost of the CELLO magnet was not made clear. The cost quoted to me was about 6.5 million French francs (about 1.5 million US dollars at the current rate of exchange), but I am not sure whether this includes Saclay scientific and engineering manpower. I was surprised that the quoted cost was so high, particularly when compared to the TPC magnet, which is larger. The CELLO magnet project is impressive.

I gave a short seminar at Saclay to people in the superconducting magnet group and the accelerator study group. There was considerable discussion about the differences in the LBL and Saclay thin magnet design technique.

Visit to the Institute für Technischephysik Kernforschungszentrum, Karlsruhe, West Germany, July 13 and 14, 1978

After a six-hour train ride across northeastern France to Metz, Saarbrücken, Mannheim and Karlsruhe, I arrived in Karlsruhe late in the evening of July 12. My contacts at KFK in Karlsruhe were W. Heinz, P. Turowski, F. Arendt, K.P. Jüngst, and W. Mauer. I was one of many visitors at the Kernforschungszentrum following the Munich conference.

A. Magnet Work at Karlsruhe on Accelerator Magnets

The magnets on display at Karlsruhe included the a.c. dipole D-2a, which has a design control induction of 4.5 T with a maximum control induction at critical current of 5.4 T. The magnet field length is 1.3 m, the warm bore aperture is 60 mm, and the field uniformity is better than 0.002; there is a quadrupole term of that magnitude. The next multipole is the sextupole which is 0.0005 times the dipole at the warm bore aperture radius. The conductor is a 12 strand IMI cable 2.1×2.6 mm, which is solder-filled. The 12 strands are wound around a copper core, and each strand contains 100 filaments 12 μm in diameter. The magnet is pulsed at 4.6 T at the rate of 0.1 Hz.

The D-2a magnet is an old magnet, tested in 1973 and 1974, which has undergone some modification. Study of the magnet of residual field will probably commence because there is an interest in building superconducting accelerator magnets for a proton ring at PETRA. A study group is being formed with the idea of building a proton ring quickly, if possible.

B. The Karlsruhe Quadrupoles

Karlsruhe has built two types of dipoles: the picture frame type and the sector type cosine theta magnet. In addition, two different quadrupoles have been built: small quadrupoles for the superconducting linac were built and successfully operated, and a doublet was also constructed. One magnet has a gradient of 37 $T{\cdot}m^{-1}$; the other has a gradient of 74 $T{\cdot}m^{-1}$. The error in the integrated gradient is less than one part in 1000. The cold bore is 60 mm. The useful field length of both magnets is 110 mm.

A quadrupole doublet has been built for use at CERN. These magnets were successful despite many training quenches. Each quadrupole has a 36 mm cold bore. The short quad has a length of 0.95 m; its gradient is 158 $T{\cdot}m^{-1}$, and its maximum induction at the wire is 4.5 T. The long quad has a length of 0.25 m; its gradient is 175 $T{\cdot}m^{-1}$, and its peak induction at the wire is around 5.0 T. The gradient accuracy for both magnets is about 1 part in 100. The quadrupoles were tested in a 600 mm diameter, 1500 mm horizontal bath cryostat which was connected to one of the 300 W refrigerators. The many graining quenches could be made in a relatively short time. Karlsruhe has had a lot of trouble with training despite tight construction and seemingly good potting technique. The vacuum schmelze conductor is considered a leading candidate for the cause of training.

C. HELITEX and TESPE

The HELITEX and TESPE programs are fusion programs, representing the major hardware fusion components being built at Karlsruhe. Karlsruhe has become the leading laboratory in Germany for large fusion magnet technology, and with the apparent demise of the Rutherford group, it is quite likely that Karlsruhe will become the leading group in Europe on superconducting technology. Saclay, Rutherford, and CERN might deny this, but none of those laboratories enjoy the funding support which Karlsruhe is getting from Bonn. A strong deutschmark and sound German government economic policies seem to be helping German science in general and Karlsruhe in particular. The major problems at Karlsruhe are bureaucratic: the decision-making process and hardware progress appear to be impeded more by unnecessary paperwork than by lack of funding or innovation. Politics plays a role here as it would any place else.

The two major pieces of large hardware I saw were the HELITEX cryogenic system and the various pieces of hardware associated with the TESPE project. (The latter is a toroid experiment at 7 T induction. It is a smaller and lower field version of the large coil project LCP.)

