Invited paper presented by Horst Meyer at the session U5 "Low Temperature Physics: A Historical Perspective", March 16, 2006, American Physical Society Meeting in Baltimore, MD

Fritz London's legacy at Duke University.

Fritz London came to Duke from the College de France, Paris, in the fall of 1939, when the World War II was just starting, and therefore he was able to escape persecution from the Nazis. His first years in his adopted country were difficult, and he felt isolated in a place so unlike what he was used to. However, with the successful completion of the war, old personal contacts were revived. Interaction with colleagues in the US. and in Europe, in particular Tisza on the two fluid model of liquid helium, and Oscar Rice in the neighboring University of North Carolina at Chapel Hill was going strong. Here is a picture of the London brothers Heinz and Fritz in 1953 in Cambridge, England.

Heinz and Fritz London (1953)

Also London's interest in liquid helium was considerably heightened by the new availability of ^{3}He in limited quantities from Los Alamos National Laboratories. In 1948 he published an article with O.K.Rice in Physical Review with the title

"On Solutions of ^3He in ^4He", and in 1949 an article in Nature, "The rare isotope of helium, 3He: a key to the strange behavior of liquid 4He". There were many ideas London wanted to see tested experimentally, but at that time foremost in his mind was the dependence of superfluidity on statistics, and whether or not ^3He was to become superfluid or would behave like an ideal Fermi gas.
Viscosity measurements on liquid ^3He at Los Alamos had shown no sudden drop down to temperatures of 1K, indicating the absence of superfluidity and London was extremely interested in the behavior of the specific heat and the nuclear susceptibility, and whether they would show the effects of Fermi-Dirac degeneracy.

London's interests are discussed in detail in the biography by Kostas Gavroglu, among them his reaction to the Landau approach to the theory of liquid ^4He, which he had great difficulty to accept. I will not delve into this chapter of London's scientific life, which was rather difficult for him. His work was only rarely acknowledged in papers published by Soviet authors.

I invite you to visit the Duke webpage of Fritz London which has various links, http://www.phy.duke.edu/people/FritzLondon/.

In 1952, to the great pleasure of London, William Fairbank was appointed as an Associate Professor at Duke University. He was strongly recommended for this position by Walter Gordy, of the Duke Physics Department, one of the pioneers in microwave electron spin resonance, who knew Fairbank during the war years when both worked at the MIT Radiation Laboratory.
Fairbank had gone to Yale in 1944 for his doctorate where he became closely associated with the work of Heinz London. With his experience of perfecting radars at MIT he decided to search for alternating current losses in super-conductors. Immediately afterwards he started working with C.T. Lane who, together with William's brother Henry was engaged in measuring the velocity of second sound and other transport properties in ^3He-^4He mixtures. He then spent two years at Amherst College, before his appointment at Duke.

A Collins helium liquefier was purchased in 1952, and Fairbank got to work setting up his laboratory with the help of graduate students, William Ard, nominally a student of Gordy, King Walters and Gene Lynch.

William Fairbank with new Collins liquefier (1952)

Here is a picture of Bill Fairbank dressed up formally, looking at his new liquefier. As most of you know, in this liquefier, the pressurized gas cools while doing work on a pair of moving pistons and is further cooled by a Joule-Thompson expansion, after which a fraction liquefies, and the rest is recycled.

Since George Zimmerman assigned me to talk about the experimental techniques in the fifties, let me digress a moment to compare this modern method of helium liquefaction with that used in the Clarendon Labs in Oxford, where I started postdoc research in 1953. This laboratory was a very large and productive LT center, as were also the Mond Lab. at Cambridge University, the Kamerling Onnes Lab in Leiden, and also Paris and Grenoble. A most important facility, at least in Leiden and Oxford, was an excellent glassblower workshop, where the various dewars were produced, and where the glassblowers were essential in constructing – and repairing - the glass vacuum systems in the racks connected to the individual cryostats.

The production of liquid helium at Oxford was done on a small scale in each individual cryostat (!) , and used an inbuilt Simon liquefier. The large laboratory hydrogen liquefier distributed twice a week the refrigerant in balloon-shaped 4-liter glass dewars. From these the liquid was syphoned via a glass vacuum-jacketed transfer tube (!!) into the glass dewar containing the cryostat.

