SAHA – PLASKETT CORRESPONDENCE

Editor's Note: *Readers are requested to read the editorial to understand the background of this exchange of letters between the two scientists. Saha's letters are undated, presumably the copies that we have received are the drafts. However, from the reply of Prof. Plaskett, it is clear that Saha's first letter was written on December 21, 1946 and the second one sometime in January 1947.*

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From

Prof. M. N. Saha, D.Sc., F.R.S.

My dear

I have been thinking of writing to you since I returned to India, but somehow I have not been able to do so.

The occasion for writing this letter was a feeling which I experienced during my two tours in Britain that there has been persistent effort on the part of British scientists to minimise (or rather should I say with a faint praise) my early contributions to Astrophysics. This is borne out by your remarks in the Observatory, April 1946, where you say :

"Saha, working in Fowler's laboratory after the end of the last War, then demonstrated that the successive appearances of these different spectra could be interpreted as bring due to the temperature and pressure prevailing in the stellar atmosphere. His work had in turn been put on a firm theoretical basis by R. H. Fowler and Milne, working at Cambridge and using the magnificent material accumulated by Lockyer, and so successfully interpreted by Baxandall."

I regret to tell you that this remark is entirely gratuitous and misleading. It gives one the impression that I derived all the fundamental ideas in astrophysics which goes under my name viz., The Theory of Thermal Ionisation and Its Applications, from Prof. A. Fowler, while I was working as a scholar under him in 1920. This is entirely misleading, for I worked at Fowler's laboratory as a guest, and never registered my name as a scholar and the work had been mainly done in India before I went to Fowler's latoratory. For the information of yourself and other scientists who are of this opinion, I may trouble you a little biographical sketch.

"I took up the study of astrophysics in 1916, a year after I took my M.Sc. degree of the Calcutta University in Applied Mathematics, and was awarded a research scholarship of the Calcutta University for work in mathematical physics. The situation was rather piquant, as there was no professor to guide us either in physics or in mathematics, so we began to look for work on our own initiative. We had read Ball's Spherical Astronomy, for our M.Sc. course and had some knowledge of physics which was my subsidiary subject in B.Sc. Honours, but beyond that we had no knowledge of any problem of physics, astronomy or astrophysics. The Calcutta University had, sometime earlier, received a large Endowment which enabled it to found the University College of Science, but could not secure the services of any competent man. C. V. Raman was then in the financial service of the Government of India and posted at Calcutta, and was carrying on researches in Acoustics, particularly Wolfe Note. He was approached to become the first Palit Professor, but he took sometime to decide and joined formally in June 1918. I mention this point because I found that it is believed outside that during some early part of my career, I had been Raman's pupil or scholar. This is completely incorrect; I never owed anything to him in life, except persistent ill-will, and attempt to harm me whenever possible. He had been working since 1907, when he took his M.Sc., on acoustics particularly Wolfe Notes and when he joined the Finance and Audit service and was posted at Calcutta, he continued these works at the laboratory of the Indian Association for Cultivation of Science at 210, Bowbazar Street. He was being talked of as the future Palit Professor and he had given me hints that it would be for my material welfare if I consented to be drawn within his orb, as many of my colleagues who had started working under him. But I had declined as I found that his knowledge of the subjects in which I was working was not such that he could render me any effective assistance.

I began from 1916 to read rather desultorily any book on Physics, Mathematics and Astronomy and Astrophysics, which came in my way or could be found in the library of my old college (Presidency College, Calcutta, or in the library of the Calcutta University. I might add that I had begun to learn German privately as early as 1911, while I was a student in the Inter science class, and by 1916 I had acquired enough proficiency to study scientific papers without the use of a dictionary. In course of these studies I came across Miss Agnes Clerke's two books on Astrophysics – one on the Sun, the other on Stars – and these excited my interest in Astrophysics, and made me familiar with some of its problems. A year later in 1917, the Calcutta University opened M.Sc. classes in Mathematics and Physics and I was asked to teach an odd assortment of subjects: Thermodynamics, Spectroscopy, Figure of the Earth, and was given charge of the Heat Laboratory. I was asked to teach thermodynamics because no one else of my colleagues would agree to take up that unpleasant subject, as they styled it. I had read no book on this subject previously.

