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FINAL REPORT OF RESEARCH SUPPORTED UNDER THE TITLE OF "LOW TEMPERATURE PHYSICS"

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I. PUBLICATIONS

As seen from the following lists, the results of this research have been published in journals and in conference proceedings which are available internationally. The titles of the following publications denote the scope of the research pursued over this 22 year period.

A. Journal Articles

1. "Experimental Evidence for Structure in the Helium II Film", Phys. Rev. 95, 279 (1954).
2. "Carbon Resistor Thermometry Between 0.3 and 2°K", Rev. Sci. Instr. 27, 240 (1956).
3. "Carbon Resistor Thermometry Below 1°K", Rev. Sci. Instr. 28, 382 (1957).
4. "Helium II Film Profile and Relative Importance of Height and Surface Pressure", Phys. Rev. 117, 39 (1960).
5. "Epoxy Resin as a Material for Constructing Cryogenic Apparatus", Rev. Sci. Instr. 32, 855 (1961).
6. "Thermal Boundary Resistance between Some Superconducting and Normal Metals", Phys. Rev. Letters 10, 287 (1963).
7. "Thermal Resistance at Indium-Sapphire Boundaries between 1.1 and 2.1°K", Phys. Rev. 235, A1028 (1964).
8. "Isothermal and Quiescent He II Film Transfer Rates over Heights to 36 cm.", Phys. Rev. 138, A1363 (1965).
9. "Effects of Deviations from Isothermal and Quiescent Conditions on the He II Film Transfer Rates", Phys. Rev. 138, A1371 (1965).
10. "Thermal Resistivity at Pb-Cu and Sn-Cu Interfaces between 1.3 and 2.1°K", Phys. Rev. 141, 615 (1966).

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11. "Isotope Effect in Superconducting Zinc", Phys. Rev. Letters 17, 255 (1966).
12. "Superconductivity of Cd-Bi Eutectic Solder", J. Appl. Phys. 38, 3667 (1967).
13. "Superconducting Transitions of Isotopes of Zinc", Phys. Rev. 164, No. 2, 565 (1967).
14. "Time Mark Generator for an X-Y Recorder", Rev. Sci. Instr. 39, 127 (1968).
15. "Magnetic Field Dependence of the Thermal Conductivity of Doped MgO", Phys. Rev. 165, No. 3, 898 (1968).
16. "Temperature and Mean-Free-Path Dependence of the Upper Critical Field in Indium-Lead Alloys", Phys. Rev. Letters 20, 1154 (1968).
17. "Specific Heat of a Spherical Type-II Superconductor", Phys. Rev. 172 No. 2, 390 (1968).
18. "Molding Epibond 100-A for Use in Constructing Cryogenic Apparatus", Rev. Sci. Instr. 39, 1214 (1968).
19. "Specific Heat of Some Polymers Used in Cryostat Construction (1.7 to 4°K)", Rev. Sci. Instr. 39, 1621 (1968).
20. "Some Thermodynamic Relations for Superconducting Ellipsoids", Am. J. Phys. 37, 519 (1969).
21. "Determination of the Ginzburg-Landau Parameters of an Indium-Lead Alloy by a Steady State Calorimetric Technique", Phys. Letters 28A, 682 (1969).
22. "Anomalous Low-Temperature Specific Heat of Austenitic Stainless Steels", J. Appl. Phys. 40, 1964 (1969).
23. "Thermal Conductivity of In-Pb Alloys down to 0.4°K", Phys. Rev. 177 No. 2, 756 (1969).
24. "Thermal Conductivity of Rare Earth Doped CaF₂ in Magnetic Fields", Phys. Rev. 181, No. 3, 1308 (1969).
25. "Isotope Effect in Superconducting Gallium", Phys. Letters 28A, 741 (1969).
26. "Low Temperature Specific Heat (1.6 to 4°K) of Six Commercial Aluminum Alloys", Rev. Sci. Instr. 40, 776 (1969).
27. "Effect of Cross-Linking on the Thermal Conductivity of Polystyrene between 0.3 and 1°K", Phys. Letters 30A, 315 (1969).
28. "An Electron Contribution to the Thermal Conduction Across a Metal-Solid Dielectric Interface", Phys. Letters 31A, 401 (1970).

29. "Evidence for Fluctuation Effects above T_c in Isotopically and Metallurgically Pure Bulk Type-I Superconductors", Phys. Rev. Letters 24, 1059 (1970).
30. "Effect of Cross-Linking on the Specific Heat of Polystyrene between 1.6 and 4°K", Phys. Letters 32A, 228 (1970).
31. "Isotope Effect in Superconducting Cadmium", Phys. Rev. B2, 4442 (1970).
32. "The Thermal Conductivity of Sapphire between 0.4 and 4°K", Phys. Letters 34A, 247 (1971).
33. "Magnon Specific Heat and Thermal Conductivity of MnF_2 ", American Institute of Physics Conference on Magnetism and Magnetic Materials, 5, 650-654 (1971).
34. "Experimental Evidence for a Low Lying Torsional Mode in Polymers", submitted to Part A-2 of the Journal of Polymer Science.
35. "Low Temperature Specific Heat of Polystyrene and Related Polymers (1.6 to 4°K)", submitted to Part A-2 of the Journal of Polymer Science.
36. "The Specific Heat (1.6 to 4°K) and Thermal Conductivity (0.6 to 1.1°K) of Stycast 2850-GT Epoxy Resin", submitted to Cryogenics.
37. "Magnetic and Thermal Properties of Austenitic Stainless Steels at Low Temperatures", submitted to the Journal of Applied Physics.
38. "Magnon Heat Capacity by Steady State a.c. Calorimetry: $RbNiF_3$ and $RbNi_{0.7}Co_{0.3}F_3$ ", to be completed and submitted.

