Volume 1

LOW TEMPERATURE PHYSICS - LT 13

Quantum Fluids

Edited by
K. D. Timmerhaus, W. J. O'Sullivan, and E. F. Hammel

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Foreword

The 13th International Conference on Low Temperature Physics, organized by the National Bureau of Standards, Los Alamos Scientific Laboratory, and the University of Colorado, was held in Boulder, Colorado, August 21 to 25, 1972, and was sponsored by the National Science Foundation, the U.S. Army Office of Scientific Research, the U.S. Atomic Energy Commission, the U.S. Navy Office of Naval Research, the International Institute of Refrigeration, and the International Union of Pure and Applied Physics. This international conference was the latest in a series of biennial conferences on low temperature physics, the first of which was held at the Massachusetts Institute of Technology in 1949. (For a complete list of previous LT conferences see p. viii. Many of these past conferences have been coordinated and sponsored by the Commission on Very Low Temperatures of IUPAP. Subsequent LT conferences will be scheduled triennially beginning in 1975.

LT 13 was attended by approximately 1015 participants from twenty five countries. Eighteen plenary lectures and 550 contributed papers were presented at the Conference.

The Conference began with brief introductory and welcoming remarks by Dr. R.H. Kropschot on behalf of the Organizing Committee, Professor J. Bardeen on behalf of the Commission on Very Low Temperatures of the IUPAP, and Professor O.V. Lounasmaa on behalf of the International Institute of Refrigeration. The eighth London Award was then presented by Professor E. Lynton to Professor A.A. Abrikosov (in absentia). The recipient's award address, as delivered by Dr. P. Hohenberg, will surely remain for all who were privileged to hear it as one of the high points of the Conference. We wish to gratefully acknowledge the members of the Fritz London Award Committee: (E.A. Lynton, chairman; J.F. Allen; P. Hohenberg; F. Reif; D. Scalapino, and M. Tinkham) for assuming the responsibility for selection of the Award recipient and for making this timely award.

LT 13, originally scheduled to be held at the University of California at San Diego, was shifted in 1971 to Boulder because facility and accommodation problems developed after the first announcement of the proposed location of LT 13. Nevertheless, the original organizers of LT 13, Professors H. Suhl and B. Matthias, contributed significantly in the initial phases of the LT organization. As originally conceived, E.F. Hammel and the staff of the Los Alamos Scientific Laboratory organized the technical programs from the very beginning. By March of 1971 invitations had been sent to some fifty U.S. low temperature physicists inviting them to serve on the National Organizing Committee of LT 13.

The first meeting of the U.S. National Committee was held in Washington, D.C. on April 25, 1971. By that time arrangements had been made for the shift of the Conference to Boulder, Colorado, and R.H. Kropschot had accepted the General Chairmanship of LT 13. At the April meeting timetables were established, an

International Advisory Committee was appointed, and the general format of the Conference was agreed upon. Strong preferences were expressed for:(a) a general low temperature physics conference; (b) approximately one half of the available time to be allotted to plenary lectures, covering new developments in low temperature physics but presented in such a way as to interest a general audience; (c) somewhat more emphasis on low temperature instrumentation and measurement than in the past, and (d) information (perhaps through a plenary lecture) on recent developments in applied low temperature physics or cryoengineering.

In order to implement these proposals, a small Executive Committee was established (listed on p. viii). It was clear from the outset that LT 13 would be a large as well as a diverse conference, and that proper development of the technical program would require the help of many experts. Consequently, the subject matter of the Conference was divided into six main divisions. Division Chairmen were appointed and were given essentially full authority to arrange both the plenary and the contributed paper sessions in their divisions. In some instances the Division Chairmen appointed small committees to assist with some of this work. A listing of the six divisions and their Chairmen is given below. Certainly the success of LT 13 was in many ways due to the superb work of this group of individuals and those who assisted them.