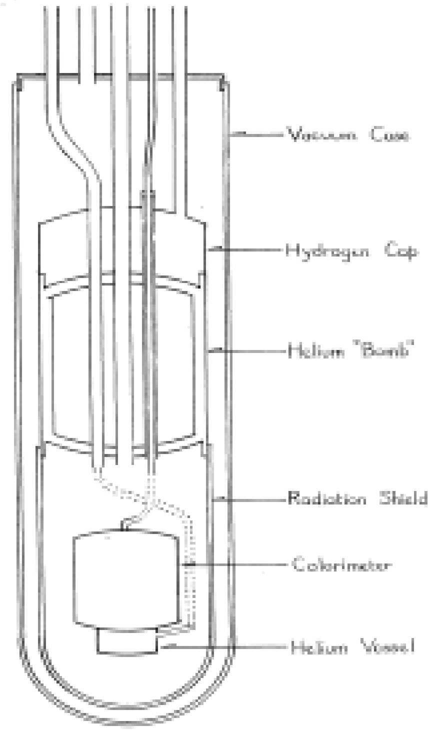

FIG. 2.1 GENERAL PURPOSE MACHINE.

Ph.D thesis Barry Ricketson
Solid H_2 calorimetry above 1 K
1956 Clarendon Lab, Oxford U.

Cryostat , Clarendon labs, Ricketson thesis 1956, fig.2.1

The latter included the Simon liquefier and the experimental cell. Here is a picture of this arrangement taken from the thesis of Barry Ricketson, who did calorimetry of solid H_2. Over night, the cryostat was cooled to liquid nitrogen temperature. In the morning, the dewar was then emptied and refilled with liquid hydrogen, and the cryostat slowly cooled to 20K. The Simon thick-walled "bomb", filled with helium under pressure of 120 bar was then cooled to about 12 K by pumping on the condensed H_2. Then the helium was adiabatically expanded, and partially liquefied at 4.2 K. Helium gas from a low pressure vessel, after being cooled via a heat exchanger, was then condensed into the cryostat by thermal contact with the Simon bomb. This liquid sample, usually of the order of 20-50 cm^3, was then used in the experiment, and could be cooled by forced evaporation to 1.0 K. Some time after lunch, the experiment could be started, and usually the helium lasted until early next morning. During this time, there were several transfers of liquid hydrogen into the glass dewar. And so we worked through the night. If we were lucky in getting more liquid hydrogen from groups whose experiment had "broken down", we could extend the duration our run.

There was also a larger Simon liquefier in a distribution room. In 1956 I was the first in the Clarendon Lab to build a metal dewar with a liquid nitrogen outer bath, and requested 1 liter of liquid helium from the large Simon to be transferred into my dewar I then carried to my cryostat which was then slowly cooled to 4 K. The helium then lasted some 24 hours in my demagnetization cryostat.

Now back to Duke. By spring 1953, there was liquid helium available and temperatures of 1K could be reached. London had many questions for Fairbank he wanted to be tested experimentally, the most pressing of course being whether ^{3}He behaved like an ideal Fermi-Dirac gas. Fairbank had already discussed this with Onsager at Yale, and it was decided to measure the strength of the ^{3}He nuclear magnetic resonance signals, in other words the nuclear paramagnetic susceptibility, as a function of temperature. Particles of an ideal F-D gas were expected to have increasingly antiparallel alignment as the temperature was decreased, which caused the susceptibility to deviate from the classical 1/T Curie law and become temperature independent when all the spins became aligned. The first measurements in '53 down to 1.2 K showed Curie's law to be obeyed. London was very interested in the results, and apparently used to come frequently, standing at the door of the lab and waiting impatiently for every datum point. After the construction of a cryostat with magnetic cooling, the liquid at saturated vapor pressure was found to show a clear deviation from Curie's law and could be fitted to the theoretical curve for a Fermi-Dirac gas with a degeneracy temperature of 0.45 K. The measurements were completed in February 1954 and confirmed London's expectations, only a few weeks before his death of heart failure on March 30.

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Fermi-Dirac Degeneracy in Liquid He³ below 1°K*

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(Received June 1, 1954)

FIGURE 5.