B. Articles in Proceedings of International Conferences on Low Temperature Physics.

1. Third International Conference at Houston, Texas, December, 1953.
 - a.) "Dependence of the Linear Velocity of the Helium II Film on the Amount of Film Initially Present on the Surface". (p. 38).
2. Fourth International Conference at Paris, France, September, 1955.
 - a.) "Some Properties of the Helium II Film". (pp. 97-100).
3. Fifth International Conference at Madison, Wisconsin, August, 1957. I served as chairman of the Steering Committee for this conference and edited the Proceedings published by the University of Wisconsin Press in 1958.
 - a.) "Specific Heat of Liquid He^4 Between 0.4 and 1.5°K". (pp. 45-48).
 - b.) "Thickness of the Static Helium II Film vs. Height to 38 cm." (pp. 61-63).
 - c.) "Effect of Crystal Structure on the Intermediate State of Small Superconducting Tin Spheres." (pp. 317-320).

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4. Seventh International Conference at Toronto, Canada, August, 1960.
 - a.) "Superconducting Transition Temperatures of Titanium and Titanium Isotopes" (pp.389-391).
 - b.) "Observations of Abrupt Changes in the Thickness of the Helium II Film". (pp. 527-529).
 - c.) "Volume Flow Rate of the Helium II Film on Glass". (525-527).
5. Tenth International Conference at Moscow, U.S.S.R., August, 1966.
 - a.) "The Isotope Effect in Zinc and Zirconium". (pp. 233-237 of Volume IIB).

C. Miscellaneous Publications

1. Eight contributed papers at meetings of the American Physical Society.
2. Three contributed papers at non-international low-temperature conferences.

II. SUMMARY OF RESULTS

The titles of the above publications constitute a topical summary of results. Additional comments will be made here under the headings of "Principles of Physics" and "Instrumentation and Techniques".

A. Principles of Physics

1. The Saturated and Unsaturated "Superfluid" He^4 Film.

We measured the thickness of the saturated helium II film at 1.40°K and found $d = Kh^{-1/3}$ for values of h up to 40 cm. This work also showed that the contour of the film was a property of the film, and could not be accounted for by considering it to be an adsorbed film subjected to a varying pressure due to gravitational effects in a vertical gas column.

The volume rates of transfer R of liquid He^4 through the film out of and into glass capillary beakers over heights up to 36 cm were measured. This was done at temperatures from 1.3 to 2.0°K under highly isothermal and quiescent conditions. Previous work of low precision done in European laboratories involving measurements over heights of some one to three cm. led people to conclude that the flow rate was independent of most variables. Our work pointed out clearly the marked dependence of R on the height of the barrier above the bulk liquid supplying the film; the height of the bulk liquid supplying the film above the "sink" liquid level to which the film is flowing; the temperature; the method of filling the beaker; the volume of liquid in the beaker; and the geometry of the reservoir of liquid supplying the film. We also conducted experiments which give insight into the dependence of R on the state of turbulence of the bulk liquid which serves as the source of the film and the bulk liquid which serves as the sink. Controlled small amounts of turbulence were introduced into each by oscillating a 1 mm. diameter superconducting sphere with an external magnetic field modulated at a very low frequency. Agitation of the source liquid increases R , and agitation of the sink liquid

decreases R . A minimum agitation is required to produce a measurable effect, and R is unaffected by agitation above a maximum value. Effects of temperature differences between the source and sink liquids were also studied.

In addition to the above studies of the saturated film, we tackled the more difficult problem of studying the unsaturated film. In contact with a gas, any solid surface will adsorb one or two layers of atoms. At a temperature below about 2.18°K the thickness of the film on a surface exposed to He^4 increases quite abruptly until it is about 70 atomic layers thick at a height of 1 cm above the bulk liquid supplying the film, and it becomes mobile flowing parallel to the surface. If one isolates the surface, on which the film being studied is located, from any bulk liquid, he can remove this film layer-by-layer and study the properties of this so-called unsaturated film. These studies are very important because they enable one to study properties of so-called superfluid He^4 in quantities of smaller dimensions than otherwise possible. Our experiments showed that there are 7 thicknesses of the unsaturated film which are more stable and/or less mobile than other thicknesses. I first reported these results at the Paris Conference in 1955. Three European theorists working on liquid helium told me privately that they considered these results to be the most significant reported there---"If they are true." Since the smallest detectable amount of radiation will evaporate the unsaturated film, studies of it must be by indirect methods. Indirect as our methods had to be, our measurements justify concluding that the properties of the unsaturated film vary in a non-simple way with its thickness.

The series of He^4 film experiments on which the above comments are based is now taking on added significance. The properties of the film represent the last group of low temperature phenomena for which there is no understanding. How can a surface increase the number of atomic layers it can "adsorb" from about 1 to about 70 very abruptly as the temperature is lowered below 2.18°K ? What is the origin of the force which accelerates atoms to move vertically upward along a surface? Although our research does not answer these basic questions, it does present quite precise measurements which rather clearly define what must be accounted for "theoretically". In the past, theorists have stayed away from the film because they did not know how or where to start. Experiments such as ours will eventually point the way. Campbell, a theorist at Los Alamos, is currently making significant progress in accounting for properties of the film. In addition to our reprints of published articles, he has copies of all of our relevant and more detailed Ph.D. theses from which these articles were "abstracted". Recent advances in instrumentation make it possible for us to study the unsaturated film much more directly than before. We hope to obtain support for such studies.

