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25. *Social Implications of Industrialization in Africa South of the Sahara* (UNESCO, London, 1956).
26. K. Davis, "Population and power in the free world," in *Population and World Politics*, P. M. Hauser, Ed. (Free Press, Glencoe, Ill., 1958).
27. W. Lippmann, "China is No. 1 problem," Chicago *Sun-Times* (14 Dec. 1959); "To live India must change its way of life. . . ," Chicago *Sun-Times* (15 Dec. 1959).
28. Nor is population a factor in political instability only in the underdeveloped areas. There are many other demographic dimensions of world politics which cannot be treated here because of limitations of space. The authors of a recent symposium volume which it was my privilege to edit include further considerations of population as a factor in world politics. Especially pertinent are the articles by Kingsley Davis, Frank Lorimer, Irene Taeuber, and Quincy Wright, from which I have drawn material for this discussion.
29. "Japan's population miracle," *Population Bull.* **15**, No. 7 (1959); "The race between people and resources—in the ECAFE region," pt. 1, *Population Bull.* **15**, No. 5, 89 (1959).
30. *Asia and the Far East, Seminar on Population* (United Nations, New York, 1957).
31. F. W. Notestein, "Knowledge, action, people," *University—A Princeton magazine,* No. 2 (1959); P. Streit and P. Streit, "New light on India's worry," New York *Times Magazine* (13 Mar. 1960).
32. See, for example, G. Pincus *et al., Science* **130**, 81 (1959); ———, "Field Trials with Norethnyodrel as an Oral Contraceptive" (Worcester Foundation for Experimental Biology, Shrewsbury, Mass., in preparation).
33. Data are based on the following: J. J. Spengler, *Proc. Am. Phil. Soc.* **95**, 53 (1951); original data (for 1937) from "Energy Resources of the World," *U.S. Dept. State Publ.* (Government Printing Office, Washington, D.C., 1949), p. 102 ff.

Sir Francis Simon

Knowledge of the properties of matter at temperatures near absolute zero has been increased greatly by his work.

P. W. Bridgman

Sir Frances Simon was born on 2 July 1893 in Berlin, the only son (he had two sisters) in a well-to-do family. He attended the Kaiser Friederich Reform Gymnasium and at first devoted himself to the classics, at the wish of his grandfather. The classics he did not like at all. He showed extraordinary talent for physics and mathematics, and it is said that it was under the influence of Michaelis of the Rockefeller Institute of Medicine in New York, an old friend of the family, that he decided to become a scientist, at the age of 14. Michaelis also persuaded his family, after considerable initial opposition, that this was the proper step. Simon graduated from the Gymnasium in 1912. After this he briefly attended the universities of Göttingen and Munich, until he was called into the army in the fall of 1913 for his year of military service. World War I broke out before his year was up, and he continued in the army, connected with the field artillery, for the duration of the war. He was badly affected by gas, and was twice wounded, the second time severely, two days before the armistice. He was confined to a military hospital until the summer of 1919, recovering from his injuries, and he thus lost altogether six years at the beginning of his scientific career. For his services in the war he was awarded the Iron Cross, first class, and was also made an officer—two unusual distinctions for a person of Jewish origin.

In 1919 Simon resumed his studies, now at the University of Berlin, working under Nernst on specific heats at low temperatures, a topic of great interest at the time because of its bearing on Nernst's controversial heat theorem. In 1921 he was awarded the degree of Dr. Phil. in *physics* under Nernst, the physical *chemist*. His other teachers at Berlin included Planck, von Laue, and Haber. In 1922 he was appointed Nernst's assistant at the Physikalische Chemische Institut. In this same year he married Charlotte Münchhausen, also of a well-to-do Berlin family, whom he had met socially two years before. Her tastes were artistic and musical, and she and Simon complemented each other perfectly. There can be no doubt but that his scientific effectiveness owed much to his happy family background (*1*). In 1924 Simon was appointed *Privat Dozent* in physics in Berlin, and in 1927, *Ausserordentlicher* (associate) professor in physics, a post which he held until he left for Breslau in 1931.

Early Publications

Simon's first published paper, on specific heats at low temperatures, appeared in 1922. During the nine years of his stay in Berlin he published 50 papers altogether; these 50 papers foreshadow most of his later scientific activity, and nearly all were connected in some way with low temperatures. At first he continued the work he had done while with Nernst—namely, on specific heats at low temperatures and, in particular, on the various anomalies at the lowest temperatures. These anomalies vitiated a smooth extrapolation of results at higher temperatures and they had to be taken into account in calcu-