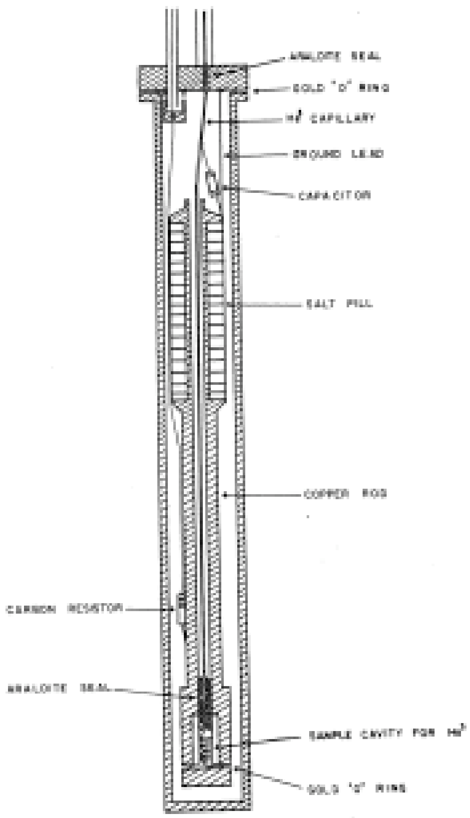

DIAGRAM OF SALT CHAMBER

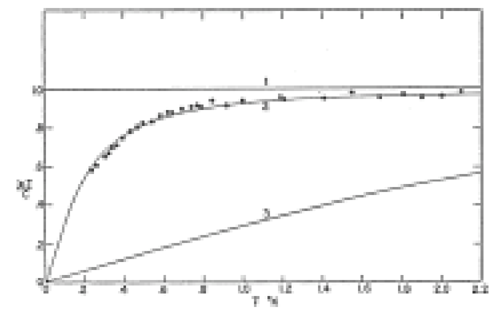

FIG. 1. Plot of $\chi T/C$ vs T. (χ=molar nuclear magnetic susceptibility of He³, T=absolute temperature, C=normalizing Curie constant.) Curve 1 represents the Curie law expected from Boltzmann statistics, curve 3 represents an ideal Fermi-Dirac gas with the same density and atomic mass as liquid He³ (T_0=5°), curve 2 represents an ideal Fermi-Dirac gas with a degeneracy temperature T_0=0.45°K. The dots represent the experimental points.

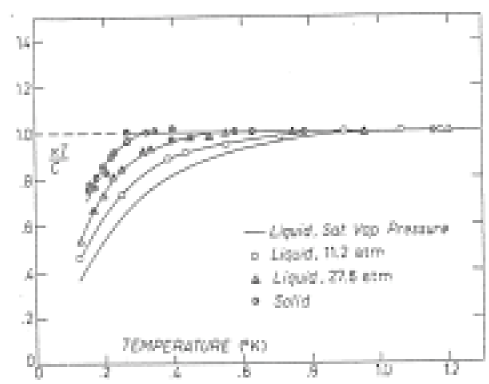

Fig. 2. – Plot of $\chi T/C$ vs. T for various pressures including the solid. (χ = molar nuclear magnetic susceptibility of ³He, T = absolute temperature, C = normalizing Curie constant).

First NMR cryostat, W. Ard, thesis Duke 1955, Phys Rev 1954 article with ³He susceptibility results Nuovo Cimento with further NMR data of ³He under pressure

Here is a picture of the early cryostat (left side), with at the top the magnetic salt pill for the cooling procedure and the NMR cell at the bottom. The results are shown in the picture (right side) which reproduces the first page from Physical Review in 1954. Note that it took only about 6 weeks between the submission of the article and its publication, the fast processing repeated in later articles in PR by the Fairbank group. A further paper in Nuovo Cimento described the susceptibility of liquid ^{3}He under pressure, and also of solid ^{3}He**.** (bottom plot, right side) These pioneer measurements already showed that with increasing density, the Fermi temperature decreased. They also showed a strong deviation from the Curie law for the solid. This last result, however, was incorrect, and must have resulted from difficulties with the production of the solid phase and the slippage of the plug along the capillary leading to the cell.