HELITEX is a cryogenic distribution system for forced flow magnet systems, consisting of two major subsystems: (1) the control dewar with its heat exchanger, and (2) the object cryostat which will contain various test coils. The control dewar has an inside diameter of 1.05 m and a length of 2.58 m; the total inside volume is 1800 ℓ; the liquid volume is 850 ℓ. The control dewar heat exchanger has an area of 6.28 m^2. In my opinion, the HELITEX control dewar is larger than is necessary to supply forced flow helium to an experiment. The object cryostat is a nitrogen shielded vacuum vessel which has a utilizable inside diameter of 2.5 m; the utilizable inside height is 1.5 m. The outside diameter of the vacuum vessel is 2.8 m, and its height is 2.45 m. The HELITEX cryogenic system permits tests of various kinds of forced flow superconducting coils. Individual sections of TESPE can be tested using the HELITEX system.

The TESPE cryostat has an OD of about 3 m, and is 3 m high. The middle of the windings will be located 2.95 m off the floor. The weight of the cryostat is about 25,000 kg. The coil forms for the first three TESPE coils are under construction; the coils are about 0.95 m high by 0.65 m wide. Copper models of the TESPE coils have been wound. The three Karlsruhe coils will be wound with a broad tape-like Nb-Ti conductor which is about 60% superconductor and 40% helium. The conductor is a flat cable with stainless steel tape in the center. The conductor current density at 7 T is 7×10^7 A•m^{-2}. Karlsruhe has developed a rather complex tensioner and a device to sandblast the conductor before winding. The peak field in the TESPE toroid is about 7 T. It was expected that there would be one forced cooled Nb_3Sn coil built by MIT, but since the time of my Karlsruhe visit, the MIT TESPE coil has been cancelled by DOE.

D. The CELLO Compensating Solenoids

The main CELLO thin solenoid is being built by CEN Saclay. The field in the main solenoid will affect the beams in PETRA. There are two methods of compensation for contracting the effect of the main solenoid on the PETRA beam: one can compensate with skew quadrupoles or with solenoid fields. The CELLO experiment has chosen the latter method.

The CELLO compensating magnets have an ID of 0.6 m and a length of 0.8 m. The design central induction is around 2.8 T, and the stored energy of the magnet pair is about 1.4 MJ. The coils are quite thick, and are designed to operate at a current density of around 2×10^8 A•m^{-2}. The CELLO compensation magnets are potted (I saw one of the magnets being wheeled out of the Karlsruhe curing oven). The second magnet has been successfully tested in a bath cryostat.

The compensation magnets will be bath cooled, which is odd, considering that the CELLO solenoid is forced cooled. The cryogenic system for the three magnets (the CELLO thin magnet and its two compensation magnets) is not clear. The refrigeration system control must be interesting.

E. The Hybrid Magnet Program

P. Turowski and W. Mauer are working on two hybrid magnet systems. A totally superconducting 15 T hybrid magnet is under construction at the KFK, and study work on 30 T combined superconducting and water cooled normal conduction is also under way. W. Mauer has been particularly concerned about the inductive coupling of the various coils in a hybrid system during a quench. It is clear to Mauer and Turowski that a high field hybrid magnet system must quench very quickly if it is to survive. Inductive coupling of the various coils in the system can cause fast quenching if the coils are properly designed.

Some experimental work has been done on a small hybrid magnet (with about a 50 mm bore) which will generate a control induction of 15 T. The magnet will consist of three concentric coils. The innermost coil is made of Nb_3Sn; the two outer coils are made of Nb-Ti which runs at two different current densities. The inner coil and the outermost Nb-Ti coils have been fabricated and tested. The combined induction generated by the outer Nb-Ti and the inner Nb_3Sn coil was just over 11 T.

Some interesting effects of coupled current were observed in the Karlsruhe test hybrid magnet. The space for the third coil in the test setup (the space between the outer and inner coils) was filled with a machined pure copper cylinder about 20 mm thick, leaving about 5 mm clearance between the cylinder and the two coils. When the magnet quenched, the copper cylinder became badly deformed: it became jammed within the bore of the outer coil. What happened is the same thing which happened in the LBL thin coil's conductive bore tube: as the magnet coils quenched they became resistive. The current (or a portion of it) in the two superconducting coils was shifted to the pure copper tube. Large currents were carried in the copper, and large current forces resulted from the interaction between currents and field. Pure copper is very soft; its yield stress is low--as a result the copper was badly deformed.

The Karlsruhe experience and discussions with P. Turowski have resulted in a change of design in the TPC magnet. The ultra pure aluminum was moved from outside the coil to where it would get support from the coil during a quench. The decision has a number of implications; for example, the time for quench back will increase if the pure aluminum circuit fails.

Karlsruhe is studying a second hybrid magnet system which might be built for a laboratory in Grenoble. This magnet will consist of a 13 T Nb_3Sn outer coil which has a large bore (about 0.5 m). Inside the superconducting solenoid is a water cooled copper magnet which will generate around 17 T; the combined induction of the two nested magnets will be 30 T. This combination should permit continuous experiments at that induction.