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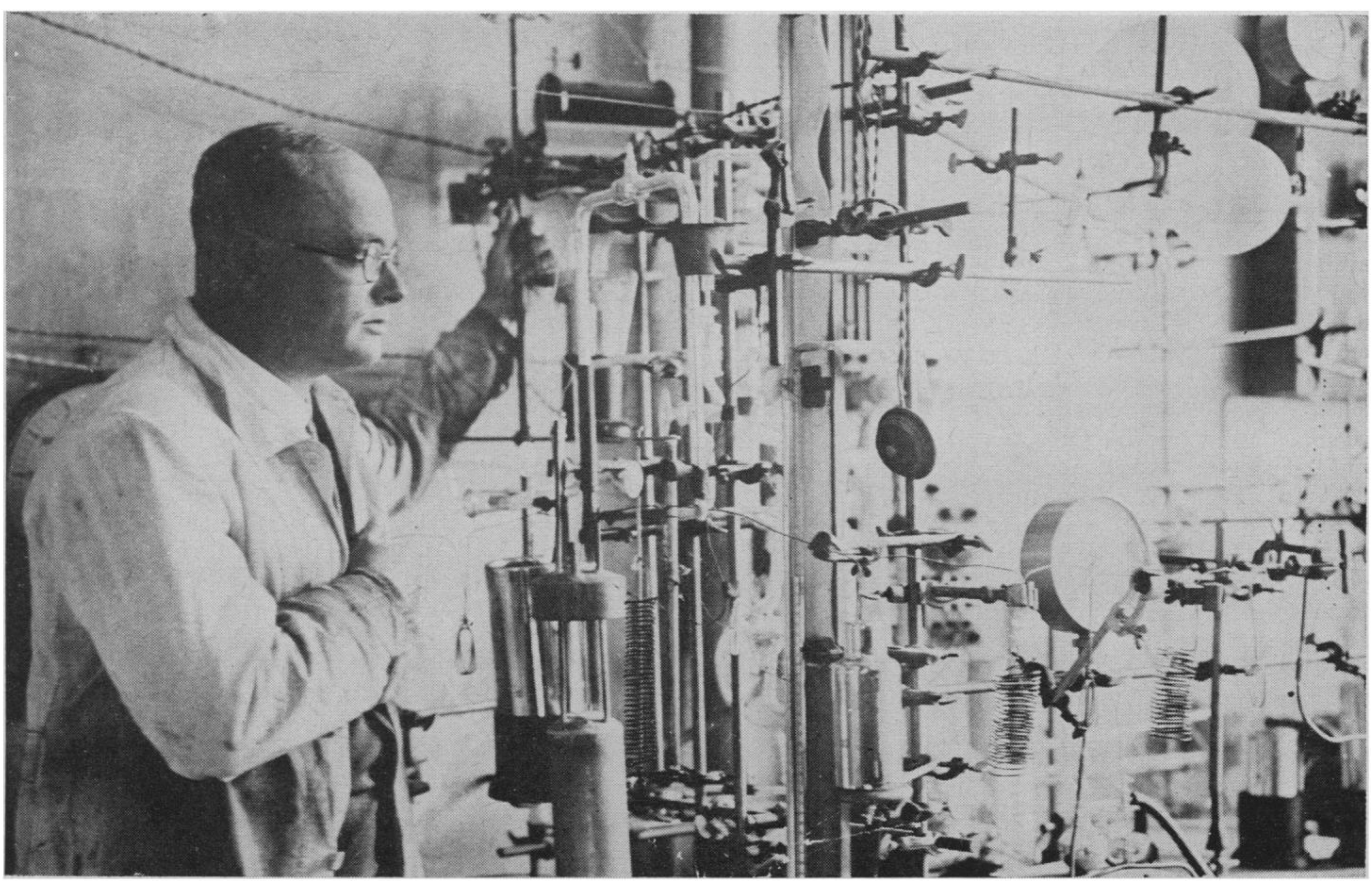
lating entropies at very low temperatures in checking the validity of Nernst's heat theorem. The quantum origin of many of these anomalies was recognized and discussed. Because of their bearing on the same question, the "chemical constants" of the monatomic gases and of mercury were determined. Also discussed was the equivalence of the Nernst theorem with the thesis of the unattainability of absolute zero. A paper had appeared on zero point energy, a result of quantum theory, which was recognized and accepted but which, rather surprisingly, did not seem to play a very vital role in Simon's picture of the limiting condition of physical systems at absolute zero as one of absolute order.

During his Berlin period Simon's reputation as an authority in the low-temperature field grew, as witnessed by his two longest papers during this period, a 55-page contribution to the *Handbuch der Physik*, in 1926, on the determination of free energy, and, in 1930, an article of the same length for the *Ergebnisse der exakten Naturwissenschaften* on the status of Nernst's heat theorem after 25 years. The latter paper is of importance as foreshadowing his later, mature attitude toward the Nernst theorem. Simon recognized the frequent occurrence of anomalies in the specific heats at low temperatures, which must be determined experimentally in order to make good calculations of the equilibrium parameters. He also recognized the existence of phases not well defined thermodynamically, but he regarded the error introduced by these factors in the calculation of equilibrium constants as of only minor importance. It is interesting that among sources of anomalies in the low-temperature specific heats the onset of order in those rotational states which play a role in molecular spectra was recognized as a possibility, although this onset was to be expected only at temperatures lower than those reached at that time. The most immediate application of this idea was to transformations like the para-ortho transformation in hydrogen. The discovery in 1929, with Mendelssohn and Ruhemann, of a specific-heat anomaly in solid orthohydrogen was a striking vindication of Simon's predictions based on Nernst's theorem. No mention was made at this time of magnetism. From this distance it seems that a connection with the phenomena of magnetism is most natural, and that here was a narrowly missed opportunity to propose the magnetic method of cooling, first proposed a few years later.

