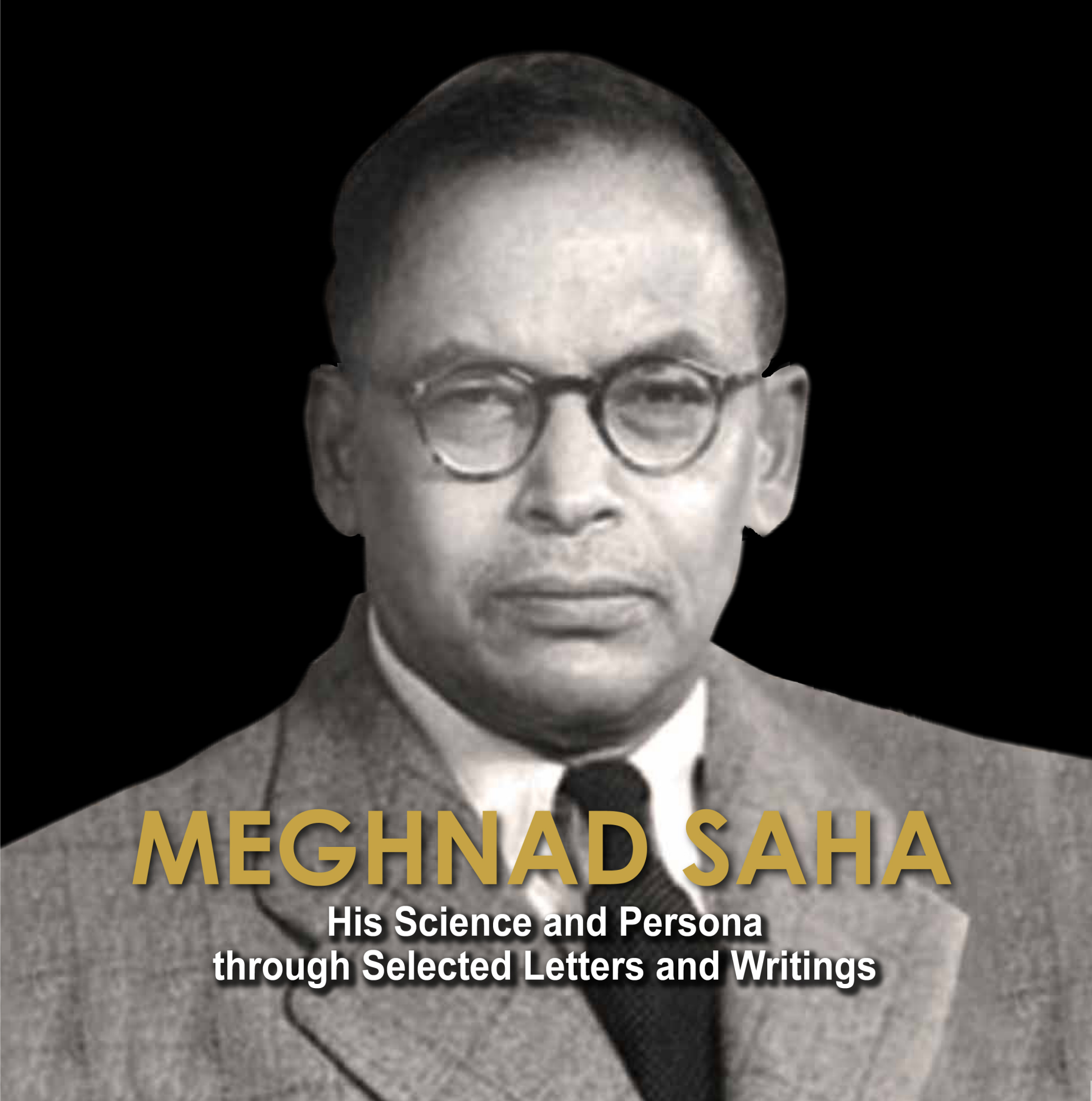

MEGHNAD SAHA

His Science and Persona through Selected Letters and Writings

ABOUT THE BOOK

This book is a selected collection of letters of, to and about Meghnad Saha and his writings to commemorate his 125th birth anniversary, supplemented by an overview of him as a scientist, social worker, activist, organizer, father and family man. The letters convey the huge impact of Meghnad Saha's scientific contributions, the respect that he commanded among his peers, his persona and undaunted spirit, and a sense of time that should be of great interest and also inspirational to the modern readers. Some of the letters contain Meghnad Saha's own narrative of the development of his famous contribution to astrophysics, the Saha Equation, and his struggles to find very modest financial resource to support his research and publications and how he got deprived of the credit for another landmark discovery of Selective Radiation Pressure. There is also a valuable collection of pictures showing Meghnad Saha with many of the stalwarts in the field of physics and astrophysics in the 20th century, many of whom he invited to India, political leaders of India, his family members and academic colleagues, and in his organizational activities. A final section contains brief summaries of the careers and scientific contributions of the letter writers.

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MEGHNAD SAHA

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through Selected Letters and Writings

6 October 1893 – 16 February, 1956

Photograph of Portrait Sketch by Amiya Saha, March 1, 1954

Meghnad Saha

His Science and Persona
through Selected Letters and Writings

Jibamitra Ganguly

with

Reminiscences of a Daughter

Chitra Roy

A Commemorative Volume on Saha's 125th Birth Anniversary

Edited and Coordinated by

Ashok Kumar Singhvi

Indian National Science Academy
Bahadur Shah Zafar Marg, New Delhi-110002
2019

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FOREWORD

Societies, Nations and their future are sculpted by the dreams of a few wise men, who contribute to it by providing a vision and knowledge based wherewithal for their development. These people provide a road map, on which the Nation treads for its well-being. Meghnad Saha was one such persona who thought of India and its prosperity determined by science.

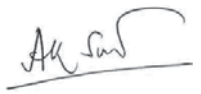
The Indian Academia and the society owe a great deal to Meghnad Saha, who besides his fundamental and path breaking contributions to astrophysics, envisioned the need for an Academy of Science in India to serve as an interface between science, society and government as well as the link with other international science bodies like ICSU. His article, ***A Plea for an Academy of Sciences***, in the Allahabad University Magazine in 1929 was sufficiently impactful that it led to the formation of UP Academy of Sciences. Later the National Institute of Sciences of India (NISI) was in place by 1935. NISI was renamed as Indian National Science Academy (INSA), in 1970. Saha was the Second President of INSA. Saha believed in Gothe's Maxim of, to *strive, to seek, to find and not to yield* and his Presidential Address to the UP Academy of Sciences should be a must read for all serious students of Science. This address presents his amazing vision almost a century ago and INSA is striving diligently to live up to this vision of Saha.

This year we celebrate the 125th birth anniversary of Meghnad Saha. We celebrate a brilliant intellect, a true patriot and a man with earthy humanness. We celebrate his 125th year, with great humility and a sense of reverence for one of the few, who provided solid foundation for the development of Science in India. This book is a mark of our deep gratitude to this goliath of Indian Science.

We at INSA are grateful to Prof. Jibamitra Ganguly, an internationally acclaimed geoscientist (and Professor Emeritus at Department of

Geosciences at the University of Arizona, USA), for his kind offer to write this book and for his immense diligence in producing such a book. We were delighted that this book has a contribution by Dr. Chitra Roy, the illustrious daughter of Meghnad Saha.

It is difficult to capture all facets of a personality like Meghnad Saha in a single book but we do feel that this book will provide an important window into his thinking and scientific development. This book was Edited and Coordinated by Prof. A.K. Singhvi, Vice President. I thank all concerned who made this book possible and feel privileged with the opportunity to write this Foreword. It is my conviction that in the years to come, this book will be a treasured possession.



October 16, 2019
New Delhi

A.K. Sood
President, INSA

PREFACE

I was given the responsibility of producing this collection of letters and writings several years ago by Prof. Chitra Roy and late Dr. Prasenjit Saha (daughter and son of Meghnad Saha), and our beloved teacher, late Prof. Supriya Roy, husband of Chitra Roy. This volume is not a comprehensive collection of Meghnad Saha's letters but a selection from what was available in the family archive. My criteria for inclusion of a letter in this volume are: (a) it should have some scientific and/or professional content; (b) it should at least convey a sense of time (social, political, cultural), especially if it is devoid of the first criterion; and (c) it should be compatible with the spirit of a celebratory volume. The science historians may take this selection and integrate it with other letters and writings of Meghnad Saha to develop a more complete picture of him as a person and a scientist. However, this collection of letters will bring out, I hope, the impact of Meghnad Saha's scientific contributions; his enormous enthusiasm and passion for science and scientific research, despite the struggle that he faced in carrying out his research in an economically underdeveloped subject nation under the British rule; his undaunted spirit; and show how highly respected he was among the stalwarts of his time that was inarguably the most revolutionary period of physics since Newton.

Meghnad Saha was one of the greatest scientists to come out of India in the modern era. His scientific contributions are versatile, ranging from Astrophysics to Nuclear Physics, as well as some areas of applied sciences that were motivated by his social commitments and Nationalistic ideals. His seminal contribution on the thermal excitation and ionization of elements, which led to the formulation of the famous "Saha Equation", gained him international acclaim immediately after its publication in 1920, when Saha was only 27 years of age (but in the prime age for making a major contribution as a physicist at that time), and secured a permanent place for him in the field

of Astronomy and Astrophysics. To explain some of the puzzling aspects of the spectroscopic data of the stars, notably the Sun, Saha came up with the bold suggestion that these data reflect different stages of excitation and extent of ionization of elements "under the stimulus prevailing in the star", rather than major compositional differences among the stars. He then went on to derive a formal relationship (1920: Philosophical Magazine) among temperature, pressure, ionization potential and relative abundances of different ionization states of an element. It was a remarkably imaginative step that combined the knowledge of the atomic structure in the new subject of quantum mechanics (the Bohr model) and statistical thermodynamics (it is said that Bohr himself was also considering the problem). The immediate success of Saha's theory and also some of his predictions led to intense synergistic activities within the Astronomy-Astrophysics community during the 1920's and 30's to further refine the theory and to determine ionization potentials of elements and their spectral characteristics, as one would appreciate from many of the letters in this collection.

The ionization theory had a profound impact in the development of the subject of Stellar Astrophysics. Augmented by subsequent experimental data on thermal ionization and theoretical refinements by Saha and others, such as Ralph Fowler and Edward Milne, it led to comprehensive understanding of the spectroscopic data of stars in terms of temperature, pressure and chemical conditions of their atmospheres. And beyond, it also fixes the epoch or thermal condition when the universe became transparent to radiation (Narlikar, 1995) and has been applied to a variety of problems in physics and chemistry such as electrical conductivity of flames, formation of electrical arc, formation of the ionosphere etc. (Saha and Srivastava, 1958). This theory was listed by Sir Arthur Eddington (plate 13) in the 14th edition of the Encyclopedia Britannica (1927) among the twelve great discoveries in the study of stars since 1596. Svein Rosseland, a leading astrophysicist of the time, remarked in his Treatise on Astrophysics (1936), that "***Although Bohr must be considered the pioneer in this field, it was the Indian physicist Megh Nad Saha who (1920) first attempted to develop a consistent theory of spectral sequence of stars[1] from the point of view of atomic theory. ..... The impetus given to Astrophysics by Saha's work can scarcely be overestimated, as nearly all later progress in this field has been influenced by it and much of the subsequent work has the character of refinement of Saha's idea.***" Also to quote from a Note by Milne in Nature

1 Spectral sequence is a scheme of classification of stars based on their spectroscopic characteristics to which the ionization theory was later applied to develop a temperature sequence, notably by Cecilia Payne (plate 12) for her famous Ph.D. thesis – one of the greatest ever in the field of Astronomy - entitled "Stellar Atmospheres: a Contribution to Observational Study of High Temperature in the Reversing Layers of Stars." In this thesis, she also showed by using the ionization equilibrium and some additional information that the Sun's atmosphere is made primarily of hydrogen and helium; this finding represented a paradigm shift as it was strongly believed at that at that time that the composition of Sun is similar to that of the Earth.

(1925): "*Six years ago, practically no explanation existed of why some lines appear in stellar spectra and not others, why some lines always decrease in intensity through the stellar sequence, and others appear, reach a maximum, and fade away again. It is to Saha that we owe the key that has unlocked these mysteries*."

The history of development of the thermal ionization theory and its subsequent modification is captured in some of the correspondences of Saha with Henry Norris Russell, Harry Plaskett and Marcel Minnaert. The last two sets also show how some thought that the originator of the idea behind the ionization theory was the eminent astrophysicist Alfred Fowler (plate 7), with whom Saha worked for a short time in the Imperial College, London, in 1920. However, Plaskett was amazed when he finally realized through a long and disgruntled letter from Saha, included in this collection, that the essential aspects of the theory was developed single-handedly by Saha in India and before he visited Fowler.

Plaskett's response to Saha's letter was remarkable and indicates that in the 1940-s Saha was already recognized as one of the founding fathers of Astrophysics. This is also reflected in the comments of some other eminent scientists in this compilation of letters. From Saha's letter to Plaskett, we also find that Saha had developed the theory of selective effects of radiation pressure on atoms[2] that could counteract the effect of gravity on the distribution of atoms in the stellar atmospheres; it helped explain such anomalous feature as the presence of calcium at greater heights than a much lighter element, hydrogen. Unfortunately, however, Saha was unable to get the full paper published in the Astrophysical Journal for his inability to pay the page charge that was mandatory at that time because of severe financial constraint in a post-world war period. The credit for this seminal contribution goes to Prof. Milne of the Oxford University who seems to have taken the cue from a short paper by Saha in Nature, and with due acknowledgment, which, unfortunately, did not catch much attention in the Astrophysics community.

Meghnad Saha belonged to what is unquestionably the most internationally acclaimed group in the history of Indian science that included stalwarts like Sir Jagadis Chandra Bose (plate 9), Sir C.V. Raman (plate 6), Satyendra Nath Bose (Meghnad Saha's classmate, plate 9), Sir K.S. Krishnan, Sisir Mitra (plate 9), Prasanta Mahalanobis and Sir P.C. Roy (plate 5 & 7). This group was part of the Bengal renaissance in the early 20th century that also saw the emergence of Rabindranath Tagore and many other eminent literary figures, artists, political and religious leaders. Meghnad Saha, Satyendra Nath Bose, and two of their classmates, the distinguished chemists Jnan Ghosh (plate 9) and Jnan

2 Absorption of electromagnetic radiation or photons by an atom imparts into the latter a velocity that is proportional to the energy of the photons. Saha showed that it is a selective process for atoms since an atom will absorb only those photons that could cause electronic transitions between quantized energy levels.

Mukherjee (plate 9), went on to become general presidents of the Indian Science Congress!

As I have remarked elsewhere (Ganguly, 2017), Kolkata during Bengal renaissance was also fortunate to have visionaries and mentors like Sir Asutosh Mookherjee (a distinguished mathematician, judge of the Calcutta High Court and later a vice-chancellor of the Calcutta University), Sir Prafulla Chandra Roy (an eminent chemist and industrialist) and Dr. Mahendra Lal Sircar (a physician who founded the first science research institute in Asia, the Indian Association of the Cultivation of Science, IACS, in 1876). They tried to provide the younger generation as much support and nurturing environment as was possible within their powers and spheres of influence. This collection of letters also includes some correspondences involving Mookherjee and Roy that would, I hope, convey a sense of their commitment and effort to promote advanced scientific teaching and research in India despite the adverse circumstances in an economically underdeveloped nation. By managing to lure Raman to a position of an endowed Chair (Palit Professor) of Physics at nearly half the salary of his well-paid position as a senior officer in the office of the Accountant General, Bengal, and appointing S.N. Bose and M.N. Saha as lecturers – all much before their attainment of international fame – Asutosh Mookherjee helped create a Calcutta School of Physics, the like of which, in C.V. Raman's words "certainly does not exist in any other Indian University, and which, even now, will not compare very unfavorably with those existing in the best European and American Universities".

Making a reasonably good living as a scientist in India was a difficult task in those days. Thus, like Raman, Meghnad Saha also entertained the idea of taking the Indian Financial Civil Service examination, but fortunately for science, he was not granted permission to do so because of his past associations with nationalist revolutionaries during his college days (Sen, 1954).

There was little financial support and infrastructure for scientific research in India at the time that Meghnad Saha and others in the above group emerged in the scene of "wonderfully blossoming Indian physics", as one of the letter writers in this collection, the Hungarian physicist Rudolf Ortvay, called it. On the other hand, the western world was rapidly moving ahead and witnessed revolutionary breakthroughs in the fields of Physics and Chemistry in the early twentieth century, spearheaded by the development of the theory of Relativity and Quantum Mechanics. Some of the letters in this collection reflect the dire financial condition in the research laboratories in India and Saha's struggle to generate modest funding from different sources from within India and elsewhere, including USA, to support the research of his group. In one of the letters, Jnan Mukherjee lamented to Saha that perhaps he should not have returned to India, while in a letter to the astronomer Harlow Shapely (plate 12, 16), Russell expressed concern about the detrimental effect that the burden of "earning his living in a small position" had on the quality of Saha's theoretical

work after his return to India.

Meghnad Saha's contribution to science was not only in the realm of research but was also in the organizational aspects and coordination of scientific work in India. He founded the National Institute of Science, later renamed as Indian National Science Academy (INSA), the journal Science and Culture and the Institute of Nuclear Physics (now known as the Saha Institute of Nuclear Physics) in Kolkata. Meghnad Saha's vision for the Academy and the Indian Science was: "*If we desire to fight successfully the scourge of poverty and want from which 90 percent of our countrymen are suffering, if we wish to remodel our society and renew the spring of our civilization, and lay the foundation of a strong and progressive national life we must make the fullest use of the power which the knowledge of Nature has given us. We must rebuild our economic system by utilizing the resources of our land, harnessing the energy of our rivers, prospective for the riches hidden under the bowels of earth, reclaiming deserts and swamps, conquering the barriers of distance and above all, we must mould anew the nature of man in both individual and social aspects so that a richer, more harmonious and a happier race may live in this great and ancient land of ours. Towards the realization of this ideal, we people must adopt ourselves to new philosophy of life and train the coming generations for the services of the community in scientific studies and research*".

In a letter included in this collection, one finds that the celebrated astrophysicist Subrahmanyan Chandrasekhar, then a graduate student at the Cambridge University, characterized Saha's organizational ability as "beyond all praise" when he sought Saha's help for the release of the great physicist Pyotr Kapitsa (plate 23) from his native country, the Soviet Union, where he got confined when he went for a visit while working in the Cambridge University. Saha's response to Chandrasekhar's letter is lost. However, his article on this issue, published in "Science and Culture" in 1935, is reproduced in the Section 8.

In the later part of his career, Saha became a social activist and got involved in politics as he felt that he must make some tangible contributions to the wellbeing of the society using his knowledge in science, as reflected, for example, in his correspondences with Dr. J.W. Whitaker, then the Director of the Indian Fuel Research Institute, Dhanbad. In fact Saha was an activist even during his school days and participated in protest and independence movements against the British Raj that got him into trouble with the establishment leading to his expulsion from the Government High School. These aspects of his life, along with many others as a father, family man, organizer etc., have been captured in a moving article in this volume by his daughter, Prof. Chitra Roy (plates 2 & 18). Science for the benefit of society spirit also led Meghnad Saha to urge the Government of India to create a Geophysical Research Institute (that I had the opportunity to visit on several occasions in the past) in order to be in a position to carry out exploration for oil and other natural resources within the country for economic developments.

The major collection of letters in this volume deals with Meghnad Saha's scientific contributions

and interactions, but several letters also deal with social and organizational issues. The first group has been subdivided into two parts: one (Section 4) dealing with correspondences with scientists in the western world, and also between scientists within the latter group that have remarked on Saha's work; the other (Section 5) dealing with correspondences with Indian scientists and academic administrators that provide a historical perspective of the climate for scientific research and education in India at that time. The letters have been organized into a group when there are more than one letter between Saha and some other scientist; a chronological order has been maintained within each group, and the starting dates of different groups. A small number of non-scientific letters have been compiled as Miscellaneous Letters (Section 6) as these seem to be interesting in conveying a sense of time, and tourist attractions in India to foreigners at the early twentieth century. This is followed by a collection of Saha's articles about different scientists, and organizational and other issues (Section 7). These articles are scattered in different journals and other types of publications that are not easily accessible, especially at this distance of time. Thus, I felt that re-publication of these writings in this volume would be appropriate as it would make them accessible to a much larger audience (a large collection of such writings in Bengali were compiled by Chattopadhyay, 1966). At the end there is a section about the scientists who wrote the letters (Section 8). The formatting of many of the letters have been modified somewhat and the letter-head details shortened or eliminated in many cases, especially when they are repetitive, to make the presentation more compact.

This collection of letters shows that Meghnad Saha tried to maintain a steady and strong connection with the western world that was the scene of most of the important scientific breakthroughs, as it is also today. With regard to interactions with British scientists, one sees a clear separation of science from his Nationalistic activities and involvement in the independence movement. He also tried to get the western scientists to visit India to give lectures and teach short courses, whenever there was an opportunity. In this respect, Meghnad Saha's outlook seems similar to that of another stalwart of his time, Homi Jehangir Bhabha, whose organizational activities have been a subject of a recent publication (Leslie and Chowdhury, 2018).

Saha's sphere of scientific connections in the western world was amazing, especially considering the means of these connections, namely postal letters, in those days. And he seems to have developed good friendship with Henry Norris Russell, Arthur Compton and Arnold Sommerfeld. There is, however, a conspicuous lack of correspondences with Indian scientists dealing with scientific problems of mutual interest. This may be due to the fact that there was a limited number of people in India at that time with whom Saha could have had fruitful interaction in science, especially in his major area of interest that was Astrophysics, and probably also because much of these interactions were through oral communications. Of particular interest to the readers and science historians

may be the interaction of Saha with the other Bengali stalwart of his time and his classmate, Prof. Satyendra Nath Bose, some accounts of which could be found in a book on Bose (Majumdar *et al*., 1994).

Several books (e.g. Sen, 1954; Chattopadhyay, 1966; Venkatraman, 2018; Mukhopadhyay, 2012) have already been written about the life and science of Meghnad Saha. Thus, one may wonder if a collection of letters serves any special purpose. While the books written about Saha collectively provide almost complete exposition of Saha's multi-faceted contributions to science and society, the letters carry a special flavor in that they bring the person to life, and together with the pictures they also serve as time capsules. The letters document the nature of both science and society in India and in the western world at that time, some of the major problems in physical sciences and future directions, as envisioned by the stalwarts, the challenges that Indian students had to face when studying abroad (as captured, for example, in a letter by Kathe Schmidt in the Section 6), and the undaunted spirit of Saha that enabled him to overcome the many obstacles that he faced in his career. An example of time capsule of historical importance is a picture of the participants in the 1936 International Conference on Nuclear Physics (plate 11); the picture is striking not only because of the physics greats that were present in the conference, but also because that there were only two women among around 60 participants, one of the two being the great physicist Lisé Meitner (a co-discoverer of nuclear fission, but unfortunately, if not regrettably, was not awarded a share of the Nobel Prize for this discovery).

Meghnad Saha received seven nominations for the Nobel Prize for his ionization theory, and most notably by Prof. Arthur Compton of the University of Chicago (himself a Nobel Prize recipient in Physics) in 1937. However, Nobel Prize aside, it is simply astounding that Meghnad Saha did not receive any major honor, except for election to the Royal Society of England – so much for the system of award and honor at both National and International levels!

Meghnad Saha was a larger than life personality and the Indian National Science Academy (INSA) must be commended for the publication of this volume to celebrate his 125th birth anniversary. The period 2017-19 also includes the 125th birth anniversaries of two other stalwarts, Satyendra Nath Bose (b. 1894) and Prasanta Mahalanobis (b. 1893), and the centenary of the foundation of J.C. Bose Institute in Kolkata in 1917. Thus, 2017-19 marks a celebratory period in the history of Indian science. But let this also be a time for reflection, greater social recognition of the importance of science and scientists, implementation of effective policies and commitments for support for further advancement of science in India at a rapid pace.

March 15, 2019
Tucson, Arizona, USA

Jibamitra Ganguly
Department of Geosciences
University of Arizona

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ACKNOWLEDGMENTS

I am immensely grateful to Prof. Chitra Roy and late Dr. Prasenjit Saha for sending me a large collection of letters and other documents relating to Prof. Meghnad Saha and giving me the opportunity to produce this volume. I only wish that I had been able to do that much earlier. Prof. Chitra Roy also gave me valuable information and advice at different stages of this work.

The production of this volume would not have been possible without the initiative, coordinating efforts and active involvement of Prof. Somnath Dasgupta. Prof. Pulak Sengupta, Prof. Suchorita Chattopadhyay (Meghnad Saha's grand daughter) and Subham Mukherjee also committed significant chunks of their time to this project that involved finding the letters from the family collection that I have selected (and some more), getting those letters photographed and typed and putting up with my repeated intrusions into their busy schedule to do and find something in a short notice. Prof. Sumit Chakraborty translated to English several letters that were originally written in German. Prof. Debashis Mukherjee read early drafts of the Preface and gave valuable advice. The Preface has also been significantly rewritten to respond to the comments by Prof. Arnab Rai Choudhuri, Prof. Kankan Bhattacharya and Prof. Rajinder Singh, as reviewers of this volume. Prof. Rajib Ganguly gave valuable advice on several astrophysical issues. Dr. Chandra Mohan Nautiyal took the trouble of looking through the collection of the National Academy of Sciences, Allahabad, and sending me a few interesting pictures for inclusion in this volume. I am grateful to each one of these individuals for their help and comments.

Thanks are due to the President of the Academy, Prof. Ajay Sood, and Vice-President, Prof. Ashok Singhvi, for deciding positively and

very promptly on the proposal for publication of this volume by INSA that was initiated by Prof. Somnath Dasgupta. Prof. Singhvi made many constructive editorial suggestions that are gratefully acknowledged.

I would also like to thank Ms. Manisha Shrivastava of Angkor Publishers (P) Ltd. for her help with the type-setting and the design of the book as well as for providing several critical inputs and correcting some aspects of the detail. Thanks are also due to Mr. Bhagavan Das of Angkor Publishers (P) Ltd. for his efforts, in co-ordination with Ms. Shrivastava and Prof. Singhvi, for such an elegant production of the book at a short notice.

Jibamitra Ganguly

1.

BRIEF BIOGRAPHICAL SKETCH

Personal Life:

- Parents: Jagannath Saha and Bhubaneswari Devi
- Born: October 6, 1893, Sheoratoli village, Dacca District, East Bengal (now Bangladesh)
- Death: February 16, 1956, Kolkata
- Married: 1918 (wife: Radha Rani Saha)
- Children: 7 (3 sons: Ajit, Ranjit, Prasenjit; 4 daughters: Usha, Krishna, Chitra, Sanghamitra)

Education:

- Kishorilal Jubilee School, Dacca, Matriculation, 1909
- Dacca College (then under the Calcutta University), I.Sc. (Intermediate Science), 1911
- Presidency College, Kolkata (then under Calcutta University), B.Sc. (honours in Mathematics), 1913
- Presidency College, Kolkata, M.Sc. in Mixed Mathematics, 1915
- Calcutta University, Kolkata, D.Sc. 1918

Visiting Scholar:

- Imperial College, London (Laboratory of Alfred Fowler) & University of Berlin (Laboratory of Walther Nernst), 1919-1921

Professional Appointments:

- Lecturer (1916-1919) and later Khaira Professor (1921-1922) of Physics, Calcutta University
- Professor of Physics and Dean of Faculty of Science, Allahabad University (1922-1934)

Honour:

- Elected: Fellow of the Royal Society of England, 1927

Organisational and Political Activities:

- Founder and First President (1931-32): The Academy of Sciences of the United Provinces of Agra and Oudh, now called the The National Academy of Sciences, India (NASI), Allahabad
- President of National Institute of Sciences in India (1937-38), this was later renamed as Indian National Science Academy
- General President, Indian Science Congress, Bombay, 1934
- President: Indian Physical Society (1935)
- Member, University Education Commission, Govt. of India, 1948
- Founded: Saha Institute of Nuclear Physics (erstwhile the Institute of Nuclear Physics) in 1949 and the Journal Science and Culture
- Elected: Member of the Indian Parliament, 1952, as an Independent Candidate supported by the Leftist parties

2.

