plague. They are engaged in a fight against cancer, which is far more persistent and no less deadly.

There was an account in Thursday's *Evening Post* of the work accomplished by physicians of the Memorial Hospital, to which Edward S. Harkness recently gave $250,000 for the purchase of four more grams of radium. From this publicity Dr. James Ewing, director of cancer research at the hospital, shied as though one were asking him to give a trapeze performance. About a year and a half ago, when the new treatment was first announced in the press, Dr. Herbert A. Quick, who has directed its application and has had much to do with its development, is said to have acted as though he were being disgraced. Mr. M. Failla, the laboratory worker whose suggestion it was that gold instead of glass be used in the "seed" tubes now implanted in cancerous growths—a vital element in the new treatment never before has been publicly mentioned in connection with the discovery. Nor has Dr. Max Cutler, who first worked out the problem on animals before it was applied to human beings.

There is a deep-seated prejudice against publicity, the heritage of many of the ablest men in the medical profession, which plays its part in the suppression of information which should be presented to the public. One of the most important reasons for this feeling lies in the constant flood of "claims" which second-rate scientists make of "discoveries" and "cures." Some of these announcements are made through overenthusiasm and some through a desire to take advantage of the public's ignorance of science.

Hence the practice has grown up among men of science of not making public announcements of their findings until they have been presented before groups of leading men in the profession, who can discuss and criticize them in the full light of scientific knowledge before the public is informed. This is a wholesome procedure. But the fear of publicity is carried too far when leading men are afraid to speak for publication even after their work has been tested in the scientific society's conferences and in the laboratories of coworkers, simply because they fear criticism from members of their own profession.

It is largely because of the reticence of the men best qualified to speak that those not nearly so well qualified occupy so much of the newspaper space devoted to science. It is also partly because of this reticence that publicists have conceived the notion that the public wants its science information jazzed up and distorted. The public never was so hungry for authentic information as it is to-day. If men of science were to speak freely where they find a disposition to report news of science sanely an *entente cordiale* between science and journalism would be established which would be of inestimable value to both—and to the public.—*New York Evening Post.*

SCIENTIFIC BOOKS

Collected Papers of Sir James Dewar. Edited by LADY DEWAR. Vol. I. 27×17 cm; pp. xxii+674. Vol. II. 27×17 cm; pp. ix+814. Cambridge and New York: The Cambridge University Press; The Macmillan Company, 1927.

To many chemists Dewar is known as the Englishman who specialized in low-temperature work and as the man who invented the thermos bottle. Only relatively few know much about his spectroscopic work in collaboration with Liveing and yet his high-temperature work began in 1872 and was continued up to 1889. Since the first low-temperature paper was published in 1884 and since Dewar was appointed Jacksonian professor of natural philosophy in the University of Cambridge in 1875 and Fullerian professor of chemistry in the Royal Institution of Great Britain in 1877, it is evident that he had made a distinguished name for himself before he ever started on low-temperature work.

New to most people will be the work on the physiological action of light, on electrophotometry, on capillarity and on the properties of nickel carbonyl. A good many people know that he collaborated with Moissan in studying the properties of liquid and solid fluorine.

What very few people realized, outside of his personal friends, is the surprising versatility of the man. When he studied low-temperature problems, for instance, he covered the whole range. It was not merely a question of developing improved methods of liquefying gases and of determining melting points, boiling-points, and densities. Dewar studied specific heats, latent heats of vaporization, diffusion, adsorption by charcoal, optical and magnetic properties, color, photochemical reactions, the effect of low temperatures on bacteria and on electrical resistance, etc., etc. It is a real pleasure to note how many sides Dewar saw to a problem. Everybody knows that Dewar was a marvelous manipulator; and this fact is impressed on the reader because, in these volumes, one runs the whole gamut in so short a time.

It is interesting to note Dewar's attitude towards physical chemistry as far back as 1888. "At the present time we may say that there are two large schools of chemistry: one school cultivating organic chemistry, in which structural or atomic building up of an atomic character is carried out on a gigantic or manufacturing scale, producing thousands of new bodies every year, and continually increasing in the energy of its work and the variety of its productions. On the other hand we have another school in which physical chemistry is predominant, where the physical relations of chemical action are minutely examined, and where the effect of the physical relations of the constituents taking part in chemical changes is being more and more elaborately studied," p. 307.