While studying these subjects for my teaching work I became acquinted with several treatises on Thermodynamics, Planck, Sackur and above all Nernst's Das Neue-warmesatz. I also got acquainted with Bohr's and Sommerfold's theory of the origin of Spectra, and Einstein's Theory of Relativity which I read in the original. I had written a number of original papers on various subjects, list of which you will find in the pamphlet enclosed. On the basis of these papers, I was awarded the D.Sc. degree in 1919.

Astrophysics

By the end of 1917 I had written a long essay on 'Selective Radiation Pressure', elaborating a theory of the role of radiation presssure acting on the atoms selectively and compensating the action of gravity on solar atoms. This paper was sent to the Astrophysical Journal for publication, but the editors replied that as the paper was rather long, it could be published only if I were willing to bear a part of the printing costs which ran to very three figures in dollars. Much as I would have to do so, it was not possible for me to find out so much money as my salary was small (about £150 per annum) and I had to maintain my old parents and a younger brother who was studying within this salary. So I wrote to the Editors of the Astrophysical Journal, expressing my inability to pay the costs of printing, but never heard anything more about the publication of this paper nor was it returned to me. Years afterwards in 1936, when I visited to Yerkes' Observatory, Dr. Morgan showed me the manuscript which was still being kept there. I got a short note published in the Astrophysical Journal, Vol. 50, 220 (1919) and submitted a duplicate of the original article for publication in our own University Journal (which had no circulation worth mentioning, on Selective Radiation Pressure and Problems of Solar Atmosphere, Journal of the Department of Science, Calcutta University, 1919) sometime afterwards. I am mentioning these facts because I might claim to be the originator of the theory of Selective Radiation Pressure, though on account of the above discouraging circumstances, I did not pursue the idea and develop it. E. A. Milne apparently read a note of mine in Nature, 107, 488 (1921) because in his first paper on the subject in *Month. Not. R. Ast. Soc.,* Vol. 84, Astrophysical determination of average of an excited Calcium atom, he mentioned my contribution in a footnote, though nobody appears to have noticed it. His exact words are: "These paragraphs develop ideas originally put forward by Saha".

It was while pondering over the problems of astrophysics, and teaching thermodynamics and spectroscopy to the M.Sc. classes that the theory of thermal ionisation took a definite shape in my mind in 1919. I was a regular reader of German journals, which had just started coming after four years of first World War, and in course of these studies, I came across a paper by Eggert in the *Physikalische Zeitschrift*, 1919, Uber den dissociation zustand der Fixsterngase, p.573) in which he applied Nernst's Heat Theorem to explain the high ionisation in stars due to high temperature postulated by Eddington in course of his studies on Stellar structures.

Eggert, who was a pupil of Nernst and was at the time his assistant, had given a formula for thermal ionisation, but it is rather strange that he missed the significance of ionisation potential of atoms, importance of which was apparent from the theoretical work of Bohr, and practical work of Franck and Hertz which was attracting a good deal of attention in those days. Eggert grasped the importance of "Chemical constant" for electron gas; it was natural enough for the idea of chemical constant was due to Nernst himself though it was given an accurate value by Sackur and Tetrode on the quantum basis. Eggert used Sackur's formula of the chemical constant for calculating that of the electron, but in trying to account for multiple ionisation of the iron-atom in the interior of the stars on these basis, he used very artificial values of ionisation potential. Later, when I met him he told me that he had not realised the improtance of Bohr's theory, or of Franck and Hertz's experiments, and had absolutely no idea that the ionisation potential can be calculated from spectroscopic data, as I had done. Further, he appears to have had no idea of problems of solar chromosphere or physical characteristics of Stellar spectra, as was apparent to one like me who had read Miss Agnes Clerke's Book. His knowledge of the problem of the Stellar interiors was derived from a talk given by Kohlochutter in the Berlin Physicale Colloquium in Eddington's Theories.

While reading Eggert's paper I saw at once the importance of introducing the value of "Ionisation Potential" in the formula of Eggert, for calculating accurately the ionisation, single or multiple, of any particular element under any combination of temperature and pressure.