LIFE WITH FATHER

Chitra Roy née Saha

The letters in this celebratory volume were collected by my eldest brother, Dr. Ajit Kumar Saha. During his later years, because of ill health, he had passed on the collection to Dr. Prasenjit Saha, our youngest brother, for safe keeping. The collection ran into volumes, the result of my father's multifaceted interests. While sifting through the material, Prasenjit decided to compile and annotate the letters, a Herculean task, to the best of his ability.

While reading the letters, the images and the spectre of the times gradually unfolded before my eyes. So numerous were the correspondences and they dealt with so many varied subjects – research problems, science planning and organisation, archaeology, flood-control, the introduction of the decimal system in coinage, calendar reform, refugee rehabilitation problems, language issues, political issues in general and of the time – that we decided that a selected corpus of letters emphasizing his science and providing a glimpse of his multifaceted persona may be handled within the scope of a reasonable time frame and number of pages.

The twenties and thirties, when he lived and worked in Allahabad, appears to be the most fruitful period of my father's scientific research life. He was also a concerned family man, and one who did not forget his roots in what was then during pre-partition days, East Bengal, and whose mother tongue was Bengali. It was a rich heritage to him, and he fought relentlessly for these issues in the Parliament, in public forums and through letters to the concerned ministers and bureaucrats.

Father had been criticised quite often, inside and outside the Parliament for turning to politics in his later years. As I see it, politics or call it nationalism, or love for his country and the urge to free it from colonial shackles, was in his bones since his school days. During the first

partition of Bengal (***Banga-bhanga*** movement) he had actively taken part in the protest movement against the British Governor's visit to his school and was expelled as a result. This was 1905. During father's centenary celebrations, Benoy Choudhury, a Minister in the Left Front government in West Bengal, told me on one occasion that father had been an active member of the Dacca Anushilan Samity (a revolutionary organisation of freedom fighters in Bengal in the early decades of the last century), headed by the well-known revolutionary Pulin Das, and that he had practised fighting with long sticks and daggers.

As youngsters in Allahabad, we were amused to see father walking on long sticks fitted with foot-holds (***Ronpaa*** in Bengali) on our spacious lawns. He would smilingly tell us that dacoits of Bengal in the early days used to cover long distances in a short time in this fashion. During the World War II and the 'Quit India movement', we were evacuees in Rajshahi. Father got hold of a trainer from a nearby '*akhraa*', an old fashioned gym, to give us (three sisters and my younger brother) lessons in fighting with daggers and sticks. Now I wonder, were these incidents left-overs from his early association with the Nationalists or ***Swadeshis***?

In his reminiscences, Bhupen Dutta, the younger brother of Swami Vivekananda, recalls the residents of the boarding house, commonly referred to as a "mess", at 110 College Street, Kolkata, many of whom were students of Presidency College. He names father among them, being actively involved with Bagha Jatin (Jatin Mukherjee, the well-known revolutionary), but we do not know to what extent. My late uncle, Dr. Kanailal Saha, a medical student at the time, had once casually told us that the students at the "mess" acted as couriers during the independence movement. Occasionally Dr. Prafulla Chandra Ray, their mentor, would go out for an outing, in what used to be called '***Garer maath'***, the spacious *maidan* adjoining Fort William. On his way, he would pick up his favourite students from 110 College Street mess to accompany him. They would have long *addas* or chats and he would indoctrinate them with the ***Swadeshi*** movement. Himself a government servant, he could do no more. But the Bengal Chemical Works, the Bengal Lamp, the Bengal Potteries, many cloth mills, Sulekha Ink, soap factories, Dye & Wash companies, sprouted up, started by many of P.C. Ray's students and disciples.

The extent of Father's involvement in the ***Swadeshi*** is still to be gauged. Father had never discussed the topic with us, the children. Other sources can be explored. For instance, in an article entitled "Saha's influence in the West: A Preliminary Account" (***Meghnad Saha Birth Centenary Commemoration Volume)***, the noted science historian DeVorkin recounted the difficult circumstances of father's election to the Royal Society. Alfred Fowler had put up his name but evidence suggests that Saha's political record was a concern among those pondering on his candidacy prior to deliberation by the Council (p. 183-184 of the above ***Birth Centenary Volume;*** article by DeVorkin). Sir James Jeans asked Holland for the full report on Saha, and Holland complied.

The intelligence report identified Saha as a conduit for Indian revolutionaries in Germany and Switzerland. Saha had apparently raised money for their cause, and advised Indian students who planned to be in Germany for training. Saha was also known to receive Bolshevik literature when in Calcutta, and "sheltered [the Bolshevik] emissary Nalini Gupta ... while the latter was concealing his presence in India in 1923."... Jeans conveyed the information to the Council members .. "*it has come to my knowledge that (Saha) is a rabid revolutionary mixed up with various sorts of anti-British propaganda.*" (the Birth Centenary Volume, p. 184). Bhupen Datta corroborated this statement in his book ***Biplaber Padachinha*** (Footprints of Revolution) (Sahitya Sangshad, Kolkata, 1999. p. 179): "***After his arrival in Berlin, in 1922, the first person whom M.N. Roy first sent to Bengal to re-establish connections with his old party, was, let us say, Ramani (not his real name), After he came to Kolkata, he was first sheltered by Dr. Meghnad Saha and then by Dr. T.N. Roy. Jaduda*** (Jadugopal Mukherjee, a cousin of the revolutionary Bagha Jatin and a regular visitor to 110 College Street) ***and Atulda*** (artist Atul Bose) ***knew him .....***" (translated by the author).

There is a discrepancy in the date 1923 and 1922, but most probably Nalini Gupta and Ramani were the same person. Earlier (p. 3) Bhupen Datta had stated that "***We had filled the mess with our own people, Meghnad Saha, Sisir Mitra, Sailen Ghosh, Jatin Seth, Jnan Mukherjee, Jnan Ghosh etc. – those who had helped Sir Asutosh Mukherjee build up the University Science Department, eight among them had taken seats in this mess. Almost all of them used to socialise with Jatinda and Sashida*** (Sashibhushan Roy Choudhury, the propagator of Workers' Education). ***Some of them had taken part in the India-Germany conspiracy. During the peak of Jatinda's activities, they had intimate access to the addas*** (chatting sessions) ***held at Nilratan Dhar's mess at 110 College Street, Hindu Hostel and elsewhere. Some stayed back in these places.***" Bhupen Datta also mentions the smuggling in of Marxist literature into the jail cells; the inspiration of Marxist literature is mentioned by DeVorkin as well. The extent of father's involvement in the Swadeshi can be known only by a full examination of the police archives of the time both in West Bengal and in Bangladesh, if these have not already been destroyed on the eve of independence.

My point in emphasising the political aspect is that one cannot separate Saha the Scientist and Saha the political man, not in the present day sense but in the sense that the political forum is also needed to harness science to serve the nation. Father believed, if necessary, one has to set up as anti-establishment and that was what he had been all his life. Again, though a great admirer of Marxism and the Soviet system, he confined his admiration to the advances in science and industrialisation and the apparent egalitarian society in Soviet Russia.

The immediate reason for Father's switching over from the Swadeshi movement and politics to science proper was of course that he was barred from competing in the Indian Financial Service. Had he qualified, it would have solved his acute

financial problems. Almost the entire joint family depended on him financially. But when he went into teaching and research, perhaps he felt he could get even with the colonial masters at least in this field. He did to some extent, but not fully. But he remained anti-establishment all through his life, be it the colonial establishment or the establishment after independence. He entered the Parliament on an Opposition ticket and riddled the government with uncomfortable questions.

Let me end this section on Father's political involvement in a lighter vein. While in Presidency College, I happened to join a procession protesting against some issue. Letters were sent to the guardians asking them to warn their wards. I had tried and failed to intercept the letter and in due time was called in and taken to task – "Next time this happens, you will be taken out of this college". Had I known at that time that he was expelled from school for leading a boycott movement against the then English Governor, I would have probably answered him back.

Researchers like Robert Anderson of Simon Fraser University, BC, Canada, has been interested in what he calls my father's 'other life'. Father's 'other life' was not wholly clear to his children as well. He did not communicate much with his children. Letters to Ma (written in Bengali) and to some of his students who were very close to him during his Allahabad days, for instance the late Dr. P.K. Kichhlu (plate 9) of Delhi University, can offer a fuller picture. Some of these have been preserved.

We used to call him Baba (as a father is commonly addressed in Bengali), though here I am going to refer to him as Father. He was a great one for writing letters. But then, wasn't that the only means of communication in those days? The letters were written in a clear, bold hand. Father did not know how to type. The single typewriter in the house was used by a relative who acted as his unofficial secretary and typed all the official letters. Personal letters were handwritten.

All kinds of physical exercises were actively encouraged for the children. Walking and swimming were favourite pastime. When he was younger and in Allahabad, Father followed a stricter daily schedule than the one he followed later in Calcutta. This included taking the children out in the evening, leaving them in the parks to play, and going for long walks with mother or paying social calls. He loved company and '*adda*' with friends and research students. The boys played cricket, badminton etc. Father, on one occasion that I remember, played what we used to call tenniquoit. There used to be picnics and outings with friends. The favourite spot was the confluence of Yamuna, Ganga and Saraswati, now dried up, called Prayag, deemed a holy place. Both my parents enjoyed bathing. One of my early memories was father dipping my younger brother, a baby then, in the river water. Father was an atheist and agnostic and he did not go to Prayag for any religious purpose, but simply to bathe. However, my mother was a very devout person, and during Magh-mela and Kumbha-mela every year, we would have

pilgrim relatives swarming in our house. Father was a good host. I also must have accumulated a lot of piety as a result of the innumerable times I was dipped in the holy water by the relatives whom I accompanied to Prayag. But we grew up Godless. Father's atheism had a stronger hold on us than mother's rather submissive and acquiescent devotion. Almost all Indian households have or used to have in those days a '*thakur-ghar*', a small room where the household deity was worshipped. Mother had none. But she would sit in a corner of the room after her bath and pray for sometime. This was a ritual with her. Some of the children reverted or should I say were converted to traditional thinking.

Coming back to sports, when we came to Calcutta (Father had joined the Calcutta University in 1938), we were lucky to buy a house on Southern Avenue (plate 4), facing the Rabindra Sarobar, the lakes. So the family joined a local swimming club, the Anderson Club. My parents enjoyed swimming and going for long walks in the evenings on the promenade adjoining the lakes. Father taught me to swim but not in Anderson Club. In 1942 we stayed for a year in Rajshahi, North Bengal (now in Bangladesh), as evacuees during the Japanese war. The small town is situated on the bank of a branch river of the Padma. During summer, the whole town used to pour out to bathe in the river. It was here in this river that Father taught me to swim. Both my parents enjoyed swimming. To Father it came naturally since he grew up in the riverine delta territory of what used to be East Bengal in pre-partition days. During the monsoons all the villages were water-bound. As a young school boy, during summer and winter, he would trek all the eight miles to school and back, with *chira*, a very simple fare, tucked in his dhoti for refreshment. During the rainy season, this stretch had to be covered by boat. Father was very proud of this hardship he had undertaken in his young days. When we were reprimanded for laziness, he would often cite this example.

As to music and cinema, he did not seem to be much interested. The cinema, I believe, was frowned upon. But mother told me she had heard him humming a tune occasionally when he was younger. Two occasions when he attended musical soirees were when Dilip Ray, a very famous singer and father's friend and contemporary, sang in 1938 in Allahabad and in 1952 in Rashtrapati Bhavan (President's house) in New Delhi. I can't make out whether the interest was in Dilip Ray or in music. But he did read novels and poetry. He even used to compose poems when he was young. Unfortunately, nothing from his younger days, not even photographs have survived. We should have made an effort to collect these documents much earlier. Now they are irretrievably lost. Except for Dada, my elder brother (the late Professor Ajit Kumar Saha), his first born and a special favourite, and in whose education he took a very keen day to day interest, the rest of the brood, there were seven of us, were left pretty much to themselves, to browse in the library, and to struggle with our syllabus. When we were in school, Father often did

not even remember what subjects we had taken up or which class we were in. But sitting with him on occasional evenings and reading aloud Tagore's poetry, his shorter plays for children, or Shakespeare's plays to him, was a pleasurable ritual.

Getting back to the ritual of reading aloud to Father, the slant, mind you, was on patriotism, social awareness etc. My second sister, Krishna (plate 2 & 18), I remember, had to read out Sun Yat Sen's "San Min Chui" (The Three Principles) when we were on vacation in Madhupur, Bihar, in 1946. She and my elder sister had to take lessons in Sanskrit as well, with him. We had a collection of Bankim Chandra's novels in our library, and he had quizzed Mother on this novelist, when he went to interview the bride to be. So he must have read this novelist. He had read Kalidasa (the Sanskrit poet and dramatist), because he knew Sanskrit and the Kalidasa Volume in our library was well thumbed. But perhaps he had not read many of the later novelists. Father knew German very well. He had taken up German as an optional subject in college, and the library was well-stocked with books on German literature. The prime concern was to read scientific papers in German. When I was in college and built up my own collection of books, consisting mostly of the classics, Marxist literature and detective fiction and my college texts, once in a while he would come up to my room (mine was on the second floor, Father's room was on the first), finger through these books and take down some to read himself to sleep. Or, he would ask me to bring down some, preferably lighter fiction, and relax with these in his bed. We were thrilled to see him handle something that was not heavy going. Once I remember, he was rebuking Dada for wasting his time on what he thought was light literature. Dada happened to be reading Thomas Mann, and Dada turned back on him and lectured him on Thomas Mann. Father listened quite sheepishly to the lecture. So I suppose he was capable of taking to new ideas outside science.

The family or part of the family, went on holidays to different places – on and off to Delhi, East Bengal (now Bangladesh) to my maternal grandfather's house in Dhaka, Bikrampur in 1937, Calcutta in 1936 and 1937, Kashmir in 1938, Darjeeling in 1940, Chhapra in Bihar and Rajshahi during the Japanese War between 1941-1943, Madhupur in Bihar in 1946, Shimla in 1949 (at that time Baba was writing the report of the Universities Commission along with other members and all were entitled to take their families. Three of us, Krishna, my second sister, I and my younger brother Prasenjit accompanied him and stayed at the sprawling Palace, with other members of the commission; it was an exciting experience (plate 18), in Delhi in 1951-52 during Father's tenure as a Member of the Parliament. These were family holidays within the country. My mother had never accompanied my Father abroad. She was supposed to do that once (see letter by Mrs. Milne in Section 6), but later her tickets were cancelled. The family had to be considered.

My mother (plate 1) was a very simple person. She had only completed middle school and got married at the age of fourteen. Father was twenty-six. She was completely dominated by father and wholly devoted to him. The only time that I had seen her upset was when with three or four families (relatives fleeing after the partition of the country in 1947) crowded in the house and the household chores became absolutely backbreaking. As a rule she was uncritical. She was Father's confidante and would patiently listen to the day's happenings after he came back home. He needed her and she was the old-fashioned accommodating wife and affectionate mother. She was extremely proud of her husband's achievements which was rather hard on us, the children. We were expected to live up to the rather high academic standard that father had set. Of course this led to all kinds of complications.

While Father was immersed in science and related organisational programmes following his early activities during the school days against the British occupation of India, he got involved in politics again later in his life after India's independence and was elected to the Indian Parliament in 1952. We were quite grown up then; I was twenty-one. I acted as one of Father's polling agents. So we were actively interested. I don't think mother came out with any positive suggestion during their discussion either of his work or politics. Mother's role was that of a passive listener.

We were a family of three brothers and four sisters. Ajit, the eldest (plate 20), was a physicist. Ranjit, a year younger, was an electrical engineer and worked for the Tata Hydro-Electrics at Bombay. Usha, my eldest sister coming next, also studied Physics, but ended up as a housewife, having been married off at the age of nineteen. It must have been very frustrating for her. My two elder brothers and elder sister have died, Ajit in 1991, Ranjit in 1993 and Usha in 1997. Very recently, on March 18, 2019, Prasenjit (plate 18 & 20), our youngest brother also passed away. Krishna, my second sister (plate 2 & 18), was a formally educated doctor, but also had to give up the idea of a professional career since she was also married off at the age of twenty. However, on my mother's insistence, both sisters were allowed to complete their education (Usha her M.Sc. in Physics and Krishna her MBBS) after their marriage. Next in the line was myself, who did rather an unambitious M.A. in English Literature, resisted all attempts to be maritally packed off and finally settled down to a teaching career in a government college in Calcutta and went on to acquire a doctoral degree. Subsequently I found a husband of my own choice. But when that was done Father was tremendously pleased. I had opened the gates of rebellion and my younger brother followed suit. Prasenjit did not study Engineering as Father had wanted him to, but took up Geology, and married a girl of his choice. In this case also Father's approval came in no time and he was about to make wedding arrangements when he died. Prasenjit later retired as the Deputy Director of the Central Glass and Ceramic Research Institute. My youngest sister, Sanghamitra is much younger

than all of us and she was just eleven when Father died. Like me, she also had taught in a college in Delhi and her subject is History. Prasenjit and I had served in government institutions. From the above recounting one can judge what a curious mix-up the family was. The girls were encouraged to study as far as they wanted to, but one had to get married at the right age. That it interrupted one's career was not considered. Going abroad for higher studies was a must for the boys. Before they reached that age, the girls were married off. Antiquated ideas persisted in the family. Yes, the girls were encouraged to take music lessons, but accomplishment in this field was required for eligibility in marriage. Do I sound cynical? But this was the prevailing climate of thought not only in our family but all around.

Father was away a lot. As we grew up we started taking interest in his travels abroad. In 1921 he had travelled to England and Germany to carry on his research in Alfred Fowler (plate 7) and Walther Nernst's laboratories. In 1927 he again travelled to Europe, to England for the Royal Society ceremony, to Italy to attend the Volta centenary, to Copenhagen to observe the full eclipse of the Sun and extensively in the Middle East to survey the relics of early civilization. My earliest memory was of his 1936 trip to the U.S.A on a Carnegie Fellowship which financed his visits to different laboratories, including the Mount Wilson Observatory at Pasadena. He took Dada (Ajit Saha), who had just left school, with him, and put him in Paul Geheeb's school in Germany, bordering Geneva. Geheeb was a friend of Rabindra Nath Tagore, and had the same ideas in running a school as Tagore had about running Santiniketan. Dada stayed there for eight months or so and was collected back on Father's return to India. The later travels abroad were in 1944 in wartime Britain, when he discovered that American and British intelligence were watching his movements. He happened to be working on a sensitive subject, nuclear physics. We listened to him telling Ma about food ration and the general hardship people in Britain were undergoing because of the war. In 1945 he visited Soviet Russia and wrote a book, "My Experiences of Soviet Russia". Planning for India's industrialization had been uppermost in his mind and the Russian 5-year plans, I suppose had been his inspiration.

He recounted how at a party he had swallowed vodka mistaking it for water. He was teetotaler. Listening to these anecdotes was exhilarating but not when he asked us girls to take shovels and spades and work in the garden like our hefty Russian sisters did. It seemed Father along with American and British scientists had been inspired by the spectacle of Russian women working the fields. The scientists were agreed on one point. The Russian girls were not silken like their American and British counterparts. They were sturdier. You might consider this episode as his wanting to encourage physical exercise for the girls. That is why, when we were in Rajshahi, the girls had to take lessons in playing with dagger and long sticks, which used to be traditional in Bengal. We

could not complete the course, but we thoroughly enjoyed the few lessons given us. Going back to shovels and spades, thank god it was a passing phase.

In 1946 Father had attended the Empire Science Congress in Great Britain. He came back with a complete set of Bernard Shaw's plays in Penguin edition and I was the lucky one to receive it as a present. I still have it with me. I had hardly read up the set when he summoned me. "Had I read Major Barbara?" "Could I take that as a model and write something in Bengali on the numerous religious and philanthropical institutions that had mushroomed all around?" a tall order for a sixteen year old. Father had read something that I had contributed to the school magazine when I was only eight. Ever since he was convinced that literature was my forte and I could be creative if I wanted to. Father made two or three more trips to the U.S.A, England, France, Russia, Dublin and Helsinki. In Dublin he also met the legendary Eamon de Valera, the leader of the Irish Freedom Movement. In Helsinki he attended the World Peace Congress, which was known to have a leftist bias. I happened to be around to pack his bags, as I did most of the time. My sisters were married, my brothers were abroad, and mother was not keeping well. Overhearing anecdotes he related to Ma, friends and students was always rewarding. Very seldom would he call us to him and directly relate his experience; there always was a distance between him and us.

Many friends and critics had found a certain abrasiveness about Father, and with reason. This put off many people, it is true. He acted on impulse and displayed certain naiveté. This was a carryover from his village days and he cared little to acquire urban sophistication. When we grew up we were delighted to listen to his broad East Bengal intonation. The abrasiveness could partly have been the result of the hardship he had to undergo as a young boy. Biographers now try to make out a story of abject poverty and backwardness in education in his native village. But Father's authorized biography (brought out on his 60th birthday and approved by him) says "*the family belonged to the Hindu middle class, who had to work hard for a livelihood.*" My grandfather had five sons and three daughters to look after, and he ran a grocery shop which provided him with "an uncertain income". As I heard from my mother, my grandmother was a wonderful housekeeper. So whatever little my two grandparents managed to scrape up, was well managed and each member of the family was well looked after. Food and health were priorities. Thanks to grandmother's efforts, the family "*though it never tasted luxury or any surplus, had not to suffer from scarcity*". There was no question of grinding poverty. As to educational backwardness, the authorized biography says "*the literacy (in the village) was high, but was confined to reading, writing and arithmetic needed for business….. It* (the village) *had about it an atmosphere of small business, money-lending and small farming…..and the religious atmosphere was devotional Vaishnavism…*".

The hardship Father had to undergo concerned educational priorities beyond the primary level and the initial opposition of his own father. After a formal "English" education up to the matriculation standard given to his eldest son failed to augment the family income, Grandfather did not much take to the idea of Father pursuing higher studies. Assisting in the grocery shop was the duty of all the sons and Father was no exception. This was so despite his brilliance and the high recommendations of his teachers. It was my grandmother and my eldest uncle who encouraged him and provided him with assistance to pursue his studies. So his father's opposition was the first hurdle he had to cross and could have contributed to his abrasive exterior.

Middle-school education was arranged at Simulia, about seven or eight kilometers away from Seoratoli, Father's home. The marathon walking feat belonged to this period. Later on it seemed such a wastage of time that his eldest brother arranged for him to stay in the house of a local doctor. Board and lodging were free and Father was grateful to no end. In later life he tried to repay this debt by setting up an extended family in his own household. He believed that to be the ideal pattern, and in fact such extended families were very common in Bengal and elsewhere in those days. Our Allahabad household I remember had three wings. The nuclear family, the relatives who needed to be looked after while they were studying, and looking for jobs or actually working. And of course, the research students whose company was so invigourating that Mother had to look after all of us – poor soul! But she was uncomplaining and father could be very generous.

Caste discrimination was another kind of hardship that father had to endure in early life and this could have contributed to his abrasiveness. This happened when he left home and came to study in high school. We do not belong to the three upper castes in Bengal, the Brahmins, the Vaidyas and the Kayasthas. Father belonged to the Vaishya community, traders and businessmen, who during the Pala era had ruled the roost financially. Brahmin disposition during Ballal Sen's time[3] downgraded them socially. But they continued to thrive both in their professions and business in Dacca city and elsewhere in the whole of East Bengal. It was this rich community which came forward to help Father financially when he was expelled from the government school. But father's own village was a far cry from these affluent centres. Despite their affluence, socially the community was discriminated against, as I am told. We didn't hear it from Father though, that during Saraswati Puja, observed in every Hindu school, Father was once acting as a priest, chanting hymns and offering prayers. Suddenly a group of upper caste boys came up and admonished him because they said he had no right to worship in that manner. Only Brahmins were supposed to do that. That must have rankled! At Eden Hindu Hostel, attached to

3 Ballal Sen was the best known ruler of the Sena dynasty of Bengal during the 12th century

Presidency College, meals were served separately for Brahmin and non-Brahmin boys. Later Father and some of his friends moved out and stayed in a mess. Among the Brahmins there is the 'sacred thread" or "upanayan" ceremony; it is initiation into Brahminhood. All are invited to it. Father made it a point never to accept invitations to such ceremonies. He was impatient with rituals which made false claims to social superiority.

Disappointment and discrimination had followed him in later life, in academic and political life. In an article on Henry Norris Russell (The *Scientific American*, 1989) De Vorken, curator of Smithsonian Institute suggests that Russell was indebted to Father for a major breakthrough in Astrophysics. But when Father had desperately wanted funds to join the team setting up a new observatory in Mount Wilson, to secure experimental verification of some of his observations, it was denied him. The letters between Russell and Father, which were later found in the Princeton University archives, make sad reading. This thing had happened earlier.

Encounters with political leaders and subservient bureaucrats were none too happy. The National Planning Committee, as far as I know, was his brain-child, in which he managed to get national leaders like Jawaharlal Nehru (plate 19) and Subhas Chandra Bose (plate 14) interested in pre-independent India. But after Independence, it was ensured that he had no useful role to play in the planning and organizing of science and industry.

Father was given a raw deal, and letters written to political leaders and close friends, express his bitterness and anguish. No wonder he had become temperamental and irritable. We did see a lot of this side of Father and we kept as safe a distance as possible. But when there were personal encounters, I have always found him willing to come down and accept the other point of view, once he was convinced it was right. And then he would appear so happy. To him nothing mattered as much as a happy family, with children, grandchildren, relatives and friends swarming around him.

Stray incidents come to my mind while I am on this topic of a happy family. Father loved entertaining and the house was always swarming with guests. There were the foreign scientists visiting during or after the Science Congress. That used to be a big affair in those days. In my memory remains Sir Arthur Eddington (plate 13) presiding over the Science Congress held in Allahabad in 1937, when mother availed of the services of the chef appointed temporarily to learn to prepare western cuisine. Father's teacher and mentor Acharya Prafulla Chandra Ray (plate 5 & 7) would often come and stay for a week or so at our place when we shifted to Kolkata. Father would take him out for drives around what is now Rabindra Sarovar. My second sister Krishna would read out the newspaper to him and I would ask for autographs for myself and my friends. The best thing was that the entire menu changed with Ma cooking light and delicious meals for Father's

guru and we the children were always looking forward to it. Whenever he came, the Acharya would bring loads of cosmetics made by Bengal Chemical Limited for my elder sister Usha. She reciprocated by baking cakes for him which my parents took with them when they visited him at his residence in the University College of Science. He had a room to himself there. The Acharya was a Gandhiite and had sharp arguments with Father who was an advocate of heavy industries. This had happened at Allahabad and I had heard about the incident. At the end of verbal skirmish, Father's teacher asked him to hurry and fetch the car because otherwise he would be late for the train to Kolkata. Father with a straight face asked him whether he wanted a bullock cart to reach the station.