Physical chemistry, as Dewar used the term, started with Black whose early lectures were entitled "Lectures on the Effects of Heat and Mixture." "Black used to institute a parallel between the phenomenon attending the hydration of lime and that which occurred when steam was condensed to water, both being accompanied by an evolution of heat and a change of state. The one might be called a chemical change, but for him it was analogous to the simpler change of physical state involved in the other," p. 313.

"The chief idea [in the set of experiments made by van't Hoff] is to correlate concentration with varying gaseous density, so that the reactions of salt solutions are brought into harmony with gaseous reactions. This is the work which van't Hoff has attempted, and very remarkable it is to consider the concentration of a solution containing a certain quantity of a salt as if the substance was in the gaseous state, and prove that the pressure of the dissolved solid is practically the same as if it had been volatilized into the same volume. Thus a new view has been opened up which brings into harmony the whole question of solutions of varying concentrations and the laws regulating their reaction," p. 314.

In 1899 Dewar published a diagram, p. 687, showing that the extrapolated resistance of a platinum resistance thermometer is zero at about $-245°$ C., and he concludes that probably the resistance curve becomes practically asymptotic to the temperature axis. In 1902 he comes back to the subject in his presidential address to the British Association, p. 785. "All known liquids, when forced to evaporate quickly by being placed in the exhausted receiver of an air pump, undergo a reduction in temperature; but when hydrogen was treated in this way it appeared to be an exception. The resistance thermometer showed no such reduction as was expected, and it became a question whether it was the hydrogen or the thermometer that was behaving abnormally. Ultimately, by the adoption of other thermometrical appliances, the temperature of hydrogen was proved to be lowered by exhaustion as theory indicated. Hence it was the platinum thermometer which had broken down; in other words the electrical resistance of the metal employed in its construction was not, at temperatures about $-250°$ C., decreased by cold in the same proportion as at temperatures about $-200°$. This being the case, there is no longer any reason to suppose that at the absolute zero platinum would become a perfect conductor of electricity; and in view of the similarity between the behavior of platinum and that of other pure metals in respect of temperature and conductivity, the presumption is that the same is true of them also." This conclusion was unfortunate because Kamerlingh Onnes showed, some years later, that the electrical resistance of gold, mercury, and platinum vanishes at the temperature of boiling helium, p. 1103.

Dewar was once the victim of the misleading experiment. In 1873 he determined the vapor density of potassium in an iron vessel and found that the molecular weight corresponded to the formula K_2, p. 72. In 1879 he repeated the work, using the Victor Meyer method which had been described the year before. The molecular weight varied from 74.4 to 90.5 with a mean value of 81.6 as against 78.2 calculated for K_2. Sodium gave values varying from 40.9 to 63.6 with a mean molecular weight of 50.7 as against 46.0 for Na_2. "Four pieces of potassium weighed in iron capsules and thrown into water gave 84.2 as a mean molecular weight, calculated from the hydrogen evolved. Four pieces of sodium similarly treated gave 49.8 as a mean. The above experiments show that no great accuracy in the determination of the vapor-density of sodium and potassium can be attained by this method of working when vessels of wrought iron are employed. The results are, however, conclusive as regards the *normal character of their respective vapor-density*," p. 177. Later in the same year Dewar and Scott repeated these experiments, using a platinum vessel, getting mean molecular weights of 26.4, 25.8, and 24.9 for sodium, and of 44.0 and 40.7 for potassium. "The new determinations of the molecular weights of potassium and sodium are just about half the former values, and would seem to support the inference that the atom of each of these metals resembles mercury and cadmium in the gaseous state, as regards molecular volume. Such a remarkable result can not be accepted without a very thorough investigation of the secondary reactions which may be induced at high temperatures. It is certain that in the platinum vessels there is much less absorption of vapor than in the case of iron, and that the action of the furnace gases is all but eliminated," p. 183.

