Speech by Dr K.S. Krishnan on the occasion of the presentation of the Bhatnagar Memorial Award

I feel deeply touched by the very kind and generous sentiments expressed by you, Sir, while presenting me the Bhatnagar Memorial Award, which I receive in all humility and with genuine pleasure. I am aware of the honour of being the first recipient of any scientific award; but this one, Sir, I appreciate the more for a sentimental reason, namely because of its association with my late distinguished friend and colleague Dr. Shanti Swarup Bhatnagar. I had the privilege of working with him for several years and under many different auspices. He had fairly wide interests in science. It was a standing comment in my laboratory -- the comment is generally regarded as having originated from me -- that whereas I came to my laboratory only once a day, Dr. Bhatnagar visited it more than once. He had an abiding faith in the potentialities of science. The large number of CSIR laboratories spread all over the country are standing witnesses of this abiding faith of his. He was, in addition, a dynamic personality which has been described variously, as an α-particle, which was at the time the most energetic particle known, or as a live wire. His resourcefulness was something uncanny. Every problem found a solution, though almost always an ad hoc one. With this combination of qualities, which he invoked freely, and his broad respect for the scientist, which extended even to those who strongly disagreed with him, he was in a large measure responsible for the rapid growth of public support for science in the country. Indeed, the support has grown today, to use a scientific phrase, to an altogether different order of magnitude, to which others, whom I need not mention here, have contributed largely.

Some of us who belong to an earlier generation of scientific workers were not only duly impressed by "the genius in the garret", but making a virtue of necessity, had almost reconciled ourselves to the consoling thought that the garret might after all be the appropriate place for drawing out the best in him. But fortunately for science, and for the future generation of scientists in particular, there was no place for the garret in Bhatnagar's scheme of thinking, not even for the genius. We owe to him primarily the numerous well-equipped scientific institutions of CSIR that have sprung up all over the country, and the momentum that he imparted to their growth has been gaining pace ever since.

The story goes back to the beginning of the last war, when the gross inadequacy of our scientific and technological organizations stood out in an embarassing way, and an immediate large scale build-up seemed imperative. The Council of Scientific & Industrial Research took shape during that period of great stress, and I had close contacts with Dr. Bhatnagar during these formative years of the Council. Indeed I was invited early in 1940, and on generous terms, to join him in organizing the scientific part of the programme. The scientific objectives were unexceptionable, and I had myself frequently expressed in my discussions with him the need for organizing various laboratories, and in particular, a Bureau of Standards for India. When the latter was decided upon, and a Planning Committee was appointed for this purpose, I did accept its membership. Following the British practice, the proposed Bureau of Standards was to be designated the National Physical Laboratory, though I would have preferred the former as a more appropriate name. I also accepted the membership of the Board of Scientific and Industrial Research. But the immediate background which prompted the organization of the CSIR did not enthuse me. Moreover with my pronounced academic leanings there was a strong resistance in me on academic grounds, to putting the cart before the horse; I felt that the development of the

Universities should have been assigned higher priority, and if the new laboratories were to be organized before the University laboratories had been greatly strengthened, it would be difficult to get adequate highly qualified scientific personnel to man these proposed laboratories, or we would be forced to eat out of the seed crop that would be needed in the Universities themselves. I could not also reconcile myself to the divorce of research from teaching, which was inevitable in the proposed new set up. The result was that I did not join Dr. Bhatnagar in 1940.

It took me many years to realize that Dr. Bhatnagar too was as keenly aware as any of us of the need for integrating these newer developments with the strengthening of the University laboratories. But the pace that he had set before himself would not have been possible, under the circumstances then prevailing, if he had adopted this academically more robust, but much the more ambitious, integrated programme. In any case, in 1947, I seemed to relax, and in the flush of newly gained independence, every problem seemed to recede to the background, in the intense faith that in an independent India every problem would get solved and every defeat would get rectified, in due course. So I joined Dr. Bhatnagar in 1947, instead of in 1940, as the first Director of the National Physical Laboratory. I was aware that the maintenance of the various physical standards is a highly specialized discipline, which involves a considerable amount of constructive work - which, as in many other spheres of human activity, is not as exciting, or as attractive, as creative work. I was also aware that much of the work would have to be in well-worn fields in Physics, far removed from some of the fertile, and for that reason fashionable, ones. But in the growing industrial economy of the country, the organization of the Bureau of Standards was one of national importance, and I had no doubts in my own mind about it.