2. Specific Heat of Bulk Liquid He^4 between 0.4 and 1.5°K .

Prior to our work, the only specific heat data in this region had been obtained in Europe from measurements of the heat capacity of a mixture of a paramagnetic salt and a small amount of liquid helium. We tried this method and found it highly inaccurate. Since the salt is a poor thermal conductor, thermal equilibrium is difficult to obtain. One must calculate the heat capacity of the salt from other measured constants, since it can't be measured accurately due to its low thermal conductivity. Since the heat capacity of

the salt is large compared to that of the helium present, one must subtract a calculated quantity from a measured quantity of almost the same value in order to obtain the specific heat of the helium alone. We also discovered another source of difficulty with this technique. A mixture of salt and liquid contains a large salt-liquid interface area. As one increases the temperature, we concluded that energy is required to change the "state of binding" of liquid layers near the surface. This appears as an "excess" specific heat, and, we believe, accounts for errors in some previous work. We would like to perform some controlled experiments to investigate this further.

To surmount these and other difficulties, we developed an open-capsule technique, and then an even better double-capsule technique which yielded what we believe to be quite precise data. Our data, together with that from Leiden where our open-capsule technique was later adopted, represent the best available. There are significant discrepancies among these data including those calculated at Los Alamos from the roton excitation spectrum obtained from their experiments involving the scattering of neutrons by liquid helium. For some time, we have had plans for and hope to obtain support for making some precision measurements of the specific heat of liquid He^4 below 1.5°K . Our double capsule technique together with improvements in instrumentation since our work - particularly the development of dilution refrigerators - would make it possible to obtain some very precise data. These data are of sufficient importance to justify the effort.

3. Specific Heat of Solids.

The specific heat of alloys was studied - measurements on 5 of stainless steel and 6 of aluminum were made in the range of 1.5 to 4°K . Data for stainless steel could not be fit to the usual relation, $c = \gamma T + BT^3$, but required an additional term, $c = A + \gamma T + BT^3$. The constant term was accounted for by ferromagnetic clusters acting as Einstein oscillators with Einstein temperatures well below 1°K . Such clusters are known to precipitate out at low temperatures. Values for γ are much too high - among the highest ever observed - to be accounted for in terms of the usual electronic specific heat. We showed that a variation in the ordering of localized magnetic moments in fields produced by some sort of magnetic ordering could contribute an excess specific heat varying linearly with temperature. Deviations from $c = \gamma T + BT^3$ for the aluminum samples were attributed to the formation of localized magnetic moments of the 3d transition elements in these alloys.

Our measurements of the specific heat of some type-II superconductors (some In-Pb alloys) showed that Maki's parameter κ_1 could be determined reliably from specific heat data. Since κ_1 depends on the electron coherence length in the pure material and the electron mean-free-path in the normal material, measurements of it give insight into the theory of superconductivity. Our measurements of κ_1 from 1.5 to 4°K for 3 samples with varying amounts of lead showed a temperature dependence in complete disagreement with existing theories.

The magnon specific heat of single crystals of MnF_2 , RbNiF_3 , and $\text{RbNi}_{0.7}\text{Co}_{0.3}\text{F}_3$ has been measured at temperatures of 1.4 to 5.5°K in magnetic fields

up to 33 kOe. These results agree quantitatively well with calculations based on spin-wave theory.

4. Thermal Conductivity.

The thermal conductivity of 4 single crystals of sapphire was measured between 0.4 and 4°K. Between 2 and 4°K the temperature dependence was as predicted by the Casimir theory, but there were discrepancies among the absolute values. Below 2° the thermal conductivity does not decrease with decreasing temperature as fast as expected. Indium was vacuum cast onto some of the above sapphire samples and lead onto others. We were then able to measure the thermal conduction across a metal-solid dielectric interface with the metal both superconducting and normal. When a metal becomes superconducting, a gap in the electron energy spectrum opens up around the Fermi energy according to the BCS theory. The width of this gap increases with decreasing temperature below the superconducting transition temperature. Thus, one can vary the electronic contribution to the thermal conduction by varying the temperature and thus the width of this gap. We found that the thermal conduction was appreciably greater when the metal was not superconducting indicating that the electrons do contribute to thermal conduction across such a metal-dielectric interface. Similar thermal conductivity measurements were made for interfaces between a normal metal and superconducting metal using Pb-Cu and Sn-Cu interfaces. With the Pb or Sn superconducting, the thermal resistance at the interface could be accounted for mainly by the acoustical mismatch between the 2 metals indicating that electron processes were relatively small. With neither metal superconducting, the thermal resistance decreased greatly, indicating that electrons play the dominant role in heat transfer across interfaces between normal metals.

Resonance spin-phonon absorption was observed and studied in MgO lightly doped with Fe^{++} ; CaF_2 doped with 0.001 to 1.0 atomic % of Dy^{3+} ; CaF_2 doped with 0.001 to 0.1 atomic % of Ho^{3+} ; and CaF_2 doped with 0.1 atomic % of Ce^{3+} . Measurements on these samples were taken from 0.4 to 4.2°K in magnetic fields of up to 40 kOe. Thermal conductivity in such samples is by phonons. Excitation of spin states removes phonons from the heat current which appears as a reduction in the thermal conductivity. With knowledge of the spin states and their dependence on magnetic field, one can calculate the change in thermal conductivity due to this spin-phonon interaction. Our measurements could be accounted for by existing theories except at the higher temperatures.