A memorable affair was the dinner hosted for Pandit Jawaharlal Nehru in 1945, after the Congress leaders were released from jail. I happened to garland him. Word had gone round that Nehru would be coming and the whole locality had assembled to catch a glimpse of the renowned leader. We could hardly control them. These were the days of the INA[4] trial after the war had ended. Father had invited the leading intellectuals of Kolkata to this dinner, including Sri Prashanta Mahalanobis and Debendra Mohan Bose, to meet Nehru. Perhaps Mahalanobis was introduced to Nehru during this dinner. Other personalities I remember were A.V. Hill very proudly displaying to us the medal awarded to him by the Royal Asiatic Society. The year was perhaps 1944. Then there was Harlow Shapley (plate 12 & 16) arriving before time for dinner. My mother and the children entertained him till Father would finally take over. After some time we found him bringing out a small glass case and he smilingly told us that his hobby was to collect bugs in the perforated glass case. The verandah adjoining Father's room where we were sitting, badly needed repair. But Professor Shapley was thrilled with the cracks and crevices from where he started collecting bugs to his heart's satisfaction. Mother was not too pleased. – "***Oh God! What is the Sahib[5] doing?***"

Other scientists who had come to dinner or visited Father at some point of time were Professors Blackett (plate 21), Needham, Dirac, Linus Pauling and the children's chief involvement was in collecting autographs.

Engagements and marriage ceremonies in the family offered amusing reactions in Father occasionally. My eldest sister Usha was a student of Physics and was about to get married in the traditional way. The whole process had to start with a "***Surya Pranam***", worshipping the sun, early in the morning, facing east. Suddenly the student of Physics, and at that stage still an atheist, revolted. Father placated her. "See this part of the ceremony is not illogical. I am doing ***Surya Pranam***. ***Surya*** after all is the life-giver." Later I

4 Indian National Army or INA was an armed force under Subhas Chandra Bose that fought against the British Raj to drive them out of their occupation of India

5 White skinned people working in India were often referred to at that time and sometimes even today as "Sahibs"

wondered whether this was a compromise or not, or whether when it came to the subject of the sun, since he had worked on it academically, now he was acting symbolically.

Funnier incidents happened during my engagement and marriage. Supriya and I were in no hurry to get married since Supriya was on a research fellowship and I did not have steady jobs. But Father was in a hurry. For him, it was ***Subhasya Shighram*** (quick action for good deeds). He called Supriya to him and tried to persuade him – "See, when you marry, your wife will always be an inspiration for you. That had worked in my case." So we had to get married at the exceptional young age of twenty-two, exceptionally for Supriya at least.

A few days later Supriya fell ill with severe bronchitis. Father paid him a visit with a book in hand – M.S. Krishnan's ***Introduction to Geology of India***. He discussed with Supriya's mother, his health problems. When he was about to leave, smilingly he presented the book he was carrying to Supriya and said, "Read this, get well soon and then write a review of the book for me." One can only imagine Supriya's reaction to this. The review was later published in "Science and Culture".

Supriya and I were married according to Brahmo rites. I agreed because Supriya was the only child and I was very close to his parents. Father was quite agreeable though he had to actively participate in the wedding rites. He had to act as the ***Kanya Karta***, the 'father of the bride'. In our Hindu weddings, the *sampradan*, or 'giving away of the bride' is done by the eldest male member of the extended family. So I was actually the only one among the four sisters who had Father beside me at my wedding. Even my wedding preparations occasioned a witty sally on my in-laws to be, who were very devout Brahmos. "You people have reduced the thirty three crores of deities of the Hindu pantheon to one. I have eliminated that 'one' also." He declared his atheism in no uncertain terms, perhaps hinting that he was making a compromise for his daughter's sake.

The figure who stands out in the memory of the younger children early in 1940 was that of Subhas Chandra Bose visiting Father in our rented home in Kolkata at 27H Manoharpukur Road. Subhas Chandra Bose (plate 14) was yet to become Netaji. I had heard that together with Rabindranath Tagore, Father had persuaded him to stand for re-election in 1939 as Congress President at Tripuri. He had won, but against the stiff resistance of Gandhiji and his followers he resigned in April 1939. War was declared later that year and perhaps future strategy was discussed in the closeting of the political leader and the politically inclined scientist. Early in 1940, we heard from Father that the great leader would arrive in our house to meet Father, and we were to make ourselves scarce. Naturally we stationed ourselves under the staircase where we would have a full view of the illustrious visitor but he would not see us. He came and strode up the staircase, tall, straight, clad in snow white Gandhi cap, Khaddar dhoti-Punjabi and chaddar and entered Father's

study and stayed there for more than one hour. No letters exchanged by these two stalwart persons have survived. I am told that a whole bunch had been destroyed. But my younger brother Prasenjit had come across important information contained in the typed manuscript of a speech by Father entitled "Netaji – the Man of Action" (1954).

I will end my memoirs with this stipulation that we are looking at my Father and other stalwarts of his time across the barriers of time. A proper perspective is required, a historical perspective, to assess their achievements. Values are changing along with changes in the socio-economic and political spheres. We and the coming generations are being swallowed by a society ruled by warped corporate and consumerist ideas. Can we expect the new generation to take lessons from history and endeavours of the leaders of the new Renaissance? Gandhiji, Nehru, Netaji in the political field differing on the means to free India politically but steady on the goal; Rabindranath, Premchand and others in literature preaching a new humanism; Ramanujam, Jagadis Bose, C.V. Raman, K.S. Krishnan, P.C. Roy, Jagadis Bose, Jnan Ghosh, Jnan Mukherjee, Satyen Bose, Meghnad Saha in the field of science, and those who helped Asutosh Mookherjee to organise science teaching generally in the country to bring about a new Renaissance in science in colonial India. Teaching of science was much against the desires of the colonial masters. The grants for setting up the science departments came not from the government but from private funds. The colonial masters would rather have Indians confined to oriental studies following the lead given by Max Müller. But men of the New Renaissance like Rammohan Roy or later Asutosh Mookherjee and Mahendra Lal Sircar thought otherwise. Questions have been raised about scientists overemphasising the role of heavy industry, technology, atomic energy etc. Heavy industry had polluted environment, technology had played havoc with traditional social norms, and atomic energy had led to the bomb. Was not the Gandhian model better, the model of a self-sufficient village or small town? The answer is, at that point of time, the time of subservience to the British ruler, all these were needed. Had Father lived longer, perhaps he would have remoulded his ideas. At least he had shown his awareness of the evil effects of atomic energy by joining the Peace movement in the early fifties. He had presided over the World Peace Congress, held in Helsinki, Finland, in the early fifties. This was a movement that looked beyond national frontiers.

I thank INSA for deciding to publish this volume for the 125th birth anniversary of my father, who also happened to be the founder of the National Institute of Sciences that was later renamed as Indian National Science Academy.

3.

A COLLECTION OF PICTURES

Plate 1a: *Wife, Mrs. Radha Rani Saha, Calcutta, 1954*

Plate 1b: *Megh Nad Saha*

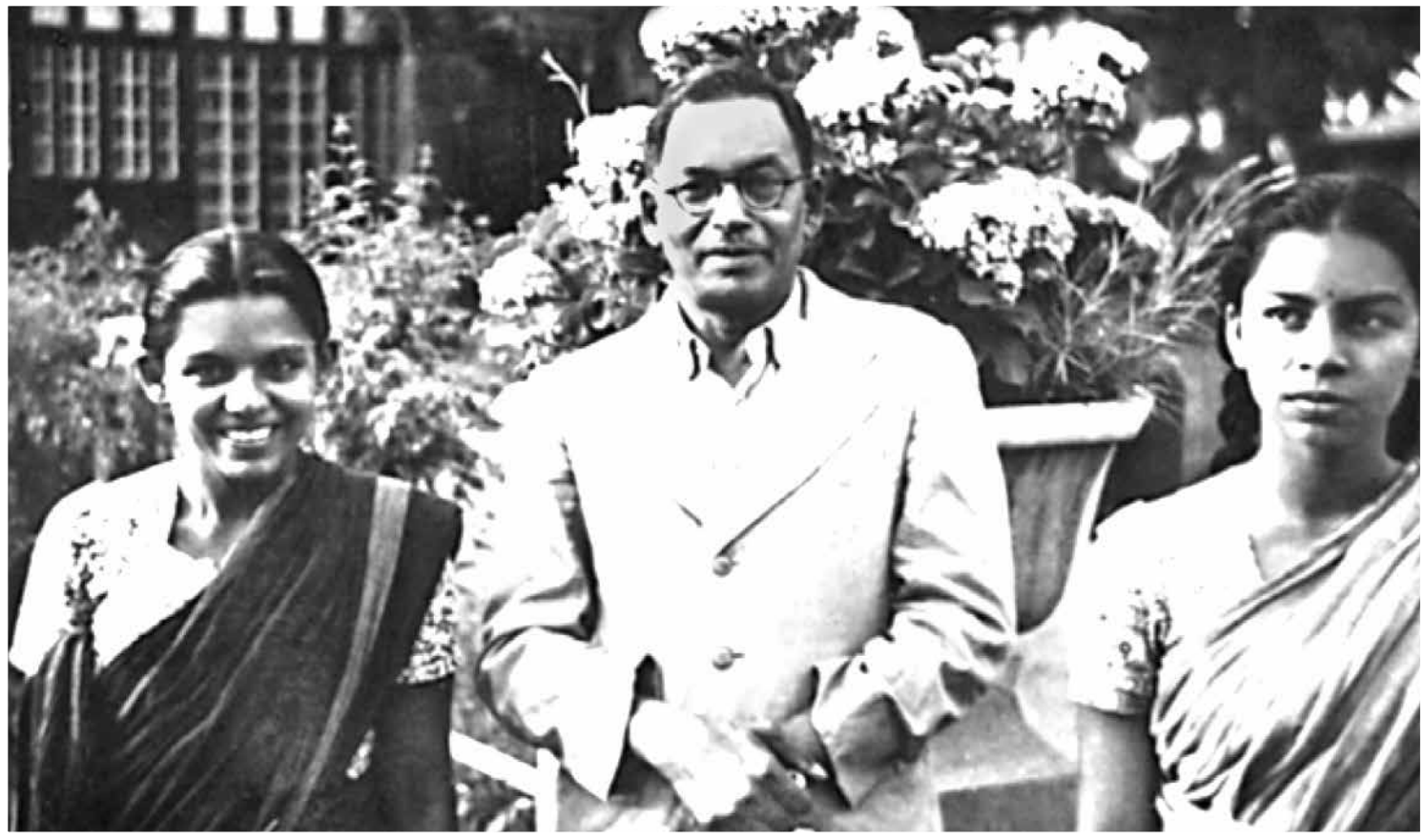


Plate 2: *With daughters, Krishna Das (left) and Chitra Saha (right), who contributed the article "Life with Father".*

Plate 3: *House of Shri Jagannath Saha in village Sheoratala, P.O. Bahadi, Dist. Dacca, East Bengal where Prof. M. N. Saha was born on October 6, 1895.*
(Reconstructed house as it stands when the photograph was taken during the early 1970s)

Plate 4: *Meghnad Saha's residential house in Kolkata (now a Heritage Building of the Kolkata Corporation)*

Plate 5: *Photograph taken in 1916 at Krishnanagar, West Bengal*

From left to right:

Back row : Meghnad Saha, ? , Jnan Chandra Ghosh

Centre row : Raj Kumar Das, Prafulla Chandra Ray, D. N. Sen

Front row : Hemanta Sarkar, Profulla Sarkar, ?

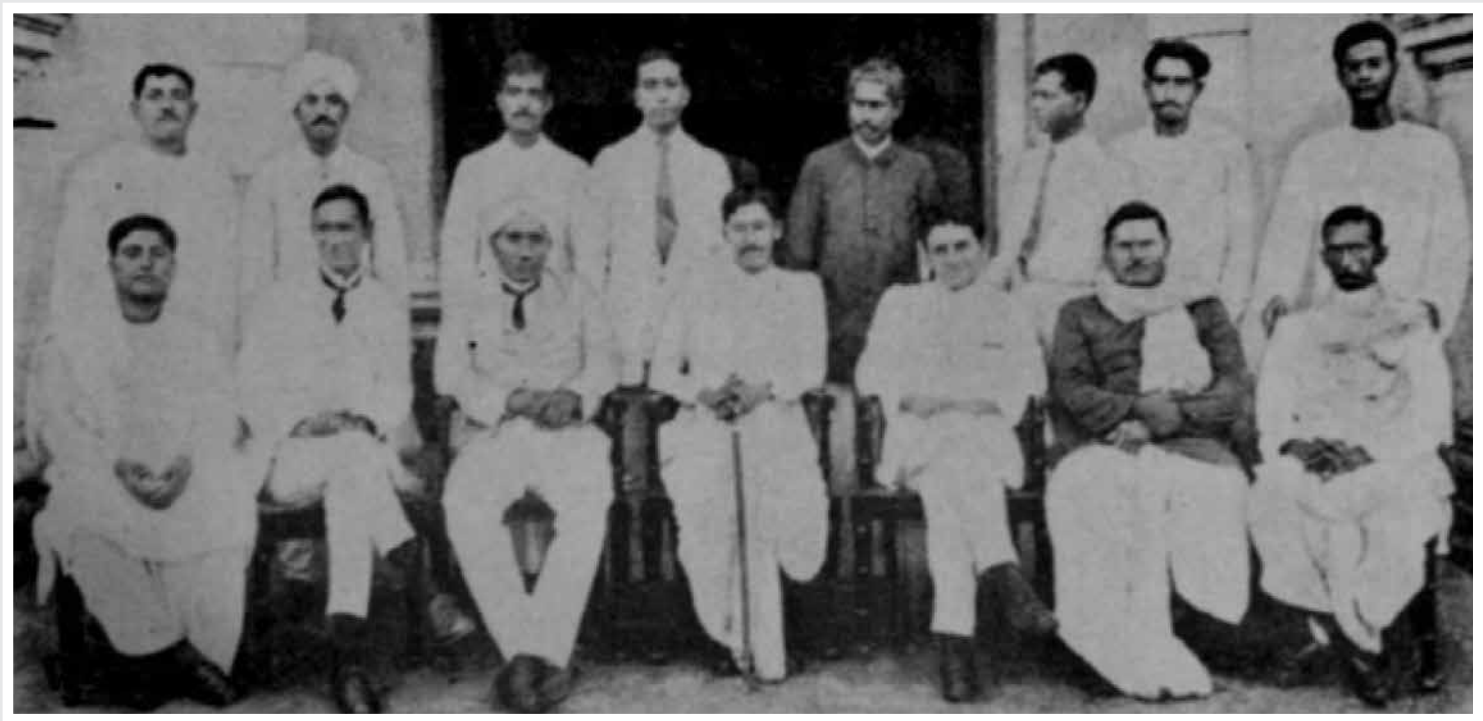


Plate 6: *Meghnad Saha's farewell before departure to UK in 1920.*

To his right is C.V. Raman and further right at the end is S.N. Bose** (*see Section 7.5; **Section 5.4, 7 & 8); behind Saha is Sisir Mitra. All were faculty members of the Physics Department of the Calcutta University and recruited by the Vice-Chancellor, Sir Asutosh Mookherjee at very early stages of their careers.*

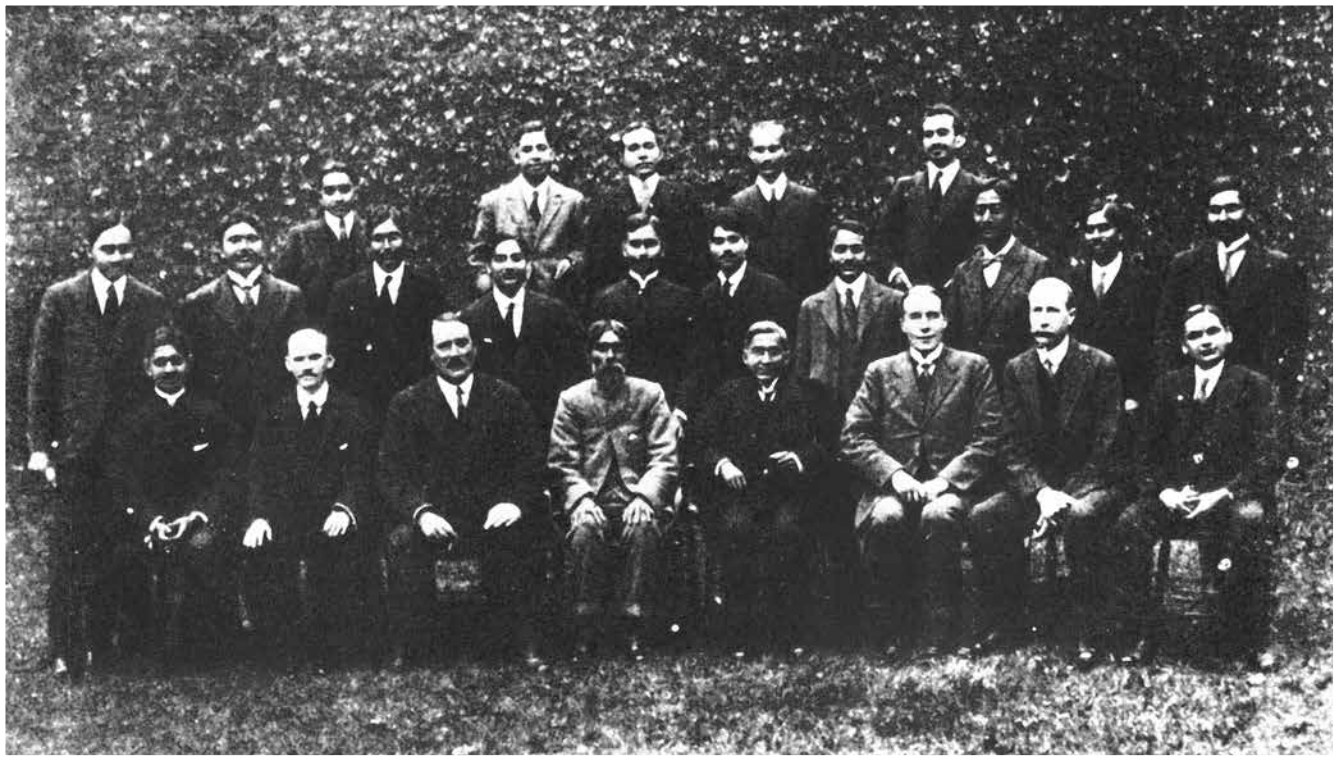


Plate 7: *Indian Research Scientists in London (1920-21)*
(From: S. N. Sen: Meghnad Saha: His Life, Work and Philosophy, 1954)

From the left:
Back row : Snehamoy Dutta, –, Mr. R. Chowdhury, Mr. K. C. Panday, –*
Middle row : Mr. K. Bagchi, Mr. H. P. Chowdhury, Dr. Meghnad Saha, Dr. Shanti SwarupBhatnagar, –, Dr. P. C. Sarbadhikari, –, Mr. S. N. Dhar
Front row : Dr. Jnanendra Nath Mukherjee, Prof. Alfred Fowler, Prof. Thorpe, Sir P. C. Roy**, Dr. Debendra Nath Mallick, Prof. F. G. Donnan, Dr. Blackman, Mr. K. G. Naik*
*(*see Section 8; **see Section 7)*

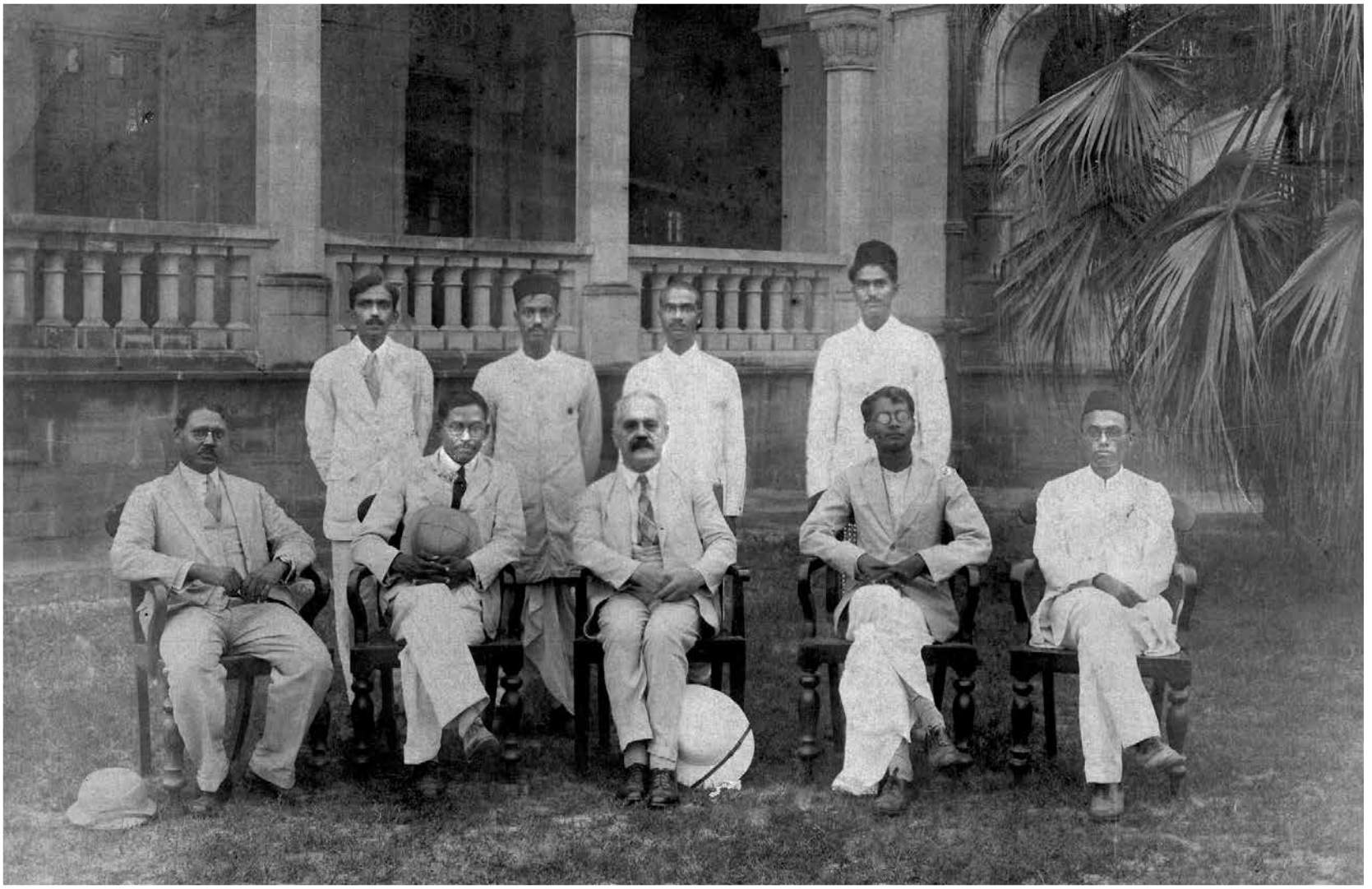


Plate 8: *A picture from Arnold Sommerfeld's visit to Allahabad in 1929.*

Sommerfeld (see Sections 4.2, 6.6 & 8) is seated at the centre, with Saha on his right, followed by Jnan Ghosh (see Sections 5.3 and 8)

Plate 9: *M. N. Saha with his mentor, Sir J. C. Bose (sitting: centre) and contemporaries.*

Sitting (from left) : Meghnad Saha, Acharya Jagadis Chandra Bose, Jnan Chandra Ghosh***
*Standing (from left) : Snehamoy Dutta, Satyendra Nath Bose, Debendra Mohan Bose, Nikhil Ranjan Sen, Jnanendra Nath Mukherjee, Nagendra Chandra Nag (*See Section *7.3 and **8)*

Plate 10: *Physics Faculty, University of Allahabad (1932)*

Standing, centre: B. N. Singh; Sitting: Meghnad Saha, A. C. Banerjee, D. S. Kothari

Plate 11: *International Conference on Nuclear Physics, Copenhagen, Denmark, June 1936*

Sitting, Front Row, Left to Right : *Prof. W. Pauli, Prof. Jordan, Prof. W. Heisenberg, Prof. Max Born, Prof. Lisé Meitner, Prof. Otto Stern, Prof. J. Franck*

Sitting, Second Row, Left to Right : *–, Prof. M. Oliphant, Prof. M. N. Saha, –, –, Prof. R. Oppenheimer*

Sitting, Fifth Row : *Dr H. J. Bhabha (behind Prof. Oppenheimer)*

Standing, First from Left : *Prof. Neils Bohr*

(See Section 8 about Max Born, Bhabha and Niels Bohr)

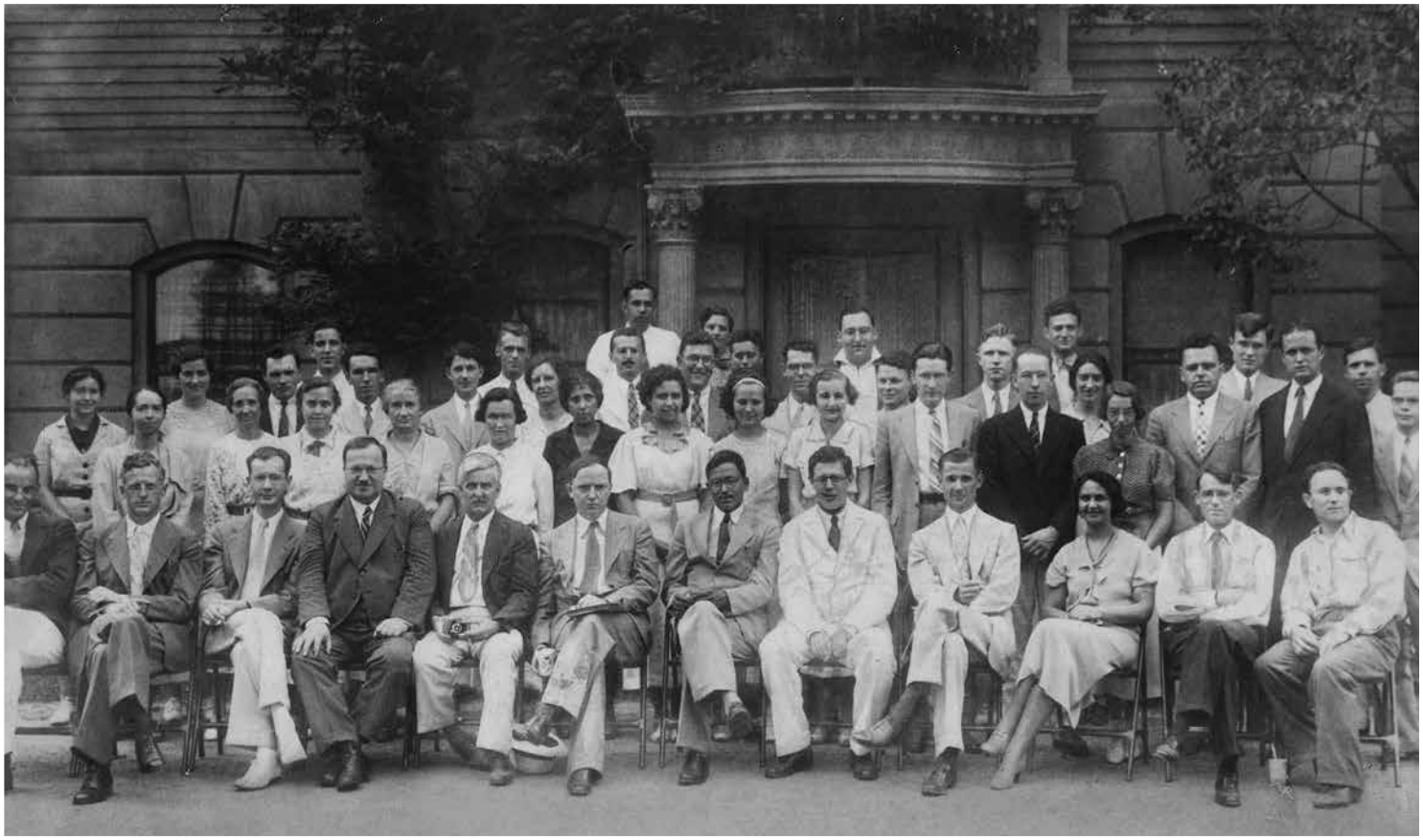


Plate 12: *Meghnad Saha (sitting, centre) in a group photograph taken at the Harvard College Observatory, 1936.*

