NEWS and VIEWS

VERY LOW TEMPERATURES

MOST OF THOSE QUOTED WERE PRESENT

About two hundred physicists, of whom twenty-five or so had traveled from Europe, attended the International Conference on the Physics of Very Low Temperatures held at the Massachusetts Institute of Technology on September 6–10, 1949. Similar meetings had been organized before the last war but only as supplements to the meetings of the International Institute of Refrigeration which were concerned with industrial questions. The transfer to the auspices of the International Union of Pure and Applied Physics marks the coming of age of low temperature research as a major branch of physics.

This conference was also a "first" in another respect, being the first major international low temperature conference in the United States, and the large number of participants is witness to the rapid growth and popularity of this line of research. This growth started only after the last war when the Collins liquefier made liquid helium available to small laboratories without a long established tradition in low temperature work.

The meeting was characterized by a homogeneity of interest; it was remarked that almost every one quoted was present. This assured a congenial atmosphere but rendered the organization of parallel sessions difficult. With about seventy papers presented this was a busy meeting and the present summary can give but general trends.

The first part of the program centered around the properties of He I, He II, He^3 and He^4. A group of papers, of which Gorter's was the most comprehensive, dealt with refinements of the two-fluid concept of He II. Mendelssohn, Jackson, Atkins, and others presented an array of ingenious experiments marking a definite progress toward the quantitative description of this elusive phenomenon. Somewhat related is the study of adsorbed layers by Long and Meyer and Frederikse (presented by Taconis).

A large number of papers was devoted to the study of He^3, He^4 solutions and some to the study of pure He^3. The discrepancies pertaining to the early experiments have been ironed out and Taconis has shown that the laws of ideal solutions may be applied to this system provided one makes the surprising assumption that the He^3 atoms are dissolved only in the "normal" part of He II.

Of great interest were the systematic investigations presented by Abraham, Weinstock, and Osborne, who consider their measurements on lambda points of He^3, He^4 mixtures and on the viscosity of pure He^3 as confirming evidence for the nonexistence of a superfluid transition in He^3. This has a bearing on the much debated question of the importance of statistics in strongly interacting systems and seems to be in favor of London's Bose-Einstein hypothesis which was the basis of Tisza's version of the two-fluid model. Yet the part played by statistics is still obscure. De Boer, presenting his calculations of the properties of liquid He^3, pointed out that although he had ignored the role of statistics, his calculations were subsequently corroborated by the Los Alamos measurements of the vapor pressure curve.

The conference marked the breaking of the deadlock which has existed for years regarding the relative merits of the two versions of the two-fluid theory. Whereas the above results make it likely that Landau's roton hypothesis has to be abandoned in favor of the Bose-Einstein theory, on the other hand Landau's predictions rather than Tisza's seem verified by the second sound measurements below 1°K. Such measurements were reported by Pellam, who applied his pulse techniques in conjunction with adiabatic demagnetization and by Maurer and Herlin working with Pellam's method in the lowest helium temperature range.

The second major group of problems was magnetic phenomena. De Klerk reported a new low value of 0.0029°K reached by adiabatic demagnetization as compared with the previous low of 0.0036°K. The difference may be actually due to an improved temperature scale rather than to an improved technique. The establishment of the temperature scale ranks in importance with the technique in cooling and was discussed also by Kürti and van Dijk. The temperature scale is problematic even in the helium range and Shoenberg offered helium vapor pressure data to all interested in order to render the data of various laboratories comparable.

Paramagnetic relaxation and particularly resonance provides a new tool for obtaining direct information on the level splitting in magnetic crystals. Gorter and Bleaney reported on this type of work.

Although two-stage nuclear demagnetization is still in the planning state, nuclear effects appeared of increasing importance. Bloembergen discussed various aspects of the nuclear magnetic resonance phenomenon, among others the serious discrepancy between the measured short spin-lattice relaxation times in contrast to the large values predicted by the Waller and Heitler-Teller theory. These discrepancies have now been traced either to impurities or to internal motions of the protons within the crystal. Conversely, the varied behavior of protons in crystals which eludes detection by x-rays could be studied by the present method.

Cooke discussed the contribution of the hyperfine-structure splitting to the specific heat and pointed out that this effect can be separated from other interaction terms.

Gorter reported on current Leiden experiments tending to line up the nuclear spins by means of the above mentioned type of interaction between nuclear spin and electronic angular momentum. Although he estimated the degree of nuclear alignment attained to thirty or forty percent, the evidence is at present inconclusive since the detection of the alignment (by the anisotropy of emitted gamma rays of tracer nuclei) is an insensitive device and may be the source of possible error.