5. Polymers.

Velocity of longitudinal and transverse sound in and specific heat of a series of polystyrenes of different origin and molecular weight, of α - and ortho-substituted polystyrenes, and of polystyrenes crosslinked with different amounts of divinylbenzene have been measured between 1.6 and 4°K. The thermal conductivity of some of these samples have been measured from 1.6° to 300°K. The specific heat of all samples shows a temperature dependence which can not be accounted for by assuming a Debye frequency spectrum for the vibrational

modes. To account for this excess specific heat, we first fit the data with a Debye T^3 term plus an Einstein specific heat with a characteristic temperature of 15 to 18°K. We suggested that this localized frequency mode may have its origin in the one-dimensional nature of the polymer chain. We made some calculations of the length of a polystyrene chain necessary to obtain these characteristic temperatures, and obtained reasonable agreement with the number of Einstein oscillators needed in the samples to account for the data.

Additional sound velocity measurements led to a wholly new and more promising approach to the problem of accounting for this excess specific heat we observe for polystyrene and observed by others for other polymers. We can account for our data plus that obtained by others for some other polymers by postulating the existence of a fourth mode with a velocity close to that measured for the two transverse modes. We suggest that this fourth mode is a torsional mode arising from the torsional coupling of the side groups along the backbone chain of the polymer molecule.

6. Isotope Effect in Electrical Superconductors.

Our work on the isotope effect in Zn, Ga, and Cd enormously increased the precision of such measurements, and contributed significantly to a better understanding of the superconducting-to-normal transition. For example, work on Zn^{64,66,68} by Geballe and Matthias prior to our work gave zero field transitions each of which was about 10 to 20 millidegrees wide. From such data it was impossible to assign transition temperatures with sufficient accuracy to determine a meaningful isotope effect. They concluded from their measurements that for Zn, $T_c \propto M^{-0.5}$ in agreement with the BCS theory. All transitions for all 3 sets of isotopes, Zn-Ga-Cd, in our work on which conclusions were based were in general less than 1 millidegree wide and most were much, much less than 1 millidegree wide. Our work for Zn showed $T_c \propto M^{-0.37}$. Our work on Ga, the only work done on this element, showed $T_c \propto M^{-0.41}$. Our work on Cd showed $T_c \propto M^{-0.385 \pm 0.015}$. It turned out that concurrent with our work on Cd, some work was being done on Cd at Zurich. They reported $T_c \propto M^{-0.32 \pm 0.07}$. As seen, our work was of much greater precision. The standard deviation of our measured points from the fitted line, from which the magnitude of the isotope effect was determined, was 0.06 millidegrees.

The excellence of our work on Zn and Cd is due largely to the pains-taking work of R. E. Fassnacht in purifying these isotopes to an extent unmatched by any other work. As received from Oak Ridge these samples were useless for isotope effect work. Their residual resistivity ratios, $r = \rho(4^\circ\text{K})/\rho(297^\circ\text{K})$, were about 1/10. After repeated vacuum sublimations, one distillation, and growing each sample into a single crystal, r was reduced to about 3×10^{-5} .

With these very pure samples we were able to discover why other peoples' transitions were so broad (often meaninglessly so); to point out a proper technique for making such measurements; and to contribute to a better understanding of electron-pair-breaking mechanisms responsible for the superconducting-to-normal transition. In short, our work showed that if measurements were made in zero magnetic field (and zero field data are needed to compare with theory); if there were no impurity atoms present; if all atoms had the same isotopic mass; and if the sample was a perfect crystal; the transition would be tremendously broad. Some of our best samples showed widths of 40 millidegrees in zero

field, yet they were less than 1 millidegree wide in fields of 0.1 to 0.2 oersteds. Thus, on each sample one must take data as a function of magnetic field H from several oersteds down to much less than 1 oersted and then extrapolate to zero field - a very lengthy process involving very precise work.

We also observed that for a given H , the less atomically and/or isotopically and/or physically pure samples showed the sharpest transition. Thus, in earlier days, samples were sufficiently impure to give, even in zero field, reasonably sharp transitions, although meaningless for isotope effect work. Thus to obtain a sharp transition - one taking place within a very small range of temperature - one needs some of the following 4 electron-pair-breaking mechanisms: a magnetic field (much less than 1 oersted is sufficient), some impurity atoms, some atoms of different isotopic mass, or deviations of the sample from being a perfect crystal. Increasing vibrations of the atoms in the crystal lattice, by increasing the temperature, will also break the electron pairs responsible for superconductivity. However, without some of the above 4 pair-breaking mechanisms, also acting, the transition will not take place at a characteristic and definable temperature.

Theories by Aslamazov and Larkin suggest that the electrical conductivity of a superconducting material in the normal state increases, as the temperature approaches T_c , because of fluctuations in the system. However, their theory suggests that in pure bulk material this is appreciable only for $T - T_c$ of about 10^{-15} degrees. Our measurements show that this increase in conductivity under excellent conditions can extend over tens of millidegrees. We suggest that this discrepancy may be explained by increased electron pair lifetimes, due to reduction of phonon scattering in the best of samples; and it is the phonons which mediate the electron pairing. This would allow higher order processes to become significant. These processes should be investigated further.