Here I mention another prediction by London, namely flux quantization in superconducting films, which inspired Fairbank to try to detect this phenomenon. He had this already in mind at Duke, but it became his first project at Stanford. He tackled it together with his graduate student Bascom Deaver, while continuing further ^{3}He research there with help of his former Duke students Goodkind and Adams. Another project of Fairbank inspired by Fritz London was the detection of the magnetic moment in a rotating superconducting spherical shell, the "London moment". The detection by Hildebrand et al. at Cal Tech in '64 and also by Bol and Fairbank became very important in the design of "gravity probe B", presently gathering data in Space.

London's fame attracted several theorists to work with him. I only mention here Paul Zilsel and Peter J. Price. Michael Buckingham traveled from Australia in 1954, but he arrived only after London's death. His involvement with the experiments of Fairbank started a long-time collaboration and friendship. Similarly Kyozi Kawasaki arrived at Duke in 1954 to work on a PhD thesis with London, but he ended up writing a thesis supervised by Buckingham.

Based on predictions by Prigogine and coworkers and by Chester, ^{3}He -^{4}He mixtures were to phase-separate at low enough temperature, and Walters and Fairbank set about to observe this phenomenon, at first with the small cell used for the susceptibility of pure ^{3}He. This showed some evidence of the phenomenon, as described in Walters' thesis. A much better cell was then designed, with three separate compartments for the fluid**.**

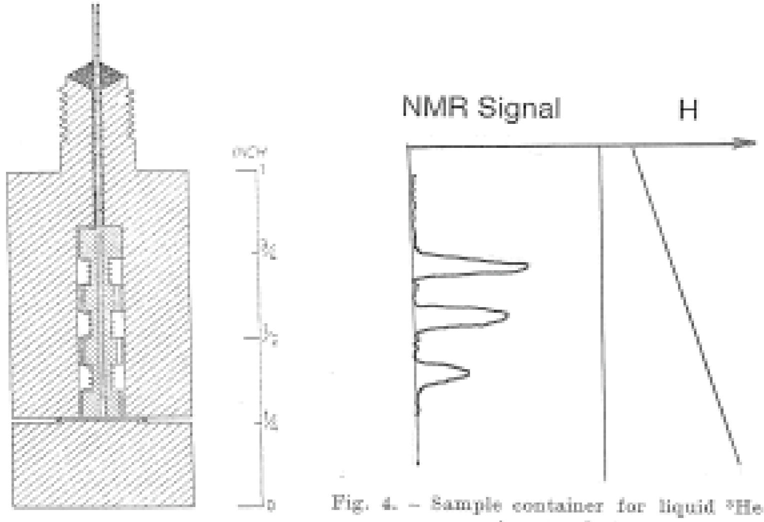

Fig. 4. – Sample container for liquid ^{3}He in nuclear resonance experiments designed to observe phase separation in liquid ^{3}He-^{4}He solutions.

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Phase Separation in He^3-He^4 Solutions*

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(Received May 10, 1956)

USING nuclear magnetic resonance techniques,[1,2] we have detected a separation of He^3-He^4 solutions into two distinct liquid phases at temperatures below 0.8°K. A separation had been predicted on theoretical grounds independently by Prigogine *et al.*[3] and by Chester.[4]

For detecting the phase separation and making quantitative measurements on the He^3 concentrations of the two phases, a sample container having three vertically arranged sections, connected by small holes, was constructed. When this container is placed in a magnetic field having a gradient from top to bottom, the solutions in each of the three sections come to resonance at a constant frequency for different values of the steady magnetic field. Thus, the resonance line as observed on an oscilloscope is split into three separate peaks, each corresponding to the resonance of the He^3 nuclei in a known section of the container. Changes in the relative amplitudes of these three peaks as a function of temperature give a measure of the concentra-

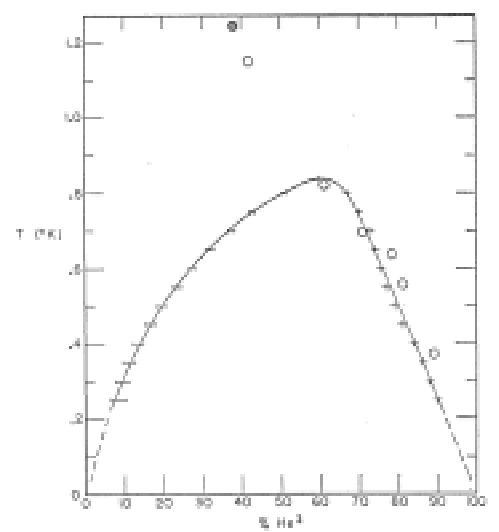


FIG. 1. Phase diagram for He^3-He^4 solutions. The open circles represent T_λ measurements of Daunt and Heer. The closed circle represents a T_λ measurement made by us.