During the Berlin period Simon began to develop techniques for reaching liquid helium temperatures on a small scale — techniques suitable for the smaller laboratories which lacked the elaborate equipment of installations such as those at Leyden. His first proposal along these lines was the "desorption" method, in which helium is first absorbed on charcoal at various pressures and temperatures reached with conventional facilities and is subsequently evacuated, absorbing heat in the process, the temperature thus being lowered still further. The point of the process is that the helium absorbed on charcoal at various pressures effectively provides a series of substances with thermal properties varying continuously between those of hydrogen and helium. It is the lack of such substances in nature that requires complicated modifications of conventional low-tempera-



A photograph taken in 1929, showing Sir Francis Simon standing beside the apparatus with which he first studied the melting curve of helium up to a temperature of 42°K and a pressure of 5600 atmospheres. [Wide World Photos] ture apparatus in taking the step from liquid hydrogen to liquid helium.

A few years after Simon suggested the "desorption" method, the "expansion" method was proposed. In this, helium is compressed to 100 to 200 atmospheres at the temperature of liquid hydrogen. Thermal contact with the hydrogen is then eliminated by pumping off an independent supply of gaseous helium, which had served as a thermal bridge between hydrogen and helium, whereupon the cooled, compressed, and thermally insulated helium is allowed to expand to a low pressure; during this time its temperature drops sufficiently that a very considerable fraction of the initial helium is liquefied—a fraction sufficient to permit the cooling of objects immersed in it and to make possible a wide variety of small-scale experiments. The method owes its feasibility to the reversal which takes place at low temperatures in the relative thermal capacity of the compressed helium and the solid parts of the containing vessels. Whereas at room temperature the thermal capacity of the metal vessels completely swamps that of the contained helium, at liquid hydrogen temperatures the thermal capacity of the metal has become vanishingly small as compared with that of the helium, so that we are effectively working with vessels with "mathematically thin" walls, as Simon liked to express it.

During this nine-year period Simon also directed determinations of the crystal structure of the solidified gases and of some other materials by x-rays, thus acquiring a first-hand acquaintance with x-ray techniques. However, he does not seem to have used these to any notable extent in his later years, although he did make use of the closely allied technique of the absorption of gamma rays in low-temperature calorimetry. Another activity which he did not pursue later was speculation about the electrical conductivity of metals in general. He proposed the rule that at equal reduced temperatures the electrical conductivity of metals with the same arrangement of electrons in the outer shells of the atoms is the same. He sought to obtain connections with the amplitude of atomic vibrations by utilizing the Grüneisen relation with atomic frequency. This occurred in 1924, a couple of years prior to the revolutionary new insight into the nature of metallic conduction afforded by Sommerfeld's application of the idea of quantum degeneracy in the electron gas of conduction electrons. Simon's line of attack was not greatly different from one which I myself was pursuing at the same time. After Sommerfeld's new insight, both Simon (to judge from his publications) and I abandoned concern with this problem.

The Melting Curve of Helium

Another line of Simon's interest, which continued throughout his life and which made pretty close connection with my own interests, began in the nine-year Berlin period. This was determination of the effect of pressure on the melting curve of many of the permanent gases. Data on this subject had already been obtained at Leyden, but the pressure range was only 100 or 200 atmospheres, barely enough to show the initial slope of the curve. Simon wanted to extend the pressure into the thousands of atmospheres; such pressures, he anticipated, would be high enough to have a significant bearing on the question of whether the melting curve ends in a critical point, as does the equilibrium curve between gas and liquid. Simon made a number of experimental attacks on this problem. Throughout, his experimental results were correlated with a semiempirical formula for the relation between temperature and pressure, which he published with Glatzel in 1929. This formula was suggested by a simple modification of the formula given by van der Waals' analysis for the vapor-pressure curve, the simple modification being to introduce the "internal" pressure of liquid or solid. This has the result of eliminating a negative infinity at zero pressure, a pressure which has physical meaning for a liquid or a solid but not for a gas. Besides work with this formula, in this nine-year period he carried the melting curve of helium to 42°K and 5600 atmospheres. The method was that of the blocked capillary. It is a disadvantage of the method that it permits a determination only of pressure and temperature, not of change of volume or latent heat. These parameters are related through Clapeyron's equation, and if they could be determined they would give some basis for extrapolating to higher pressures and temperatures and therefore some basis for judging whether a critical point is imminent, a judgment which is not possible in terms of pressure-temperature values only. To rectify this lack Simon also made some measurements of the latent heat of melting of helium up to nearly 2000 atmospheres and 20°K. The measurements were too rough to give any significant information. To make a direct determination of latent heat under high pressure is extraordinarily difficult and, in the ordinary range of temperature, is not feasible at all, because of the masking of the heat capacity of the melting substance by the heat capacity of the containing vessel. So far as I know, this measurement by Simon of a direct latent heat of melting under pressure is the only one of any value which has ever been made, except for a similar measurement made under his direction later. It does not now appear to offer a practical line of attack on the problem of the existence of a critical point, because with increasing pressure, temperature is rapidly carried to values so high that the heat capacity of the pressure vessel is of dominating importance.