The study of aluminum would be very revealing in these regards. Single crystals of very high atomic purity can be obtained. Since Al has only one isotope, there would be no isotopic impurity. Thus, measurements in zero field should show an extremely broad transition. We were cooling down our first sample of aluminum, when our building was bombed at 4 a.m. Monday, August 24, 1970, killing Dr. Fassnacht and completely destroying my laboratory and the equipment necessary for the above type studies. To minimize thermal gradients giving rise to stresses which cause leaks, we often cooled our refrigerator slowly over a weekend to be ready to take data on Monday. I had checked on the apparatus and its cooling rate on Sunday morning, and my wife came with me at 7 p.m. Sunday to check again. Had the revolutionists decided to bomb it then, they would have got both of us. Fassnacht was going to check on the apparatus soon after midnight. His wife said he overslept a bit and did not come until 2:30 or so. He had finished checking the apparatus and adjusting the cooling rate and was out in the hall visiting with the night watchman and Mr. Schuster, both of whom were seriously injured but not killed by the bombing. The night watchman said he must go on, and Fassnacht said he too was going to leave. However, to make one last check on the apparatus, he went back into the lab where he was crushed by concrete rubble. Our lab was next to where the truck load of explosives was parked on a driveway just outside.

In discussing above why our isotope effect work was better than that of others, I should have mentioned that it was due in large part to the very special refrigerator and apparatus we had developed for work below 4°K - particularly for work below 1°K . We started developing and building this refrigerator in 1959, soon after He^3 became available in sufficient quantity to use as a refrigerant in work below 1°K . This was designed to operate from a 4.2°K bath to permit runs of long durations. Other refrigerators required a bath to be pumped to 1°K . Thus, to replenish the bath required warming the low temperature section up to 4.2°K . This refrigerator had a high cooling rate and large experimental chamber - 14 cm diameter x 22 cm long. This large chamber made it possible to study 3 isotopes at once and have each one separated by about 7.5 cm from any other one. Thus, distortion of any applied magnetic field due to the transition of one sample would not affect significantly the field at the site of any other sample. This proved to be valuable for our very low field measurements. The large cooling capacity made it possible for us to immerse all three samples and the thermometer in separate chambers, which were filled with and interconnected by liquid He^4 . The good thermal conductivity of this liquid provided excellent isothermal conditions.

We also studied the isotope effect in Ti and Zr. Our work on Ti showed dramatically and painfully the effect of imperfections in crystal structure. Matthias of Bell Labs, where they then had no facilities for work below 1°K , obtained the first separated isotopes of Ti and loaned them to us to study. They were in the form of pellets which obviously had large internal stresses. They could be reduced to a size to fit into our chambers by "shattering" them by hitting them lightly with a piece of drill rod. However, the thinking then was that such stresses were insignificant. Although the transitions were expected to vary but slightly around about 0.39°K , we found the transitions of these samples to be at 0.22, 0.36, and 1.11°K . They were also in the wrong order for any sensible isotope effect. The great pain came from the fact that work below 1° requires a change in technique from that above. Since we could not conceive of a transition being above 1° , and did not at first look there since we did not expect any transition to be anywhere near 1° , we were puzzled for some time as to why one sample appeared to have no transition. As discussed above, our work clearly shows that the transition is very, very sensitive to many things. Our subsequent work on Ti and Zr has been somewhat the use of our transition temperature measurements as a metallurgical analysis tool - a tool to motivate and assess the progress of metallurgical work on these elements at Oak Ridge. At the time of the bombing, we had 3 Zr and 2 Ti samples from Oak Ridge, which represented great (hopefully sufficient) improvement in sample quality - the third isotope of Ti was delivered after the bombing. By sifting through tons of rubble, we found all of these samples except Zr^{94} .

We hope to obtain support to resume this work and other work requiring a refrigerator like that we had developed and built. We have not started rebuilding this refrigerator, since there are at least 2 commercial concerns currently marketing refrigerators. Although presently available units are not a suitable replacement for what we lost, an adequate one should soon be available at a price less than what we could build it for. Although I can not conceive of a commercial unit being better for our work than the one which was completely destroyed, a commercial unit should soon be available, which will be adequate, and should incorporate some better engineering than one we could build.

B: Instrumentation and Techniques

As seen from the titles of some of our publications, we made significant contributions to the development of instruments and techniques for research at very low temperatures. The first He^3 refrigerator was described by the Los Alamos group at the 5th International Low Temperature Conference here in 1957. It was clear that such were destined to replace adiabatic demagnetization techniques we were then using for research from 1° to about 0.35°K . As described above, we pioneered in the development of such refrigerators starting in 1959 as soon as He^3 became available to non-government laboratories in sufficient quantities for use as a refrigerant.

Starting in September, 1950, my AEC contract was one of the very early ones in support of low temperature research. While at the M.I.T. Radiation Lab during the war, I became aware of the development of the "Collins Liquefier" by Prof. Collins and the A.D. Little Co. It was evident that this liquefier could make liquid helium available at many places and thereby open up a most fruitful field of research. Upon joining the staff here in 1947, I started laying plans for such research - including competing for space, getting about \$23,000 from the University with which to purchase the liquefier, and getting it delivered and installed. This was completed by September, 1950.