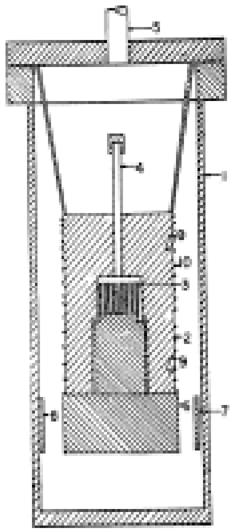
Three-compartments cell for phase separation (Nuovo Cimento) and diagram of the NMR signal with vertical field gradient. P.R. 1955 article with phase separation results.

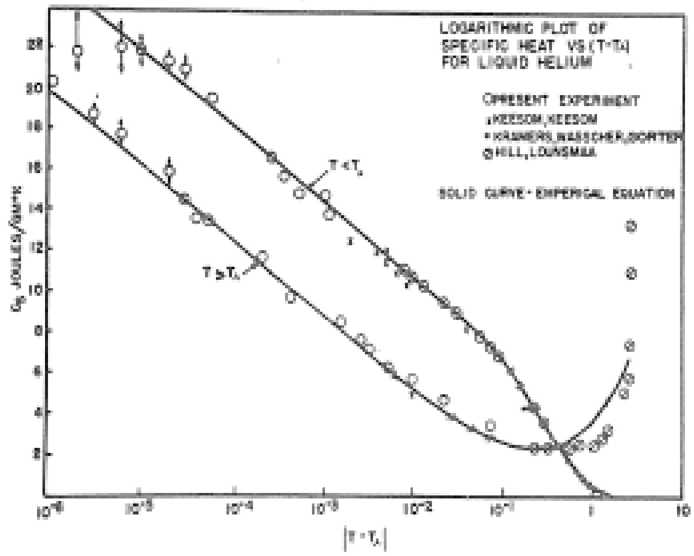
This was in fact the early version of a magnetic resonance imaging arrangement., where a vertical magnetic field gradient was imposed, which upon the modulation of the applied magnetic field yielded three resonance peaks. The NMR signal height in each of the compartments was then used to estimate the ^{3}He concentration. Below 0.9 K, enrichment of ^{3}He in the top cell and depletion in the bottom cell were recorded. In the data analysis an adjustment was made for the deviation of ^{3}He from Curie's law , and the results are shown in the next figure, taken from the short Phys. Rev. paper. in 1956. **bottom** (**Picture of phase separation**) The open symbols show the superfluid transition data obtained by Daunt and Heer at Ohio State University, another early and very successful low temperature lab. Both the nuclear susceptibility of liquid ^{3}He and the phase separation results attracted wide attention. I remember well the seminar given in '56 by Laurie Challis, then a graduate student at the Clarendon Lab.

There is still one more experiment by Bill Fairbank I want to talk about, this one in collaboration with Buckingham and Fred Kellers, a graduate student. The challenge was to test the prediction by Blatt, Butler and Schafroth that the specific heat at the lambda point of ^{4}He had a peak of a few milliK wide. By contrast, the 2D Ising model predicted a sharp logarithmic singularity at the phase transition, and so Fairbank made a bet with Blatt that the transition was going to be sharp.

Specific heat of liquid ^{4}He near the lambda point

Fairbank, Buckingham and Kellers Proc. LT Madison, WI, 1957





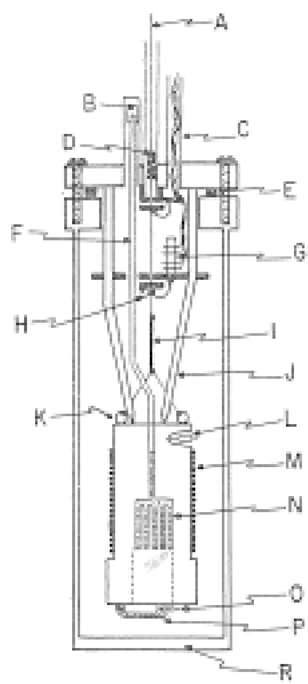


Fig. 1. Schematic diagram of adiabatic chamber for specific heat measurements[1].