I thus arrived at the formula which now goes by my name. Owing to my previous acquaintance with Chromospheric and Stellar problems, I could at once see its application. I prepared in course of six months of 1919 (February to September) four papers and communicated them for publication in the Philosophical Magazine from India within August to September.

1) On Ionisation in the Solar Chromosphere (later published in Phil. Mag., 40, 472, 1920).
2) Elements in the Sun – Phil. Mag., 809, 1920.
3) On the Harvard classification of Stellar Spectra (later changed at Prof. Fowler's suggestion "On a Physical Theory of Stellar Spectra"), P.R.S. (A), Vol. 99.
4) On temperature radiation of gases, Phil, Mag., 41, 267, 1921.
5) On Electron Chemistry and its application to problems of Radiation and Astrophysics, Journ. Ind. Ast. Society (now extinct), 1920.

In the meantime I had been awarded a Foreign scholarship to enable me to proceed to Europe. I had already received my D.Sc. degree of the Calcutta University on certain other thesis on Relativity and on an experimental work on the Pressure of Radiation, so I thought that I would rather try to find out a place where I could obtain experimental verification of the thermal ionisation theory and I should not, like some of my other Indian friends, spend my time in taking a D.Sc. degree of some British University. This was a rather dangerous decision to take, for British or European degrees had a much higher market value than Indian degrees, and I ran the risk of being passed over in the all-important matter of getting a suitable University post by some pass B.Sc. from Oxford or Cambridge. But nevertheless, I decided to take the risk. I consulted some Indian scientific friends who were acquainted with conditions in England; they said that for the verification of spectroscopic and astrophysical data utilized in my paper, the best place was the laboratory of Prof. Albert Fowler in the Imperial College of Science and Technology. For the verification of the work on the thermal side, they could recommend no place in England, because nobody there at that time was carrying on high temperature work. I had no personal acquaintance with Prof. A. Fowler except that I had read his paper on the spectrum of ionised helium.

On my arrival in England, I saw Prof. Albert Fowler, who at first thought that I had come to work for the D.Sc. degree of the London University like other Indian students working under him, but when I explained to him that I wanted to work there only for a short period to obtain verification of my theory, he did not show himself very enthusiastic, but allowed me to read and work in his laboratory. Probably he had not much time to listen to me at the first meeting. This was in November of 1920. If you look at the records of the Imperial College, you will find that I never got my name registered for any degree work. In the meantime, my first paper 'Ionisation in the Solar Chromosphere' communicated from India had appeared in the Phil. Mag., thanks to a personal call which I made on Mr. Francis, the publisher of the Journal. After its publication, Prof. Fowler began to take more lively interest in my work and in my views. I showed him the paper 'On a Harvard Classification of Stellar Spectra'. He read it very carefully, and argued with me that the Harvard astrophysicists including Prof. Pickering and Miss Cannon were not the only persons who had made contributions to these subjects, but the pioneering credit for such researches must be given to Lockyer. He gave me all the papers of Lockyer and his pupils and of himself to read, and from their perusal, I was convinced of the correctness of Fowler's views, and at his suggestion the title of the paper was changed to 'On a Physical Theory of Stellar Spectra'. The original paper (3) was withdrawn from the office of the Philosophical Magazine, and Fowler was kind enough to communicate it to the Royal Society where it was published. Papers (2) and (4) were published in the Phil. Mag.

I took about four months in rewriting this paper, and all the time I had the advantage of Prof. Fowler's criticism, and access to his unrivalled stock of knowledge of spectroscopy and astrophysics. Though the main ideas and working of the paper remained unchanged, the substance matter was greatly improved on account of Fowler's kindness in placing at my disposal fresh data, and offering criticism wherever I went a little astray, out of mere enthusiasm.

For example, he would repeatedly say that hydrogen in stellar spectra and helium in the solar chromosphere do not at all obey the ionisation theory. These facts later on gave rise to the idea of hydrogen excess in stars, but I do not think that for the helium anomaly any satisfactory hypothesis has yet been put forward. I have suggested one in my paper "On a Physical Theory of the Solar Corona". The paper "On a Physical Theory of Stellar Spectra" was read at a meeting of the Royal Society, and there Prof. Fowler spoke very enthusiastically about the work. He said that it was the greatest contribution in Astrophysics since Kirchoff's discovery of spectrum analysis in 1859, and predicted a great number of works being stimulated by it.