Cooling by Expansion and Adiabatic Demagnetization

In 1931 Simon left Berlin, becoming *Ordinarius* (full professor) and director of the Institute for Physical Chemistry at the Technische Hochschule of Breslau, a post in which he was Eucken's successor. Late in 1931 his stay in Breslau was interrupted by a visit to the United States. He attended the AAAS meeting in New Orleans in December of 1931, where I had the pleasure of renewing my earlier acquaintance with him, and in the spring semester of 1932 he was visiting professor at the University of California at Berkeley. Even as early as this Simon was astute enough to forecast correctly the course of political events in Germany. The prospect alarmed him so much that he removed his family to Switzerland for the duration of his stay in the United States. On his return to Breslau he was instrumental in obtaining for me some special German steel for my experiments on high-pressure helium, without which the experiments could not have been successful. No steel at that time manufactured in this country was sufficiently free from mechanical imperfections to support high-pressure helium without leak. At Breslau Simon apparently had a good deal of administrative work; early in 1933 he wrote me that he had little time for scientific work because, in his capacity as dean of chemistry and mining, he was actively engaged in the merging of the university and the Technische Hochschule.

It was during his stay in Berkeley that he developed his well-known expansion method for the liquefaction of small quantities of helium. He was able to demonstrate it as a lecture-table experiment and, with it, to demonstrate various phenomena connected with supraconductivity. The temperatures were indicated by his direct-reading gas thermometer. On his return to Breslau he set about converting the lecture-demonstration apparatus that he had used in Berkeley into a research tool for use at liquid helium temperatures, a tool particularly adapted for small laboratories. From Breslau during this period he published, with Kurti, a paper on the specific heat of gadolinium sulfate at low temperatures. His interest was connected with the then recently made proposal of Giauque and Debye for reaching low temperatures by the adiabatic demagnetization of paramagnetic substances. Kurti and Simon showed that there are low-temperature anomalies in the specific heat of gadolinium sulfate connected with the energy differences between the different orientations of the elementary magnets which would make the proposed method for attaining low temperatures less efficient than had appeared from the preliminary calculation of Giauque, but which nevertheless would not lessen the efficiency enough to prevent this from being a fruitful method for reaching temperatures hitherto unattainably low. However, this presumptive diminution of the effectiveness of the magnetic method caused Simon to turn to an examination of the possibility of reaching lower temperatures by another method, that of adiabatic decrease in volume. He proposed to use adiabatic decompression of liquid helium, carried out in a number of successive stages. Barring the appearance of unknown properties in the behavior of helium at unreached low temperatures, there appeared to be no theoretical limit to the temperatures which could be reached in this way. For this, Simon worked out an elaborate arrangement with metal bellows for the expansion engine. Since, however, the maximum theoretical gain for a single stage of the process was a lowering of temperature to one half the initial temperature, the practical application appeared too clumsy to make a serious working out of the idea worth while, and it appears to have been abandoned.

At Oxford University

The political situation continued to deteriorate, with Hitler in power, and by the summer of 1933 things had reached such a pass that Simon resigned his Breslau post and in September 1933 assumed the duties of a research position at Oxford—a post which he owed to the interest of F. A. Lindemann (later Lord Cherwell), head of the Clarendon Laboratory. I know from my own correspondence that Simon had been contemplating this step for some time and had made a pretty thorough canvass of the various possibilities, of which there were not many. His departure from Germany was greatly facilitated, as I know from personal communication, by the fact that he had been awarded the Iron Cross, first class. The help which he received was doubtless informal rather than official in character, the decoration being held in high esteem by all sorts of persons with whom he came in contact during this period. At Oxford his position did not at first amount to much more than a foothold in a new country, since the resources available there did not permit him to duplicate or even approach the facilities which he had left. He had only one room for his experimental work, and because of the dearth of technical and secretarial help he had to perform many routine tasks, including the typing of his own letters. His position gradually improved; in 1935 it was to a certain extent regularized by his succeeding A. C. G. Egerton (later Sir Alfred Egerton) as reader in thermodynamics, but expansion of the facilities continued to be slow. As late as 1936 Simon was not clear in his own mind whether, in justice to his own scientific career, he should regard his position as permanent. Matters gradually improved, however; a new laboratory presently came in prospect, and with the expanded facilities to be anticipated Simon could afford to bide his time a little longer. In 1939, just before the outbreak of the war, he acquired British citizenship, and in this he took extraordinary satisfaction.

The outbreak of World War II not only interrupted Simon's scientific work but upset his family life as well. In June 1940 his wife and two daughters were evacuated to Toronto, accepting the invitation of the faculty of Toronto University to care for the families of the Oxford faculty. Acceptance of this offer seemed essential to the Simons in view of the expected invasion of England, in which, it could be expected, they would be singled out for special treatment because of their German and Jewish origin. Simon stayed behind in England where for the first year of the war he experienced bitter frustration, since, in spite of his eagerness to make any technical contribution he could, his offers of help in the war effort were repulsed. This was due to the initial reluctance of the authorities to employ ex-enemy aliens on secret work. Debarred from "official" war work, Simon began to interest himself in the problem of the atomic bomb, in particular the large-scale separation of the uranium isotopes, well before investigation of these matters received government support. When, in the latter part of 1940, official work on the atomic bomb began, Simon became associated with it and was throughout the war one of the leading figures of the "Tube Alloys" project.