In receiving, Sir, in all humility, the Bhatnagar Memorial Award, I feel strengthened in that faith that prompted me to join

Dr. Bhatnagar in the great and useful national task that he had set before himself and in which he felt that he was not receiving adequate support from the scientists in the country. By the award I also feel strengthened in the thought that during the intervening years, I had not proved wholly unacceptable to CSIR.

Allow me, Sir, to express my very sincere thanks to the authorities of CSIR, for this generous award, which I shall always cherish.

I now wish to give a brief outline of the research work done by me in collaboration with some of my colleagues at the N.P.L. on which the present award is based, as distinguished from the work of the N.P.L. as a whole. My work has been mainly on the physics of the solid state and I should like to refer in particular to three major lines of work.

One is on the thermionic properties of metals and semi-conductors done in collaboration with Dr S.C. Jain. As is well-known electrically heated elements play an important part in many electronic and other instruments as a convenient source of electrons that can be injected into the system in a controlled manner. A detailed and precise knowledge of the emission of electrons from the hot surface of the metal is naturally of some importance. Any small trace of gases adsorbed by the surface of the metal makes a marked difference to the thermionic emission and hence before any thermionic study is made, the surface has to be degassed by continued heating in high vacuum for a hundred hours or more, and even so the direct measurement of the emission from the surface does not permit one to obtain the precise values of the thermionic constants of the metal.

Dr Jain and the present writer developed a new technique which in the first place dispenses with the need for degassing the surface, which, as I mentioned, is a laborious process, and secondly enables not only the thermionic constants, but their temperature coefficients too to be determined with precision. The method was first applied to graphite, and is

based on the determination of the saturation vapour pressure of the electron gas inside a graphite chamber kept at different constant temperatures. In practice this is done by finding the rate of effusion of the electrons through a small aperture in the wall of the chamber. From the known saturation vapour pressure of the electrons at different temperatures it is easy to calculate the latent heat of evaporation of the electrons from the graphite walls, which is its thermionic work function. Since ultimately it is a measurement of equilibrium pressure, it is independent of the transmission coefficient of the surface for the electrons from the inside, and hence independent of any adsorption of gases at the surface. The method is analogous to that of obtaining black body radiation from the narrow mouth of a heated chamber, which naturally is independent of the emissivity of the wal s of the chamber.

Many incidental problems arising from contact potentials, space charges, etc., get either eliminated in this method, or elegantly solved.

By covering the inner walls of the graphite chamber completely with a thick coating of any metal, the constants of the metal can be determined in the same manner. The method has also been applied to semiconductors by keeping the semiconductor inside the chamber, and in electrical, but not direct mechanical, conta with the walls. At the low temperatures at which semiconductors can be operated conveniently the electronic vapour pressure of graphite is negligible in comparison with that of the semiconductor. It is analogous to measuring the ordinary vapour pressure over a liquid in a glass tube, the pressure of the vapour from the glass walls being negligible.

The thermionic constants for several metals and some semiconductors have been measured by us by this metho , and the results discussed in a series of four papers in the Proceedings of the Royal Society. Among them I would like to refer in particular to the results

for the monovalent and the transition metals, which present some interesting features.

The experimental methods developed by us in the course of these thermionic studies for the determination of the spectral and the total emissivities of metals have now become more or less standard methods for such determinations.

A question of some importance that immediately arises is this: what are the conditions under which a length of the tube equal to that of the chamber near the centre is of sensibly constant temperature? The answer requires a knowledge of the distribution of temperature along an electrically heated tube. The problem had not been worked out previously, nor even the simpler problem of the distribution of temperature along an electrically heated filament. This is rather surprising at first sight, since heated filaments have been in use extensively in electric lamps, and in many scientific instruments. The parameters involved are also few and simple. Taking a small element of the filament, the gain in energy due to the electrical heating is known, as also the loss due both to radiation from the filament, and to conduction through the material of the filament towards the colder ends. Hence the differential equation defining the distribution of temperature can be readily formulated. Even particular solutions can be obtained in a straight-forward manner. But these particular solutions are not in an explicit form in which they can be combined in order to obtain a general solution. Hence arises the difficulty in obtaining the general mathematical solution of the problem, though there are numerous empirical formulae that are in common use in the trade.

The problem was taken up by Dr Jain and me, and it was found that a practically complete solution can be obtained, owing to a certain special feature pertaining to the particular solutions, namely this: there is a small region near the centre of the filament in which conditions are favourable

for our being able to combine in a simple manner, the two particular solutions in order to obtain a general solution. As one moves out from the centre the contribution from one of the solutions increases rapidly, while the contribution from the other decreases rapidly, and before one has moved out of the small favourable region the latter contribution has become negligible in comparison with the former. Hence we have the very satisfactory position that throughout the region where the conditions for combination are not favourable, there is only one significant contribution, and the need for combination does not exist.