You will see that the suggestion made by you in your remark in the Observatory that the inspiration of two work was due to my presence in Fowler's laboratory is somewhat gratuitous. I had greatly profited by my four months' stay in Fowler's laboratory (1920, November to 1921 February) and Prof. Fowler never grudged my intrusion of his time and knowledge. I used to go to his room at any time. 'Come on' he would say softly in response to my tap of the door as if he knew who it was. He would stop his own work, listen to my enquiries and then take a file out and say "you will find the information you want there". These informations and Fowler's criticism improved the quality of my third paper, but did not materially change it, the main ideas remained very much as I had worked out in India, as you will find from the papers published in the Phil. Mag. Fowler treated me as a colleague and a guest, never as a student. I have very great respect for him as a scholar, as a man, as an astrophysician, and for the unselfish and generous way he treated me, which I now find is rather unique, and I am quite sure that were he living now, he would have been the first man to resent your suggestion.

Though this letter is becoming rather long, I might state why I did not continue my stay at the Imperial College and continue tho work on the line I started. This question has been asked by many persons. Fowler actually wanted me to stay at the Imperial College as he thought that spectroscopic part of the work regarding stellar spectra required to be worked out in greater detail, and there was the question of peculiar stellar spectra which he repeatedly mentioned. You must remember that at that time (1921) only the knowledge of the spectra of only the first and second group were available and we had no knowledge of the ionisation potential of elements of other groups. The spectroscopic analysis of the elements like Si, C, and many others important for astrophysical work were yet to come, and Catalan's work which gave the clue to these works had just started. In fact, he arrived at the Imperial College while I was there and started his work on "Multiplets" of Mn. He said that he had performed some 150,000 subtractions to get the multiplets of Mn. In spite of the success of the Bohr-Sommerfeld theory in explaining simple spectra, Fowler rightly guessed that the full theory of origin of spectra of atoms was yet to come. He was puzzled particularly by the calcium-multiplets discovered by a Russian called Popoff working in Paschen's Laboratory, and often asked me "how could these multiplets which are undoubtedly due to two-valence elements be explained by the Bohr-Sommerfeld theory? As you know, study of these clues led to the Russell-Saunders theory of LS-coupling and ultimately to Pauli's principle and Hund's final work on spectra of atoms. Fowler even offered to try to get for me a Royal Society readership worth five hundred pounds per annum (I think it was the Messel Readership), but I was very anxious to obtain experimental proof that atoms can be ionised by heat, so I did not agree to Fowler's suggestion. This point is not now considered very important, but at that time, the prevailing idea was that no gaseous atoms could be ionised at temperatures available at the laboratories, as you will find from remarks on p. † of O.W. Richardson's 'Emission of Electricity for incandescent Solids'. But I obtained from my calculations given in paper that at least Cs, Rb and K should show very considerable ionisation even at as low temperatures as 2000°K. As my heart was intent on this work, I decided to leave the Imperial College, and Fowler understood my viewpoint completely. We parted as very good friends, and he was later kind enough to propose my name for the Royal Society. I think I owe my election in 1927 chiefly to him, Eddington and Chapman. I was elected after R. H. Fowler (1925) and E. A. Milne (1926), which gave rise to some comments in India.