While pointing out some of the contributions to instrumentation and techniques we made, I shall point out some of the prejudices and "hardships" we had to overcome. In several cases, without independent AEC support, I would not have been able to overcome these. My comments in this regard constitute very well deserved compliments for the AEC, the very high quality of its personnel, and its method of operation. All site visitors during my 21 year connection with AEC have been helpful, competent physicists who never once told me what to do nor objected to my proposals - even when they included trying to do things which had never been done before. I can not say the same for members of my department here. My comments in this regard will also reflect something of the history of the rapid development of work at low temperatures. I believe no field has ever developed as rapidly as this one. In 1950, one did not store nor transport liquid helium from one place to the other - just producing low temperatures and establishing a temperature scale for their measurement was an end in itself - so much so, that one who did it was called a "low temperature physicist". Some 12 to 15 years later, techniques for producing and measuring such temperatures had become quite routine and used by a wide variety of scientists. My department still lists me as a "low temperature physicist". However, this is mainly a reflection on my age - meaning only that I did work at these temperatures when just producing them was somewhat of an end in itself. Now, some 8 other professors in our department and many in other departments do research at these temperatures and are not classified as "low temperature" scientists. They started work at these temperatures after liquid helium became available "on tap" as is liquid nitrogen, distilled water, etc.

To start with, in 1950 my department could spare barely enough space in the basement for the heavy liquefier. I was given a room on the second floor for research. I did not object, for I had designed a 5 liter liquid helium storage and transfer vessel and had ordered stainless steel for it. However,

my department invited an "expert" to come here and "check" on me. To be an expert in those days one had to have worked in a European lab for a while, where research at low temperature started first. This expert told the department that if I intended to liquefy He in the basement and carry it up 2 floors, I did not know how to handle the stuff. True! I liquefied the first helium I ever saw. He had been to Leiden where He was neither stored nor transferred, and after all, Sam Collins thought it could not be done and had designed his liquefier with no provision for removing any liquid. At some loss in efficiency, provisions are made in this liquefier for doing experiments only with apparatus mounted down in the bowels of the liquefier. A transfer tube was built into the liquefier only after the first models. Yet, my straightforward heat-loss calculations showed storing and transferring liquid He to be quite possible. I was one of the first to take liquid from one place to another. I took some to various schools around the state for demonstration lectures. Prior to this, I know some of the M.I.T. boys had taken some down Mass. Ave. to show people at Harvard. Now, for several years it has been trucked around the country in 10,000 gallon trailer trucks, and is so readily available it is cheaper to buy it in the liquid phase than to liquefy your own.

This prejudice against storing and transporting liquid He resulted from the fact that the Europeans did not do it. They did not do it because in doing so they were afraid they would lose an atom of He of which they had so little - having to get it from the atmosphere - not having gas wells as we do. This fact was made dramatically clear to me when I visited W. Meissner's lab in Herrsching, Germany, in 1955, in connection with my going to the Paris conference to present a paper - a trip made possible by AEC Funds for which I am grateful. There, for all the many years of his great work, he had a total amount of He equivalent to about 150 cubic feet of gas at NTP. Seldom is any work done here that does not blow away into the atmosphere an amount of He greater than this in a single experiment. Soon after that a colleague of Meissner's visited labs in this country. He stopped here first and immediately asked to see some He and to see us transfer it. I shall never forget the expression on his face as he saw liquid collecting in a glass dewar and gas escaping into the atmosphere. However, Americans, coming previously from visits to Europe, seeing them not transfer or store liquid He, concluded that they did not because it could not be done, rather than because they were afraid they would lose some. Such Americans, thinking of the relatively very low heat of vaporization of liquid He and its very low boiling temperature, jumped to the conclusion that many things could not be done with it - basing such conclusions on observations of what the Europeans did rather than making calculations. I suspect my department would have got rough with me then had it not been for my AEC support. I suspect the senior members felt that if AEC was foolish enough to support me in doing the "impossible" they should not interfere.

AEC support was most valuable to me soon after the above bout with "prejudice" against transferring and storing liquid He. I had asked the University Research Committee for \$1,500 with which to buy a high capacity pump, for use in connection with instrumentation for measuring the specific heat of liquid He⁴ below 1°K in an adiabatic demagnetization refrigerator we had built. This was refused and my proposal almost got me kicked out of the department. Our departmental representative on the Research Committee and our departmental chairman had asked another "expert" who said specific heats -

even of liquid helium which are large compared to solids - were just too small below 1°K to measure. One senior colleague said to me, "a good man would not propose to do the impossible". Prior to that time most all studies below 1°K had been of paramagnetic salts which were cooled in the adiabatic demagnetization process. True enough, heat exchange with such salts is difficult. However, it looked very possible to me. We pioneered in such work and our open-capsule and double-capsule techniques not only enabled us to obtain good results for liquid He, but represent significant contributions to research techniques for such work in themselves. Now, specific heats of most all solids, of significance in this respect, have been measured and are rather well known down to temperatures of about 0.1°K . AEC provided funds with which to buy the needed pump for which I am grateful.