G. Other Discussions at the KFK

I had some interesting discussions with W. Heinz concerning a second phase for PETRA. Karlsruhe is forming a study group under the direction of K.P. Jüngst to study design for a proton option for PETRA. There are apparently two schools of thought about the central induction desired in such a magnet. The first calls for a quick inexpensive ring with a control induction of 4 to 5 T. The second involves a higher induction solution (7 to 9 T). The prevailing thought is that Voss at DESY will choose the low field solution in order to out-distance the United States further. The Karlsruhe study group is supposed to issue its first report in February 1979.

I met with G. Hartwig who continues to study, in an academic way, the properties of various materials at cryogenic temperatures. Hartwig is particularly interested in the development of epoxy type resins which are ductile at 4°K. Hartwig would like an epoxy resin which is hard at room temperature and behaves like polyethylene at 4°K.

I gave a two hour seminar to the Institute für Technische Physik on July 14, 1978. I described various aspects of the LBL superconducting magnet program.

APPENDIX

This Appendix consists of a biographical listing of the papers and booklets I brought home from my trip to Europe. A trip itinerary and list of contacts is given in the first section of this report.

A. Papers given to me at the Rutherford Laboratory:

P. Clee, N. Cunlitte, J. Simkin, C.W. Trowbridge, M. Watson, "A Preliminary Design of a Superconducting Wiggler Magnet for the Dareskary Laboratory SRS," RL-75-065, Dec. 1974.

Science Research Council Central Laser Facility, Annual Report to the Laser Facility Committee, 1978, RL-78-039.

V.W. Edwards and M.N. Wilson, "The Effect of Adhesion Between Turns on the Training of Superconducting Magnets," March 1978.

"Laser Drunsen Implosion of Gas-Filled Micro Balloons," RL-78-028, Feb. 1978.

D.E. Baynham, P.T.M. Clee, N. Marks, "The 5 Tesla Superconducting Wiggler Magnet for the SRS."

R. Newport, "Specification and General Characteristics of the RCBc for the EHS," CERN/EF/EHS/TE 77-3, June 1977.

N.H. Cunlitte and R.L. Roberts, "A Superconducting Uniform Solenoidal Field Magnet for a Large Polarized Target," July 1978.

M.N. Wilson, C.R. Walters, "Development of Superconductors for Fusion Technology," RL-76-038, April 1976.

C.R. Walters, "Development of Superconductors for Fusion Technology," RL-77-131/A, Nov. 1977.

N.H. Cunlitte, P. Houzego and R.L. Roberts, "A Superconducting 2.6 T High Accuracy Magnet," RL-77-099/B, Sept. 1977.

B. Papers and booklets given to me at the 7th International Cryogenic Engineering Conference in London, England:

Publications pertaining to the conference:

"ICEC-7 Program and Abstracts"

"Addendum to the Program"

"List of Conference Participants"

"A special issue of cryogenic pertaining to ICEC-7"

"An announcement ICEC-8 in Genoa Italy"

Literature from various firms participating in the ICEC-7 Exhibition:

"BBC Brown Boveri Superconductor Data Sheet"

"CEA commisariat a l'energie atomic."

Circular 13/78 on cryopumps

Circular 14/78 on superconducting dipole CESAR

Circular 18/78 on the CELLO solenoid

"VAC vacuumschmelze superconductor data sheets"

"IMI Imperial Metals Industries superconductor data sheets"

"Sulze Refrigerator data sheets"

"Cryoservice South Yorkshire data sheets"

Papers from the conference and papers given to me by conference participants which were given elsewhere:

J.H. Coupland, R.V. Stovold, "Superconducting Hemipole Magnet," RL-77-096/13.

A series of papers published by Brown Boveri BBC:

"Insulation System for Magnets Used in Experiments for Nuclear Fusion and High Energy Research."

"Superconducting Coils for the Muon Channel System at the Swiss Institute for Nuclear Research."

"Losses in Filament Superconductor in Magnetic Fields Varying with Time."

"Supraleitende Kabel für Magnetpulen."

"Superconductors for the Magnet Coils of the OMEGA Spark Chamber at CERN."

"The D shaped Main Field Coil for the ASDEX Fusion Experiment of the Max Planck Institute for Plasma Physics at Garching near Munich."

A series of papers by H. Hillman of Vacuumschmelze, Hanau, W. Germany:

"Entwicklung harter Supraleiter, vorzugsweise an Beispiel Nb-Ti."

"Technisch Filamentleiter mit Nb_3SN Diffusions Schichten zum Erzeugen von Starken Magnetfeldern."

"Ausscheidungen und Flussverankerung bei Hochfeld-supraleitern aus Niotitan."