I am able to say very little about Simon's connection with the bomb and atomic energy. I had no personal connection with it, and in the nature of things his activities have not been published. It is well known that he was in charge of a major part of the isotope separation work of the British, and in connection with this work he visited this country a number of times during the war. His connection with the project, and in particular with the British establishment at Harwell, continued after the war on a part-time basis. The importance of his work was recognized, and in 1946 the order of Commander of the British Empire was bestowed upon him. In 1954 he was further rewarded, with knighthood. He took a frank and lively pleasure in these honors, and I have a vivid recollection of his acting out in front of my fireplace, with the fire tongs for a sword, the ceremony in which Queen Elizabeth dubbed him "Sir Francis." He at one time said of himself that he was doubtless the only person who had ever received an Iron Cross, first class, and also a CBE.

Immediately after the war he was made professor of thermodynamics, a new position created especially for him, and a Student of Christ Church—evidence of his full acceptance into the British academic community and tradition which he highly appreciated. From then on, with his new laboratory and adequate resources, his work rapidly expanded in importance and influence, until his laboratory at Oxford became one of the world's recognized centers for low-temperature research. In 1956, on Lord Cherwell's retirement, he was made Dr. Lee's professor of experimental philosophy and head of the Clarendon Laboratory. He entered on his new duties only a few weeks before his death, on 31 October 1956.

Temperatures of a Few Hundredths of a Degree

We return now to Simon's scientific work at the time of his removal to Oxford in 1933. In spite of the meager facilities, a first paper appeared in *Nature* early in 1934, written jointly with Kurti, who had followed him from Breslau and remained associated with him at Oxford up to the time of his death. We have here a classical example of a fruitful scientific collaboration between two men, each complementing the other.

The subject of the first experimental paper with Kurti from Oxford was the production of very low temperatures by magnetic cooling, which, having been proposed in 1926 by Giauque and by Debye, was first carried out by Giauque in March 1933. As mentioned above, Simon had speculated a good deal on the experimental possibilities, and the fact that he had not attempted to test them earlier is evidence of his meticulous scientific courtesy and his unwillingness to trespass on a field opened up by another man before the latter had had opportunity to reap the first harvest. As a first result of reaching a temperature of 0.1°K, Simon and Kurti were able to announce a new supraconductor, cadmium. This experiment was made possible, as might be suspected, by Simon's expansion method for liquefying small amounts of helium. At about the same time Simon wrote a purely theoretical paper in which he showed that the unusual property of helium of remaining liquid to the lowest attainable temperatures and requiring the application of a pressure of 25 atmospheres to become solidified, was due to the unusually large role played by zero point energy, because of the weakness of the interatomic forces in helium.

One would never guess from Simon's bibliography after his transfer to Oxford that he was working with meager resources. In all, he published 49 scientific papers from Oxford between 1934 and 1942, when his scientific publication was suspended because of the war. His principal interest during the earlier part of this interval was development of the magnetic method of cooling. He did a number of experiments in Paris, using the magnet at Bellevue, at that time the largest magnet in the world. One of the most interesting of his discoveries was that of the ferromagnetism of a number of salts below their Curie points of a few hundredths of a degree—salts which above this point are paramagnetic. His work in Paris would hardly have been possible without his expansion method for working with small quantities of liquid helium.

An important problem in the newly accessible temperature region of a few hundredths of a degree is the measurement of temperature on the absolute scale, and Simon devoted considerable effort to this problem. Experimentally the simplest procedure, but a provisional one, on first entering this field, is to specify the temperature in terms of an assumed extrapolation of Curie's law that magnetic susceptibility is proportional to the reciprocal of the absolute temperature. However, this extrapolation is known to fail at low temperatures, as indeed it must, because if Curie's law continued to hold, absolute zero could be reached by an adiabatic demagnetization. The problem becomes, therefore, one of converting the apparent Curie temperatures into absolute temperatures. Simon did this by direct application of a Carnot cycle between the two temperatures, using the fundamental formula of the second law:

$$Q_1/T_1=Q_2/T_2.$$

Here T_2 is the unknown temperature in the new region (defined and reproducible, however, in terms of its "Curie" temperature), and T_1 is a temperature in the region of higher temperatures in which the absolute scale has already been established. Application of the second law and determination of the absolute temperature T_2 therefore is reduced to a determination of the quantities of heat Q_1 and Q_2. Q_1 may be assumed to be measurable by methods already in our command, but the measurement of Q_2 in the new region is not simple. Simon solved the problem of measuring Q_2 by absorbing a known quantity of gamma rays, a most ingenious method which he utilized on a number of other occasions, and which he subjected to a careful analysis to be sure that there were no flaws in it.

In connection with this question of reduction to the absolute scale of temperature, Kurti has called it to my attention that Simon in two of his earliest papers effectively established absolute temperatures in the hydrogen region by a direct application of the second law of thermodynamics to the sublimation equilibrium of solid hydrogen at different temperatures, using the triple point as the fiducial point at which the absolute temperature is assumed known. Such a direct thermodynamic establishment of an absolute temperature is frequently discussed in the textbooks but is practically never carried out.