Division I
Quantum Fluids—Prof. I.I. Rudnick, *UCLA*

Division II
Quantum Solids—Dr. N.R. Werthamer, *Bell Telephone Laboratory*

Division III
Superconductivity—Prof. T.H. Geballe, Stanford and Dr. P. Hohenberg, *Bell Telephone Laboratory*

Division IV
Magnetism—Dr. S. Foner, *Francis Bitter National Magnet Laboratory, MIT.*

Division V
Electronic Properties—Dr. P. Marcus, *IBM Laboratory*

Division VI
Measurements and Instrumentation—Prof. J. Mercereau, *California Institute of Technology*

The International Advisory Committee for LT 13 also contributed significantly to the success of LT 13 by forwarding to the Organizing Committee information on exciting new work in low temperature physics as well as the names of those younger scientists whose presence at LT 13 should be encouraged.

Within this framework many other individuals helped in the organization and execution of this Conference. Special thanks are due to W.E. Keller, R.L. Mills, L.J. Campbell, W.E. Overton, Jr., R.D. Taylor, W.A. Steyert, and E.R. Brilly of the Los Alamos Scientific Laboratory, who helped with the handling of abstracts and manuscripts of the contributed and plenary papers. At Boulder L.K. Armstrong and L.W. Christiansen did an equally fine job with the local arrangements both prior to and during the Conference.

Also at Boulder, the task of preparing and distributing the Call for Papers, the subsequent announcements, and the Conference Program was carried out by the Conference Publications Committee consisting of W. O'Sullivan and K.D. Timmerhaus. Since both these individuals are continuing to serve as the General Editors of the Conference Proceedings, our grateful thanks are hereby extended to them for their contribution to LT 13 before, during and after the Conference. The Editors acknowledge the services of graduate student Michael Ciarvella, University of Colorado, in the process of preparing the manuscript for publication.

The Conference organizers are also deeply indebted to Professors G. Uhlenbeck and S. Putterman for arranging an evening session on "The Origins of the Phenomenological Theories of Superfluid Helium," to Professor J. Allen for the showing of his new motion picture on Liquid Helium II, and to the organizers of the numerous impromptu but extremely valuable sessions that were developed in addition to the regular program. Clearly the organization and operation of a conference as large as LT 13 can succeed only with the help of many dedicated people. We had hoped that LT 13 would be a stimulating and enjoyable conference. The fact that it met both those expectations is due to all those who wanted it to be a fine conference and worked hard to realize it.

Finally, we wish to express our appreciation to the Session Chairmen and to the individuals who reviewed the manuscripts prior to publication in these Proceedings.

E. F. Hammel
R. P. Hudson
R. H. Kropschot

Acknowledgment

The editors take great pleasure in recognizing the outstanding assistance which Mrs. E. R. Dillman of the University of Colorado provided in the preparation of these four volumes comprising the Proceedings of the 13th International Conference on Low Temperature Physics. Words cannot express our gratitude for conscientious and devoted effort in this thankless task.

K. D. Timmerhaus
W. J. O'Sullivan
E. F. Hammel

Biennial Conferences on Low Temperature Physics

1949 LT 1	Massachusetts Institute of Technology, Cambridge, Massachusetts, U.S.A.
1951 LT 2	Oxford University, Oxford, England
1953 LT 3	Rice University, Houston, Texas, U.S.A.
1955 LT 4	University of Paris, Paris, France
1957 LT 5	University of Wisconsin, Madison, Wisconsin, U.S.A.
1958 LT 6	University of Leiden, Leiden, The Netherlands (scheduled to celebrate the 50th Anniversary of the liquefaction of helium by Kamerlingh Onnes)
1960 LT 7	University of Toronto, Toronto, Canada
1962 LT 8	University of London, London, England
1964 LT 9	Ohio State University, Columbus, Ohio, U.S.A.
1966 LT 10	Moscow, U.S.S.R.
1968 LT 11	University of St. Andrews, St. Andrews, Scotland
1970 LT 12	Kyoto, Japan
1972 LT 13	University of Colorado, Boulder, Colorado, U.S.A.