The complete solution and its many implications, and detailed experimental verification of these implications, form the subject matter of a series of six papers, again in the Proceedings of the Royal Society. It was mentioned a little earlier that there are numerous empirical formulae that are in general use in the trade. These formulae come out elegantly from the general solution as particular cases.

The temperature distribution is parabolic near the centre of the filament, logarithmic a little further out, and approximates to that of an infinitely long filament near the ends. Both the logarithmic and the parabolic regions can be used conveniently for determining the thermal conductivity of metals, particularly at high temperatures. A new experimental method has been developed based on this, and is described in detail in a paper by Dr Jain and me in the British Journal of Applied Physics, which gives also some conductivity measurements at high temperatures made by this method.

Arising out of these investigations on the filaments, is a result of some mathematical interest to which I may refer here. Testing of electrical fuses is one of the common problems in a standards laboratory. When the current through it is switched off the decay of temperature can be represented in practice, by an exponential decay curve involving a single

relaxation time. On the other hand, theoretically each of the terms in the Fourier expansion is assigned its appropriate relaxation time. This discrepancy gets resolved mathematically in an unexpected manner in the present investigations. It turns out that by choosing a certain suitable length of the filament as the appropriate base for the Fourier expansion all the terms except one disappear to a close approximation, and hence one is left with a single term with a single relaxation time. At the beginning of the decay the approximation is very close indeed, and even after a very long time the approximation is as close as π^2 to 8 and π^3 to 32.

The problem of temperature distribution along a heated tube has also been solved by Mr R. Sundaram and the present writer. The new feature here is the radiative transfer of energy in the hollow core of the tube. This transfer can be calculated in a straight-forward manner and corresponds to a certain effective thermal conductivity which is proportional to the cube of the temperature, as should be expected. As is well known the thermal conductivity of a gas at ordinary temperatures may be ascribed to the diffusion of the molecules, and the conductivity will naturally be the larger the longer the mean free path of the molecule between two collisions. On the other hand if the gas is at a high temperature it is not so much the diffusion of the molecules themselves that account for the conductivity, but the diffusion of the radiation quanta emitted by the molecules. The mean free path is now the distance to which the quanta can travel without sensible absorption and it can be quite large. Now the radiative conductivity in the core of the heated tube may be regarded as the limit case of that of a hot gas, when the pressure is reduced to zero, and the free path is determined by the dimensions of the tube rather than by the gas. Comparing this limiting conductivity with that calculated for radiative transfer in the core, one finds the free path comes tonbe

just the diameter of the tube.

The expression bears a close analogy to that of the lattice conductivity of a dielectric cylinder at very low temperatures in which the diffusion of the phonons is responsible for conduction, and their free path again is determined by the diameter of the cylinder.

There is a further analogy with the passage of a highly rarefied gas through a narrow tube, in which again the free path is determined by the dimensions of the tube, and the well known slip coefficient that occurs in the latter, due to specular reflection of the molecules from the walls of the tube, find a parallel in radiative transfer too.

As an interesting side result may be mentioned the expression obtained for the radiation from the mouth of a uniformly heated long cylinder, with specularly reflecting walls, in which the fraction of the radiation incident on any element of the wall surface that is not absorbed, is regarded as specularly reflected. It is found that when the tube is sufficiently long, and closed at the other end, the flux of radiation from the mouth approximates very closely to that of an ideally black surface covering the mouth. This is so along every direction over the whole of the hemispherical solid angle. This is a result of some importance in the construction of radiation standards suitable for calibrating an extended thermopile, for example.

I would like to refer next to a theoretical investigation with Dr A.B. Bhatia, followed later by experimental work with Mr A.C. Joshi, on the electrical conductivities of metals and alloys.

Taking first a pure metal, if the atoms in it were perfectly regularly arranged, the conduction electrons can move freely in the crystal lattice, and the metal will have no electrical resistance at all. The normal thermal agitations of the atoms, however, disturb this regular arrangement, and hence there will be some scattering of the electrons by

the thermally agitated atoms, and hence a finite resistivity.