Liquid Helium II

The properties of liquid helium was another subject actively pursued by Simon during the prewar years in Oxford. An important procedure in this work was the first explanation (with Rollin) of some of the very puzzling phenomena shown by helium II as being due to a thin creeping film which covered all the accessible surfaces of the containing vessel.

Nernst's Heat Theorem

Simon spent the first couple of years after his return to Oxford after the war in equipping the new laboratory. Novel apparatus and methods were used which were the subject of numerous papers. His first paper of the postwar period was not published until 1948, six years after publication of his last previous paper. This interval of six years is, by coincidence, exactly the same as the time lost from his scientific career because of military service during World War I.

In his postwar period Simon returned to re-examining and making more precise his earlier work with Nernst's heat theorem. To this he devoted several papers, of which by far the most important is the Guthrie lecture to the Physical Society, delivered in March 1956. In this lecture a detailed history is given of the various developments, and the magnitude of Simon's own contributions appears particularly clearly. Simon was qualified as no other man to give this summary, both because of his early association with Nernst and because of his own contributions. Simon's contention in his lecture was that Nernst's heat theorem has by now, by general recognition, become truly a third law of thermodynamics, coordinate in importance and range with the first and the second. A great deal of the early skepticism about the law was based on the many anomalies in specific heats at low temperatures. It was largely due to Simon's own work that nearly all these anomalies were recognized to be quantum effects which result in abnormal and temporary liberations of energy during the degeneration of various degrees of internal freedom, followed at lower temperatures by domains in which the complete order is attained, a situation which means zero entropy for all the regular phases.

In addition to his work on the many substances with recognizable quantum anomalies, Simon was probably the first to recognize and insist on finding an explanation for another class of apparent exceptions to the third law, exemplified by the glasses and subcooled solutions. Here we are dealing with "frozen-in" degrees of freedom, by virtue of which the substance is incapable of coming to a state of true thermodynamic "equilibrium." According to Simon, such systems are not properly thermodynamic systems at all, and it is not to be wondered at that the third law of thermodynamics does not apply to them.

In arriving at this position Simon was led to examine with great care the concept of a "glass," and he made the very important factual discovery that in the comparatively narrow temperature interval within which the degrees of freedom are in process of being frozen in, other characteristic phenomena occur, such as abnormal behavior of the specific heats or of the temperature-dependence of viscosity. It seems to me that it is here, in his factual discoveries, that Simon's important contribution is to be found, rather than in his insistence that we have here a true third law of thermodynamics, which appears to me to be to a certain extent a verbal matter. For after all, conventional thermodynamics does control the ordinary behavior of glasses; no one would hesitate to apply to them such a formula as, for example,

$$\left(\frac{\partial C_p}{\partial p}\right)_T = -T\left(\frac{\partial^2 v}{\partial T^2}\right)_p$$

which is derived from the first and second laws.

Another topic which occupied Simon in his postwar period was the nature of the melting curve. He was never satisfied that my own experiments to high pressures on various melting curves had sufficiently established the improbability of a critical point between liquid and solid, and he always ascribed a particular significance in the answering of this question to a determination of the melting curve of helium to the highest feasible pressures. Already, before leaving Germany, he had, in collaboration with Ruhemann and Edwards, followed the melting curve of helium to 5600 atmospheres. With an altered technique he was now able, in collaboration mainly with G. O. Jones and D. W. Robinson, to carry the melting curve to nearly 10,000 atmospheres and 60°K. It was still a disadvantage of his method that he was not able to determine all the parameters necessary to completely characterize the transition thermodynamically. This effect was now rectified by new work at Oxford by Dugdale, who was able to measure directly the specific heat of compressed solid helium and the heat of melting up to 3000 atmospheres. It appeared from these thermal measurements that the entropy difference between liquid and solid increases along the melting curve with increasing pressure and temperature. Since at a critical point the entropy difference vanishes, here is strong presumptive evidence that the melting curve of helium, at least, is not headed for a critical point.

Simon closely correlated his experimental work on this topic with the formula which he had derived as early as 1929 on a semiempirical basis, namely,

$$\log (p + a) = c \log T + b.$$

At that time a connection was indicated, by analogy, between the constants of the formula and the internal pressure of van der Waals. On Simon's return to the problem after the war, various collaborators succeeded in strengthening the theoretical implications of the formula, in particular Salter, by showing a connection with the internal pressure (of the gas). Now the experiments on helium are unique in the enormous range of both pressure and temperature when expressed, as reduced parameters, in terms of the critical constants of vapor-liquid. The attainable temperatures and pressures for helium are of the order of ten times the critical temperature and 500 times the critical pressure, whereas for many ordinary substances these factors may be of the order of unity and 100, respectively. Because of this Simon felt justified in applying the same type of formula by extrapolation to other substances, and in particular to the melting of iron. This formula predicts, for the pressure at the interior of the earth, 4×10^6 atmospheres and a melting temperature of the order of 4000°K, a result of obvious interest to the geophysicist.