LT 13 Executive Committee

J. Bardeen	R. H. Kropschot	W. J. O'Sullivan	K. D. Timmerhaus
E. F. Hammel	D. G. McDonald	R. N. Rogers	L. K. Armstrong
R. A. Kamper	F. Mohling	H. Snyder	G. J. Goulette

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Fritz London Award Address

A. A. Abrikosov

Landau Institute for Theoretical Physics
Moscow, USSR

First of all I would like to thank the Fritz London Award Committee for the high appraisal of my work expressed by their awarding to me this prize. It was a particular pleasure for me since the last Soviet physicist to receive the London Award was my teacher, Landau, to whom I and many other Soviet physicists are greatly indebted. His early death in a tragic accident in 1968 was a great loss for science.

I would like to recount some memories of a period of approximately a decade which was of great significance for my scientific life, during which time I had the opportunity almost every day to communicate with Landau and to profit by his advice. Maybe this is the reason why this period was so fruitful for me.

In 1950 Ginsburg and Landau wrote their well-known article on superconductivity. Without the microscopic theory the meaning of several quantities entering their treatment remained unclear, above all the meaning of the "superconducting electron wave function" itself. Nevertheless this theory was the first to explain such phenomena as the surface energy at the superconducting–normal phase boundary and the temperature and size dependence of the critical field and current of thin films.

The experimental verification of the predictions of the Ginsburg–Landau theory concerning the critical fields of thin films was undertaken by my friend Zavaritzki, who was at that time a young research student of Shalnikov's. I often discussed the matter with Zavaritzki. Generally his results fitted the theoretical predictions well. He even managed to observe the change in the order of the phase transition with decreasing effective thickness (i.e., the ratio of the thickness to the penetration depth at a given temperature). To do this, he used the hysteresis of the dependence of the resistance $\rho(H)$ on the field. One day Zavaritzki slightly altered his technique of sample preparation. Usually he evaporated a metal drop onto a glass plate and then put such a mirror into the Dewar vessel. Instead of this, he began to carry out the evaporation inside the Dewar vessel, with the glass plate at helium temperature.

Now we know that in this case the atoms reaching the plate are trapped at the sites where they hit the plate and are unable to move and to form a regular structure. Therefore an amorphous substance is produced, which at every effective thickness will be a type II superconductor. But at that time this was not known, of course.

The critical field versus thickness dependence measured by Zavaritski did not follow the formulas given in the article by Ginsburg and Landau. This gave the impression of a paradox. Apart from its beauty, the theory really explained a lot of things and we were surprised to see that suddenly it had failed.

Discussing with Zavaritzki the possible origin of this discrepancy, we came to the idea that the approximation $\kappa \ll 1$ based on the surface tension data (where κ is the Ginsburg–Landau parameter) could be incorrect for objects such as low-temperature films. Particularly one could suppose that $\kappa > 1/\sqrt{2}$. According to Ginsburg and Landau, the surface energy should be negative under these conditions. Intuitively it was felt that in this case the phase transition in a magnetic field would always be of second order, and this was in fact what Zavaritski observed.

When I calculated the dependence of the critical field on the effective thickness with $\kappa > 1/\sqrt{2}$, it appeared that the theory corresponded to the experimental data. This gave me the courage to state in my article of 1952 containing this calculation that apart from ordinary superconductors whose properties were familiar, there exist in nature superconducting substances of another type, which I proposed to call superconductors of the second group (now called type II superconductors). The division between the first and the second group was defined by the relation between the quantity κ and its critical value $1/\sqrt{2}$.

After this I tried to investigate the magnetic behavior of bulk type II superconductors. The solution of the Ginsburg–Landau equation in the form of an infinitesimal superconducting layer in a normal sea was already contained in their article. Starting from this solution I found that below the limiting critical field, which is the stability limit of every superconducting nucleation, a new and very peculiar phase arose, with a periodic distribution of the Ψ function, magnetic field, and current. I called it the mixed state.