† page number was missing in the original

At Fowler's advice, I went to see Sir J. J. Thompson at the Cavendish Laboratory to find out whether it would be possible for me to carry out tests on the experimental verfication of the theory of thermal ionisation by observations on heated Cs and Rb-vapour. Sir J. J. had just then retired from the directorship of the Cavendish Laboratory, but used to come at very odd hours, just before lunch time, to his place in the laboratory. On the advice of an Indian friend, I went to see him without previous engagement and was fortunate to obtain his permission to talk for a few minutes. I gave him an abstract of the work published in India (No. 5). He read it twice over without asking me a single question, and then thought for about 10 minutes with closed eyes, and then began to bombard me with question after question which went on for one full hour. He said that he had calculated the ionisation of Sodium in the flame; it could not be more than 10^{-60}, but after all these calculations were of a provisional natures, and there was no harm in trying the experiment on the line I proposed. We discussed the possibility of carrying out the experiment at the Cavendish, but ultimately it was found that there was not sufficient apparatus and materials in the Cavendish for carrying on the work. It was during this visit that I first met Milne at the house of Sir A. Eddington, to whom I went to pay my respects. Milne said that he was reading my papers, but I had no idea from conversations with him that he had planned joint works with R. H. Fowler, which came sometime later.

I informed Fowler of my talks with Sir J. J. Thompson and of my intention to proceed to Nernst's laboratory at Berlin. Fowler agreed rather ruefully. I had written to Eggert who was Nernst's assistant, and asked him to get Nernst's permission for me to work at his laboratory. I received an encouraging reply both from Eggerts and Nernst himself and went to Berlin in February, 1921.

On arrival at Berlin, Nernst received me very warmly, and gave me facilities at his laboratory for verifying the theory experimentally. He was not much impressed with the astrophysical side, importance of which was not realized either by him or his assistants. It appeared to me later that he was interested in my work because it seemed to him to afford an additional verification of the Nernst Heat Theorem, about which he was carrying on fierce controversies with Arrhenius and others. In fact, when Arrhenius visited the laboratory, sometime in August of 1921, he introduced me as one from India who had been trying to verify the Nernst Heat Theorem from the high temperature side, just as Simon (now at Oxford) was trying to verify it from the low temperature side. I carried out certain experiments at Nernst's laboratory on the electrical conductivity of heated caesium vapour which, to my mind, completely established the truth of the ionisation theory, but Nernst found several objections, and I never heard any further of the manuscript which I left with him in October 1921. I returned to India in November 1921, and tried to contact Nernst, but I could elicit no reply from him regarding the fate of the paper. So I got an account of the work published in the Journal of the Department of Science of the Calcutta University, 1922. After a little while, I found that Nernst had suggested the solution of the problem on indirect lines – from observations of the ionisation of caesium vapour in contact with heated tungsten filaments. The experiment was carried out at Nernst's laboratory by E. Meyer (Ann. d. Physik) and almost simultaneously by Langmuir (Proc. Roy. Soc., Vol. 107). My mistake was to publish the work I did at Nernst's laboratory at Calcutta. I ought to have published it in the Phil. Mag.; then I would have got due credit.

After return to India, I was appointed to a small Professorship (small because the salary was low and no laboratory was provided) created for me, but I had to encounter at Calcutta the persistent ill will and hostility of C. V. Raman, who had been appointed Palit Professor of Physics in 1918. He had been to Europe in 1921, had heard very good reports about my work which he had done his best to pooh-pooh in India. He very much disliked that I was coming into prominence on account of my work. This led to several very unpleasant incidents, which I need not mention, and I was glad to leave Calcutta, when in October 1923, I got offer of the University Chair in Physics at the Allahabad University.

6 January 1947.

UNIVERSITY OBSERVATORY,
OXFORD.

My dear Saha,

Thank you for your most interesting letter of 21 December. I have read it with the greatest enjoyment and profit, and as you suggest I have given it to Milne for reading, transmission to Chapman and ultimate return to me. What was quite new to me was ~~first~~ the fact that the early part of your work had been done in India, not Germany, before you came to Fowler's laboratory. The knowledge that you had done so much without help and backing in India only serves to increase the admiration I have always felt for your great contribution to astrophysics. I only regret that I did not know of this at the time of my presidential address, and can only ascribe my ignorance to a probably incorrectly remembered statement of Russell on his return from Engalnd in the early 1920's.

I am not conscious myself that there is a tendency in this country, or indeed anywhere else, to minimize the importance of your fundamental work. With the concept of thermal ionization your name will always be associated, and though inevitably the subject of astrophysics has moved on very far since your pioneer investigations your place in the history of the subject is secure for all time. So much so indeed, that it seemed to me worth while to try and correct a tendency (prevalent perhaps in some quarters in the United States) to regard astrophysics as stemming from the work of Pickering and yourself, forgetting entirely the indispensable contributions of Fowler and Lockyer.

