HISTORY OF THE VACUUM FLASK*

BY SIR WILLIAM BRAGG, O.M, K.B.E., PRES. R.S.

FEW laboratory devices have achieved the popularity of the vacuum flask. Since Sir James Dewar designed it for the purpose of preventing his liquid air from rapid evaporation, the flask has become a household friend and an invaluable tool in the laboratory and the workshop. It is so very simple and yet so very efficient. Each of the ways by which heat can pass from place to place, by convection, by conduction and by radiation, is almost entirely blocked, with the result that everyone knows.

Fig. 1.

GENERAL FORM OF A CURVE SHOWING THE RELATION BETWEEN THE RATE OF TRANSFERENCE OF HEAT ACROSS AN AIR SPACE AND THE PRESSURE OF THE AIR.

Quite apart from the aptness of the design, the gradual discovery of the principles on which it is based makes a story of great interest. The pioneers in the study of heat believed that an examination of the methods by which heat is transferred would offer the best chances of discovering the nature of heat itself. Consequently their experiments were remarkable in themselves; and moreover, their interpretations of what they observed throw a curious light on the workings of their minds as they tried to establish one or other of the widely differing theories then held.

Thus Rumford in 1785 hung a thermometer by a thread in a closed vessel and set out to find whether or no heat was helped or hindered on its way to the thermometer by the air through which it must pass. He expected to find that removal of the air would hasten the transference of heat,

because he believed that heat was associated with motion, and that the transference was effected by a wave motion in a medium which could not be evacuated. The air in his vessel would not on that theory be the actual carrier of the heat and might well be a hindrance. He formed a Torricellian vacuum in his vessel by first filling it with mercury, which was heated to drive off any attached air, and then draining it in the usual way. It then appeared that the transference was less rapid than when the vessel contained air at atmospheric pressure. This may not have been what he expected, but nevertheless it favoured his theory because it seemed to show that heat could cross a space void of air. As a matter of fact, there must have been enough air left to be important, but he could not have known that. His critics, whom we may take to be represented by Leslie—whose experiments will be described later—supposed that air was alone responsible for the transfer and refused to believe that heat could cross a vacuum.

As we know now, the relation between the flow of heat across any space and the pressure of the gas in that space can be represented by a curve of which the general form is shown in Fig. 1.

The positions of the bends at *A* and *B* and the dimensions of the curve depend on the form of the experimental vessel, the condition of its walls and the nature of the gas which it contains. If the emitting and the absorbing surfaces within a vessel are small compared with the dimensions of the vessel, convection will play a large part unless the pressure is small, and the bend at *B* will then be far to the left of the figure. If convection is not serious, a long level stretch from *B* to *A* will show that the conduction is independent of pressure—as Maxwell proved—until at *A* the free path of the molecules becomes comparable with the dimensions of the vessel. If evacuation passes this point, conduction rapidly ceases and radiation acts alone.

Rumford's vacuum was good enough to take him some way round the corner at *A*, but he could not have known how far he had yet to go. Air was still doing most of the work. He may perhaps have reduced the pressure to a tenth of a millimetre.

A little later (1798), Rumford carried out his famous experiment of the generation of heat during

* Substance of lectures at the Royal Institution delivered on December 7 and 14.

the boring of a cannon. About the same time he compared with great care the weight of a mass of water when hot and when cold and, finding no difference, placed the result to the credit of the 'motion' theory. The supporters of the theory that heat was a form of fluid were by no means impressed by all these arguments. Davy and Young supported him, but they were very much in the minority. Leslie was an energetic opponent. "In the infancy of science", wrote Leslie ("An Enquiry into the Nature of Heat", p. 139 (1804)), "heat was supposed to consist in certain intestine vibrations". He had not changed his mind when he wrote "Dissertation Fourth" prefixed to the seventh edition of the "Encyclopædia Britannica" (1820).

Leslie supposed that light traversed space in material form and entered into some kind of combination with the matter of any body on which it fell; in that way heat was produced. Heat itself, as such, could not cross a vacuum. The transfer of heat across an air space was accomplished through the simultaneous agencies of air pulses and of air jets directed from the hot body. Herschel in 1800 showed that a thermometer when placed in the spectrum and also when placed well outside the red end of the spectrum showed the receipt of heat without the agency of light. It was then asserted that his experimental arrangements were at fault. "This bold hypothesis was for a time regarded with wonder and applause but the delicate observations of Berard soon demolished the fabric. The notion of dark rays of light which enveloped the science in mystery stands now therefore without any proof and is utterly discountenanced by sound philosophy" ("Dissertation Fourth", p. 636). Herschel's own interpretation of his experiment is very interesting. He supposed that there were two forms of radiation, one constituting light and the other heat, and that they overlapped. "Those who would have the rays of heat do also the office of light must be obliged to maintain the following arbitrary and revolting proposition: viz., that a set of rays conveying heat should all at once in a certain part of the spectrum begin to give a small degree of light" (*Phil. Trans. Roy. Soc.*, 90, 508). If this attitude seems surprising to us, we may well remember that we are less inclined in these days to consider our natural senses to be the perfect overriding judges of natural effects. Herschel would think that light and eyes were made for each other, that the intensity of light in different parts of the spectrum both as sensation and as cause of sensation was what the eye could see there, and that it was "revolting" to suppose that there was something of the same nature as light which the eye could not see.