The third major group of problems was related to

superconductivity. Numerous experiments were reported on the penetration depth of the magnetic field, on the intermediate state, on the effect of rotation, and on the heat conductivity of superconductors. We will concentrate here on the last item, because some novel features were put forward in this connection. It is known that the heat conductivity of metals drops considerably when passing from the normal to the superconducting state. Using these results, Daunt and Heer suggested an ingenious method for making a cyclic cooling engine for operation below 1°K. The gist of the matter is that the establishment or severance of the heat contact between the working substance, the heat bath, and the system to be cooled can be achieved simply by varying the magnetic field around the critical value.

Another interesting effect relating to the same subject was presented by Mendelssohn. He had concluded that under certain conditions superconductors should allow the establishment of an internal circulation of normal and super current in much the same way as is the case in He II, leading to an increase rather than decrease of heat conductivity in the superconducting state. These conclusions were borne out by his experiments. Somewhat similar results (Hulm's) were presented by Shoenberg, although the theoretical interpretation given was different.

The present status of the theory of superconductivity was summarized by London who emphasized the absence of any satisfactory molecular theory. Meissner remarked that experiments were in progress to test a consequence of Heisenberg's theory. He pointed out the importance of a small electronic volume for the existence of superconductivity.

On the supplementary program, Tisza presented the results of recent work. He claimed the development of a quantum mechanical formalism which is general enough to account for the possibility of superconductivity without ad hoc assumptions. On Friday afternoon Gorter, van Itterbeek, Shoenberg, and Simon reported on the work of their respective laboratories in Leiden, Louvain, Cambridge, and Oxford. There were several reports on the various liquefaction plants and finally a number of miscellaneous items were considered. A few random selections are: Lane, Shoenberg, Marcus on the de Haas-van Alphen effect; Estermann and P. H. Keesom on semiconductors; van Itterbeek on ultrasonics; and Laine on the resistivity of thin films, all at low temperatures. A new type of magneto resistance effect was discussed by Sondheimer.

This résumé might best be closed by the remark that a comfortable balance was achieved between the number of questions settled and the number of new questions raised.

—Laszlo Tisza

NATIONAL SCIENCE FOUNDATION

LEGISLATION FAILS

The shelving of the bill for a national science foundation by the House of Representatives went almost unnoticed amid September's flurry of turbulent and weighty problems. It had for some weeks been touch and go whether the bill could survive either or both committee and House floor debate; as matters now stand it is spared for another day and will presumably be dusted off for further consideration and action next year. In any event, this marks the third time in as many Congresses that the lower chamber has blocked passage of legislation to create a national science foundation by the simple expedient of failing to act.

A product of many compromises, and designed to eliminate reasons for the Presidential veto which killed a foundation bill last year, the present bill has received the endorsement of scientists, if not their complete enthusiasm. But some others have been opposed. At hearings earlier in the year before the House Committee on Interstate and Foreign Commerce, for instance, the whole idea of a national science foundation was bitterly attacked by the National Patent Council, with the support of others wary of further encroachment of Government power into traditionally independent areas.

In his presidential address to the American Chemical Society at Atlantic City a week or so before the bill was shelved, Linus Pauling observed that it has now been four years since the persuasive recommendations of the Bush Report were submitted to the President, two years since the writing of the perhaps even more convincing Steelman Report—and still no foundation has come into being. He asked that a strong program of public education be launched to make understood the overwhelming importance of federal support of scientific research, as advocated in these two reports.

Agreeing at the same time that there is possible danger of bureaucratic domination in having basic scientific research supported exclusively or predominately by the Federal Government, Pauling suggested that this might be met by a comparable fund provided from non-Federal sources to insure that the Government would and could not be left to dominate the field by default.

Specifically, he proposed a foundation supported by the nation's industry which would supplement the support given research under a national science foundation. Such a foundation, if assured of continuity and independence, would help provide new fields for industrial effort, Pauling said, and would provide the opportunity for industrial leaders "to help to avert the danger that they foresee by themselves providing a more significant part of the support of pure research in our universities and research institutions."

PHYSICS OF POWDER METALLURGY

SYLVANIA SYMPOSIUM IN LATE AUGUST

We may in time find industry performing a function hitherto associated entirely with scientific societies and universities, if it follows the lead of the Sylvania Electric Products, Inc. which recently sponsored a symposium on the physics of powder metallurgy at Fort Totten in Bayside, Long Island. Walter E. Kingston, manager of Sylvania's Metallurgical Research Laboratories, was the moving spirit of the meeting, which was organized and financially supported by his company independently of any technical society. Kingston assembled a program of