"New Measurements of Critical Data of Optimized Nb-Ti Superconductors."

G. Pasotti et al, "A Preliminary Design of the Toroidal Magnet System for the Demonstration Reactor Fintor-D."

J.W. Dean and W. Stewart, "Performance of a Liquid Helium Refrigerator Operated Above the Critical Temperature."

E. Eber, H. Quack, C. Schmid, "Gas Bearing Turbines with Dynamic Gas Bearings and their Application in Helium Refrigerators."

CTi Cryogenics, "Technical Memorandum, Number 91477, Model 1410 Standard liquifier.

H. Borner, D.W. Schmidt and W.J. Wayner, "A Fast Response Superconducting Thin Film Probe for Detection of Second-Sound Shock Waves in Superfluid Helium."

F. Kirchner, "Operation of the Superconducting Solenoid SUPER SOLO and its Switch."

Y. Brunet, J. Mazuer, R. Renard, "High Field Critical Current Densities and Resistive Transition of Nb-CN Superconducting Films."

F. Arendt and H. Katheder, "Cryogenic Design and Performance of Two Quadrupoles Used in the CERN Hyperon Beam."

J.S.H. Ross and R.A. Smith, "Superconducting AC Generators--Some Recent Experimental Investigations."

C.A. Wingate and C.S. Lettel, "An Ultra Reliable Miniature Cryogenic Refrigerator."

H. Laeger and Ph. Lebrun, "Cryogenic Equipment and Operation Procedures for Testing Superconducting Quadrupole Magnets for the CERN Intersecting Storage Rings (ISR)" CERN ISR-BOM/78-14.

W. Herz, W. Lehman, "Cryogenic System for the Karlsruhe Superconducting Proton Linac."

P. Buttner, G. Klipping, H.D. Denner and I. Klipping, "Experimental System for Studying Cryopumping under Operating Conditions as in Fusion Machines."

M.O. Hoenig, "Design and Development of the US-TESPE Coil."

H. Leager et al, "Long Flexible Transfer Line for Gaseous and Liquid Helium."

C. Papers and booklets from the Nonmetallic Materials and Composites at Low Temperatures Conference in Munich, W. Germany:

Conference Booklet with program and abstracts.

ICMC conference list of participants.

J.W. Elkin et al, NBS, "Effect of Strain on Epoxy Impregnated Superconducting Composites."

E. Tesler, "Flashover Behavior of Spacers at Low Temperatures."

A.C. Muller, "Properties of Plastic Tapes for Cryogenic Power Cable Insulation."

S.G. Ladkany, "Laminated Fiberglass Composites for Cryogenic Structures in Underground Superconductive Energy Storage Magnets."

E.L. Stone, L.O. Fl-Marazki, and W.C. Young, "Compressive Fatigue Tests on Unidirectional Glass/Polyester Composite at Cryogenic Temperature."

E.W. Johnson and G. Walker, "An Outline Design for a Cryogenic Internally Insulated Liquefied Natural Gas Pipe Line for Arctic Gas Recovery."

G. Claudet, F. Disdier, M. Locatelli, "Interesting Low Temperature Thermal and Mechanical Properties of a Particular Powder Filled Polyamide."

D. Evan, J.V.D. Langridge, and J.T. Morgan, "The Manufacture and Properties of a Glass Fabric Epoxy Composite Bellows," RL-78-063.

E.L. Sharpe and R.G. Helenbrook, "Durability of Foam Insulation for LH_2 Fuel Tanks of Future Subsonic Transports."

J.W. Eikin of NBS, "Fatigue and Stress Effect in Nb-Ti and Nb_3SN Multifilamentary Superconductors."

D. Drawings from CEN-Saclay, 91 Gif Sur Yvette, France:

CELLO thin solenoid drawings:

Ensemble General - Solenoide

Ensemble General - S/ENS Tirants (2 drawings)

E. Papers from the Institut für Technischephysik, Kerforshungszentrum, Karlsruhe, W. Germany.

G. Krafft "HELITEX--A Test Facility for Large Scale Cooling Experiments with Liquid Helium."

Kerntechnik, June 1978

Verlag, Karl, Thiomig, 8000 Munchen 90

H. Hillman, "Development and Manufacture of Technical Superconductors."

G. Bogner et al, "Development and Test of a Superconducting a.c. Cable."

F. Arendt et al, "Superconducting Magnets for Research Purposes."

P. Komarek et al, "The Large Coil Task--An International Contribution to the Development of Superconducting Magnets for Nuclear Fussion."

A Beck et al, "Uses of Four-fold Coaxial Corrugated Piping in Low Temperature Technology."

G. Hartwig, "Tieftemperatureigenschaften von Epoxidnarzen."

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