A — Stainless steel wire for closing heat switch
B — Brass cap on filling capillary
C — Wires connecting to heater and resistors on sample
D — Cotton plus dyed with carbon black as radiation trap
E — Indium "O" ring
F — Filling capillary
G — Kovar seal used as thermal short for wires to sample
H — Radiation shield and thermal short for heat switch
I — Nylon cord
J — Three prongs of heat switch (copper)
K — Indium coating and suspension for sample
L — Temperature sensitive resistor
M — Heater
N — Sample cavity
O — Temperature sensitive resistor
P — Copper shield over resistor
R — Calorimeter wall

Top left: First Calorimeter , and first reported lambda-point experiments, 1957.
Bottom: Newer version of calorimeter (Buckingham and Fairbank, Progress in Low Temp. Phys. Vol. III (1962)

A simple thick-walled copper cell with grooves inside to increase the liquid-solid contact surface was designed, and filled at liquid nitrogen temperature under a pressure of 130 bar, then sealed off. It was then suspended by fibers in the cryostat. This method insured that there was no heat leak into the cell from superfluid flow through a capillary. These were the first high-resolution temperature measurements, where the temperature of the cell close enough to the lambda point was monitored versus time when a small constant heat was applied. The first published results are shown on the right side **(top right of figure),** where indeed it is shown clearly that the specific heat at saturated vapor pressure diverges nearly logarithmically, the peak sharper than 1 microK, contrary to the prediction by Blatt et al. These experiments were done with very simple electronic equipment. The temperature was measured by an AC bridge method where a 30 Hz Army surplus tuned amplifier was used. An improvement in later measurements was the incorporation of a home-made phase-sensitive detector. Also the suspension and thermal contact mechanisms were much improved **(bottom of figure: picture of improved calorimeter**). When at his PhD oral exam Fred Kellers was asked by Fairbank what he felt was his most important contribution, he replied that it was the design of the lock-in amplifier.

This experiment inspired several scientists interested in the ^{4}He lambda point to repeat this experiment and improve the data. The experiments by Ahlers and later by Chui and Lipa, and also those by the group of Gasparini in constricted geometry have the highest standard in temperature resolution and data quality. Furthermore, people interested in second-order phase transitions, such as the liquid-vapor critical point and the superconducting-to-normal transition were stimulated to measure the specific heat with high temperature resolution. This contributed to the explosive growth of critical point investigations in the sixties and well beyond.

There is no time to describe the other experiments in the Fairbank group, which I list here :

1) Amplitude dependence of second sound in liquid ^{4}He by Alex Dessler,

2) NMR relaxation times in solid ^{3}He by John Goodkind using pulse techniques,

3) Experiments on critical velocities of isothermal flow in superfluid ^{4}He by John Kidder,

4) Nuclear susceptibility in liquid and solid ^{3}He by Dwight Adams,

5) NMR of solid H_2 under very high pressures by Bill McCormick,

6) Density of liquid ^{3}He by John Rives.

Furthermore:

Robert Romer, a faculty member of Amherst College, spent a year in the Fairbank lab., measuring the intrinsic longitudinal relaxation time of ^{3}He nuclear spins in the liquid phase .

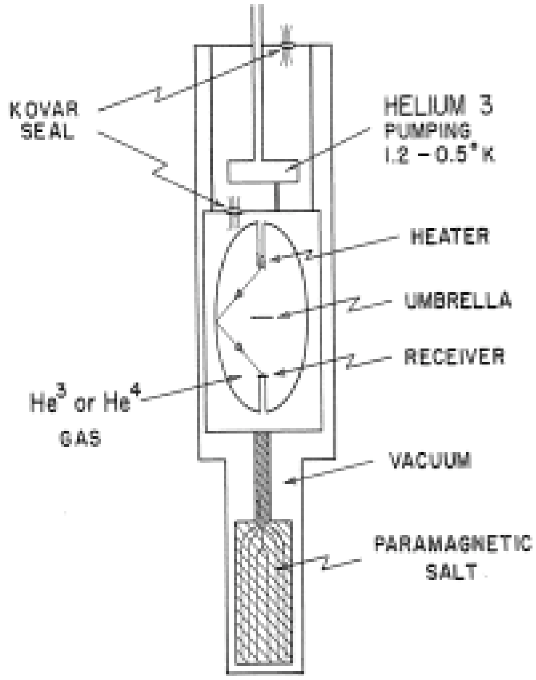
Bill Fairbank was a very friendly, informal, likeable and optimistic person, dreaming all the time of experiments, very enthusiastic about Physics. He loved to be in the lab, enjoyed taking measurements and discussing his various ideas of further experiments any place at any time. At several conferences in the sixties he asked me to room with him, and these were nights where I got very little sleep, because Bill loved to discuss his ideas and plans at any time, and woke me up periodically.