If there is any feeling of doubt about your work, it is probably to be found amongst the physicists in Oxford who feel that not sufficient credit is ever given to Lindemann's pioneer work in this same field. I do not share this doubt. It is true that Lindemann was the first to solar apply thermal ionization to the problem of the ~~stellar~~ atmosphere, but it was you who showed how fruitful this concept was in describing the stellar sequence in terms of the parameters of temperature and pressure.

Thanking you again for writing so fully and frankly to me, and looking forward to the receipt of the second instalment,

Ever yours,

H. H. Plaskett

H.H.Plaskett.

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Saha's reply

My dear Plaskett,

Thanks for your letter of January 6. I am glad that you did not find the letter boring. I could not finish the second part of my letter earlier, as the Indian Science Congress intervined and we were busy with our foreign guests, and other works.

As regards Lindemann's part in the business, I was myself unaware of any work by Lindemann on this line till I met him for the first time in the halls of the Royal Astronomical Society sometime in 1921. I remember this occasion vividly because Prof. S. Chapman was reading on this occasion a paper on magnetic storms. This paper was subjected to very violent criticism by many people present. Chree's criticism in particular was expressed in language which appeared to me rather harsh, and ungraceful. He said, if I remember correctly that Russians had prohibited the use of Vodka, I wish somebody could prevent the presentation of such theories as presented by Chapman before learned assemblies. Lindemann wound up the debate by saying that theories are only attempts at understanding physical phenomena, and but for such attempts, all observational and experimental material would be a chaotic jumble of facts which would only confuse the brain.

I was presented by somebody, I do not remember now who it was, to Lindemann, who had read my paper on the Ionisation in the solar chromosphere, published in the February number of the Phil. Mag. He told me that he had published something of a similar type somewhat earlier in the Phil. Mag. I looked up the references given the next day, and found that he had deduced the thermal ionisation formula for hydrogen, taking the correct value of the I. P. for hydrogen and had tried to find out whether hydrogen was completely ionised in the solar chromosphere. This was in connection with his hypothesis that a cloud consisting entirely of electrons would be completely dispersed by forces of mutual repulsion, and therefore such a stream, coming from the sun, could not account for the magnetic storms, unless an equal number of protons was present. But there was no indication in this or any subsequent paper, that he was aware at that time, of the importance of using actual values of ionisation potential for other elements in the formula, or whether he was conscious whether accurate values of I. P. could be obtained from spectroscopy, or from the experiments of Franck and Hertz, which were exciting a good deal of interest at that time. There was also no indication that he was conscious that the general problems of solar or stellar atmospheres could be attacked on those lines. In a later paper, I mentioned Lindemann's contribution, but I do not see any justification for the Oxford group's grievance, because in course of my conversations with Lindemann or from his previous writings, I had not the faintest idea that he had intended to deduce a general formula for thermal ionisation for all elements and intended to use them for the elucidation of problems of solar or stellar atmospheres.

Now to return to my own story.

I joined the Allahabad university in October, 1923. Here I found myself in an entirely different atmosphere. In Calcutta, we had a Research Atmosphere, in fact University professrs like myself were not burdened with any teaching responsibility and could devote all their time to research work if they so desired. At Allahabad, there was practically no "Research Atmosphere", though the new regulations, under which we were appointed, aimed at creating Research Schools. But such transitions are not easy to work out.