When we started our work below 1°K , measurement of the susceptibility of a paramagnetic salt was the only "approved" method - established again by the Europeans - for setting up a temperature scale. Since the susceptibility of pyrex glass, stainless steel, etc. are too large - even though small - we had to build our apparatus out of soft glass. European glass blowers are expert at working with soft glass, but American blowers (even those at the H.S. Martin CO.) won't touch it. They directed me to a retired German glass blower in Evanston, Illinois, who still did some work in his garage. So, I used to take boxes of complex, soft glass apparatus on the streetcars of Chicago out to his garage. I could make this apparatus out of thin wall stainless tubing much more compactly and with much less heat leak into the experimental chamber than out of this relatively thick wall soft glass. However, susceptibility thermometry was not possible with stainless steel. We were among the first to work with carbon resistors below 1°K , making the use of stainless steel apparatus possible. We calibrated these resistors against the vapor pressure of He above 1° and extrapolated below 1° . These turned out to have such a very large dR/dT as to make them ideal for specific heat and other work below 1°K . When Clement of the Naval Research Lab and I told of measurements based on such thermometry at the 1955 Paris Conference, we were almost laughed at. One physicist from Leiden said essentially, "How can any knowledgeable low temperature physicist insult us with such thermometry?" Carbon resistor thermometry has been used all over the world for such work now for several years. One peculiar turn of events still remains. As seen from some of our publications, in those early days one had to get several carbon resistors from Allen-Bradley and sort out the "bad" ones to obtain a good one. The bad ones would show all sorts of erratic behavior - sometimes almost discontinuous responses - when used as thermometers. Each of my students had 2 or 3 so selected resistors which he hoarded away for his personal use. Then, even the good ones changed their calibration significantly after being warmed to room temperature. Apparently the Dutch had based their condemnation of carbon resistors on the erratically behaving ones and were unwilling to accept, as a thermometer, a device which had to be sorted out from several. Now, for some years every resistor one gets from Allen-Bradley works perfectly as such a thermometer, and on successive cool-downs after being warmed to room temperature the calibration changes but very little. I have talked with the Allen-Bradley people with no success in determining what is responsible for this happy turn of events.

We also pioneered in the construction of apparatus for low temperature research out of epoxy resins such as Araldite and Epibond 100-A. This material has many advantages over stainless steel, glass, etc. It is non-magnetic and non-electrically conducting. Thus, changes in applied magnetic field do not induce eddy currents which add unwanted heat. We were able to machine apparatus from this material to have very thin walls. With this, thermal conduction heat leaks were very small. To be able to machine it to small thicknesses, we had to develop a method for casting it essentially free of gas bubbles, which the manufacturer said could not be done. Such materials also have the very great advantage of being able to be sealed together at room temperature, whereas, glass requires heating to a high temperature to fuse the glass, and metal apparatus must be heated to solder it together. In addition, for such work, usually some 10 to 20 leads must be brought out of the experimental chamber. There is not room for glass-to-metal seals with this many feedthroughs, and such require high temperatures to solder them on. With apparatus made of these epoxies one can drill many small holes into the chamber - insert leads - and seal them in with more epoxy all at room temperature. We also found Stycast 2850-GT to be useful as a sealant. As our publications show, we measured the specific heat and thermal conductivities of these materials - data which are needed in the design of apparatus using them.

Yes, we 'missed the boat' sometimes - at least twice. In the very early days, we wound a small magnet out of superconducting Nb wire before such could be obtained coated. We strung it back and forth in a room and painted it with glyptol varnish. We had occasion to unwind and rewind it a few times. Each time we did, we found the critical current to be higher. We thought this was nonsense, but it turned out to be good physics. Laverick at Argonne later published an article on this, and superconducting wire is now supplied pre-work-hardened.

Also, in the early 1950's, Soviet physicists, using bismuth probes, showed that, upon applying a magnetic field, flux re-enters a superconductor in a non-continuous way. It appeared to us as if flux re-entered in some form of quantized fluxoids - appearing as if a certain volume of material was needed to sustain one of these fluxoids. They had worked with large spheres with slots into the equator into which they could insert probes to map the re-entry of the flux. We thought the way to investigate such was to study the variation in magnetic moment of very small spheres as a function of applied field. We studied tin spheres of about 1 mm. diameter. Magnetic moments were measured by oscillating these spheres slowly between 2 pick up coils. With a little more insight and/or more study of Fritz London's papers we should have discovered flux quantization.

III. STUDENT TRAINING AND OTHER EDUCATIONAL ASPECTS OF THIS WORK.

A. Students Trained to the Ph.D. (Their current positions are given.)

1. W. C. Knudsen, Staff Scientist, Lockheed Research Lab., Palo Alto, Calif.
2. P. J. Fleming, Professor of Physics and Dean of the College, Wellesley College, Wellesley, Mass.

3. D. C. Pearce, Sr. Research Scientist, E, T, and D Laboratories, Ft. Monmouth, New Jersey.
4. T. L. Finch, Assoc. Prof. of Physics, St. Lawrence University, Canton, New York.
5. J. H. Werntz, Jr., Prof. of Physics and Director of the Center for Curriculum Studies, Univ. of Minnesota, Minneapolis, Minn.
6. A. H. Markham, Associate Director of Projects Sponsored by the American Society for Engineering Education, Washington, D. C.
7. O. T. Anderson, Assoc. Prof. of Physics, Bucknell University, Lewisburg, Pennsylvania.
8. R. G. Netzel, Acting Deputy Director of Education, American Association for the Advancement of Science, Washington, D. C.
9. L. J. Barnes, Member of Teaching Staff, The Marlborough School, Los Angeles, Calif. - formerly Senior Physicist, Atomics International, Canoga Park, Calif.
10. D. A. Neeper, Staff Member, Los Alamos Scientific Lab., Los Alamos, New Mexico.
11. R. W. Selden, Physicist, Lawrence Radiation Lab., Livermore, Calif.
12. D. M. Jones, Staff Member, Los Alamos Scientific Lab., Los Alamos, New Mexico.
13. R. E. Fassnacht, Deceased - killed by the stupid bombing of our physics research laboratories by revolutionary students at the University of Wisconsin on August 24, 1970.
14. G. T. Fox, Assistant Prof. of Physics, University of Santa Clara, Santa Clara, Calif.
15. Paul Zoller, Neu-Technikum Buchs, CH-9470 Buchs, Switzerland.
16. B. L. Berman, Asst. Prof. of Physics, Wells College, Aurora, New York.
17. M. W. Wolfmeyer, Asst. Prof. of Physics, Winona State College, Winona, Minn.
18. D. H. Liebenberg, Staff Member, Los Alamos Scientific Lab., Los Alamos, New Mexico.
19. R. P. Madding, Institute for Environmental Studies, Univ. of Wisconsin, Madison, Wisconsin.
20. D. L. Fehl, he has received some support from these contracts to date and will complete work for the Ph.D. within the next few months.