Diamond-Graphite Transition

A closely related topic which received Simon's attention after the war was the diamond-graphite transition and equilibrium. The calculation of the location of the transition line afforded a natural application of the third law, with some unusual features because of the abnormally low specific heat of diamond at room temperature. As early as 1926 Simon had published an estimate of the equilibrium relation between pressure and temperature. After the war he returned to the question and with Berman published a paper in which the most recent thermodynamic data were used, including certain new data obtained by other collaborators at Oxford, in order to rule out the possibility of unsuspected low-temperature anomalies. This calculation of the probable course of the equilibrium was unique in its careful estimate of the uncertainty introduced by various doubtful factors. Simon and Berman calculated an equilibrium pressure at 1200°K of approximately 40,000 kg/cm², with a linear rise above that at the rate of 27 kg/cm² per degree. Before the paper appeared in print, the General Electric Company had announced the successful synthesis of diamond. The details have not yet been published, but from everything that has appeared it would seem that the General Electric conditions fall within the limits of Simon and Berman's estimates. Simon also had a novel idea about the synthesis of diamond. His idea was that the great reluctance of graphite to take up the stable diamond form at low temperatures and high pressures could be overcome by neutron bombardment, and he had some calculations to justify this. He constructed an apparatus in which an ostensible pressure of nearly 30,000 atmospheres could be imposed on a piece of graphite of sufficiently small dimensions; the graphite was then subjected to prolonged neutron bombardment in the pile at Harwell. The results were negative. Simon believed that the apparatus actually did not deliver the required pressures and expected to repeat the experiment under more favorable conditions.

Thermal Conductivity

Another area of postwar activity for Simon was thermal conductivity, particularly of dielectric crystals. The measurements demanded control of temperature over the entire range, from room temperature down, and, as such, demanded certain changes in his customary technique. Working with Berman and Wilks he clearly demonstrated the two fundamental processes responsible for thermal resistance in dielectric crystals: the Umklapp type of process between phonons, first proposed by Peierls, and scattering at the boundaries, proposed by Casimir. These experiments were subsequently extended in considerable detail and illustrated the influence of lattice defects.

Nuclear Orientation and Cooling

Without doubt the most important part of Simon's postwar scientific work was that connected with nuclear orientation and, in particular, with nuclear cooling, in which the magnetic moments of the nuclei of the atoms at excessively low temperatures are utilized in very much the same way as the paramagnetic moments at higher temperatures. This possibility was suggested soon after the paramagnetic method had been proposed, by Gorter in 1934 and nearly simultaneously by Simon and Kurti in 1935. There were, however, formidable technical difficulties when it came to putting the idea into practice. The initial temperature from which the demagnetization was to occur had to be of the order of 0.01°K. Heat transfer between the magnetic and the nonmagnetic parts of the system also introduced complications. After long preparation, successful results were obtained by Simon, with Kurti, Robinson, and Spohr, and the announcement that temperatures of about 2×10^{-5} °K had been attained was published in *Nature* only a few weeks before Simon's death. Thus, a new field was opened for understanding the properties of matter.

Public Figure and Humanitarian

Simon's connection with the atomic energy project both during and after the war brought him into such close contact with industry and public affairs that it was only natural that concern with broad questions of public policy should occupy a large part of his time after the war, and he became increasingly recognized as a public figure. His usefulness in the field of public policy was increased by his extensive travels, which had given him a wide first-hand acquaintance with conditions in Europe and America. At the invitation of the *Financial Times* of London he wrote an extensive series of articles, which received wide attention and doubtless exerted considerable influence. In these articles he emphasized especially the importance for England of an adequate supply of scientists and engineers, he called attention to the woeful lack of educational facilities, and he strongly urged the immediate establishment of several technical institutes of the caliber of the Technische Hochschule in Zürich or Massachusetts Institute of Technology. He saw the widespread ignorance of scientific matters, on the part not only of the lay public but also of the members of the boards of directors of many large British industries, as part of the same picture and as the cause of the country's having fallen far behind in the technological race with other countries. A number of his articles on this theme were collected into a little book, *The Neglect of Science*. He was vividly aware that a crisis was approaching in the country's power supply, chiefly in the supply of coal, which was continually becoming less adequate. He called attention to very wasteful practices in the regular use of coal. He put his finger on the single most important factor in this waste—the almost universal habit of heating houses with open coal grates. More efficient methods of house heating would, he calculated, save 20 million tons of coal a year, a vital amount in the present state of the economy. As a simple method of eliminating this waste he suggested replacing the open grates with closed stoves, and he urged that the government distribute such stoves free to all who would use them. Many people recognized the justice of his comments, but it does not appear that anyone has done anything about it.

In connection with the fuel-economy picture, Simon was much concerned with the development of atomic power for industrial purposes, and he wrote a number of articles on the practical aspects of the utilization of atomic energy and the probable timetable for such utilization. He was concerned lest people should be too optimistic and think that we have here an easy solution of all our troubles. On his return from the Geneva conference of 1955 on the

Sir Francis Simon

peaceful uses of atomic energy he wrote me that he was disturbed because he felt the small nations had been given an all-too-rosy picture of the prospects for them.