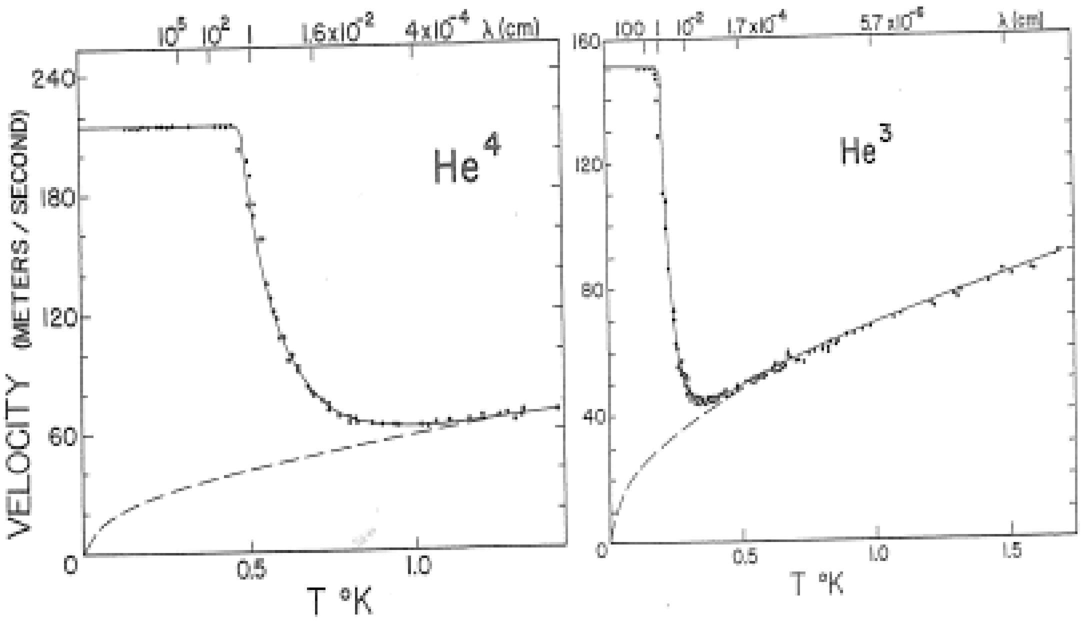
What now of Fairbank's legacy at Duke? When I joined the Physics Department at Duke in the fall of 1959, several students of Fairbank had not finished their experiments, and some parts in their experimental cells had to be modified before publishable data were obtained. I was quite new in this field of liquid helium, but I had experience in NMR and was very interested in getting involved and in developing further several of the projects.

Here I might mention an experiment which evolved from one originally designed by Dessler for second sound experiments. A cell consisting of two halves of a copper shell with a perfect ellipsoidal inside space and polished walls, a heater at one focus and a thermistor at the other, were used. **(See top of figure** : **Picture of the cell**) The propagation of ^{4}He and ^{3}He beams was studied below 1 K when the vapor pressure became so small that the mean free path became of the order of the ellipsoid dimensions. The time of flight of the beam, specularly reflected by the wall and concentrated in the other focus, was measured as a function of temperature, heater power and pulse repetition rate**. (Bottom : pictures for ^{3}He and ^{4}He velocity data)** These were among the first Low Temperature molecular beam experiments, later extended in a different geometry at Ohio State U. by Edwards and his group.