Before we joined, the activities of the teachers of the Allahabad University were confined to training students for the B.Sc. and the M.Sc. degree. There were about 90 students for the B.Sc. degree, and 10 for M.Sc. The staff provided was one Professor, one Reader, one full time lecturer, and 2 demonstrators, one of whom performed the functions of clerk and storekeeper. My predecessor in the office of Professor was one Mr. Durach, who had come from New Zealand to the Cavendish Laboratory at the same time as Lord Rutherford, and after a short stay at Cavendish, was sent to Allahabad as Professor. I had never met him, but from accounts I heard of him, I concluded that he found himself probably ill at ease during all the 15 years of his residence at Allahabad, and had made no attempt to organise teaching on a higher level, no to speak of setting up any research school. But he used to buy apparatus quite liberally, and had told the people that the Allahabad University had the best stock of physical apparatus in the whole of India. This was probably true, but the apparatus were all stocked in the Almirahs, as many professors still do, and as Lindemann told me in 1921 his own predecessor at Oxford used to do. These facts had to be remembered, because when I approached the University authorities for a grant for purchase of apparatus which would enable me to set up experiments on Thermal Ionization I got the inevitable answer: – Your predecessor Mr. Durach had assured us that the Allahabad University has the best stock of physical apparatus, why do you require more ?" This state of affairs lasted for five years, and I seriously contemplated returning to Calcutta after two years of probation. For the Executive Councillors, who were mostly lawyers, it was difficult to understand that the old apparatus for ordinary class teaching were of no use to me, but for research work new types of apparatus were needed.

Apart from these circumstances, there was practical no workshop, only skeleton of a library, and the staff was barely sufficient for the number of students we had to handle. There were one or two old tradle lathes which used to be run by the feet, and nothing else. Our routine class duties were from 10-30 A.M. and 3-30 P.M. after which most of the teachers went home. But when myself and my colleague in the Chemistry Department Dr. N. R. Dhar and a few of our colleagues and research students began to stay up to 6 P.M., for our own research work, we were regarded in some quarters as fools, in other quarters as over enthusiasts. There was practically nobody with whom any intelligent discussion could be held. In course of time we were fortunate in securing a few colleagues and research scholars whom we could infect with our own enthusiasm for work. I should particularly mention Dr. N. K. Sur, lecturer in a local college, for whom I could, after great efforts got a full time appointment in our University, who published with me some papers on Spectroscopy and Thermal Ionisation but in 1927 joined the Meteorological Department of Government of India and Dr. P. K. Kichlu who worked on Spectroscopy and Active Nitrogen, and later became Professor at Lahore, where he founded a flourishing school in spectroscopy. But the tragedy was that I had myself to work hard, sometimes along unfamiliar lines, for enabling all these workers to do work of sufficient merit for the D.Sc. degree, and when they were sufficiently trained, I could not help them in my own laboratory for helping me in my own work, for they got jobs elsewhere on far better salaries and left my company.

Probably all these details would not interest you or others, but you cannot possibly understand the want of activity on my part on astrophysical lines unless you have the proper background. I had followed the subsequent developments in the line initiated by me, but saddled as I was with an overdose of teaching work, and having no proper library for a long time, and no intellectual comradeship with anybody it was difficult for me to find time for any active work. I could do nothing for the first four or five years, except watching, and doing some theoretical work on spectroscopy, for that was the only line on which we could start work as the laboratory had some equipment and particularly Sur and Kichlu have made notable contributions which enabled them to get the D.Sc. degree of Allahabad. I had myself come very near Hund's theory, which completed the theories on the origin of spectra, but my publication came five months after Hund's work was published and naturally I got no credit for it. I laid very much stress on a proper understanding of the origin of spectra of atoms, because I was convinced that without all this knowledge, it would be impossible to unravel the mysteries of the atmospheres of stellar bodies.

Things began to improve when due to efforts by Prof. A. Fowler and other unknown friends (possibly Eddington, Chapman, and R. H. Fowler) I was elected to the Royal Soeiety in 1927. This was then regarded in India as a rare distinction and I was congratulated by the Governor of the Province, Sir Wm Marris, and many other distinguished men. Taking advantage of the favourable change in the atmosphere I approached the Governor for an annual research grant to enable me in my research work and through his efforts, annual a sum of Rs. 5000 (£ 333) was granted to me. This and a grant of £250 (probably in 1928 or 1929) from the Royal Society enabled me to put the library, laboratory and workshop in a tolerable position, and enable me to purchase apparatus for experimental verification of the formula of thermal ionisation. But it took a number of years before I could get all that I needed. I had also succeeded in increasing the staff, many of whom were my own pupils, so that by 1932, I had more time to devote to research work. But in 1934, there started another scuffle with C. V. Raman, which spoiled some of my time, but that is quite another affair. ❐