Sitting from left: –, –, Dr. Menzel, Dr. Lundmark, Dr. Campbell, Dr. Harlow Shapley, Dr. M. N. Saha, –, –, Dr. Cecilia Payne-Gaposhkin***
*(see Section *8 and **Preface)*

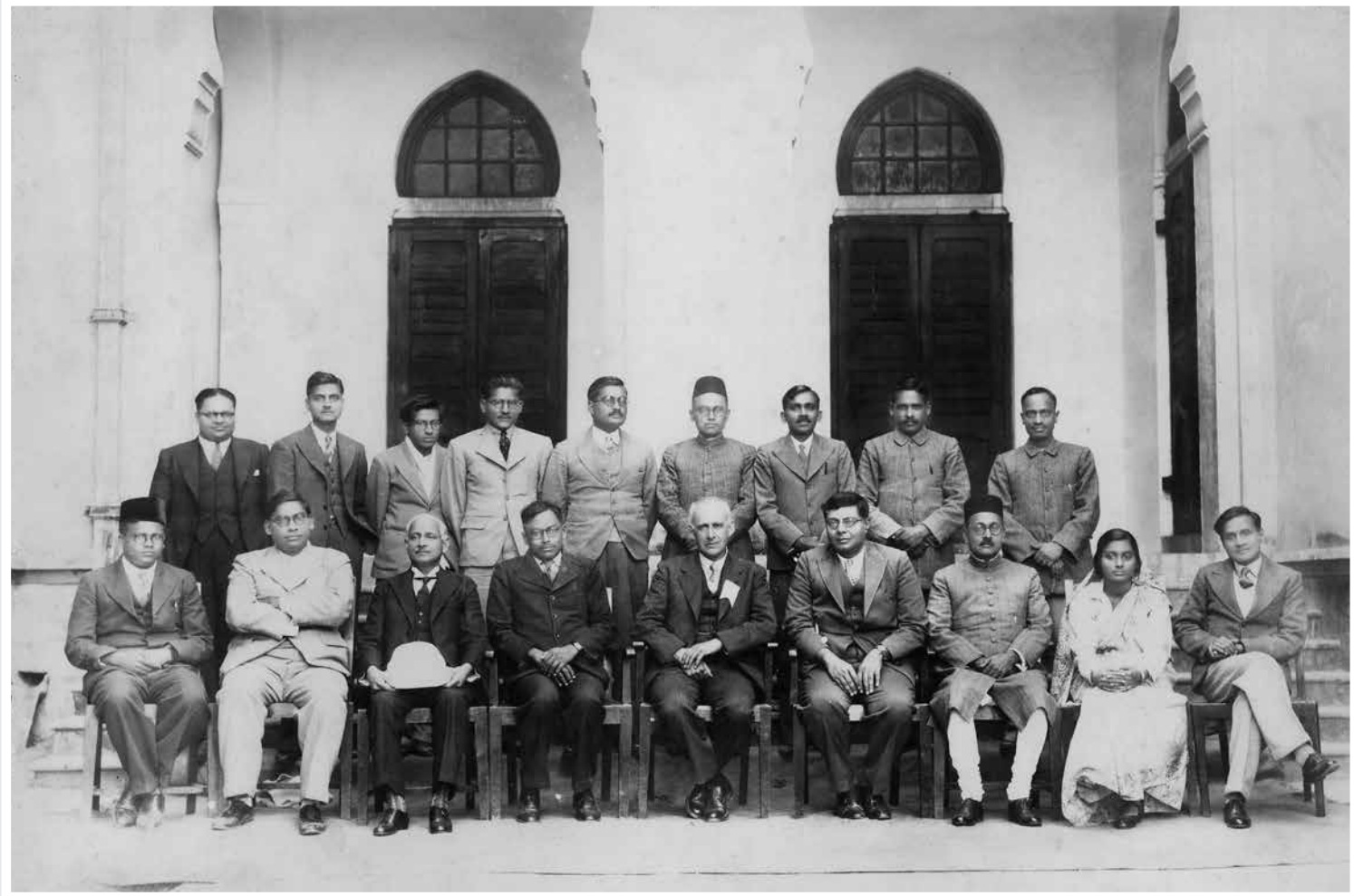


Plate 13: *Arthur Eddington's visit to the Allahabad University, 1934*

From left:

Standing : A. N. Chandrikaprasad, B. D. Nagchaudhury, K. B. Mathur, B. N. Srivastava, P. K. Kichlu, R. N. Rai, G. R. Toshniwal.

Sitting : N. R. Sen, Sir M. Suleiman, Prof. M. N. Saha, Sir Arthur Eddington, Prof. A. C. Banerjee, Prof. Tarachand, Mrs. Bibha Majumder, R. C. Majumder (*see Section 8)*

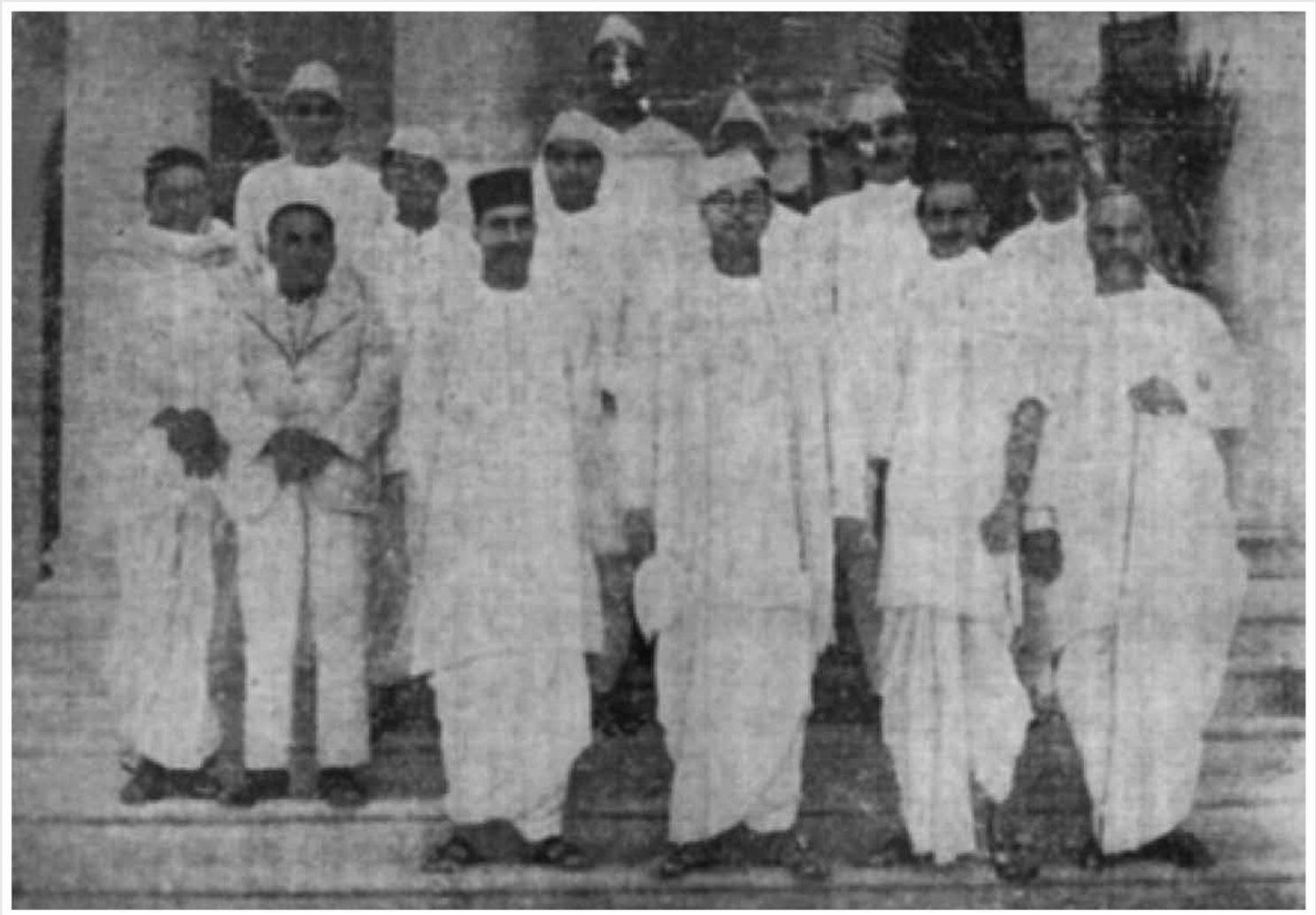


Plate 14: *Conference of Industries Ministers of the States (where the Indian National Congress came to power as a result of the General Election of 1937), held in October, 1938, at Delhi.*

Prof. Meghnad Saha (standing, left) was invited to this meeting by the then Congress President, Subhas Chandra Bose (centre). G.D. Birla is at Bose's left; P.B. Kripalini and V.V. Giri at his right. Reproduced from a newspaper.

PASADENA STAR-NEWS, January 4, 1945

Plate 15: *Indian Scientists at Caltech*

Left to right: Sir Shanti Swaroop Bhatnagar, director, Scientific and Industrial Research, Government of India; Prof. J. N. Mukherjee, professor of chemistry, University College of Science, Calcutta; Dr. Nazir Ahmed, director, Indian Central Cotton Committee; Dr. Robert A. Millikan, chairman of the executive council, California Institute of Technology; Sir Jnan Chandra Ghosh, director, Indian Institute of Science, Bangalore; Prof. S. K. Mitra, professor physics, Calcutta University; Prof. Megh Nad Saha, professor of physics, Calcutta University. Standing at the back of Dr. Millikan is Frank S. Coan, representing the State Department. (*see Section 8)*

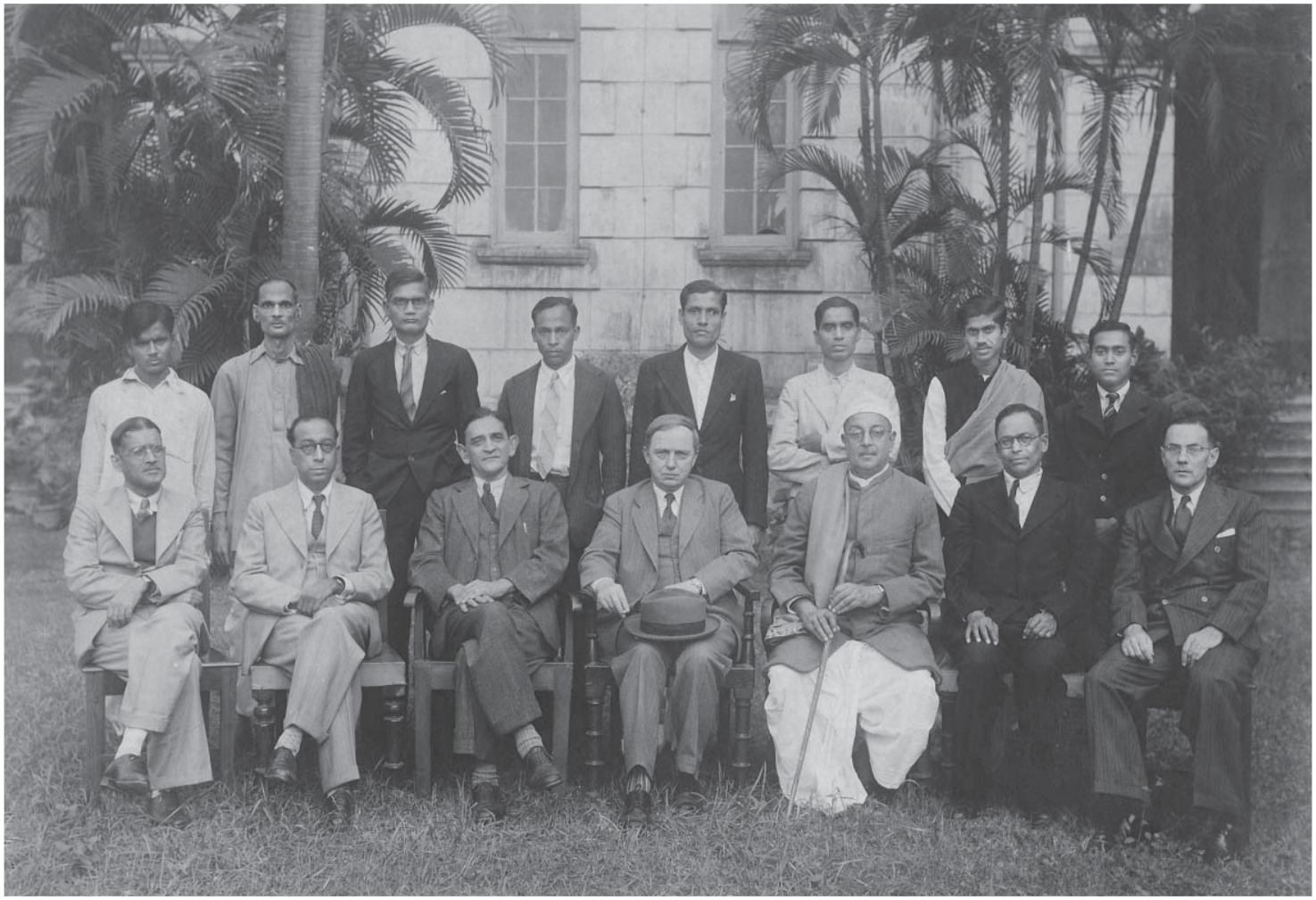


Plate 16: *January, 1947, Prof. Harlow Shapley, Director, Harvard College Observatory, Cambridge, Massachusetts, U.S.A., with the personnel, office-bearers and patrons of the Indian Science News Association at the campus of the University College of Science & Technology, 92, Upper Circular Road, Calcutta.*

Sitting (left to right) : *Prof. P. Ray, Prof. S. K. Mitra, Dr. D. M. Bose, Prof. Harlow Shapley, Dr. P. C. Mitter, Prof. M. N. Saha & Dr. W. D. West, Director, Geological Survey of India, Calcutta*

Standing (left to right) : *Murai, Mr. P. Biswas, Mr. A. K. Ghosh, Mr. H. N. Saha, Mr. K. S. Chakraborty, Mr. R. L. Dutt, Mr. K. Bagchi, Mr. S. N. Sen*

Plate 17: *University Commission at Bangabasi College, Calcutta, 14th January, 1949*

Left to Right:
Rai Bahadur K. N. Mitra, Dr. Arthur Morgan (Member), Principal P. K. Bose, Dr. J. F. Duff (Member),
Dr. Zakir Husain (Member), Dr. K. N. Bahl (Member), Vice-Principal S. N. Mitra, Prof. M. N. Saha (Member),
Dr. N. K. Sidhanta (Member), Mr. P. N. Banerjee (Vice-Chancellor, Calcutta University)

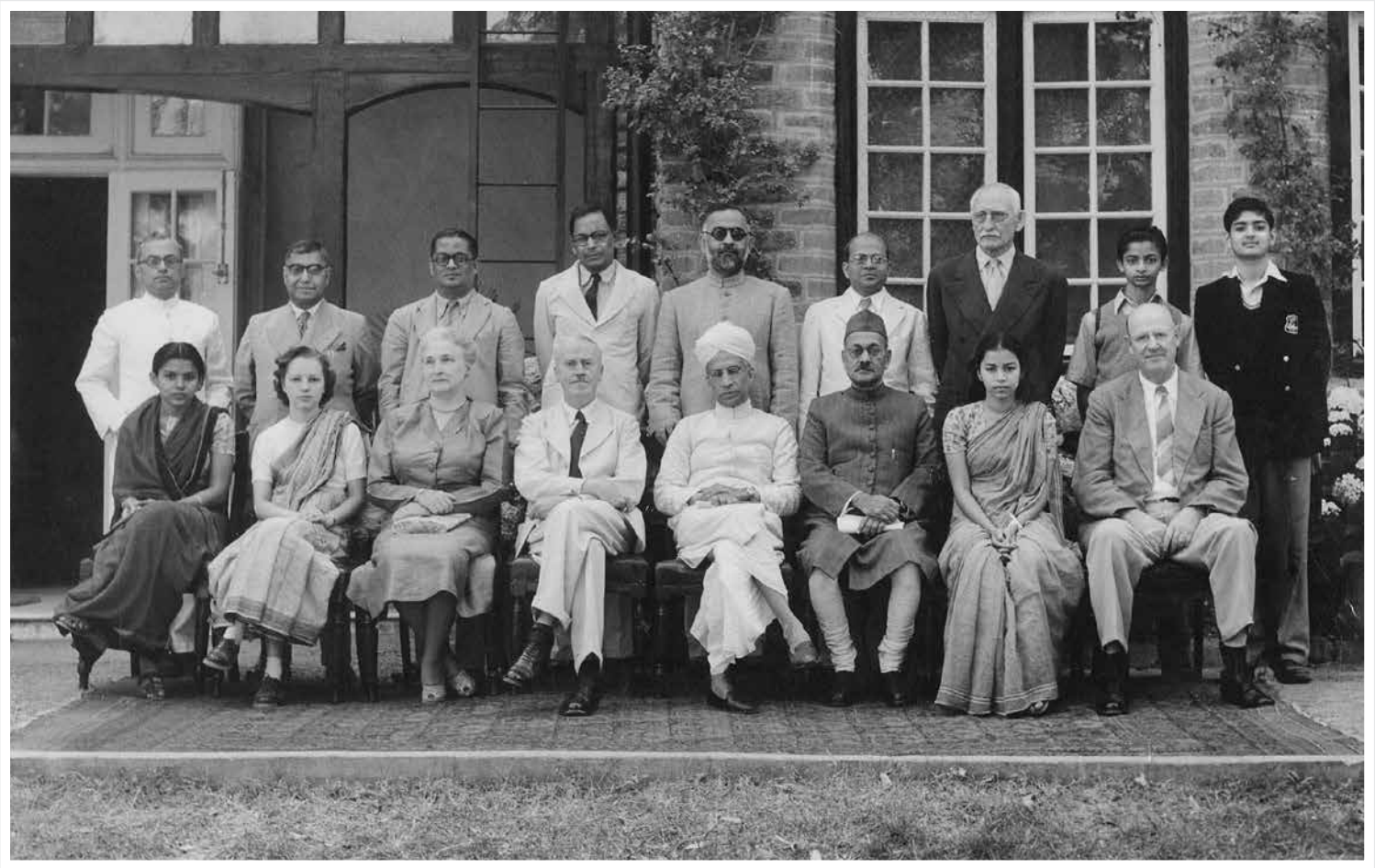


Plate 18: *University Commission: "Snowdown", Simla, May-June, 1949*

Left to Right:
Sitting : Krishna Das[1], –, –, –, Dr. S. Radhakrishnan, Dr. Tarachand, Chitra Saha[1], –
Standing : P. K. Kichlu, –, –, Dr. Meghnad Saha, –, –, –, Prasenjit Saha[2], –, (Megh Nad Saha's [1]daughters and [2]youngest son)

Plate 19: *Opening day (August 26, 1950) of the Central Glass & Ceramic Research Institute, Jadavpur, Kolkata.*

From left : Dr. Bidhan Chandra Roy (Chief Minister, W. Bengal), Justice Charu Chandra Biswas, Pandit Jawaharlal Nehru (India's first Prime Minister), Prof. Meghnad Saha, Dr. Atmaram (Director: CGCRI – Central Glass and Ceramic Research Institute)

Plate 20: *Felicitation of Prof. Meghnad Saha upon his election to the Indian Parliament in 1952.*

Sitting, from left : Miss Sanghamitra Saha[1], Prof. Saha, Dr. Santimay Chatterjee;
Standing, from right : Ajit Saha[2], -, Prasenjit Saha[2] (Prof. Saha's [1]daughter and [2]sons)

Plate 21: *Prof. P. M. S. Blackett* with Prof. Saha during his visit to Kolkata (probable period 1953-1954).*
*(*see Section 8)*

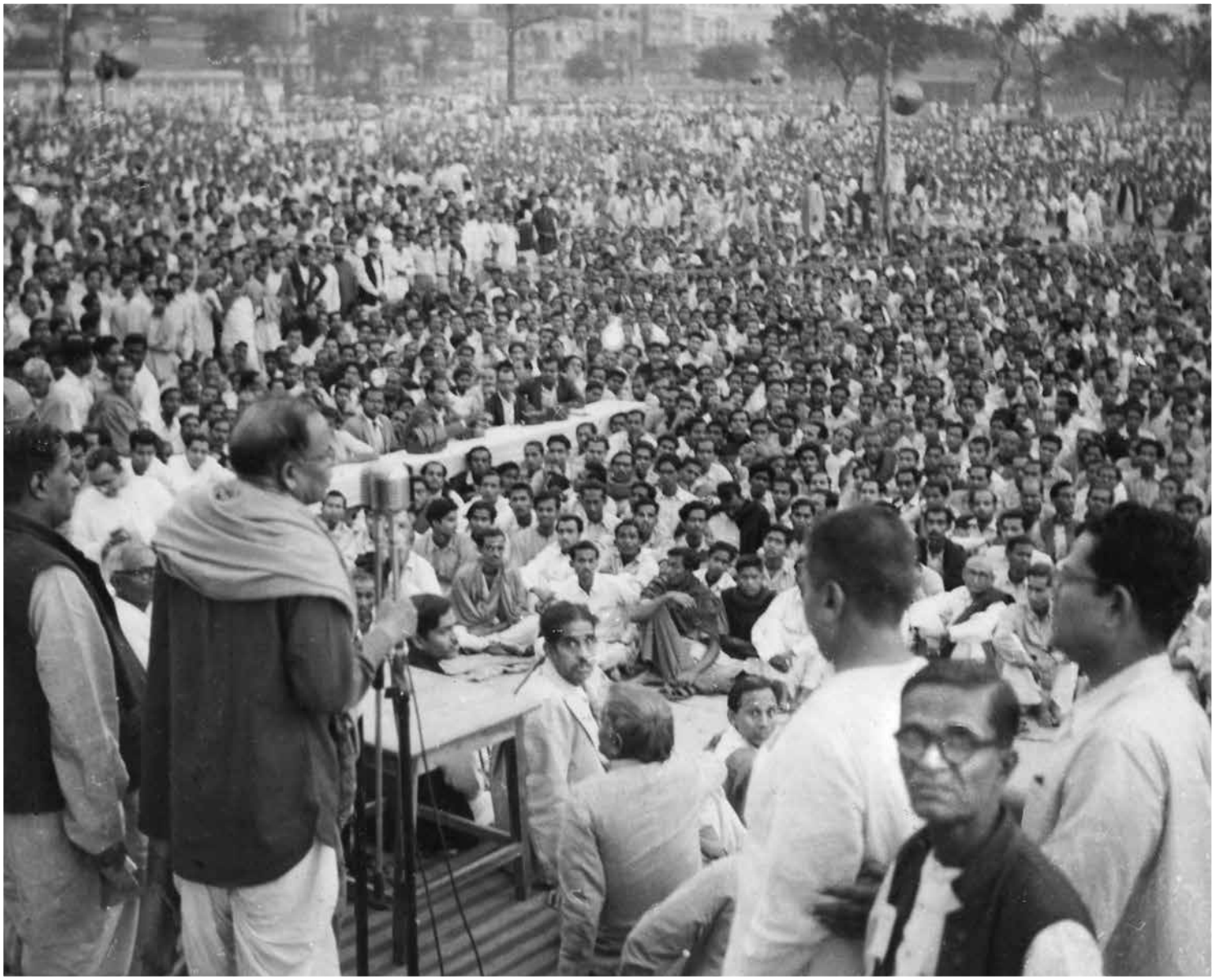
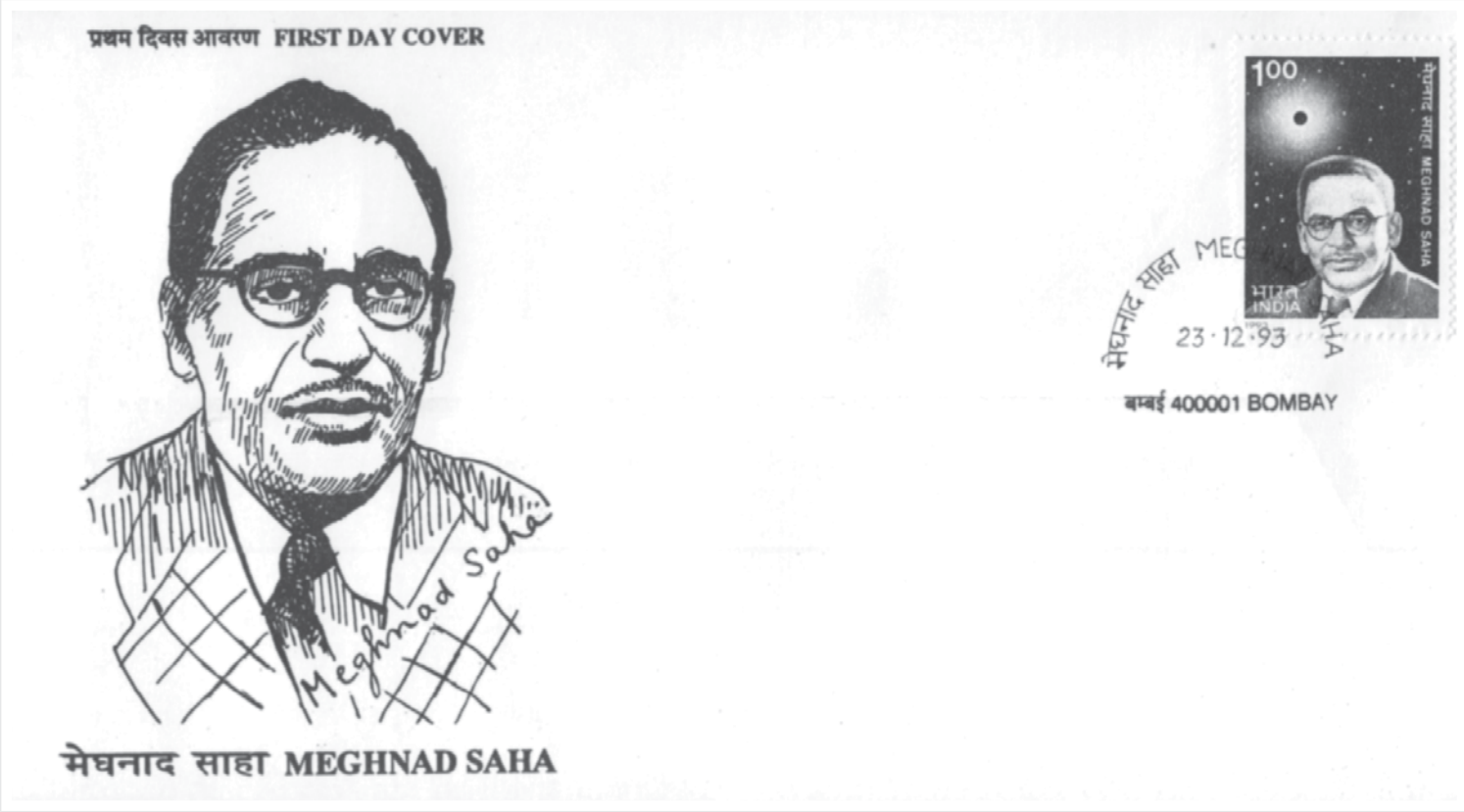


Plate 22: *Prof. Saha addressing a protest gathering against the proposed Bengal-Bihar merger in 1955 at the Kolkata Maidan*

Plate 23: *Prof. Meghnad Saha with Soviet physicist Prof. Pyotr Kapitsa* and space scientist Anna Masevich during his visit to the Soviet Union in 1955*
*(*see Section 7)*

Plate 24: *Prof. Saha, elected member of the Indian Parliament, with India's first President, Dr. Rajendra Prasad (1954)*

Plate 25: *Prof. Saha, founder of the National Academy of Sciences, India, Allahabad, addressing the opening of the Silver Jubilee celebration in Lucknow in 1955.*



The Department of Posts of India released a Postage Stamp and First Day Cover in December 1993 in honour of the great physicist Prof. Meghnad Saha

4.