Velocity of 4He and 3He vapor D. T. Meyer et al., Cryogenics 1963

Sound velocity for λ << cell dimension
Beam velocity for λ >> cell dimension





Ellipsoid experiment (D.T. Meyer) with beam experiment below 1K. Plots of velocity in ^{4}He and ^{3}He

The arrival in 1962 of Earle Hunt from Rutgers, with expertise in pulsed NMR, permitted us to start a program of studies of the longitudinal and transverse NMR relaxation times in solid ^{3}He below 1 K. This series of experiments, which was the PhD thesis work of Bob Richardson, provided values of the nuclear exchange interaction energy in bcc and hcp ^{3}He. For the bcc solid near the melting pressure, the resulting nuclear ordering transition was predicted to be of order of 1 milliK, and decreasing with increasing density. This got Bob interested in searching for this transition after his appointment at Cornell University. His teammates were Doug Osheroff and David Lee, and the rest is history.

In 1963, Bill Fairbank's brother, Henry, became the Physics Department Chair at Duke, and he brought members of his Yale group with him. One of them was Bud Bertman, another Michael Crooks, and new students were recruited. The research interest was that of transport phenomena in liquid and solid helium, both ^{4}He, ^{3}He and mixtures.
In their research technique, great care was taken growing crystals at constant pressure in a temperature gradient and annealing them for the thermal conductivity measurements, as was first done by Mezhov-Deglin in Moscow. In those measurements, a sharp peak in the conductivity versus temperature had been observed and the region of Poiseuille flow was clearly shown.

A crucial postdoctoral appointment was that of Robert Guyer, a recent PhD in theory of J. Krumhansl at Cornell, who strongly interacted with the Fairbank group. He was instrumental in calculating the experimental parameters necessary for the observation of second sound in solid ^{4}He. This sound was detected in 1966 by Ackermann, Bertman, Fairbank and Guyer and the results appeared in Phys. Rev. Letters in 1966.

The top of figure shows the cell with the copper post at the bottom where the crystal was nucleated and grew towards the top in a temperature gradient. **The plot (bottom)** shows the shape and arrival time of the observed second-sound signal as a function of 1/T. This was an important milestone, followed by further very careful transport measurements by this group.

VOLUME 16, NUMBER 18 PHYSICAL REVIEW LETTERS 2 MAY 1966

SECOND SOUND IN SOLID HELIUM†

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(Received 28 March 1966)

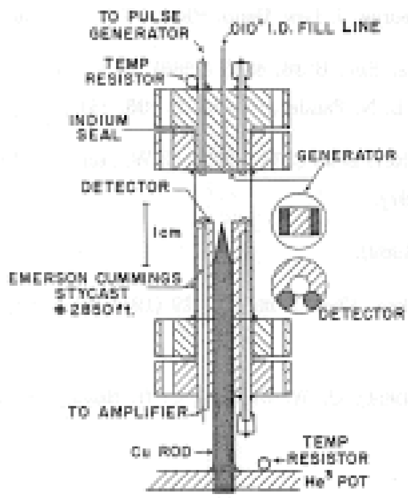

FIG. 1. Sample chamber.

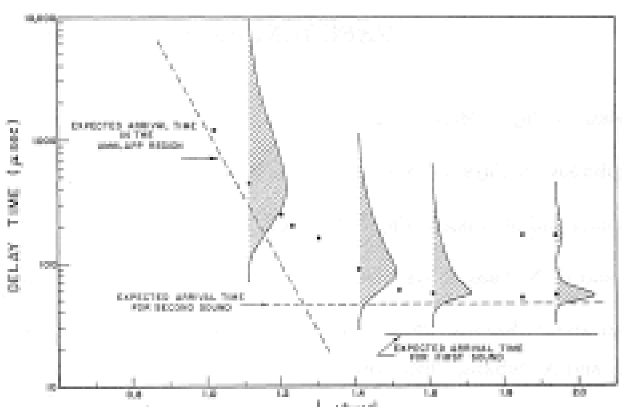

FIG. 3. The arrival time of the received pulse as a function of reciprocal temperature. These arrival times are taken to be the time of maximum $d(\delta T)/dt$ of the received pulse [e.g., from Figs. 2(b) and 2(d)]. A plot of the $d(\delta T)/dt$ curves of some of the received pulses are superimposed.

Second sound experiment in solid ^{4}He: Phys Rev. Lett. 1966 Plot of second sound arrival times.

In conclusion this talk has described the influence London's predictions and influence had on the development of low temperature research at Duke University, which took place under the inspired and imaginative leadership of William Fairbank.