Kurti has remarked that there seems to be a single guiding motif running through all Simon's concern with public affairs—namely, his horror of waste in any form, fostered by his life-long experience with the third law of thermodynamics and his vivid appreciation of the significance of the growth of entropy. When, at the end of the war, he abruptly stopped smoking, he remarked, perhaps jocosely, that for the money that went up in smoke he had better help someone in need.

It remains only to pay a tribute, however inadequate, to his personal qualities, through which he endeared himself to all with whom he came in close contact. Perhaps his most outstanding trait was his intense interest in people for their own sake. His interest manifested itself in many practical works, which ranged from helping a student find a boarding place for himself or his family to elaborate efforts to get refugees out of Nazi Germany, which resulted quite literally in the saving of lives. His home in Oxford became a sort of clearinghouse for scientists who had to leave Germany and for whom he tried to find positions. As a result of his own experience in getting out of Germany and of his later experience in evacuating refugees, he acquired a certain know-how, not without an element of cynicism, for dealing effectively with officialdom. At the time of his visit to this country in 1953 to attend the Rumford celebration of the American Academy of Arts and Sciences I happened to be sufficiently behind the scenes to witness Simon's tactics to ensure that he did not encounter the inconveniences in getting an American visa that were being experienced by so many other foreign scientists at that time; these tactics can only be described as masterly!

The rapid turnover of his secretaries was legendary, it being the almost invariable rule that after a year or two they left to marry a graduate student or one of his young colleagues—something which gave Simon the keenest pleasure in spite of the obvious inconvenience to him.

He had a refreshing and somewhat Puckish wit, of which the subject might equally well be himself or someone else. He had his foibles; his dislike of drafts and of the insufficiently heated English rooms (conditions which caused him to don, as occasion demanded, cap and sweater and muffler) is well known. Although he flew a great deal, he did not like flying, and before pressurized cabins were common he always traveled with an oxygen bottle, apparently for very real reasons of health. However, his colleagues were not always sympathetic about such measures, and on at least one occasion Simon's maneuvers to supply himself with his oxygen flask without public knowledge provided much entertainment to those who knew of them.

Simon's adaptability in transplanting himself to a foreign country at the age of 40 and making himself so valuable as to be officially rewarded in the way he was is well-nigh unique in these times. He felt himself thoroughly at home in England, although he continued to hold the view that in some respects the English were a little mad. He even acquired some of the exterior markings of an Englishman. I remember that after he had appeared in the dining-room of the local hotel, in the New Hampshire town where I have my country home, one of my friends, on learning that it was *Sir* Francis Simon, remarked, "I spotted him at once for a typical Britisher." I am told, however, that his British colleagues were under no such illusions. His English was fluent, but he never felt that he had mastered all the niceties of the language, and he always gave his papers to friends for editing before submitting them for publication.

To a high degree his was a vital, almost exuberant, personality. To it was due in great measure the richness and fertility of his scientific ideas, which permitted him to catalyze so successfully the work of his many students and colleagues. It is hard to realize that we shall no more see him in our midst, and hard to justly assess the magnitude of our loss. His memory will long be with us.

Note

In preparing this article I received valuable assistance from a number of Simon's intimate associates, in particular N. Kurti and Lady Simon.

Charles Judson Herrick, Neurologist

The leitmotiv of Charles Judson Herrick's scientific career was the integration of the several disciplines involved in the understanding of the nervous system. When such a plan was originally envisioned by his elder brother Clarence, in 1891, the factual basis was meager, and a frontal assault on the barriers between university departments could not be tolerated. In the course of 70 years, "C.J.," as his friends called him, contributed a wealth of histological detail on the fundamental structure of the nervous system and correlated his findings with the rapid advances of the 20th century in psychology, physiology, and psychiatry.

From early boyhood his brother had been his teacher in "natural history," and before graduating from college Herrick began to study the brains of fishes. In 1893 his brother had to resign his professorship at Denison University and hasten to Arizona because of tuberculosis. "C.J.," two years out of college, undertook to conduct, single-handed, all of the scheduled courses in biology. He assumed an even heavier burden in saving the *Journal of Comparative Neurology*, which his brother had founded, "because its suspension would break my brother's heart and retard his recovery." He became editor, business manager without a secretary, proofreader, and supervisor of the printing of engravings. Until 1908 he paid the inevitable deficits of the journal from his own salary. On one occasion he felt called upon to decipher a manuscript in longhand which had been charred black in the fire which destroyed his laboratory. Many a robust man would have been overwhelmed by these labors. They did not keep C. J. Herrick from continuing his research.

After a year's leave of absence in 1896 he completed his doctoral dissertation, a monograph of 302 pages. It was the first complete analysis of the cranial nerves and contributed more than any other single work to the establishment of the "American School," which was interpreting the structure of the nervous system in terms of its functions. By 1907 he had published 17 papers on the nervous system of fishes in which certain afferent systems are highly developed and their centers in the brain are hypertrophied, so that the connecting fibers can be followed without experimental interference. A study of the behavior of normal catfish (1902) clearly demonstrated the gustatory function of the "terminal buds" which are distributed over the skin of head and trunk.

The reputation established by these studies resulted in an invitation to Herrick to become professor of neurology at the University of Chicago when H. H. Donaldson resigned to go to the Wistar Institute in 1907. Herrick hesi-