LETTERS: WESTERN SCIENTISTS

4.1 RUSSELL LETTERS: 1921–1945

4.1a *Between Russell and Saha*

Carnegie Institution of Washington
Mount Wilson Observatory
Pasadena, California

Dr. Megh Nad Saha,
Wallenweber Str. 12, Berlin

August 3, 1921

My dear Dr. Saha:

Dr. Hale has shown me your recent letter to him, and I am very glad to learn of your present address, for I have long wished to write to thank you for the copies of the reprints of your first two papers, and to tell you of the very high opinion which I hold of your work.

I much regretted that you were not in England when I was there last spring for I wanted to meet you. In my opinion, you have made a contribution to astrophysics which is of the highest value and importance and opens great opportunities for future advance.

When I was appointed as a Research Associate of this Observatory I planned at once to do what I could to carry on work on the lines which you have suggested. Some of the results will be seen in the enclosed carbon copies of notes which have been prepared for publication in the Publications of the Astronomical Society of the Pacific, or in the Annual Report of the Mount Wilson Observatory.

You will see that your predictions about the lines of the alkali metals in the sun-spot spectrum

have been completely verified. Rubidium is present. We will investigate caesium as soon as proper photographs of the spot spectrum near λ 8500 can be obtained.

The lines of the alkaline earths also behave in a manner which agrees very remarkably with your theory – except that the spectroscopic evidence indicates that barium is a good deal more easily ionized than sodium. By comparison of the behaviour of the lines of other elements I find data strongly supporting your conclusion that the ionization potentials of Ti, Fe, Co, Ni—Ce lie between 6 and 9 volts. You do not state how you reached this estimate. I should be glad to know. Dr. Hale, Dr. Adams, Dr. St. John, and other members of the staff here are greatly interested in the various applications of the ionization theory.

We had already discussed the desirability of work along almost all the lines suggested in your letter and it is probable that a great deal will be done, either here or at the California Institute of Technology, which is to develop a great physical laboratory, under the direction of Dr. Millikan[6].

I have a paper ready for press which deals with the sun-spot spectrum, and, on my return here next year, or perhaps before, Dr. Adams and I hope to work on stellar spectra, following the path you have laid out.

Meanwhile Dr. King is greatly interested in the laboratory end of the research. Before I close, may I note one or two small points on which I think that your theory may be modified?

First, when several elements are present in a mixture the electrons resulting from their ionization form a "common dissociation product" and the equations for all must be solved together. Let X be the percentage of the atoms of any element which are ionized, and $\bar{x}$ the ratio of the total number of electrons present to the total number of atoms. The law of mass action gives

$\frac{x}{1-x}\frac{\bar{x}}{1+\bar{x}}$ P = K (when K is the ordinary function of the temperature, as given by Nernst) that is

$$K = \frac{U}{const.RT} - \frac{5}{2}\log T - C$$

When an element is ionized in two stages, the equations are

$\frac{x}{1-x+y}\frac{\bar{x}}{1+\bar{x}}$ $P = K_1$ x, y proportions of singly and doubly ionized atoms.

$\frac{y}{x}\frac{\bar{x}}{1+\bar{x}}$ $P = K_2$

If no other element is present $\bar{x}$ = x + 2y.

These equations, which are not given in the enclosed abstract, suffice to prove the propositions there stated.

Secondly, the H and K lines of calcium are strong in stars of Class Mp. I have Dr. Adam's unimpeachable authority for this. They can only be shown by long exposure, because the continuous spectrum in the violet is so faint. Indeed, King has got H and K in emission in the furnace at 1650° and in absorption with the hottest part of the furnace at about 2000°.

6 See plate 15,

Again, the lines present in the O-stars are certainly hydrogen, and not due to He^+. The difference in wave lengths between the two could not fail to be detected in many cases.

I believe that both these results will be found to support the theory of ionization when the effects of the presence of other elements are considered, and when account is taken of the fact that very small traces of an element suffice to give it fundamental lines.

Let me say in closing once more how greatly I value your work. I hope that you will continue to discuss the theory of excitation of radiation in an atom, for there is a great deal more to learn about it. If you happen to have any spare copies of your last two papers "On the Temperature Radiation of Gases" and "A Physical Theory of Stellar Spectra" I should be glad to have one of each. My present address is Princeton University, Princeton, New Jersey. Please let me know yours, and I will send you copies of my papers when they come out.

Wishing you all success in your admirable work, and with kind regards,

I am,

Very sincerely yours,

(Henry Norris Russell)

●●●

Paris, Oct 11, 1921

Permanent Address:-
University College of Science
Calcutta, India

My dear Prof. Russell,

Thanks very much for your letter of the 3rd Aug., and the accompanying papers. It is needless to add that your fine appreciatory remarks on my poor attempt, and as well as the discovery of the lines of Rb in the Sunspot Spectrum has afforded me the highest pleasure.

In your letter, you have supplied me with such a large amount of food for reflection that I must take some time before I can be ready with a full reply. Unfortunately I am returning to India on the 14th inst, and have very little time for doing any serious work. You will therefore excuse me for delaying the answer till I return to India.

I hope before long I shall be favoured with an intimation from you announcing the discovery of the lines of Cs.

Again, thanking you heartily for your interest in my work, and with kind regards,

I am, Very Sincerely Yours
M. N. Saha.

Paris, Oct 11, 1921
Permanent Address:
University College of Science
Calcutta, India

My dear Prof. Russell,

Thanks very much for your letter of the 3rd Aug., and the accompanying papers. It is needless to add that your fine appreciatory remarks on my poor attempt, as well as the discovery of the lines of Rb in the Sunspot spectrum has afforded me the highest pleasure.

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I hope before long I shall be favoured with an intimation from you announcing the discovery of the lines of Cs.

Again, thanking you heartily for your interest in my work, and with kind regards,

I am, very sincerely yours

M.N. Saha

●●●

University College of Science
Department of Physics
92, Upper Circular Rd., Calcutta, India

December 7th, 1921

My dear Dr. Russell,

I arrived in India about a fortnight ago. I hope you received the small note which I addressed to you just before leaving Europe.

I am sorry to say that reprints of my papers on "The Temperature Radiation of Gases, and on a Physical Theory of Stellar Spectra" which I shipped from Hamburg along with my other books have not yet arrived at Calcutta. I shall send them to you as soon as I get them.

Re: your remarks about the excitation of the H and K-lines of Ca^+. I am not quite sure if you have noticed my remarks on pp. 482 of "Ionization in the Solar Chromosphere" where I have mentioned King's work on Furnace spectrum. The data about the variation of intensity of these lines inside the M-class of stars are rather meagre and often contradictory but I think I am on sure grounds in saying that there is a general decline

as we proceed downwards. In a recent paper in the Astrophysical Journal, Merrill states that they are entirely absent from _____ which is a star of the N-class. However, I feel that more systematic works ought to be done on this point.

Re: the ionization of Barium, I quite agree that it is more easily ionizable than Na. But I am not quite sure if the principal line of Barium is quite absent from the solar spectrum as you state. Mitchell gives a line at λ = 5535 Å.U, reaching a height of 400 kms. in the chromosphere and identifies it with the emission-line of Barium. You state that it is absent from the spot-spectrum as well. I shall be glad to know if this point has been critically tested, also whether the lines of Ba^+, Sr^+ and Ca^+[7] show any weakening the spot-spectrum.

In the theory as it now stands, the ionization potential has been regarded as the prime–factor in deterring the degree of ionization, but this cannot be quite correct. For example Sr has a higher ionization potential than Na and therefore Sr should be less ionized than Na and the principal line of Sr ought to be quite comparable in strength to the corresponding line of Na. But in fact in the solar spectrum the Sr-line. λ = 4504 (the principal line of Sr) is very faint while, the corresponding Na-lines (D_1 and D_2) are extraordinarily strong. This is not due to a paucity of Sr-atoms in the sun for the Sr^+ fundamental lines (215, 1077) are quite strong in the solar spectrum.

This shows that the degree of ionization of an atom in any stellar spectra depends on certain other factors besides the ionization-potential. But what these factors may be, it is very difficult to say. In my theory I had to regard that each element existed by itself, uninfluenced by the presence of other elements. This is of course a very rough assumption but when scores of elements are simultaneously present I do not know what other steps can be taken to improve matters. Regarding your suggestion in the equation of the reaction – isochore

$$log \frac{x}{1+x} \cdot \frac{\bar{x}}{1+\bar{x}} P = K, K = -\frac{U}{2.3RT} - \frac{5}{2} \log T + C$$

where x fraction of any atom ionized, $\bar{x}$ = ratio of the total number of electrons to the total number of atoms present.

I am not quite sure if you have worked out the deductions of this assumption completely. If we regard the sun as a whole uncharged[8], (and this to apply to every part of the sun) as appears quite natural – then the total number of electrons is equal to the total number of positively charged atoms of all different kinds. Hence $\bar{x}$ is the average ionization in any part of the sun, the average being made over all the elements present. Thus $\bar{x}$ should be a constant for all elements and varying only with height from 1/2 at the end of the chromosphere ($\bar{x}$ = 1), to a small fraction at

7 Ba^+ λ = 4554, 4904 Sr^+ λ = 4044, 4215 Ca^+ λ = 3934 (H), 3968 (K)

8 I do not quite exclude the idea of space charges i.e. charges of one sign accumulating at particular points. But this cannot be a steady phenomena for owing to mutual repulsion, the changes would soon disperse.

the photospheric level. It is worth seeing if such a modified equation will improve matters.

You do not state any reason in support of this assumption or of the statements (1) and (2) in your note on 'Ionization in Solar and Stellar atmospheres'. I shall be glad to know if there is any.

For the last one year, I have not done much for improving the theory. I had to travel much and the spare time I spent in doing an experimental work at Berlin, with Prof. Nernst. This related to another aspect of my theory, whether gasses can be made to conduct electricity under the action of heat alone? J.J. Thompson and McLennan had tried the same problem before but they worked with mercury and other difficultly ionizable substances, and got negative results. I worked with Cs, Rb, and K, and got results quite in accordance with my theory. I wished to test the Nernst equation quantitatively, but unfortunately the experimental difficulties were so great that I had to be satisfied only with a qualitative proof.

Re: the ionization of Fe, Ti, Co, Ni.....what I stated were more or less guess-work based on the behaviour of the lines of these elements in the arc and the spark, and in stellar spectra. The lines of these elements have not yet been classified in series, but recently Mr. Catalan working at the Laboratory of Prof. Fowler, has succeeded in reducing the spectra of manganese – which, in complexity, is not far behind that of Fe. He shows that the strong triplet, 4030.76, 4033.07, 4034.48, which occur strongly in the solar-spectra are the principal lines of Mn, and the lS-term is 59939, from which the ionization–potential comes out to be 7.38 volts. These lines reach in the chromosphere the level of 750 kms[9], and in the stellar spectra, have the following intensities.

	A	F5	F8	G	K	Mb[10]
(4030	1	2	5	5	5	8

The behavior of the element is quite in accordance with my theory. Catalan finds that the line 4344.4, which is the characteristic line of Lockyer's Protromanganese, is not included in the series – spectra of Mn and is due (to) Mn^{+}. But he has not been able to assign the exact position of this line in the spectral frame of Mn.

The principal lines of Fe are either the triplet

4383.720	}	or 4045.973	}	or 3720.08	}
4404.927		4063.759		3737.28	
4415.293		4071.908		3745.86	

or all of them (In the case of Mn Catalan finds 3 sets of principal series each consisting of Triplets[11].

9 Mn-lines

	Intensity		Height
	Photosphere	Chromosphere	
4030.76	9	12	750
4033.07	8	9	720
4034.48	6	8	750

10 Du Gramont – Sur la repartition des raies ultimes dans les spectres stellaire

11 The IS-term is single; there are e sets of 2p-terms each consisting of a triplet. This is somewhat similar to the constitution of Neon-spectrum (Paschen, Ann. d. Phys. Vol. 60)

They disappear from above the B8-class

	B8	A0	F5	F8	G0	K	M
4383.72	1	2	4	5	11	10	13
4045. 98	1	2	4	4	10	13	7

which shows that the ionization-potential of Fe is between 7.5 and 8.5 volts. A direct experimental confirmation would be greatly welcome, but I think that the experimental difficulties would be insuperable. Mr. Catalan told me he would next try to find out the morphology of the Fe-atom from spectral data.

The ionization potentials of Ni, Co and Ti were derived from similar data. It is a great pity that the spectra of metals of the third group La, In, Yt are so ill-known. The contrast between the Fraunhofer spectra and the chromospheric spectra of these elements in quite marked and thus they represent quite a rich filed for investigation.

I am very glad to hear that the Mount Wilson Astrophysicists are taking much interest in my work. I am looking forward with great expectations to the results of their works, and I shall consider myself sufficiently rewarded if I can be of any service to them.

In concluding this letter, let me once more repeat my thanks to you for your appreciatory remarks on my poor attempt.

With kindest regards,

Yours very sincerely,

Megh Nad Saha

●●●

January 17, 1922

My dear Dr. Saha:

I was very glad to get your letter of December with its very interesting contents. I received copies of the reprints of your paper on the Temperature Radiation of Gases and On a Physical Theory of Stellar Spectra, some time ago and am very greatly obliged to you for them. I take pleasure in sending you, under separate cover a number of reprints of papers of mine, dealing with stellar and spectroscopic topic, which, I hope, you will accept with my compliments.

To take up the specific points mentioned in your Letter, my evidence for believing that the H and K lines were strong in the spectra of the Mb Stars was a very explicit statement to that effect from Dr. Adam who is unquestionably in a position to know. These stars give so little violet light that it takes a long exposure to get H and K but with sufficient exposure they appear and are very intense. When I get to Mr. Wilson, I will ask Merrill about the H stars.

As for the ionization of barium and strontium compared with that of sodium I think that the clue

may be found in considerations which Professor Compton and I have presented to the meeting of the American Astronomical Society last December. I enclose a copy of an abstract which will explain my points. My conclusion regarding the absence of Ba 5535 from the spectra of Sun and spots was based on a very careful examination on the original negative on which the Mt. Wilson sun spot map is based where again I had the great advantage of Dr. Adams' criticism. Unless Schultz's measurements seriously wrong this line is not present in the sun. The fundamental enhanced lines of Ca, Sr and Ba are not weakened in the spots. The enhanced lines of subordinates series are inaccessible for Ca. weakened for Sr and practically unaffected for Ba.

You will find these points rather fully discussed in my paper which is to appear in the Astrophysical Journal, and of which I enclose the only proof which I have. My theoretical conclusions concerning the amount of ionization when several elements are presentation agree with those which Milne published independently in the Observatory for September, 1921 and I think that they are correct.

Your results upon conduction of electricity in the vapor in the alkali methods are extremely interesting and very valuable, I Hope that they will very soon be published.

What you say about Mr. Catalan on manganese is of very great interest as are also your suggestions about iron spectrum. I hope to discuss these with my friends at Mt. Wilson. I believe that King is working on the spectrum of yttrium, neodymium and zirconium. I quite agree with you that the metals of this group are very important, spectroscopically. I am leaving next week for Mt. vision, where I hope to do my principal work upon the extension of your theory in further details to stellar spectrum, in collaboration with Dr. Adams and Dr. King.

With most hearty good wishes for the further success of your very important work, believe me.

Very sincerely yours,

(Henry Norris Russell)

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My dear Dr. Russell,

I received your interesting letter of the 17th Jan. and reprint of your paper in the Astrophysical Journal, but took some time in realising the full significance of its contents. In the meantime, I find that the paper has appeared in the Astrophysical Journals. My best thanks to you for the interest which you have created for my work.

I have been quite convinced of the correctness and importance of your extension of the theory to the case where several gaseous mixtures are present. My first impression was that the extension was of an "ad hoc" nature, but I find that I was mistaken, for it is based upon the solid basis of the classical law of "mass-action". To take up the other specific points in your letter, viz. why the alkaline earths as a rule are more ionised than their

I.P. would indicate, I believe I have just got an explanation. The consideration which do not claim finality, are enclosed in the accompanying leaflets. You will see, that some empirical assumptions had to be introduced, but we cannot do otherwise till we have a more intimate knowledge of the spectra of Sc, Ti, Mn and Fe – when we shall come to clearer understanding of the value of the "Steric factor".

The institute I am working at is rather poor in literature on Astrophysics for which I have to work under great difficulties. I am thinking of asking the authorities for a collection of up to-date literature and stellar maps. I shall feel much obliged if you can kindly recommend to me some publishing or order supplying book-firm in the U.S.A. who will be able to supply me these. I am particularly in need of Rowland's Table for Solar wave lengths, Rowland's map of solar spectra, the Mount Wilson maps of spot spectra, spectra of typical stars, and lanterns and slides useful for lecture purpose.

Looking forward to the pleasure of hearing more from you on your very important work.

Very sincerely yours,

(M.N. Saha)

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My dear Prof. Saha:

The very interesting papers which you sent me arrived a few days ago. Let me congratulate you most heartily upon this admirable extension of your work. Your introduction of the "steric factor" certainly constitutes an important advance in the theory of ionization. If the effective ionization potential of the metals diminishes with rising temperature it will be easier to account for the fact that hydrogen exhibits its lines in stars for which practically all the enhanced metallic lines have disappeared.

I am not so certain, however, that the astrophysical evidence indicates that manganese is more highly ionized in the AO stars than calcium. I interpret the intensity of the lines as indicating roughly the concentration of neutral atoms in the star's atmosphere. If an element is present in small proportions its lines may become faint although the percentage of its atoms which remain neutral is still fairly considerable. From equation 6 of my paper (*Ap. J. 55. 121, 1922*) it appears that at the temperature of 10,000º and with a difference of ip of 1.23 volts, the percentage of atom of manganese which remain neutral should be between four and five times as large as that for calcium — and on the simple theory which neglects the steric factor. Now calcium is a very much more abundant element than manganese in the earth's crust, according to Clarke and Washington. If the relative proportions are the same in a star's atmosphere the number of neutral calcium atoms present at 10,000 should be about xxx[12] times

12 The number is not clear in the letter

greater than that of neutral manganese atoms, in spite of a higher degree of ionization and this effect will persist at higher temperature. A line of a relatively abundant element may, therefore, persist higher up the spectral sequence than those of a relatively rare element of higher ionization potential.

Of course, we cannot be sure that the relative proportions of the elements on earth is similar to the star, but I think it is nevertheless, the best guide that we have.

Your tribute to Sir Norman Lockyer at the end of your paper is justly deserved. His insight was extraordinary and it is a very great pity that certain superficial personal qualities hindered the acceptance of his theories.

I enclose herewith your manuscript as I do not know whether you have a duplicate copy. I should be obliged for a reprint when it is published. I hope to send you reprints of my latest papers when I receive them for distribution.

It gave us great pleasure at Rome to add your name to the membership of the committee on spectral classification of the International Astronomical Union. We hope that your cooperation will be of much value in the further work of this committee.
With best regards,

Very sincerely yours,

(Henry Norris Russell)

●●●

September, 7th, 1922

My dear Prof. Russell,

I was glad to receive your letter of the 23rd June. It was very nice of you to add my name to the Committee of Stellar classification of the International Astronomical Union, and I do not know how to thank you for this friendly service, and for the interest in the ionization theory which you have been instrumental in creating among the astronomers of the U.S.A. and Europe. The investigations of Dr. St. John on the Spectra of solar faculae and of Dr. A.S. King on the Electric Furnace experiments involving ionization phenomena, which I had the pleasure of reading sometime ago in the pages of the Astrophysical Journal, already show that we are on the right track, though much work still remains to be done. I was glad to find that your ideas regarding the effect of mixtures of elements having different ionization potential on spectroscopic phenomena have been brilliantly confirmed by Dr. King.

As regards the points raised in your paper, I have entirely to depend upon the observation of the previous workers. It appears to me that any conclusion regarding the concentration of an element in the stellar atmospheres from the

intensity of the lines alone will be lacking in quantitative precision. This is due to the following reasons: Firstly, we have as yet no quantitative theory of thermal radiation of gaseous mass; secondly, no meaning except a qualitative inference could be attached to the intensities of lines as given by different observers. To make my points clear, I shall take the liberty of referring to some of my unpublished ideas. Let us suppose that there is a background of continuous light (emitting like a black body) with a column of absorbing and radiating gas before it. The photospheric light will be absorbed in its passage through the column of gas. The transmitted light is made up of what remains of the photospheric light after absorption plus the emission of the gaseous column. Let 'f' be the ratio of this transmitted light to the original emission from the photosphere. 'f' may be styled as the Fraunhofer absorption. Then it can be shown that

$$f = (1 - e^{-pt})\left(1 - \frac{I'}{PI}\right), or \left[1 - e^{-\frac{P}{\beta}(1-e^{-\beta t})}\right]\left[1 - \frac{I'}{PI}\right]$$

where

I = emission from the photosphere

I´ = emission from the gaseous column at the wave length

P = absorption coefficient

The first formula refers to the case of finite thickness of homogeneous gas of thickness 't', the second refers to the case when the distribution of gases in the stellar atmosphere follows the law $e^{-\beta h}$, where h = height of the layer.

The case where 't' is very large, so that the first term is sensibly unity may be regarded as "Saturation". This is the case with stronger lines like D_1, D_2 ... H, K etc., but probably not with the lines of the subordinate series. The second term, (1 - I´/PI), depends on the emissive powers I of the photosphere, which can be obtained from the black body law and emissive powers I´, and absorption coefficient 'p' of the gas. This part of the problem awaits solution.

If, the gaseous layer is in thermal equilibrium with the photosphere, the temperature may be taken to be $\frac{T}{\sqrt[4]{2}}$ in accordance with Schwarzschild's theory of radiative equilibrium. It may be supposed, as has been done in some quarters, that $I_o/p = I$ at the same temperature, but I do not believe this to be correct. But experiments can be devised by Dr. King, or those who have similar resources at their disposal, for finding out experimentally the value of I´, p for an element like Na, or Ba, for the principal as well as the subordinate series lines.

If we apply the above considerations to the consideration of stellar spectra, we at once realise the difficulty that estimations of the intensity of stellar absorption lines require a thorough revision. The intensity of lines in the spectrograms of the sun or the stars is to be measured according to the principles of Koch's microspectro-photometer. The Fraunhofer absorption will then be quantitatively obtained from the ratio of the minimum of the curve (say for the K-line of to the normal intensity of the solar spectrum in the neighbourhood of the

K-line. The intensity will be simply the inverse of this ratio.

This is a brief sketch of my ideas on the future lines of investigation of the problems arising from the ionization theory. I have taken the liberty of troubling you with this brief outline, as a detailed discussion will require much time. I hope to send to you at an early date a full sketch of these ideas.

With best regards,

Very sincerely yours

Megh Nad Saha

●●●

October 12, 1922

My Dear Prof. Saha,

Many thanks for your letter of September 7, and for your kind congratulations upon the observational confirmation of some of my ideas.

I quite agree with your statement that at present we cannot obtain any precise idea of the concentration of an element in a stellar atmosphere from the intensity of its lines, and I am very glad to know that you are working upon this subject. My colleague, Prof. Stewart, is also considering these questions and I hope that before long our knowledge will be much advanced. I agree heartily with your suggestion that the intensity of spectral lines should be measured with some instrument like Koch's microspectro-photometer. Any other suggestions that you may care to make about experiment would be very welcome either to Dr. King or to myself. I shall look forward with great pleasure to seeing anything further about the subject that you may wish to send me.

With best regards,

Very sincerely yours,

(Henry Norris Russell)

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March 23, 1923

My Dear Prof. Saha,

Your letter has remained in my hands longer than I realized, and I return your manuscript with apologies for the delay. I have shown the paper to my colleague, prof. Stewart, who is deeply interested in this problem, and the comments which follow come from us both.

One, on page 2, I think that I denotes the emission and not the emissivity. On page 3, your reasoning is based on the assumption that the extinction in the gas is due to true absorption of

light. There must, however, be also a good deal of scattering of light by resonance, a quantum of radiation being absorbed and re-emitted after a very short interval. This would also give Fraunhofer lines, but the law of blackening would be quite different. Instead of giving,

$$E = E_0 \, e^{-\alpha X}$$

in the direct beam, we have

$$E = \frac{E_0}{1+\frac{1}{2}\alpha x}$$

for the direct beam and scattered radiation together.

The relative importance of scattering and true absorption probably depends mainly on the density of gas. I suspect that absorption (that is the conversion of radiant energy into thermal energy in the gas) takes place only by some process involving collisions, and that the same is true of true thermal emission by the gas. When the mean free time of an atom between collisions is very long in comparison with the time involved in the absorption and re-radiation of light by resonance (which latter time is probably of the order 10^{-8} of the second) I should expect scattering to be the predominant factor.

It is only under these conditions that I should expect to find fine sharp absorption lines. Consequently I feel that resonance scattering is probably very important in the production of stellar spectra. This convinces me also that the pressure and density in the reversing layer are very low. Stewart has just given good reasons to thickness of a few km if the pressure is greater than 0.1 atmospheres. You have probably seen his note in "Nature". We are collecting evidence on this matter and it seems pretty clear that the pressure in the reversing layer does not exceed 10^{-4} atmosphere.