Under Pictet's experiments on the liquefaction of hydrogen, we read, p. 169, that "hydrogen in the liquid state at a temperature of $-140°$ has a pressure of about 320 atmospheres and appears to solidify in the tube when the fluid jet is allowed to escape. The jet of liquid has a steel blue color." The color was not surprising in those days when it was thought that solid hydrogen was probably a metal. We know now that hydrogen is colorless both as solid and as liquid. Consequently the steel blue color was either due to an impurity or was a structural color.

In a paper on "The Coming of Age of the Vacuum Flask" in 1914, Dewar points out that as far back as 1873 a highly exhausted annular metallic vessel was used by him in calorimetric experiments. It was not until 1893, however, that he described the use of glass vessels. During the war the all-metal thermos bottle was developed, thus harking back to the original type.

While one can not fail to be impressed by the brilliancy and versatility of Dewar's work, it is surprising that there is so little in the way of theory. The truth of the matter seems to be that Dewar was what the reviewer has called an accumulator rather than a guesser. He was intensely interested in experimentation and he did not care at all for the theoretical bearing of the experiments. He did a good deal of work on adsorption of gases but he never cared about the laws of adsorption. This appears even more strongly in his work on soap films. The results are fascinating and are veritable triumphs of experimental ingenuity. Bubbles were blown four feet in diameter which lasted several hours; one bubble, 46 cm. in diameter, lasted sixty-three days; and a horizontal black film, 20 cm. in diameter, lasted for a year. Dewar gives all details; but he draws no theoretical conclusions from them and makes no effort to do so.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE GINS METHOD OF DEMONSTRATING CAPSULES OF BACTERIA

WHILE using the Burri India ink method of studying bacteria, especially for demonstrating the various forms of the diphtheria bacillus, the idea occurred of attempting the demonstration of capsules of bacteria through staining of the India ink films. The method was tried with excellent success. Subsequent search of the literature has shown that the method in all its essential details has been described previously by Gins.[1] Since the method has worked out very well in class use and does not appear to be generally known in this country, I am giving the technic for the information of others.

The ordinary India ink sold in this country for drafting purposes can be used for this purpose providing it is free of bacteria. This, however, was not the case in several samples recently purchased, there being large numbers of organisms present. An ink which is prepared especially for the purpose is that of Grübler, known as Pelikan tusche No. 541. This ink probably contains a preservative since bacteria have been absent in the samples examined.

The ink usually works better when it is diluted with an equal amount of sterile distilled water. A drop of the diluted ink is placed near one end of a very clean slide and a loopful of the bacterial suspension is carefully mixed with it. The mixture is then spread across the slide with the edge of a second as when making a blood smear. A properly made preparation should be uniformly spread and of a grayish color rather than black. After drying in the air, the film should be fixed by heating or preferably by dipping in methyl alcohol. The slide may now be stained with any of the ordinary bacteriological stains including the Gram method. If the film is too thick it sometimes loosens after fixation, but properly spread films seldom give any trouble in this respect. A cover-glass may be used to protect the film but such protection is not needed if the slide is carefully handled.

Under the microscope well-stained organisms can be seen lying in lacunae in the film of ink. The margin of the capsule is sharply delineated by the ink, and the margin of the bacterial cell is sharply delineated by the stain. Between the two is a clear space which represents the capsular substance. If the film of ink is too thick, shrinkage of the film may produce separation of the ink from the cell wall, thus giving rise to an artifact which resembles a capsule. When the ink is properly diluted this difficulty has not been met with.

This method has been used successfully in class work using the Friedlander bacillus, the anthrax bacillus and the pneumococcus as test organisms. The anthrax organism shows a thin capsule even in cultures which have been continuously in artificial media for many generations. A similar capsular substance has also been evident in the streptococcus cultures which have been examined by this method. These organisms do not show any evidence of capsular substance when examined by other capsule demonstrating methods and are not capsulated in the ordinary sense of the word. Apparently some sort of intercellular substance exists in all chain-forming organisms and it is this material which is demonstrated by this method.

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[1] Centralbl. f. Bakteriol., Parasitenk. u. Infektionskrankh. Abt. I Orig. 1911, Bd. 57, 477.