Adopting the well-known mathematical technique which one uses for the calculation of the diffuse scattering of X-rays in a crystal, the scattering of the Fermi electrons in the metal can be calculated in detail, along different directions. It is found that the scattering can almost wholly be attributed to the local thermal fluctuations in density of the type studied by Smoluchwoski and by Einstein many years ago, and can therefore be readily calculated. In the case of an alloy, say a binary one, the number of atoms of either type present in an element of volume will fluctuate. This fluctuation can be separated into (A) a fluctuation in the total number of atoms in the elements, the relative propositions of two types, i.e. the concentration, remaining constant, and (B) a fluctuation in the concentration, the total number in the element of volume remaining constant. The two can be referred to as the density and the concentration fluctuations respectively.

In a binary alloy the contribution from the fluctuation in concentration to the resistivity of the alloy is found to be much larger than that from the fluctuation in density, thus accounting for the well known large increase in resistance due to alloying. This simple method of approach is found to be very effective in the discussion of the electrical properties of metals and alloys, particularly of those which exhibit order disorder phenomena, and also the changes that take place on melting.

The change in the cross section of the atoms for scattering with the change in the wave-length of the Fermi electrons, can also be studied conveniently on this basis.

I wish to refer here to a result of some interest in pure mathematics, which emerged incidentally from these investigations, as a typical example of the interdependence of the different branches of science.

I referred just now to the classic investigations of Einstein on light scattering, whose technique was adopted by Dr Bhatia and me for the corresponding problem in electron scattering. In calculating the scattering coefficient, he obtained an infinite series of the type $\sum_{n=-\infty}^{+\infty} \frac{\sin^2 n\alpha}{(n\alpha)^2}$ where n is an integar. Under the usual conditions of experimenting α is small, and he therefore equated the series to the corresponding integral, and thus obtained the value of the coefficient. But from purely physical considerations we had reason to believe that the scattering coefficient should be applicable under other conditions too, where one finds that α is not small. If this were so, the obvious conclusion that one is forced to draw is that the series is equivalent to the integral even when α is not small.

Its significance is this: If we plot $\sin^2 x/x^2$ against x the area subtended by the curve with the x-axis can be obtained accurately just by simple quadrature, i.e. by erecting ordinates at equal intervals, and adding them up and multiplying by the interval.

Dr Bhatia and I found this was so, for values of α as large as π and we also supplied a trignometric proof. Soon after we had the proof I had occasion to reproduce this at a lecture, and made some remarks incidentally about the proofs not being elegant though it was rigorous, adding that it should be possible to find a more elegant one by applying the method of Fourier transforms. Professor Norbert Wiener, who was in the audience supplied the more elegant proof before I concluded my lecture. On the basis of Wiener's proof we have been able to construct a large class of functions which have the some property. We could also construct several interesting series among which are some which appear prominently in one of Ramanujan's papers.

Finally, I should like to refer to some extensive investigations by Dr Sanat Kumar Roy and me on the frequency and the anharmonicity of

some of the normal modes of vibration of ionic crystals. The results appear in a series of papers in the Philosophical Magazine and in the Proceedings of the Royal Society. I shall first consider the principal mode which corresponds to the oscillation of the lattice of positive ions with respect to the lattice of negative ions. Separating the two lattices by a short distance γ the potential energy can be expressed as a power series in γ, in which the coefficient of the square term gives the frequency and that of the higher term gives the anharmonicity. The anharmonicity comes to be of the right sign, and of nearly the right magnitude required by some specific heat observations at high temperature. The frequency so calculated, is not the frequency of the principle mode of the crystal, but that of the constituent oscillators, whose mutual influence has to be taken into account before obtaining the frequency of the normal mode of the crystal. This influence comes out to be just the effect of the well-known Lorentz polarization field; and the magnitude of the Lorentz factor is almost exactly verified. This is the only case known in which all the conditions postulated by Lorentz hold, and it is significant that this is the only case in which the factor is exactly verified.

One gets from this an insight on the effect of the polarization field on the refractivities of dense media. The effect can be taken into account in two alternative ways, which are mathematically equivalent.

One may regard it as affecting either the strength of the oscillator, without affecting its frequency, or the frequency of the oscillator without affecting its strength. The lead to the well known dispersion formulae of Lorentz and Drude respectively, which express the refractivity as a function of the frequency. It resolves a long standing controversy regarding the merits of the two formulae, and the solution happens to be a Pickwickian one, namely, that the two are mathematically equivalent the frequencies that appear in the Lorentz formula being those of the constituent oscillators, while those appearing in Drude are those of the medium. Many significant results follow therefrom.

I have tried to give a brief outline of the main lines of investigation in which we have been engaged during recent years. In conclusion I wish to express my sincere thanks to my colleagues, for their valuable collaboration.

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