I would suggest, therefore, that you take scattering into account before you complete your discussion. Your suggestions about experimental work are very interesting and I hope to discuss some of them with Dr. King at Mount Wilson, where I am going in a few weeks.

With kind regards and best wishes for all your work.

Very sincerely yours,

(Henry Norris Russell)

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University College of Science
92, Upper Circular Road, Calcutta, India

The 1st August, 1923.

My dear Prof. Russell,

Your letter of the 23rd March was duly to hand, but I did not reply because my knowledge about the general effect of scattering was rather meagre, and I thought I would acquaint myself with the literature before I could present the paper for publication. Let me record my best thanks to you for your kindly drawing my attention to the influence of the scattering factor, and for your valuable criticism.

I have seen Prof. Stewart's note in 'Nature', and have been looking forward expectantly for his full paper. You state (and Dr. Plaskett also confirms it in a footnote to his paper on the O-type of Stellar Spectra) that from certain investigations, you have come to the conclusion that the pressure in the Reversing Layer of star is of the order of 10^{-3} Atms. Fowler and Milne have come to the same conclusion from their theory of maximum intensity of subordinate lines, but I object to their treatment because they have taken a divergent series [b(ϑ) in Fowler's paper – ***Fowler Phil. Mag. 18***] as the basis of their operations. Nevertheless they seem to have struck an entirely new, and promising line of thought, but their treatment may lead to valuable results if they can find out a convergent partition function in place of b(ϑ). You will find my arguments stated in a note to the Phil. Mag. Meanwhile, I am looking forward for your paper on pressure in the Reversing Layer of Stars.

As regards the effect of scattering, I think Milne proves rather convincingly that it is quite small as far as the solar atmosphere is concerned. Of course, your idea of "resonance scattering" may alter it. But I have not been able to grasp the distinction between absorption and resonance scattering. Are they not the same phenomenon?

In a recent note communicated to Nature, I have adduced arguments to prove that the continuous radiation from the stellar disc must fall far short of the black body radiation, as in the case of metal filaments. This is probably the reason why an indiscriminate use of black body formulae lead us to such divergent temperatures. They may also explain the phenomena of bright lines in stellar spectra.

Lastly, I would like to make a delicate request. Can you not kindly arrange to send to me a copy of the spectrogram of sunspots in which you detected the rubidium lines?

With kind regards,

Very sincerely yours,
Megh Nad Saha

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November 14, 1923

My dear Prof. Saha:

Your letter of August 1 reached me while I was on vacation, and became mislaid in a package, which I only found today.

As for your questions concerning scattering of lights in stellar atmosphere, I should distinguish between absorption and resonance as follows: In the first case, the energy is converted into heat, that is, into molecular motion in the gas, whereas in the second it remains as potential energy within the atom until it is radiated again. Darwin, with whom I talked in Pasadena, pointed out to me the great importance of "collisions of the second kind" in producing true line absorption. These are collisions between an ordinary atom or electron and an "excited" atom, in which the energy of the latter is released and transformed into kinetic energy. They are the exact reverse of the collisions which excite resonance radiation.

Fowler and Milne have done a beautiful piece of work. I am not so much concerned as you are about the divergence of the series with which they work, because they have advanced good physical arguments to show that only a few term will be of practical importance. But as they themselves point out, their curves indicate that lines should remain visible at temperature far above those corresponding to the maximum intensity, and this does not actually happen. The theory evidently needs improvement.

Last spring at Mount Wilson, Adams and Joy got some spectra of faint dwarf stars which confirmed your theory very beautifully. The aluminium lines between H and K are stronger than the latter pair (which nevertheless are heavy lines). The titanium triplet at 5210, 5192, 5173 are stronger than the magnesium triplet 5183 etc. The former are ultimate lines, and the latter belong to a subordinate series, so everything is just as you predicted. I have a graduate student working on line intensities this year. By Dr. Shapley's kindness, he is using the great Harvard collection of spectrograms.

I am writing Dr. Adams, asking him to have a copy of the sun-spot spectrum in the red sent to you. You certainly deserve to have one.

With best wishes,

Very sincerely yours

(Henry Norris Russell)

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University of Allahabad
Department of Physics
Allahabad, India

Prof. H.N. Russell
The Observatory, Princeton University
Princeton, New Jersey

Dated 18th September, 1924

Dear Prof. Russell,

May I Just take a few minutes of your valuable time with a personal affair which is troubling me for sometime.

Ever since my return from Europe, I have been trying to get a research grant which will enable me to undertake experimental work and extend the ionization theory. I approached the Government, as well as some private individuals but I have never been able to obtain any grant. At Calcutta, I had no laboratory whatsoever. At this place I have got a very good laboratory, and very enthusiastic colleagues, but here is no apparatus with which we can start our work. Our annual grant is just sufficient for running the classes and cannot be utilised for research purpose. Still myself and a colleague or mine have spent from our private pocket about £ 100/- to start our work. I shall send you a reprint of the work which is in course of publication but we are poor people just like school teachers all over the world, and it is not possible for us to spend more from our private funds.

My object in writing to you is to ascertain if you could help me in getting a research grant from some charitable American institute – say the Rockefeller Trust, or the Carnegie Trust. About £ 2000/- will suffice for my work I want to buy a good quartz spectrograph, a diffraction grating, a low tension transformer and certain other accessories. If we have the apparatus, we can install them in this laboratory and we would not require much afterwards, for labour is very cheap, and our post graduate students and research scholars would be only too glad to join in the work.

I have seen in "Nature" that the Rockefeller Trust has given Prof. Bohr the sum £800/- for organizing a spectroscopic laboratory. Of course I am not comparing my work with Prof. Bohr's which stands on a much higher level, but probably we could also contribute our humble quota to the advancement of scientific knowledge if we could get the opportunity.

Of course, if is a very delicate matter to approach you with a request like this and I have done this after much hesitation. I would not be surprised or displeased if my venture does not succeed. I have approached you since I know that you would never grudge me this little service if it is in your power. I am also writing an identical letter to Prof. Millikan.

I hope you will kindly send to me from time to time reprints of your papers.

Very sincerely yours,

Megh Nad Saha

•••

October 15, 1924

Professor Megh Nad Saha
University of Allahabad
Allahabad, India

Dear Prof. Saha:

Your letter of September 18 has reached me today and interests me deeply. I am very glad to hear of your better laboratory facilities, and your pleasant situation at Allahabad and your plans for further research are very attractive and deserve every encouragement.

I am not closely in touch with the large foundation which make grants for such purposes and fear that I cannot give you much advice as to how to appropriate them, except to say that the Carnegie Institution appears to me less likely to have such funds available than the Rockefeller foundation. Prof. Millikan is, however thoroughly in touch with these matters and his advice would be very valuable. I shall be glad to support any application which you may make by a letter of commendation detailing my high opinions of your valuable work.

I am awaiting the reprint of present work with great interest. I have been very busy recently on spectroscopic studies, mostly upon the titanium arc and spark and the iron spark, all of which have proved thoroughly amenable to analysis. When these papers are printed it will give me pleasure to send you reprints.

With best wishes for the further progress of your work,

Very sincerely yours

(Henry Norris Russell)

•••

Dated Feb. 10, 1925

My dear Prof. Russell,

Probably it is now too late to thank you for your kind letter of the — but it was (due) to the fact 'that' I was waiting for your paper on the spectra of 'Ti'. You might have noticed a short note in the Nature sometime ago by my colleague Mr. N. K. Sur on the same subject. Immediately on learning that you were also doing the same work, I asked Mr. Sur to stop his work. But we are sorry still not to find your paper in print. We hope that it has already come out.

After some hesitation, I have made up my mind to try my luck with the Rockefeller foundation, and have sent an application. I have named you as one of the referees, and I hope you would not mind the trouble.

You might have noticed a short note in Nature (if it has come out at all) on the effect of radiation on ionisation equilibrium. I think that at last, I have been able to tackle the problem, but the refinements have still to be worked out.

With best regards,

Yours sincerely

Meghnad Saha

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7 Beli Road, Allahabad

23. 10. 33

Dear Professor Russell,

My friend and former pupil Dr. Ramesh Chandra Majumdar who worked at Jena with Vogt on the application of Fermi-Dirac statistics to the interior of stars has written to me that he wants to apply for an International Research Fellowship given by the Rockefeller foundations. He got his doctorate at Jena and is now at Cambridge doing work on the Expanding Universe etc. and attending lectures of Dirac, Eddington, and van der Waerden on the recent theories of matter. Probably you have already received some communications from him, I shall be personally highly obliged if you can kindly help him in any way.

Dr. Majumdar is a gifted young man of high character, and as you are probably aware he has already done very good work in astrophysics. He has acquired a very good proficiency in modern developments, particularly application of tensor and spinor-analysis to problems of matter. The Deutsche Akademie Munich gave him a scholarship which enabled him to stay in Germany for two years. The Calcutta University gave him a further scholarship which enabled him to stay in Europe

for another year. It is now not possible to extend these scholarships. I think that his education will be complete if he is enabled to work for a year more under Bohr and Heisenberg. He will prove to be a fine recruit to astrophysics.

With kind regards,

Yours sincerely,

M.N. Saha

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Professor M.N. Saha
Beli Road, Allahabad, India

February, 25, 1933

Dear Professor Saha,

Thank you for letter about Dr. Majumder. I have just received a letter from him, stating his plans. I have read with much interest some of his excellent papers, and am very glad to add a letter endorsing his application for a fellowship.

I am busy here with many things, but can spare time for spectroscopy. I have just had a very enjoyable time working on the vanadium arc spectrum, with data generously lent me by Neggers. It is nearly cleaned up now, end, of course, is in perfect agreement with Hund's theory.

With best regards,

Very sincerely yours,

(Henry Norris Russell)

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Princeton University
Princeton New Jersey
University Observatory

November 30, 1934

Dear Professor Saha:

I have just received your letter. I take great pleasure in sending you, under separate cover, copies of Miss Moore's compilations, and also of a couple of papers of mine dealing with solar physics, with our best compliments.

I agree with you that the attribution of the coronal lines to neutral oxygen is by no means conclusive. Up to the present there have been no decisive developments in the matter.

With best wishes for all your work.

I am sincerely yours,

(Henry Norris Russell)

●●●

December 23, 1935

Dear Professor Saha:

Permit me to acknowledge with very hearty thanks the copies of your two fine books, which I found awaiting me when I returned here from Mount Wilson a day or two ago. They will be very valuable additions to my library.

In my first glance at them I turned naturally to the sections on spectroscopy, and I would congratulate you on their lucidity and comprehensiveness. The only criticism which I would make is that you have been too modest in refraining from referring to some of your own work by name, when you give full credit to other investigators.

I am greatly interested and pleased to hear that you will make so extended a trip next year as a Carnegie Scholar, and that you hope to visit the United States. I fear, however, that mid-summer is not the best time for such a visit. At Princeton, for example, our Observatory is closed during the long vacation from about June 20 to September 20, and almost all, if not all, the members of the staff are likely to be absent. Much the same situation exists at Yale. At Harvard, Dr. Shapley has now organized a Summer School at which distinguished visitors are present. But you will be going there anyhow in September. Work at Mount Wilson, Yerkes and the Lowell and Lick observatories, is much more nearly continuous. But

I think that you will find few of the physicists in their laboratories at any American university during July and August.

I greatly hope that you will be able to visit us in Princeton during the University Term. If you are not pressed for time on your return to India, it might be well to defer this until about October 1, when the University will be in session. I expect to attend the Harvard Tercentenary and to see you there, if not earlier, and also here in Princeton.

I am much interested in your reference to Dr. Higginbottom, whom I have met more than once in Princeton and greatly admired.

With all good wishes,

Very sincerely yours,

(Henry Norris Russell)

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Academy of Sciences
(United Provinces of Agra and Oudh)
7 Beli Road,
Allahabad, India

17.11.35

Dear Prof. Russell,

I have read with much interest your George-Darwin Lecture published in the Monthly Notices. It was very kind of you to have referred to my early works in such glowing terms.

I have asked, my publisher to send to you my two books "Treatise on Heat, second edition, and Treatise on Modern Physics, vol. I". Perhaps you will be interested in my treatment of spectroscopy in the second book, which gives a detailed description of spectroscopy.

You will probably be interested to learn that I intend visiting the States next year, as a Carnegie Scholar. I intend to start from India about the middle of February, by the overland route and expect to reach England by the middle of the March. I wish to remain in Oxford up to June 15, when I wish to go to the States. I shall try to spend some time with you at Princeton, and then go to Mount Wilson, and spend there till September. Then I have to come back to the Atlantic Coast to take part in the Tri-centenary of the Harvard University on Sept. 16-18. I have been appointed a delegate by our University for this function. After that, I may return to India via England, or via Japan.

I have no personal knowledge of the conditions in the States, but my friend Dr. Sam Higginbottom, and other American friends (Dr. Rice, brother-

in-law to the Comptons) who are in the local American Mission Colleges tell me that this is the best programme possible. I shall be glad to know your opinion. It has been my dream to come into first hand contact with the American investigators particularly astronomers, but, somehow, my hopes have not so far been realized. If necessary, I shall be willing to modify my programme, provided the British Committee of the Carnegie Corporation agrees.

With kind regards,

Yours sincerely

M.N. Saha

P.S. My occupation here as Prof. of Physics hardly leaves me anytime for astrophysics. But for some time past, I have been reading the literature on astrophysics which has accumulated and I have got several problems in hand which I want to discuss with you and other friends during my stay in Europe and America.

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Harvard College Observatory
Cambridge, Massachusetts

July 13, 1935

Dear Professor Russell,

I have been here since July 7, and I am told that you would be coming here towards the end of July. I meet your son-in-law. Mr. Edmondson, who has been very nice towards me. I shall be here up to September 19, when I return to England.

I was doing some work on the upper atmosphere of the Earth. I believe I sent you a reprint of my paper. I am giving some talks here on the subject on July 17th. I wanted some information about the infrared data on the spectra of the Night Sky obtained by Dr. Adams of the Flagstaff observatory, but I have been told that he neither publishes his work (except at long intervals) nor is willing to let others have access to them. It has been suggested that you are the only person who can get something out of them. I would be awefully obliged to have your suggestion on the matter.

Briefly speaking, I believe that I have been able to demonstrate, at any rate to my satisfaction, that the sun's spectrum below λ 3000, is much stronger than that given by a black body at $T = 6000^{o}K$, and probably the resonance lines of H, He, He^{+}, Fe^{+} come out as strong emission line on a continuous background. It would have been nice to be able to discuss the matter with you. At least, I shall know on July 17 what the astrophysicists here think about the work.

With kind regards,

Yours sincerely

M.N. Saha

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4.1b *Between Russell and Other Western Scientists on Saha*[13]

13 References to or discussions of Saha's work have been *italicized*

PRINCETON UNIVERSITY
University Observatory

December 17, 1920

Prof. W.S. Adams
Mt. Wilson Observatory
Pasadena, California

My Dear Adams,

Let me acknowledge with many thanks your letter of Nov. 30th and of two slides showing the spectra of typical giant and dwarf stars of various spectral types ranging from F to K. They are very beautiful reproductions and I studied them with much interest. I shall not be able to use them in actual classification, because we have no spectra here to work upon, but perhaps the very fact that I look upon them with an inexperienced eye may enable me to see some things that are less obvious to the experienced student. I do not mean characteristics of the spectra themselves, but simply the availability of the reproductions for ordinary purposes. It is probably that because of my practical inexperience that I found it very much easier to identify and study the lines in the spectra of Arcturus, which was of perceptible width, than in the other very narrow spectra, which, even though somewhat enlarged, really require to be examined with considerable magnifying power to reveal the finer details. Might it not be worthwhile, in preparing the final set of reproductions of spectra to be published for the use of astronomers generally to exhibit them in two forms, as spectrograms for the worker with the slit spectrograph, and as widened spectra for comparison with objective prism photographs and for the sake of physicists, who are not accustomed to very narrow spectrum?

I remember that Miss Cannon told me once that when one first began to study slit spectrograms (which I believe were sent to Harvard from the Lick Observatory) she found the narrowness of the spectra very confusing, but soon got used to it. For general illustrative purposes, I am inclined to believe that the widened spectra will prove most useful.

I have had a good deal of discussion with Schlesinger recently about the parallax catalogue. I am delighted to know that your spectroscopic parallaxes will be in it and that you hope to assign probable errors to them. I expect to contribute to the same catalogue a lot of hypothetical parallaxes – or as I now prefer to call them,

dynamical parallaxes — of double stars. I have looked over the situation and anticipate that we can get values for about 1500 stars, with a probable error averaging about twenty five or thirty percent the parallaxes themselves. I have already come upon a number of interesting instances of dwarf stars. You may find it interesting to put some of these on your spectroscopic lists.

To change the subject, have you read a paper by Megh Nad Saha, in the last Phil. Mag. On Ionization in the Chromosphere? I found it extremely suggestive and I believe that within a few years we may utilise knowledge of ionizing potentials, and so on, to obtain numerical determinations of stellar temperature from spectroscopic data — or at least of relations between temperature and pressure. Saha's conclusion that low pressure and density are strongly favourable to double ionization and the appearance of enhanced lines is very interesting, in connection with the remark which I remember in a letter of yours years ago, to the effect that the absolute magnitude lines for giant stars are all enhanced lines. At equal temperatures, the low density of the giant stars should directly favour the appearance of such lines. Saha's calculations about the dissociation of hydrogen seem to solve a problem that has always puzzled me, namely, why we do not get the secondary spectrum of hydrogen in astrophysical spectra? He calculates that dissociation should be complete except at considerable pressure (and) temperature below 4000°. At 3000°, however, there should be a good bit of molecular hydrogen if the pressure was high.

Now the familiar hydrogen lines are almost absent in the dwarf M stars, though fairly strong in giants of the same type — which leads me to suggest that careful study of the spectra of dwarf M's, to see whether the secondary hydrogen spectrum can be found, might be well worthwhile. You have the best existing spectra of such stars. Might it not (be) an amusing thing to try?

With best wishes for the Christmas season.

Very sincerely yours,

(Henry Norris Russell)

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Prof. A.S. Eddington
The Observatory,
Cambridge, England

October 14, 1921

My Dear Eddington:

In a conversation that I had with D.L. Webster when visiting Stanford University, we hit upon a mode, which I think is new, of stating the physical meaning of your beautiful equation for radiation pressure in the stars. You will find it discussed in the enclosed note. After you are through with it, I would be greatly obliged if you would send it on to the Secretaries of the R.A.S. for the Monthly Notices.

Incidentally, I see no reason why a string of letters should be put after my name. But, if custom demands this, then Webster should also be labelled as Professor of Physics in Stanford University. I should prefer, however, to have these titles dropped.

I have had a grand time this summer starting out in May and visiting the Grand Canyon and the Yosemite on my way to California, also the Lowell and Lick Observatories. Mrs. Russell was with me on this part of the trip and we had quite a vacation. My two months at Mt. Wilson were even better than I expected. ***I got very much interested in applications of Saha's theory of Ionization and have a paper in press, confirming almost all the predictions and adding some new material.*** The opportunities at Pasadena are superb and will be still greater, when Millikan gets his new laboratory in commission. Hale hopes that a good many distinguished visitors will spend a term or two working at Pasadena, as Lorentz will do next spring. Someday I hope you may be among the number, and I can assure you that you will find it an attractive place. He wants me to come out every year for a couple of months and you may be sure that I will.

Please give my best wishes to all the Cambridge Astronomers and in particular my congratulations to Milne on his admirable presentation of the Ionization theory in the Observatory. With kind regards to your mother and sister,

Very sincerely yours,

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Dr. Walter S. Adams
Mt. Wilson Observatory
Pasadena, California

November 8, 1921

Dear Adams:

Your letter regarding stellar classification came yesterday, and I will get off an answer at once.

I approve of almost all your specific points. Page 3, III; you are right about difficulties with Qd, Qe, etc. Yet, I think that Q1, Q2, etc. violate our principle that we do not use numbers unless we are sure of a sequence. We might be bold enough to suggest Qu, Qv, Qw, Qx, Qy, Qz which would, at least have the advantage of being quite different from any other notation. It may be that we cannot get out a satisfactory preliminary classification of Novae in time, but I feel strongly that such a classification ought to be possible and that it would be very desirable.

Page 5, A; I quite agree with Miss Cannon with respect to the c-characteristic among early B-stars. I think we came near to being guilty of leaving these types out of our classification because we did not know enough about the absolute magnitudes, thereby violating our own principle of basing classification on spectra only.

Page 6, II; I think you are right that it will be best on the whole to treat all double spectra as composite, as you suggest. The other notation has got into hopeless difficulties.

I am much interested to hear of St. John's work on the faculae. *It is really what one should have expected, but Saha got ahead of us, as on several other occasions.* Have you ever tried, by the way, photographing the spectrum of a Sun-spot with moderate dispersion, such as we use on the stars, to get its spectral type? It would illuminate things considerably if we could describe the Sun as a G0 star, with f8 faculae and N2 spots.

I suppose that I shall go to Rome, though I have made no detailed plans as yet, and will have a good deal of hurrying getting everything in. I wish you were going too. I have been realizing of late that I must not rush too hard, but I think that I can get a reasonable amount of work done.

With cordial regards,

Very sincerely yours,

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Carnegie Institution Of Washington
Mount Wilson Observatory
Pasadena, California

Nov. 9, 1921

Dear Professor Russell,

Your very welcome letter arrived in due time and it turns out that I should have answered it at once. However, you wrote as if the manuscript of your paper was about to be sent back to the Observatory and I did not gather that any further consent of mine was necessary for the inclusion of a reference to the experiments made here. When the manuscript came not so very long ago, I noted that no mention was made of these experiments, and thought you had judged it best to wait till more material was available. Mr. Hale, to whom I had spoken of your intention, said he thought it would be well if you included a brief note in your paper that is if you thought it belonged there now. I might have something written by the time its final proof was ready, or if not it can come later. He said this only yesterday, so my writing is late for various causes. I would rather have a good assortment of data available before writing this up, but if it will help your argument I shall be very glad to have you use its material now in any way you wish.

All that I have gotten further along this line was to repeat the experiment with Ca and K using metallic potassium instead of the chloride. The weakening of H and K was the same as before. With strontium and potassium it was the same story. 4078 and 4216 suffered when potassium was introduced. I tried Ca and Mg for the 2800 Mg pair, but the difference in strength of this region and the H and K region is too great for comparison with the 15' spectrograph. This can be done with the 1 meter spectrograph by shielding the H and K part of the film during part of the exposure. Why I have not done this and other possible mixtures is another story. I had to run down some points in ultra-violet and infra-red spectra and while playing with the latter, I observed that the A band at 1600 appeared in absorption with an air path only 12 meters long between the lamp and the photographic plate. The shortest previous observation (visual) was 80 meters of air. So there were a lot of tedious experiments requiring nothing except some photographic techniques to see for how short a path A and especially B would appear. A was finally gotten as close as 7 meters and B at 40 meters. The water-vapor band "a" showed in very dry weather as close as 9.5 meters.

So much for digressions. I have to put in a good deal of time on office work, but can now go ahead with the ionization experiments with an interesting side line. Dr. Noyes wants to study ionization phenomena in tubes, and I having the equipment and experience must do the bulk of the

experiments. This may work in very well with the spectroscopic efforts under the same conditions.

When you write again, I will be glad if you will outline more definitely how you think the I.P.s of titanium and scandium can be gone after. I have not thought much about it as yet, but will be glad to have your suggestions. We are all looking for much help from your cooperation in the future.

Sincerely yours,

Arthur S. King

P.S.: I want to look closer into the matter of lines appearing in absorption before emission. *Saha may be right as to initial appearance*. My experiments show that with a well developed spectrum the emission and absorption for the same thing are very nearly identical, but it may be that much careful.....

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November 16, 1921

Dr Arthur S, King,
Wilson Observatory
Pasadena, California

Dear Dr. King;

Many thanks for your letter of November 9. Let me confess that when I was finally getting off the manuscript of my paper, I forgot that I had been so bold as to ask if I might refer there to your admirable work. I should prefer, however, to have the first notice of such an important thing appear as a communication from you. There are some advantages in an early announcement of so striking a confirmation of theory, but I can see just why you would not want to publish a formal paper on the subject until you had more experimental material. Might it not be a good plan to run in a little note in the A.S.P. timed to come out shortly after the publication of my Ionization paper announcing your results and referring to its confirmation of my theoretical predictions? 1 think that this is now the best thing all round, for there can be no possible doubt of the reality of the phenomenon and, therefore, no reason, that I can see for delaying an announcement of the fact.

I am very glad to hear that you have confirmed the effect with strontium 4078 and 4216. Another element of great interest is barium. ***You remember that I found that barium in the Sun appears to be much highly ionized, than sodium, although on Saha's theory they should be equally ionized***. Experiments in the furnace with barium alone, barium and potassium, and barium and sodium should show whether this peculiar behavior is exhibited also in the laboratory. The critical lines (are) 4554, 4934 as compared with 5536.

The work which you plan to do with magnesium is also very interesting and important. The results which you have recently obtained on the A, a and B atmospheric lines are exceedingly interesting and should have important bearing on the atmosphere of Venus and Mars. I am delighted to hear that you and Dr. Noyes are to cooperate in work on ionization in hot tubes and also to learn that you are planning further and more precise investigation of the question, whether lines appear in absorption before they do in emission? The question is so important theoretically that it is worth the work.

As for the ionization potential of scandium and titanium, I would suggest from the spectroscopic side, a set of experiments similar to those which you have made on calcium and strontium trying successively strontium and potassium, scandium and sodium, calcium, magnesium and zinc with potassium, all the enhanced lines should be greatly weakened which will give us the first real proof in the absence of series relation that such lines are due to Sc^{+}. From the relative (?) of the various other metals, I believe that it should be possible to make a pretty close estimate of the ionization potential. Titanium can be treated similarly and might be the best to begin with as it is so much easier to get, although the furnace will have to be pretty hot to show the enhanced lines. Incidentally, I think that a study of zinc on the general lines of your work on iron, titanium, etc. for the various classes of furnace lines would be desirable.

Here a lecture which I must give five minutes hence calls me away. I hope to write again before long, with cordial good wishes,

Very sincerely yours,

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Prof. Alfred Fowler
Imperial College of Science
London, S.W. 7

January 18, 1922

My dear Fowler:

A letter which I have just received from Dr. Saha contains some exceedingly interesting statements about Mr. Catalan's work on the spectrum of manganese.

I am just going out to Mt. Wilson for my second term of duty there, and I am planning to work with Adams on the further interpretation of stellar spectra on the basis of the ionization theory. It would be exceedingly valuable to us to know about Mr. Catalan's results — the more the better. If you or he would care to communicate them to us in advance of their publication, and also anything which you may have got on iron, or others of the many-lined spectra, it would be also a very great help to us. We will be glad to follow any suggestions of yours with respect to publication, if you want things to see the light first in your

own papers.

I make this somewhat bold request because I had such a lot of fun last summer working out some results of the ionization theory that I am eager to get at as much more of it as I can. The paper which I wrote about it has been somewhat delayed in publication, but is coming out in the next number of the Astrophysical. Some of the results are mentioned in the last number of the A.S.P. You may be interested in particular in the conclusion that the ionization potentials of all the elements between calcium and zinc lie between 6 and 9 volts. I seem to have hit it nearly right in the case of manganese anyhow.