Landau showed a notable interest in this work and wanted me to publish my results for the vicinity of the upper critical field, which I named H_{c2}. But I wanted to understand how the new mixed state looks in the total range of fields.

At this time I became ill and had to stay in bed for almost three months. One day Landau visited me. The conversation, as in most cases, concerned everything but physics, and Landau sipped with great pleasure from a glass of glühwein, which was not at all like him. And then suddenly I destroyed all this paradise by telling him what I had invented for the mixed state, namely, the elementary vortices. As Landau's eyes fell on the London equation with a δ function on the right-hand side, he became furious. But then, remembering that an ill person should not be bothered, he took possession of himself and said, "When you recover we shall discuss it more thoroughly." Then he hastily bade farewell and disappeared.

He did not come to me any more. When I felt better and appeared at the Institute and tried to tell him again about the vortices, he swore rather ingeniously. At that time I was still very young and did not know the temper of my teacher well enough. He had seen in his life many kinds of pseudoscience, and this made him suspicious toward unusual statements. However, by making some effort and disregarding the noise which he made, one could always "drag" him through any reasonable idea. But at that time I sadly put my calculations in my table drawer "until better times."

But in fact the idea was not so bad. Analyzing the solution that I got close to H_{c2}, I saw that in the plane perpendicular to the field there are points where Ψ becomes zero. The phase of the Ψ function changes by 2π along a path around such a point. I thought about why such singularities should appear, and saw that it could not be otherwise. Indeed the Ginsburg–Landau equation contained not the magnetic field but the vector potential. If the magnetic field does not vary in sign over the whole sample, then the vector potential must increase with the coordinate. But the physical state in a uniform field (this is true close to H_{c2}) must be uniform or at most vary periodically in space. So the increase of the vector potential must be compensated by a change of the phase of the Ψ function. Consider Fig. 1. Let the field be along the z axis and let us choose $A_y = Hx$. Consider the (xy) plane. Let the black points be those I noted earlier. If we want to have a unique determination of the phase we must draw cuts in the plane. We draw them through the black points parallel to the y axis. From the figure it is evident that when going around the points the phase increases by $(\Delta\varphi)_1 = \pi y/a$ if we move along the lower path and by $(\Delta\varphi)_2 = -\pi y/a$ if we move along the upper one. That means that at every cut the gradient of the phase $\partial\varphi/\partial y$ undergoes a jump $2\pi/a$. Using ordinary units (at that time I used the dimensionless Ginsburg–Landau units), one sees that the compensation of the increase of A_y demands

$$(2e/c)\,Hb = 2\pi\hbar/a$$

or

$$Hab = \pi\hbar c/e = \Phi_0$$

which is the flux quantum. Since I used dimensionless quantities, I did not mention the flux quantum on the right but I understood that with a decreasing magnetic field the cell dimensions ab must increase, and as a limit one vortex must be considered where the phase of Ψ changes by 2π in going around it. On the z axis one must have $\Psi = 0$ since otherwise the Ψ function is not uniquely defined. Such a picture gave me the possibility of obtaining the lower critical field H_{c1} and the magnetization curve $M(H)$.

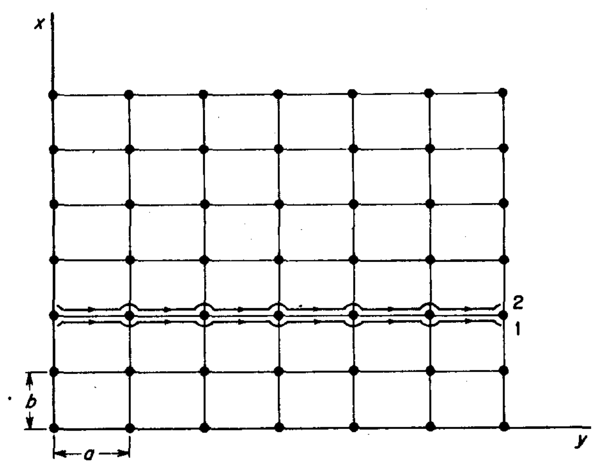

Fig. 1