B. Training of Other Students.

Five other graduate students worked in our group on projects supported by the above AEC contracts. They later transferred to other groups where

they applied low temperature techniques learned with us on their Ph.D. thesis research. One student received valuable training with us before being drafted into the Army. Two graduate students received valuable training with us and now have positions of responsibility, but were discouraged from continuing to the Ph.D. because of poor performance on our written examinations. Some 4 or 5 undergraduate physics majors have worked with us as hourly employees for short periods of time. This experimental research work served as a valuable supplement to their course work.

C. Other Educational Aspects.

Starting in 1950, AEC support made it possible for me to have one of the early laboratories in the country in which liquid helium was available, and to have facilities for doing research at temperatures below 4°K. By that time the Europeans had discovered many of the "startling" phenomena, and serious, systematic study of them was just beginning. These phenomena were of great interest to many people with various levels of understanding of physics. Indeed, just to see some liquid helium, to be assured it was at a temperature of about -450°F, to see some solid "air", etc. was intriguing to all. After one demonstration lecture at a meeting of college physics teachers, which involved some properties of superconductors and superfluid helium, a teacher from Princeton University said, "I can die happy now that I have seen some of these things which have intrigued me for so long."

The above circumstances led me to give many general lectures on the properties of matter at temperatures near absolute zero. A quick look at my University Activities Reports indicates that I have given over 60 such lectures. They have been at places such as institutes for high school science teachers, seminars for college teachers, special colloquia at colleges and universities, dedications of science buildings, National Science Foundation summer institutes for high school and/or college physics teachers, etc. To about 1/2 of these I took some liquid nitrogen and liquid helium and included demonstrations of some properties of superconductors, superfluid He, etc. as part of the lecture. Although all of these lectures were given as part of my duties here at the University without financial support from AEC, support by AEC of my low temperature research enabled me to gain the knowledge, techniques, and experience which made such lectures possible. Since most of these audiences were science teachers, describing and demonstrating phenomena characteristic of atoms at low temperatures, and discussing the extent to which results of research at low temperatures have amplified and clarified principles of physics categorized under heat-thermodynamics-kinetic theory- and statistical mechanics, have constituted a significant and beneficial influence on the general education of our people. In addition to imparting factual knowledge, such lectures should have also pointed out the far-reaching and often unpredictable value of pure research.

IV. AN ADDENDUM.

I add these comments here in hopes that resolution of some current international tensions may be facilitated by a further understanding of how barriers - often false ones - have been created between the Soviet Union and

our Country in the past. The Steering Committee in planning for the 5th International Low Temperature Conference here at the U. of Wisconsin in August, 1957, applied for and received a grant of \$5,000 from the AEC in support of this conference. When some AEC "business agents" asked me, as chairman of this committee, how we would use these funds, I said to help defray expenses of physicists from foreign laboratories and included the Soviet Union. I was told that so long as it was "even our intent to invite some Soviet physicists", our grant would be withheld. I was also told that these were orders from the State Department. By letters and telephone I could not resolve this with the State Department. Feeling sure I could get this order reversed, and knowing the Soviet Scientists should know of the conference and our intent to extend formal invitations soon in order to arrange to come (we had already extended invitations to all others), I took it upon myself to call Kapitzka. We had a most cordial conversation. He told me to talk with Topchev, then Head of the Soviet Academy of Science. I called him while he was eating dinner - the only 30 minute interval during which I could place a call to the Soviet Union. He did not speak English. Instead of his saying sharp words to me for assuming he spoke English and not having an interpreter with me, he told the operator to tell me to call back the next day and he would bring an interpreter home with him. I did, and we had a very cordial conversation.

At the next meeting of our steering committee - during the APS Spring Meeting in Washington - I reported my calls to the Soviet Union. J. Howard McMillen of the National Science Foundation and a member of our committee first doubted that I could do this. Then he asserted that I had to be investigated before they would let me talk as had the Russians. When I assured him I was put on the line as soon as I contacted the operator, and the Russian being called started talking as soon as connections were made, he was amazed and said he would not have done what I did for the world. He assured me that my calls would have been recorded by our Government and serve as a black mark against me forever.

While in Washington, I spent much time working my way through people in the State Department. This was particularly difficult since some came late in the morning - left early for lunch - and at 2 or 3 p.m. were still not back from lunch. When I raised my voice, after each told me this was a Presidential order but could not show it to me, I would get referred to another office. At the fourth office I got a promise that they would look into the source of the order banning invitations to the Soviets.

It turned out that a federal agency had itself arranged earlier for a group of Russians (architects, I believe) to visit here, and there were some embarrassing incidents in Ohio, I believe. In connection with this, the New York Times had reported the President as having said that federal agencies should not themselves take the initiative in inviting whole groups of Russians here. The State Dept. was basing its order on what they had read and misinterpreted in the N.Y. Times. After this was cleared up, I quickly got permission to invite Soviet Scientists through official channels. However, they replied that it was late for them to plan to come, and that the end of August was a bad time for them to leave their work there.