I shall return from Mt. Wilson early in March, pick up my luggage here and sail for the Mediterranean on March 21. Mrs. Russell is going with me and has the most conveniently available boat, — stops at Piraeus. We plan to have a week in Athens or thereabouts and then proceed to Rome, where I shall look forward to meeting you and many other good friends. Afterward we have for a few weeks in Sicily and shall sail home from Palermo. I will admit that the prospect is not unpleasant.

I shall be glad to receive any information which there is to give about the arrangement in Rome, headquarters, hotels, etc. I would suggest that letters for me be addressed to Princeton, where they will be forwarded. Mails are so slow these days, that I am not sure when a letter would have to leave England to catch me at Mt. Wilson.

If you care to send the information about manganese, it might be better to address it to Adams, so that we will have it to work with even if it gets to California.

With most cordial good wishes and very pleasant anticipations of seeing you in Rome.

Very sincerely yours,

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October 20, 1922

Dr. H. H. Plaskett
Dominion Astronomical Observatory
Victoria, B.C., Canada

My dear Harry,

In my first answer to your letter of November 27, I promised further remarks when I got time, which I will make now.

First, concerning the relative abundance of elements, I am inclined to believe that the theoretical principle which you use is approximately correct, namely that the appearance of a line in the spectrum demands that the number of atoms in the

star's atmosphere which are capable of absorbing radiation must exceed a certain definite fraction of those which would be capable of absorbing the whole of the radiation of this sort, which is furnished by the atmosphere (providing that they were all acting at once with full efficiency). Your further conclusion that it should take fewer atoms per square cm to produce a just perceptible line in the spectrum of a cool star than in that of a hot one, also seems reasonable.

But when it comes to determining the numerical value of the constant involved, everything depends on the assumed pressure in the sun's reversing layer. As I pointed out in my last letter, if we assume, for example, a pressure of 0.001 atmospheres, we shall find very a little neutral barium to be present. The constant K will be much diminished, and there will no longer be any difficulty as regards the appearance of lines of such elements as cobalt or nickel.

For such elements as scandium, gallium, and germanium, the difficulty remains. I am inclined here to agree with you that these elements are relatively more abundant in the sun's atmosphere than in the earth's crust, and I think that Stewart and I may have an explanation for this.

We believe that the main agent in holding up the reversing layer is radiation pressure. If then, in the solar atmosphere atoms of one kind are very abundant, only a fraction of these can be held up by the radiation, and the rest will sink down out of sight, while the radiation pressure will tend to bring up from below atoms of the rarest sorts until as many of them have got into the reversing layer as the radiation is able to hold up. This reasoning may require some modification, but I think it indicates that the relative abundance of the elements in the reversing layer should be much more nearly equal than in the denser layers below. This would not apply, by the way, to very heavy atoms — or at least one would expect them to be present in smaller relative proportions. Of course it is well known that the lines of such elements are very faint or absent.

On these principles and with Saha's theory modified to include the ionizing influence of radiation, I think that almost everything can be fairly well explained, except the absence of lithium lines in the sun. I see no way at present to account for this except by assuming that lithium is actually much rarer in the sun (even in the deeper layers) than on the earth. By the way, you might just as well use rubidium as lithium, to get a value for your constant K; it will give a much smaller value.

Taking up some minor points in this section of your letter, let me say first, that I would not have made my remarks about the enhanced lines of sodium if the paper by Pocto(?), Keggers & Mohler, had been published when I wrote. I notice, however, that they state (*Ap. J. 55, 150*) that it requires 14 volts to excite the enhanced lines of sodium, but that a higher voltage is prerequisite to displace an electron from the L-ring, it therefore seems probable that the true resonance potential for these lines is probably high. There is an excellent discussion of the theory in the paper already mentioned.

As for the barium lines, it seems to me that the evidence is fairly conclusive that the enhanced lines are present and strong. I do not see much force in your argument about the duplicity of 4554 and 4934. Why should not these lines be reversed in the sun? They reverse strongly in the laboratory. As for 6496 Burns put the iron line 0.45 Å away, while the solar lines agree almost perfectly with the barium line. As I wrote before I agree that Fowler is right in saying that 5853 and 6496 are not the principal pair of the diffused series, but Fowler gives them as enhanced lines and I have Saunders' authority for classifying 6441 with the other two. The evidence that the system of doublets in barium is due to the singly ionized atom, appears to me to be substantially as strong as in the case of the H and K lines of calcium. I am inclined to fear that you may have been straining things a little when you allowed yourself to doubt the sodium lines. I quite agree with you that the great strength of the D lines and presence of the subordinate series are results of the great abundance of the element. In my original paper I laid stress, not on the absolute intensities of the lines of any element, but on the relative intensities of the ordinary and enhanced lines of the same element – that is really on the relative abundance of the neutral and ionized atoms from this standpoint. I still think that the evidence is conclusive, that Barium is very highly ionized in the sun, but the assumption of low pressure relieves almost all the difficulties.

Now to take up your "minor points"; I am much obliged for the reference to Merton's paper. I doubt, however, whether the phenomena observed in vacuum tubes are strictly comparable with those in stellar atmospheres. I hope to look into this matter further.

As for the classification of O. stars; my remark about a star (with no lines in its spectrum) was of course, speculated. I think, however, that with an exceedingly high surface temperature (100,000°) it might easily happen that all the atoms had lost their easily removable electrons. In this case, although they still absorb "super-enhanced" lines, the resonance lines would lie far in the ultra violet, and lines in the first region would be produced by quantum changes between outer orbits of high ordinal number, which would be improbable. The number of atoms which absorb these radiations might then be so small and the flood of radiation to be absorbed so great that no lines of perceptible intensity might be produced. I think that something of this sort may explain why we get so very few "unknown" lines, even in the hottest stars. Of course, a more fundamental reason is that a few dozen volts in the laboratory give us an excitation surpassing anything that we can well expect to find on the surface of any star. Incidentally, why do you write B.D. 35°3930 instead of 35' 3930? I thought the latter was a standard usage.

I am awaiting with great interest the paper which I hope to see you write on line intensities in stellar spectrum. Measurements here are badly needed.

I hope my other letter reached you in time and persuaded you to cut out all printed reference to my having assisted you in eliminating a mistake,

and that you were able to put in a note about the great influence of the assumed pressure on your constant K. I hope to publish my note on pressures in stellar atmospheres before long, but I am pretty busy now with teaching. All my work comes in this term to leave the second term free so that I can go to the Coast then. I do not know just when I am going west next spring, but I shall certainly try to visit Victoria if I can and have the pleasure of seeing you again. I am returning your manuscript herewith, with very hearty thanks for your letting me have it so long.

With most cordial regards to your wife and daughter, and the same to your father and mother,

Very sincerely yours,

(Henry Norris Russell)

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Leander Mccormick Observatory
University, Virginia

S.A. Mitchell
Director

Professor Henry Norris Russell
Princeton, N.J.

December 2, 1922

My Dear Russell,

I thank you for your letter *regarding Saha and your criticisms*. You and I agree in the matter. Apparently Saha is not very well up on matter of astronomical interest, and being a physicist we could not expect him to be. His value of 7500° for the sun was very surprising.

I was interested in seeing Pannokoek's suggestion in B.A. N. regarding the spectrum of corona. Like you I feel that until we know something of the spectrum of Ca^{++} we can speculate as much as we please.

Unfortunately I shall not be in Boston for the Christmas meeting. I am trying to finish the book on eclipses and the Christmas vacation will see my nose close to the grindstone.

With very best wishes to you, and many thanks, I am,

Very sincerely yours,

(A. Mitchell)

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Dr. A.S. King,
Mt. Wilson Observatory
Pasadena, Cal.

December 20, 1922

Dear Dr King:

Very many thanks for your letter of December 7, with the very interesting photographs of the titanium spectrum. Your results are exceedingly interesting and show how powerful a method of investigation this new scheme puts in our hands.

The lines at 3341 and 3361 are evidently due to the neutral atom. It will be interesting to find what is the physical reason for their behaviour in the spark. Possibly they are fundamental lines like 4227 of calcium which are strong in all sources which contain neutral atoms.

Your suggestion of a classification of enhanced lines similar to that of the arc lines opens up a field of great importance. ***I notice that Saha, in the last Phil. Mag., suggests that lines of doubly ionized titanium should be fairly easy to excite***. These lines should be more strongly suppressed by potassium than the others, but I would hardly expect to find them in the furnace at all. There is a fascinating field here for further research.

Congratulations on your paper on iron in the last Astrophysical. I hope that you will be able to extend your list of low temperature lines towards the red and the ultra-violet by working in absorption. It is among these lines that we must look for the principal series and the larger range of spectrum we can get the better. I see that Kiess is getting at the chromium spectrum and finds that the great triplet about 4200 belongs to a principal series. To find the equivalent for iron would be of extreme interest, the great triplets which disappear at low temperatures must belong to subordinate series.

We will have a great deal to talk about when I get out to Pasadena again which I expect to do about May 1. I appreciate very much your congratulations on the draper Medal.

With best wishes for Christmas and the New Year,

Very sincerely yours,

(Henry Norris Russell)

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Prof. H.G. Gale
Dean of the Graduate School of Science
University of Chicago
Chicago, III.

November 12, 1923

My Dear Gale:

I am glad you sent me the note by Hulburt, which I return herewith. Though it is a little "thin", I think it deserves publications; in fact, I am anxious to see it published, because Stewart and I are just going to send you a much more comprehensive paper on the same subject, not, however, dealing with this particular question in detail, so that it would be inexcusable of me to suppress his note. I am convinced, however, that it is worth publishing anyhow. There are two points which, however, need correction.

First, on page 1, he assumes that the mean electric field is equal to that of an electron at the mean distance. This is wrong, of course. It is the old confusion between the mean of the reciprocal and the reciprocal of the mean. However, this will only amount to a factor of two or three, and is not very important in view of other uncertainties, which Hulburt clearly states (page 2), in a manner which excuses his approximations.

Secondly, in using Saha's equation (3, page 2) Hulburt has fallen into the trap of supposing that all the electrons and ions in the stellar atmosphere come from the ionization of hydrogen, whereas, the vast majority of them come actually from the easily ionized metals. I would suggest that he revise his calculations, working out the "electron pressure" (xP, of equation 2). This will come out very much smaller than the values of table I. The ratio of the total pressure in the atmosphere to the electron pressure can be roughly estimated from the tables in my paper, (*Ap. J. 55, 134, 1922.*) It will not be more than two or three, except for the cooler dwarf stars.

This last correction is of great importance, and I think should certainly be made before the paper is published. The other is relatively insignificant. With this correction I think the paper (is) quite worth publishing.

Congratulations upon your new and distinguished office in the University. I hope that it will not keep you too busy to do a little astronomy now and then.

With cordial good wishes,

Very sincerely yours,

(Henry Norris Russell)

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California Institute of Technology
Pasadena
Norman Bridge Laboratory of Physics

Professor H.N. Russell,
Princeton University, Princeton, N.J.

October 21, 1924

Dear Russell:

I have just received a letter from Saha asking if I can recommend him to the International Education Board or to the Carnegie Foundation for a grant of $10,000 for the equipment of his laboratory. He tells me in the letter that he has addressed an identical letter to you[14].

I am as you know, a very firm believer in this type of grant where it is an assured thing that the personnel and the whole set-up is such as to make it likely that the funds will yield returns. With sufficient care in the looking up of the situation, I think that three or four times as large results per dollar of expenditure can be obtained in this way as in any other way with which I am familiar. But it goes without saying that without great carefulness in choice there is the possibility here of frittering away funds.

It so happens that Professor Raman of Calcutta is on our Faculty for this fall, and since I knew that Saha had occupied a position in his own University of Calcutta for a time I showed him Saha's letter and asked him if he would give me a judgment as to whether such an expenditure of funds would be a wise one if a donor could be found. His reply was that while Saha has done some excellent theoretical work, he is in no sense an experimentalist or an organizer, and funds spent in the way Saha requested would not be likely, he feared, to represent the wisest expenditure which could be found. Indeed, he said that if Saha had inspired confidence in India in his ability to get results through the organization and direction of research he was confident that it would not have been necessary for him to apply in this country.

I have seen a good deal of Raman and rate his judgement very highly. I further rate his character highly, for one reason because of his willingness to express an adverse judgement where his real convictions require it. It is in view of this judgement that I am making the kind of a reply to this letter which I enclose.

Professor Raman will undoubtedly wish this correspondence to be confidential, but I have thought that you ought to be apprised of his judgment, and perhaps Dr Rose of the International Education Board as well.

Very sincerely yours,

R.A. Millikan

14 See Saha's Letter to Russell, p. 59, Russell's response to Millikan's in the next letter and Millikan's letter to Saha, p. 96, about the outcome of this fund raising effort

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Professor R.A. Millikan,
California Institute of Technology
Pasadena, California

October 27, 1924

Dear Millikan:

I am greatly obliged for your letter about Saha's request. I agree entirely with your point of view, and had felt some of the same doubts which you suggest. Consequently I tried to write a somewhat cautious letter to Saha, as you may see by the enclosed copy. You were a little more judicious than I in the form of your offer of commendation, but I do not think I have pledged myself to do anything more than you intend to do.

Some remarks, which Fowler made to me about Saha's attitude while a student in his laboratory, are quite in line with comments which you have heard.

I am very glad to hear that Professor Raman is at Pasadena. I formed a very high opinion of him from what I saw him at Toronto. I only wish that he was likely to be still in Pasadena when I get out there in early spring.

Everything is going well here. I have about finished with titanium spectrum, and am getting ready to write things up — also lecturing in spectroscopy to a dozen graduate students, which I find very good fun, and highly educating to myself.

With kindest regards to Mrs. Millikan, and anticipations of seeing you before long.

Very sincerely yours,

●●●

Professor Harlow Shapley
Harvard College Observatory
Cambridge, Mass.

January 24, 1927

Dear Shapley:

Many thanks for your letter with the enclosed manuscript from Gerasimovic. I find it difficult to criticize it in detail for two reasons. First, I am unfamiliar with the details of the theory involved; second, and more important, his paper, as far as its mathematical portion goes, is merely an abstract. He does not give details enough to enable anyone except a very few experts to follow his reasoning and even they, I suspect, would have to do a good deal of algebra. I believe that his reasoning should either be published in fuller detail, or else that the heavy mathematics should be omitted from

the Harvard Circular and published elsewhere.

I think that the policy of publishing full details is the more desirable, because Professor Gerasimovic's conclusion differs very remarkably from Milne's, who, if I understand him at all, believes that the temperature in the chromosphere is effectively lower than that corresponding to black body equilibrium. However, from page 7 of the manuscript, it appears that he and Milne may be talking about different regions in the atmosphere.

I have not time to go into the theoretical details of this matter at present, but I would suggest that in the case of legitimate controversial material of this sort, (that) the mathematical deductions be given in full. I may add that, with respect to the "barium problem" (page 8 of the manuscript), I have grave doubts of the validity of Saha's "steric" factor. ***Unless I misunderstand Saha, he assumes that excited atoms have a sensitive side on which collision must occur to produce effects, and this does not look to me like good quantum physics.*** On considering this problem, I am inclined to believe that the main factor in the case is the existence of the metastable D states in both the neutral and ionized atoms. If I have time, I will send you a little note on this thing in a few days. Just at present we are abominably rushed with the Index of the second Volume of our book, and with what we hope are almost the final proofs.

I am writing Kiess about zirconium.

I had a grand time at Harvard, and have been rather sleepy ever since; but I certainly enjoyed it.

With best wishes to everybody,

Cordially yours,

●●●

Dr. Theodore Dunham, Jr.
Mt. Wilson Observatory
Pasadena, Calif.

October 11, 1927

My Dear Dunham:

The ionization diagram was sent on from Flagstaff and reached me here in Princeton. I have not had time to go over the matter yet as I could wish, on account of a variety of incidentals which eat up time. It is rather hard to advise you at this distance how much theory to put into your thesis. I don't think you need to put in any for strict thesis purposes. It is a fine and useful piece of work anyhow. But it may be worthwhile to put in some on your own account to get it published, particularly if you have a little spare time. I wouldn't hold up publication much though for the sake of it.

You are very kind about the note which I sent in manuscript to Dr. Adams. ***It is by no means " the biggest thing" since Saha's theory, but I***

think it will be useful as affording direct evidence that Boltzmann's theory actually applies to the distribution among the various energy states.

This, by the way, makes the famous " hydrogen problem" much more acute than ever. Perhaps we can get that cleared up someday. I hope so!

I hope, too, that you had as much fun in the Kapteyn cottage as Mrs. Russell and all the family did when we were there in June. It is a delightful spot.

With cordial good wishes to you both, in which Mrs Russell joins me,

Very sincerely yours,

●●●

University of California
Department of Physics, Berkeley

Prof H.N. Russell,
Princeton University

Oct. 19, 1932

Dear Professor Russell,

Dr. Struve, in his answer to a copy of the letter sent to you on Oct. 5, suggests that I publish a short note on the Saha equation for the Astrophysical Journal. I have accordingly written up the enclosed, and am submitting it for suggestions before formally sending it in for publication.

In it I have discussed also the other general constant of the Saha equation. This second constant is far more complex, and the calculation of its probable error is really quite a task. I remember in 1929 struggling for days over the probable errors of the various forms of the Sackur-Tetrode constant. It wasn't quite so bad now, as I at least know how to go about it. Incidentally I made a direct numerical calculation of the value of C_2 in the two forms given in the enclosed. The agreement of the two numerical results furnished a good check on the accuracy of the value of S_o' that I published in 1929.

I shall wait until I hear from you and Dr. Struve, and then incorporate any suggestions you have to make. I got the "chemistry" of this from Prof. Giauque and I am quite sure that part is correct. I never know just where to find such things in the literature.

Yours sincerely,

Raymond T. Birge

●●●

Dean L. P. Eisenhart
Nasseau Hall Campus

January 18, 1945

My Dear Eisenhart:

I have a letter from Meghnad Saha, the Indian physicist. He is with the Indian Scientific Mission and will be in New York from January 27th to February 2nd. He would like to come to Princeton on January 30th. He writes, "I hope this date will suit you. I would like to see Profs. Einstein, Bohr, Pauli and others." I did not know that Bohr was here. How should we go about making connections with these worthies? *Saha is a man of real distinction and should be an honoured guest*. We would be glad to entertain him, or it might be that a luncheon could be arranged elsewhere.

Very sincerely yours,

(Henry Norris Russell)

●●●

Harlow Shapley,
Harvard College Observatory
Cambridge 38
Massachusetts

February 13, 1947

Dear Shapley:

While I was away at Aberdeen the enclosed manuscript came with a characteristic brief note of yours. *Saha's history of life is really interesting, and as good an example I know of the importance of providing assistance to a competent man. I had often wondered why Saha's activity in the theoretical field fell off after his return to India, and his account gives the explanation*. Whether he is correct in his opinions about Raman I cannot judge but it is a pity that he was buried earning his living in a small position. Unfortunately the interruption lasted so long that his recent contributions to theory do not seem to me to be at all of the same importance as the old. His history affords a very good argument that the Indian universities and perhaps the government should be actively interested in securing proper opportunities for men of proven ability. If it would be of any use to you to quote me to that effect you are most welcome to do so. *I have always been careful to give Saha the credit for starting the astrophysical application of the ionization theory. I do not think I knew till seeing this letter how much he had done previously in India*.

You will be glad to hear that Spitzer's appointment has been approved by the Committee of Three. Action by the Trustees is a foregone

conclusion but cannot take place until April. Bok has probably told you of other developments. Schwarz's child was here and told us that DuBridge wants to get him at Cal. Tech. I am not trying to influence your judgement as to the relative advantages to science of his going there or coming here. But if you favour the latter, a word to him might be of some help. I hope you can get a little rest now after your strenuous time in India of which Mrs. Shapley told me something. Now if you can't be 'away' be as 'away' as you can.

We are happy in the news that William Ashley Russell saw the light a week ago yesterday at Greeley. He and his mother are reported very well.

With hearty good wishes to you all,

Cordially yours,

(Henry Norris Russell)

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4.2. BETWEEN ARNOLD SOMMERFELD AND SAHA*: (1921-1936)

(*Plate 8)

(Translated from German)

Institute of Theoretical Physics
Munich

The 18, April 21

Respected Dear Colleague!

Many thanks for your letter and announcement of your visit to Munich. Your work on the solar lines is very interesting. I request you to lecture on that in our colloquium in Munich if you know enough German. You will let me know the exact time of the aforementioned visit before the trip.

Respectfully yours,

A. Sommerfeld

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(Translated from German)

To (Mr.) Prof. Geheimrat A. Sommerfeld,

April, 1921

Very respected Mr. Geheimrat,

Please receive my sincerest thanks for your lines, and for your friendly invitation to Munich. It would give me great pleasure to be allowed to speak in your colloquium about my work. I have written out my work in German, but am afraid that my pronunciation is not comprehensible enough. It will be a special honour for me to get to know you personally. I am thinking of travelling to Munich at the end of July.

Respectfully yours,

(M.N. Saha)

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PROF. ARNOLD SOMMERFELD

MÜNCHEN, GUNANTSTR. 6

14. August 1932

Lieber Jaha! Ich danke Ihnen herzlich für Ihr schönes Buch. Es war mir sehr interessant zu sehen in welchen (nicht gedruckten) Punkten Ihre Behandlung sich von meinen Vorlesungen unterscheidet und in welchen Punkten wir übereinstimmen. Auch ich bringe Thermodynamik und Statistik in derselben Vorlesung. Beim II. Hauptsatz unterscheide ich die Existenz der Entropie (= erster Teil des ersten Hauptsatzes) und ihre Tendenz zum Maximum (= zweiter Teil des zweiten Hauptsatzes). Ebenso beim I. Hauptsatz: die Existenz der Energie als Zustandsgröße (= erster Teil des ersten Hauptsatzes) und ihre Äquivalenz mit Arbeit und Wärme (= zweiter Teil des ersten Hauptsatzes). Die Existenz von Entropie und Energie kann man (vom reinen) zwar statistisch beweisen, muß sie aber thermodynamischen Standpunkt aus postulieren, ohne auf die Postulate vom perpetuum mobile zurückzugreifen.

Da ich nächstens einen Vortrag im Planetarium von Stuttgart zu halten habe, über Spektroskopie und Astrophysik, werde ich Ihr Kapitel XIV genau durchlesen und vorzüglich gebrauchen können.

Ich freue mich, dass Herr Majumdar sich so gut
entwickelt hat. Mein früheres ungünstiges Urteil
über ihn, das sich auf Mitteilungen meines Freun-
des Joos in Jena stützte, möchte ich ausdrück-
lich zurücknehmen, wie ich es auch Raman ge-
schrieben habe. Leider aber bin ich mit [illegible]
gar nicht vorwärts gekommen. Es fehlt ihm
an mathematischer Schulung und an physika-
lischem Verständnis. Persönlich ist er fleissig
und sympathisch. Ich habe ihm einfache numer-
ische Aufgaben gestellt die er sorgsam ausgerech-
net hat. Aber auch da war es schwer [illegible]
[illegible] verständlich zu machen. [illegible]
[illegible] nicht, dass er hier promovieren kann.

Ich würde mich sehr freuen wenn Herr [illegible]
man aus Dacca herkommen könnte; mit ihm
würde ich sehr gern zusammen arbeiten. Sie
und Ihre Collegen möchte ich bitten, in Zukunft [illegible]
vorgeschrittene Studenten herzuschicken, die
keine Schwierigkeiten bei den elementaren Dingen
haben.

Meine Erinnerungen an Indien sind immer
äusserst lieb. Auch die Tage in Allahabad waren
sehr schön. Agra war herrlich. Dann habe ich
zwei schöne Tage in Santiniketan verlebt.

Ich hoffe Sie bald in Europa
wiederzusehen. Ihr getreuer

A. Sommerfeld

(Translated from German)

Prof. Arnold Sommerfeld
Munich

14, August 1932

Dear Saha,

I thank you very much for your beautiful book[15]. It was very interesting for me to see on what points your treatment differs from my (unpublished) lectures and on what points we agree. I, too, cover thermodynamics and statistics in the same course. With the second law I differentiate the existence of entropy ("first part of the second law") and its tendency to increase ("second part of the second law") and its equivalence with work and heat ("second part of the first law"). One can indeed prove the existence of entropy and energy from statistics but one must postulate these from a purely thermodynamic standpoint, i.e. return to the postulates about perpetuum mobile etc.

Because I have to deliver a lecture shortly on spectroscopy and astrophysics in the planetarium at Stuttgart, I will study your chapter XIV in detail and be able to make excellent use of it.

I am pleased to know that Mr. Majumdar[16] has developed so well. I want to explicitly retract my earlier negative judgment about him, which I based on reports from my friend Joos in Jena, as I also wrote to Raman. Unfortunately, however, I have not made any progress with Mr. Chatterjee. He lacks mathematical training and physical understanding. Personally he is very hardworking and nice. I have given him simple mathematical tasks which he had carefully worked out. But even there it was difficult to explain the problem to him. I, therefore, do not believe that he can receive his doctorate here. I would be very pleased if Mr. Krishnan[17] could come here from Dacca; I would be very happy to work with him. I would request you and your colleagues to send only advanced students to us in the future, who do not have any difficulties with elementary things.

My memories of India are very dear to me. The days in Allahabad[18] were very nice. Agra was excellent. After that I spent two beautiful days in Santiniketan.

I hope to see you again soon in Europe.

Your colleague,

(A. Sommerfeld)

15 Treatise on Heat – by M.N. Saha and B.N. Srivastava, first published in 1931

16 Dr. Ramesh Majumdar, a former student of Prof. Saha

17 K.S. Krishnan, co-discoverer of Raman Effect

18 Prof. Arnold Sommerfeld visited Allahabad in 1929, and stayed at Prof.Saha's house in 7 Beli Road.

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(Translated from German)

Munich, the 13 December 1935

Dear Saha,

This letter is not only an "acknowledgment", but also heartfelt thanks for pleasing me yet again with your new book[19]. Your books seem to grow from the ground like tropical plants! I astound at your productivity. My book (Atombau = Wavemechanics) is making only very slow progress. You must wait for at least another year, until I can send it to you as a return gift. In the meantime, please do please yourself with some reprints.

On statistics, which you handle very beautifully and penetratingly, is it not inconsistent (I am myself guilty of the same in earlier lectures) to use μ-space in §463 and Γ-space in Note 11 (Notation of Ehrenfest in the Encyclopedia of Mathematical Sciences).

Perhaps Prof. Ortvay from Budapest is going to visit you next year — a fine and clever man, whom I can recommend to you very strongly as an ex-student and good friend of mine. With Born this is, of course, redundant.

When will you come again to Europe?

With heartiest greetings

Always yours

A. Sommerfeld.

19 The "new book" is most probably "A Treatise on Modern Physics" by M. N. Saha and A. K. Saha, first published in 1934

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4.3 LETTERS FROM WILHELM WESTPHAL: 1921-1922

(Translated from German)

Professor Dr. Wilhelm Westphal
Zehlendorf, Berlepschstr. 72a

6.9.1921.