Since Rumford made these first experiments, a number of workers have examined the passage of heat across a gas-filled space, not with Rumford's purpose of discovering whether air is necessary, but as tests of theories connecting amounts of transference with difference of temperature. Dulong and Petit did not use a high vacuum; they speak of 2 mm. mercury as being usual (*Ann. Chim. Phys.*, 7, 245; 1817), so that their evacuation cannot have taken them round the corner at *A*. In 1875 Kundt and Warburg (*Pogg. Ann.*, 156, 177; 1875) carried the experiment to the limit. They had good pumps and they cooked their containing vessels so as to drive off gases adhering to the walls, a procedure which they found to be absolutely necessary. As they arrived at the same figure for the transference no matter whether air, hydrogen or carbon dioxide had been in their vessel before evacuation, the last

Fig. 2.

THIS FIGURE IS TAKEN FROM DEWAR'S PAPERS, VOL. 2, P. 1265. IT SHOWS THE RELATION BETWEEN THE AIR PRESSURE IN THE ANNULAR SPACE OF A VACUUM FLASK AND THE LEAKAGE OF HEAT MEASURED BY THE AMOUNT OF LIQUID AIR EVAPORATED FROM THE INNER VESSEL. THE AIR PRESSURE IS VERY LOW, SO THAT THE EFFECTS OF RADIATION ARE CLEARLY SHOWN.

traces of gas must have been gone before the measurements were made. The remaining transference must have been due to radiation alone: in their case it was of the order of the fourth or the fifth of the amount when the vessel was full of gas, but of course this figure has no absolute meaning since it depends on the conditions of the experiment.

When Dewar began to construct his vacuum vessels, he also found that the greatest care must be taken to remove the last traces of gas: very often the vessels had to be kept in the heated oven for many days.

The next feature of the vacuum flask to be considered is the silvering of the walls bordering

on the evacuated space. In the early days, to which I have already referred, the connexion between the amount of radiation emitted or absorbed and the surface of the emitter or absorber was also a subject of interest, because, as already said, it might be expected to give information as to the nature of heat. Rumford and Leslie made experiments in this field, those of the latter being perhaps the best known. Their methods were similar, and their results also. Both used a 'thermoscope' resembling the earlier instrument which Leslie had made for his studies in hygrometry. Leslie's results were published in March 1804 in his "Inquiry into the Nature of Heat": Rumford's results were published in various places, some of them, for example, in the *Transactions of the Royal Society* in January 1804.

The two authors were probably unaware of each other's work and their publications may be considered to be simultaneous. Their interpretations were, of course, poles apart. Leslie supposed that metal surfaces reflected well, but emitted or absorbed badly because there was a layer of air in a special condition next all surfaces across which heat pulses could not pass, and that this was narrow in the case of non-metallic surfaces but wide in the case of metals unless the surface was roughened, in which case the peaks projected into the outer air. Rumford did not attempt such an intimate explanation, but merely recorded his results in terms of vibratory motion. He made one interesting mistake. He ascribed a definite frequency to each temperature, not in the modern sense that for each temperature there is a range of frequencies of which that existing in greatest intensity is characteristic of the temperature, but on the assumption that for each temperature there was one frequency only. When a hot body *A* radiated to a cold body *B*, the former emitted rays of a higher frequency which were "calorific" to *B* and *B* emitted rays of lower frequency which were "frigorific" to *A*. Rumford was further in error in supposing that fluids did not conduct heat; he was misled by the magnitude of the part played by convection and missed the relatively small part played by conduction in the experimental arrangements which he made. Dalton and others pointed out his mistake.

It will be remembered that Rumford discoursed widely on the consequences of the laws of heat displayed in natural phenomena. He discussed also the proper application of the laws to human activities, and it is not surprising to find him pointing out that if heat is to be prevented from passing between two vessels, one of them inside the other, it is well to silver the outside of the inner vessel and the inside of the outer ("Complete Works of Count Rumford", 2, 243). The same idea

must have been put forward frequently when the main laws of heat were understood. For example, in a book written by Gompertz in 1850 (see letter by G. H. Gabb, *Daily Telegraph*, Oct. 12, 1935), a 'fireproof box' is described which is to consist of one metal box supported on springs within a second. The space between is to be evacuated of air to prevent conduction and convection, and the metals are to be well polished to prevent radiation. Probably the author of this idea never tried to carry it out, or he would have discovered the difficulties of construction.

Dewar was obliged to face those difficulties when during 1892-94 he was in urgent need of effective containers of liquid air. With characteristic drive and energy, he succeeded in obtaining all the vessels he wanted of many forms and sizes. One of the chief technical difficulties was naturally the join of the two vessels, inner and outer: it was difficult to get a join that could stand—as Dewar put it—"the torture to which the glass is put". A rubber cap was often provided if—in the case of the early vessels—it was necessary to pour the liquid air over the junction. The first vessels were made in London by Muller and by Gillingham: they were afterwards obtained in large quantities from Germany. Mr. George Gabb has told me that he helped Muller with some of the first silvering; and Mr. Gillingham has shown me some of the appreciative letters which Dewar wrote to him.

A very great improvement in the production and subsequent maintenance of the vacuum was made when Dewar made use of the absorbing power of charcoal. Dewar points out that Davy employed caustic potash to remove traces of carbonic acid left after his air pump had done its best: and further, that de Saussure in 1815 made an extensive examination of the absorbing powers of charcoal from different sources. Tait and Dewar used this effect in 1874; they found coconut charcoal to be the best. When large containers were required, especially during the War of 1914-18, it was necessary to make them of metal, as the mechanical strength of glass was insufficient. Without charcoal the vacuum in these vessels could not have been maintained.

Dewar made splendid use of his ingenious device. His experiments had a special directness, and some were of great beauty. It is so long since they were shown in the theatre of the Royal Institution that many may like to see them, either because they have not seen them before or because they remember when Dewar performed them. As the Institution still has the services of Mr. Green, who helped Sir James in much of his later work, it is possible to repeat them in good style, and I hope that this may be done shortly.