Dear Mr. Saha,

I am sending you today 30 copies of your work as printed matter, for a start. I assume that this will be enough for you for the moment. Further copies are lying ready for you with the caretaker of the Physical Institute. I already told you that I needed a larger number in Potsdam. Besides, various colleagues had turned to me for reprints, which I have then sent them. There is a huge interest. Because in all likelihood I am going to get more such requests since you mention me so friendly at the end of the work, it would be good if you would want to leave me as many as possible from the copies lying in the Physical Institute. I would ensure their suitable use. After all, for Indian and English readers the work does not carry the same significance as for us Germans, because essentially the same material is already there in your English publications.

Pity, that we missed each other in Potsdam. I hope that the afternoon was as nice for your (women) friends as it was for me. We had a jolly time together and it had particularly pleased me to get to know Indian women once.

I am counting on your definitely coming to Jena. I will be already there on the afternoon of 17.9. But should you somehow be prevented from coming, then please do write me the address of your friend, the musician[20]. You see, we would not see each other in Berlin anymore, because from Jena I travel first to Hamburg and then to the Black Forest and will be back here only around 20.10 again.

Hopefully you are having a good time in Switzerland. Please greet Prof. Edgar Meyer, whom you are going to meet most likely, for me. With best wishes from my wife and me.

Yours

Wilhelm Westphal

Prof. Dr. Wilhelm Westphal

20 Probably refers to the famous Bengali singer, Dilip Kumar Roy

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(Translated from German)

I realized with horror that I have sent you a letter written to Mr. Berliner while Mr. Berliner received the letter to you. Please destroy the wrong letter. You would have seen from the enclosures that the reason for my writing was the bill from Springer.

By the way, I hope to hear from you really soon whether you are doing well, what you are working on, and everything else that is interesting that you have to report. I am stuck between all kinds of official responsibilities at the moment and have little leisure to work.

You would do me a great favour if you would send me reprints of all interesting work that is published in India and that we would not get to see otherwise. I will take care that the people who are concerned with such work get to know about them. That is of course also in the interest of the Indian researcher. In particular I will work on getting seminar papers in the Physikalischen Berichten and the Naturwissenschaften. Could you not perhaps organize that we get all publications from Kodaikanal?

We have many foreign visitors, Americans, Japanese, Chinese. At the moment Langmuir is here.

So please do not forget us, in particular me. I hope that our time together remains also for you a pleasant memory. I will always consider it a particular gain that we have come closer to each other not only scientifically but also personally and hope that the future will allow our friendly relationship to be increasingly firmly cemented.

With heartiest greetings I am

Your

Wilhelm Westphal

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26.1.1922

Dear Saha,

I thank you very much for your friendly letter. I forwarded the check immediately to the springer firm. The work of Russell interests me very much. I have presented it in the colloquium, where there was a lively discussion. I will be very thankful to you, if you continue to communicate me such things.

I believe I have not told you yet, that since 4 weeks I am not working at the Physikalischen Institut any more. Namely, I have accepted a call to join the Ministry of Science, arts and Public Education, where I work in Higher Education Department (Universities, Polytechnics and academics in general). But I continue to hold my classes. I have decided to take this step because I believe there I will be able to be of a lot of use

to academia. It is probably good when someone who has spent a large number of years in the business of academia takes over the academics there, particularly in these, such difficult times for our German science. Within my responsibilities are included the cultivation of academic relationships with foreign countries. Should one of your friends happen to come here, it would be a particular pleasure for me if you would give him a short letter for me (Address: Unter den Linden 4). I would then receive him very gladly, also in my official capacity. It would particularly interest me, if you would keep me updated about the interesting issues of Indian University life.

My essay on your work has found a very friendly reception and I have had to re-order copies several times to send them out, for example, to Mr. Voute in weltevreden in Java.

It will interest you to know that Nernst has become the President of the central Physical–Technical Office (Physikalisch-Technicschen Reichsanstalt), as successor to Warburg. Who takes over his Institute is still not known.

In my house we had unfortunately a lot of illnesses. My entire family was down seriously ill with the flu and my wife is so badly affected that she has been in bed since 5 weeks and hardly stands up. Now we have harsh cold since almost three weeks and I am jealous of you with your warm climate at home.

Hopefully I will hear soon again from you. With the hope that you and your family are doing well and that you continue to produce real nice work, I am

In friendly thoughts, yours

Westphal

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4.4 LETTER FROM ALFRED FOWLER*: 1921

(*Plate 7)

Imperial College of Science and Technology
(Royal College Of Science)
South Kensington
Londons. W.7.

April 29, 1921

Dear Mr. Saha,

I was very glad to have the notes on **Neon** which you were good enough to send me a few weeks ago. I have utilised them in drawing up a rather fuller account for my work on series, which is now on the way to printing.

Your recent letter to the college with regard to fees has been shown to me, and I think it will be possible to modify the original amount.

With regard to the government grant which has been assigned to you, I hope you are clear in your mind that the grant was made on the understanding that the work would be carried on at the Imperial College, and that the sanction of the Committee would be necessary if you change your plans. I am sorry that you did not get the full amount you asked for. I suppose there was not enough to meet everyone's wishes.

I hope that you are enjoying your visit to Germany & that you are profiting of the opportunities.

Yours Very Truly,

A. Fowler

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4.5 LETTERS FROM RALPH H. FOWLER: 1923-37

Trinity College
Cambridge

April 14 (year unknown)

Dear Saha,

I have sent off the papers which you were good enough to ask me for I should be grateful if you could spare me copies of your papers on stellar & Solar Spectra. I am (with Mr. E.A. Milne) writing a paper which is a material extension of your work, on the basis of general statistical theory, to the exact positions of maximum intensities of lines & the results come out very satisfactorily.

R.H. Fowler

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Cromwell House, Trumpington
Cambridge

June 28 (year unknown)

Dear Prof. Saha,

I am sorry I missed you and shall look forward to seeing you in October when I shall be regularly available in the Cavendish Lab.

I do not think we shall have any difficulty in coming to agreement about the points I discussed in the review of my book! It will be very interesting to try anyhow.

Yours sincerely,

R.H. Fowler

●●●

Nov 7 (year unknown)

My Dear Saha,

I have to thank you most sincerely for your letter & the message of sympathy officially passed by your university. Rutherford's work & influence spread so widely over the world, didn't they? There is no scientific group anywhere I think which does not feel a sense of loss. And here in Cambridge as you realize it is very great indeed. It is especially tragic for Indian scientists to feel that they must miss his presence presiding over the first joint meeting of the two associations for the advancement of Science. He was so exactly the right man. His address as President was however completely finalised, and I believe it will still be read to you. It is his last written work. But alas it won't' be read by him.

With best wishes

Yours sincerely

R. H. Fowler

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4.6 LETTER FROM ROBERT A. MILLIKAN*: 1924

(*Plate 15)

Professor Megh Nad Saha,
Department of Physics,
University of Allahabad,
Allahabad, India.

Dear Professor Saha:

I am just now in receipt of your letter of September 18.

I think it is true that the International Education Board has extended some assistance to Professor Bohr, but so far as I know they have not yet extended assistance to any of the laboratories in this country, many of which are sorely on need of funds, and I suspect that they are not making it a policy to extend largely this type of benefaction.

I think that the Carnegie Institute had a rather unfortunate experience with a policy with which they began of distributing a considerable number of small grants, and has therefore rather abandoned that policy[21].

I am appraising you of this situation because you will wish to know it if you make an appeal to either of these bodies. I know that the head of the Rockefeller International Education Board made a very careful personal study of the conditions existing in Bohr's laboratory before making him the grant to which you refer, and I feel pretty certain that their policy is going to be to make no grants until after the most careful investigation. Should you make application it should go to these bodies directly.

I could speak in the highest terms of your theoretical work on the ionization in the atmosphere of the sun and stars, upon which I congratulate you most heartily.

Very sincerely yours,

R.A. Millikan

21 Also see correspondence between Millikan and Russell about Saha's request for funding (p. 79-80)

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4.7 LETTERS BETWEEN MARCEL MINNAERT AND SAHA: 1955

October 19, 1955

My Dear Minnaert,

You may remember that at Dublin, I talked to you of a paper written by me on the Solar Corona, during the War, which has been practically ignored by astrophysicists. I am sending you a copy of this paper which I am afraid, has not been read by many astrophysicists of note. I gathered information at Dublin which indicated that my views may after all prove to be correct. I would refer you to p. 105, where I quoted you.

"Anderson has shown that the corona cannot be in equilibrium if the ordinary physical laws are valid. Instead of assuming as he does, that very hypothetical modified laws must be applied, we may attempt to account for the corona by assuming that it really is not in equilibrium, and that its particles are continuously projected towards space" (Minnaert).

At the present time, fanciful temperature amounting to 10^8 C – 10^{10} C have taken the place of fanciful physical laws mentioned by you. It is as if Temperature has gone mad.

My view is quite different. I consider most of the abnormal solar phenomena – flares, burst, coronal ionisation, to be unsteady phenomena not due to any temperature. They must be due to some kind of nuclear explosion taking place inside the photosphere, and then propagated outwards.

You will grasp my ideas if you care to read through my paper.

At Dublin, ----[22] told me that they had traced the green normal line 5308(?) down to a height of 20000 km. Of the solar photospheric -----, Robarta of Sacramento told me that he has traced 5208 down to a height of 3000(?) km. These finding, if they are true, completely demolish in my mind the temperature theory of coronal emulsion.

I shall be glad to have reprints from your laboratory, as I am again turning my attention to astrophysics after retirement from my professorial duties.

I am not afraid of "criticisms". When I wrote my first paper on "Thermal Ionisation" published in *Phil. Mag. vol 39, 1920*, I asked Evershod to communicate it to the M.N.R.A.S. He refused saying that he could not understand the paper. I sent it straight to the M.N.R.A.S. it was refused forthwith. The Thermal Ionisation "Theory" gained acceptance only when my paper on 'A Physical Theory of Stellar Spectra' was admitted by the Royal society of London through the god offices of the late Prof. A. Fowler. I wrote this paper in India, but it was revised at Fowler's laboratory. It is quite wrong to say, as Plaskett and others have sedulously propagated, that I owed the ideas to

22 Illegible

Fowler. The equation of Thermal Ionisation was evolved in India in 1919, as *Phil Mag* paper shows, a year before I met Fowler. I wrote an article on 'Selective Radiation Pressure' for the *Astrophysical Journal in 1919*, it was kept in a drawer by the Yorkes astronomers, till it was handed back to me by Dr. Morgan when I visited Yorkes in 1936. The very same thing was worked out by Milne later. But due to propaganda by Plaskett and others I have been hardly given any visible form of recognition by astronomical society for the work on thermal ionisation, e.g. I have never been asked by the Royal Astronomical society of Great Britain to become their honorary member, or was awarded any other form of honour.

With kind regards.

Yours sincerely,

M.N. Saha

●●●

Prof. M.G.J. Minnaert
Director of the Observatory,
Zonnenburg 2, Utrecht,
Netherlands.

Utrecht,
Zonnenburg 2

30 November 1955.

Dear Dr. Saha,

Many thanks for your kind letter with the reprint of your important paper on the theory of the corona. It was extremely interesting to read how difficult it has been to introduce the ionisation theory in astrophysics. I must say that these historical details were unknown to me and that they enhance my admiration for the man who first proposed this theory.

However about the corona I cannot well assume that you are right. In the corona no appreciable systematic motions are found and the equilibrium theory explains beautifully the observations. The high temperature is fully in agreement with:

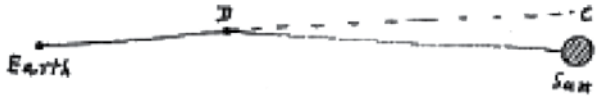
(1) the high ionisation;
(2) the great width of the lines;
(3) the enormous vertical extension of the corona;
(4) radio astronomical measurements. The agreement of all these methods is very convincing.

I shall now try to discuss some of your objections.

On p. 104 you wonder whether an iron atom can be deprived of 13 or more electrons. In papers like that of Woolley and Allen, M.N. you will find a calculation, showing that the observed ionisation can quite well be explained with temperatures of the order of one million degrees; the excitation

is mainly due to electron collision, the emission is due to recombination.

On p. 105 you express disbelief about the existence of dust particles in the outer corona, which would be necessary for the explanation of the continuous coronal spectrum. But the question is that the diffusing dust grains do not occur near the sun, but all on the way between the earth and the sun; they are seen projected in C,



on a region around the sun. Particles more outside do not contribute, because the scattered light quickly diminishes with the angle of scattering[23].

Your remark concerning the width of the emission lines, which has been found to decrease higher up in the corona, is certainly interesting; as far as I am aware of, this observation has been more or less forgotten. I should like to see the original material in order to see whether it is entirely reliable. In any case we must remember that a line of sight, passing at 20,000 km above the solar surface, has also passed through layers at very much greater heights. I agree that this is not an explanation; it only shows that things are not so simple as they look.

There are many other difficulties which also remain incompletely explained. But these are only details compared to the very successful general theory. To substitute for this a hypothetical nuclear process seems to me very unsatisfactory; the atomic velocities needed for such a process would be much higher than our one million degrees!

Your considerations about the capture of electrons by highly ionized atoms are in any case useful and may be applied to the detailed calculation of the equilibrium between the different ions in the corona. But I cannot well think that your general conception is right.

Please excuse that I express so frankly my opinion. It was a great pleasure to meet you at Dublin and to have a very interesting talk during the American dinner party. With best remembrances and wishes,

Yours, very truly,

M. Minnaert

23 Hand drawn Figure from the original letter

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4.8 LETTER FROM ERICH REGENER: 1934

(Translated from German)

Physikalisches Insitut der
Technischen Hochschule
Stuttgart

Stuttgart, the 24 November 1934
Wiederholdstrasse 13
Fersprecher Nr. 2 18 28

Respected Dear colleague:

I thank you very much for your friendly lines from 26 October of this year and the interest that you show in our work.

I also thank you for pointing out the Fraunhofer lines that we could expect to see still deeper within the Ultraviolet. The spectrograph with which we made our first recordings was intentionally made to have very low light sensitivity, so that there would be no disturbance from scattered light. We are building a more light sensitive one now and then we hope to come deeper in the Ultraviolet, so that perhaps we could also observe intensities on the other side of the Hartley Brands at approximately 2200 A.

I thank you for your tip about recording the protuberances. Unfortunately it is a very difficult business because for this purpose one has to point the instrument directly at the sun. We have been able to avoid that so far through the use of a diffuse reflector.

With the same mail I am allowing myself to send you reprints.

I am yours faithfully,

E. Regener

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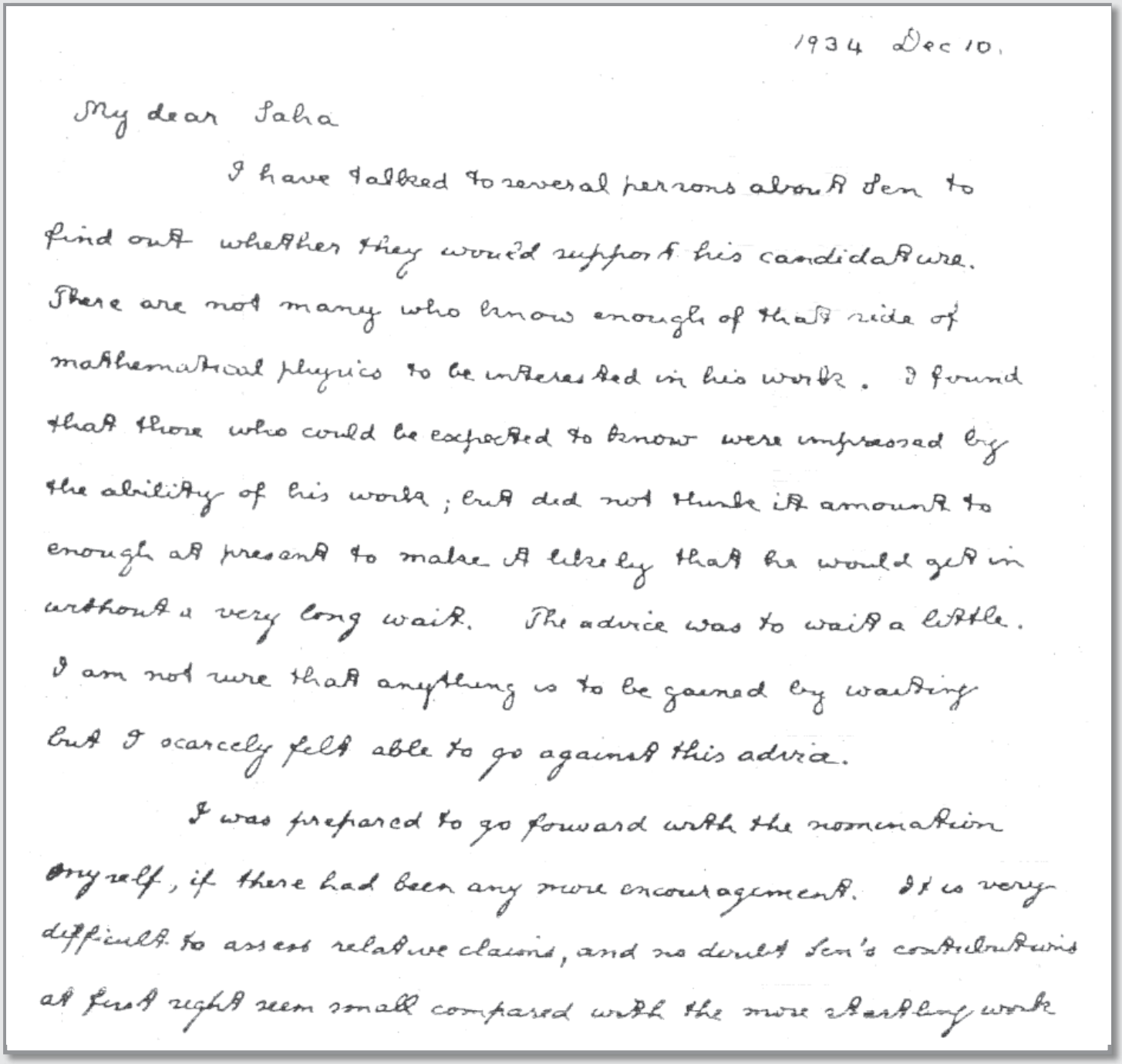
4.9 LETTERS FROM ARTHUR S. EDDINGTON*: 1934-1937

(*Plate 13)



1934 Dec 10.

My dear Saha

I have talked to several persons about Sen to find out whether they would support his candidature. There are not many who know enough of that side of mathematical physics to be interested in his work. I found that those who could be expected to know were impressed by the ability of his work; but did not think it amounts to enough at present to make it likely that he would get in without a very long wait. The advice was to wait a little. I am not sure that anything is to be gained by waiting but I scarcely felt able to go against this advice.

I was prepared to go forward with the nomination myself, if there had been any more encouragement. It is very difficult to assess relative claims, and no doubt Sen's contributions at first sight seem small compared with the more startling work

in quantum theory. On the other hand if a relativist is to get into the Royal Society the obvious man at present is Jon.

As regards the paper which I thought highly of, A still impressed me as a workable and brilliant piece of work. I am inclined however to agree with Lemaître that A does not reach the true answer. It makes it more difficult in pressing his claims not to be able to quote it as an unequivocal advance.

Perhaps we might consider the matter again next autumn

Yours sincerely

A.S. Eddington

1934 Dec 10

My Dear Saha

I have talked to several persons about Sen to find out whether they would support his candidature. There are not many who know enough of that side of mathematical physics to be interested in his work. I found that those who could be exposed to know were impressed by the ability of his work; but did not think it amount to enough at present to make it likely that he would get in without a very long wait. The advice was to wait a little. I am not sure that anything is to be gained by waiting but I scarcely felt able to go against this advice.

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As regards the paper which I thought highly of, it still impressed me as a notable and brilliant piece of work. I am inclined however to agree with Lemaître that it does not reach the true answer. It makes it more difficult in pressing his claims not to be able to quote it as an unequivocal advance.

Perhaps we might consider the matter again next autumn.

Yours sincerely

A.S. Eddington

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1937 Nov, 23

Dear Professor Saha

Thank you very much for your letter. I shall be glad to fall in with all the arrangements you propose. I accept with gratitude the address from the corporation of Allahabad.

I am not quite clear whether the arrangement means that I return to Delhi from Dehra Dun, or omit the visit to Dehra Dun altogether. But anyway you may take my acceptance as definite, and I will travel from Delhi with Sen, P.M. Sulaiman and Dr. Kothari on the night of the 26th.

Yours very sincerely

A.S. Eddington

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4.10 LETTER FROM EDWARD A. MILNE: 1935

Oxford
24th July, 1935

Dear Professor Saha,

I was very glad to receive your letter saying you are coming for some months to Oxford. You are paying Oxford mathematicians & physicists a great compliment in selecting us to visit, & I hope we are equal to it. Lindemann, Simon & Plaskett, as well as myself will be there, but Zanstre(?) is only here in summer vacation, & A.H. Compton has now returned to America. On the other hand we – Plaskett & I have some able & stimulating research students with whom we run a couple of weekly Colloquiums. I may be lecturing in stellar structure, but I have done lately some exciting work in fundamental dynamics that I may forge my lectures formally on that. I have got the whole dynamics of a perfect "fluid" & can complete Einstein's work in a surprising direction.

On the question of accommodation my wife will write to Mrs. Saha what she thinks[24]. I should say that Oxford is the dearest place to live in, outside London, in England, so you should extract a pretty substantial sum for living expenses from the Carnegie people. Bruggencotés took lodgings – one living room & one bedroom – in N. Oxford for their stay. Others have begun by living "en pension" – we can give an address of a boarding house. The First Class Hotels are too expensive even for a short stay, in my opinion, but there are a number of less well-known secondary hotels. Furnished houses fall vacant from time to time, & I imagine cost some £4 a week. But you might write to Mallam, Payne & Dorn Estate Agents, Cornmarket, Oxford, mentioning my name & asking them to keep a lookout for you in advance, stating your requirements. If they ask where you want to live, I should mention that Boars Hill is an expensive residential region in S. Oxford, a short motor-car ride distance – not to be recommended. Hewadington is better, well served by a service of motor omnibuses, but fewer people live there than in N. Oxford, where we live & where the majority of the dons live. This is divided into three main districts (a) houses on & off the Banbury Road (the best houses); (b) houses on & off the Woodstock road, cheaper but very nice indeed & thoroughly to be recommended (here the B's found rooms) (c) Summertown, a non-university district with shops, smaller houses & a solid middle-class population to the N of the dons' residential districts. If I were you, I would certainly accept a house in Summertown if a suitable one offered, as it is well served by busses – it is on the Banbury Road [Woodstock Road also has a 'bus (omnibus) service] & the extra distance is negligible. Victoria

24 Mrs. Milne's letter to Mrs. Saha is in Section 6: Miscellaneous Letters

Road, Summertown is a good road. In Northmoor road, which is parallel to Barnbury Road, houses rarely fall vacant, & they are higly rented. The Plasketts live in a road near Northmoor Road off the Banbury Road, where the Simons have found a house – not too cheaply I imagine. The Schrödingers live near Northmoor Road in a much more expensive house than ours. Frenclay Road & other roads off he Woodstock road are exceedingly nice. Southwell lives in Lathbury Road, between the W. (Woodstock) & B (Banbury) Roads.

You would probably do best to live in a boarding house near us till you find a house, unless you can receive one in advance through Mr. Dorn of Mallam, Payne & Dorn, to whom I am known personally, & through whom we got our house to rent. All the roads I have mentioned are a 1½ d (one & half penny) or 2 d (two penny) bus (omnibus) ride from the collegiate & laboratory centres of Oxford. I can easily introduce you to the library authorities. I have no doubt that Plaskett would be delighted to give you reading & writing accommodation in his observatory & we have also now a mathematical institute, in which there is a little used common room, where you would be most welcome. This is adjacent to the Radcliffe Science library, under the same roof as my professorial room. I hope you will be willing to give some talks or lectures at my invitation during your stay.

I must mention one point of extreme delicacy – Lindemann always seems to me to be rather jealous of you work – he has never forgiven himself for not discovering the physical nature of the stellar spectral sequence himself, which we all recognise was due to you. He will be, I think, extremely courteous to you – he always is, to everybody – but he may make difficulties if you require laboratory accommodation – partly because his lab is already overcrowded & partly because he appears to me to be not very well disposed to Indians. He once put difficulties in my way when I mentioned the possibility of an Indian student coming to work with me. He gave me, very kindly, the use of, lecture room on the Clarendon Lab. & has been extremely kind to me always, so I mention those matters with extreme reluctance & only in the strictest confidence. Probably you know them well enough already. He is rather a power in the land of Oxford, & I simply suggest all circumspection in any approach to him that you may make. He is sometimes a disturbing element in an otherwise very happy Oxford family, yet his intentions are always of the best & he never bears any malice if one disapproves his proposals as we sometimes have to do. You may be interested to learn that Mrs. Carnegie, the widow of Andrew Carnegie, founder of the Carnegie Mellon, lives in Skibo Castle, a mile or two away from Dornoch where my wife & family are staying on a visit to my wife's father. I passed the lodge-gates this morning.

Now I must stop.

With kind regards,

Yours very sincerely,

E.A. Milne

4.11 LETTER BETWEEN HARLOW SHAPLEY* AND SAHA: 1935-1953

(*Plate 12)

Harvard College Observatory
Cambridge 38, Massachusetts

Professor M.N. Saha
7 Beli Road
Allahabad, India

December 23, 1935

Dear Professor Saha:

I have just received your letter of November 18. I am glad that you are to be here as a delegate from your university. I hope you will find it possible to attend at least a part of the Conference that is held from August 31 to September 11 of next year. There will be a large number of foreign scholars in all fields of learning taking part in the symposia. Among the physicists we expect Bohr, Heisenberg, probably Einstein, Millikan, and Compton. Eddington and Pannekoek will be here also. Dr. Lundmark of Lund will be here at least in the early part of the summer.

I should like to have you plan to attend especially the symposium on Friday, September 4, at which time the principal speakers will be Dr. Pannekoek on "The Temperature Scale for the Stars" and Professor Russell on "The Composition of the Stars." After these principal papers are given, we shall ask for 15-minute reports or discussions, prepared in advance if possible, by Dr. Otto Struve of the Yerkes Observatory, perhaps by Dr. Theodore Dunham of the Mount Wilson Observatory, and by you, if you will. The next day there will be another symposium at which Eddington and Levi-Civita will give the principal papers, and possibly Hubble and Tolman will be invited to contribute shorter discussions.

During this first week of September the American Astronomical Society will be meeting here at Harvard, and you will thus have an opportunity of easily meeting most of the leading astronomers of America.

I shall see that you are later supplied with more detailed information concerning these synopsis.

Nearly all of the astronomers of Harvard Observatory will be here from July 1st throughout the next three months. Beginning about July 5 we shall have our six weeks of the Graduate School of Astronomy and Astrophysics. This summer school consists largely of informal seminars for advanced students and for visiting members of the departments in other colleges and universities. We began the school last year, and I enclose

a brief descriptive circular of the program for these weeks. Next summer the teaching staff will include: Dr. Merrill of Mount Wilson, Professor Russell of Princeton (for one week), Mrs. Cecilia Payne Gaposchkin, Mr. K. Lundmark of Lund, and probably Professor Schlesinger of Yale (one week or so). We should like to have you arrange your schedule so that you can take part in these conferences. I do not know to what extent the terms of your Carnegie Scholarship permit your undertaking seminar lectures or conferences, but I have written to the Carnegie Corporation in New York. When I hear from them, I shall immediately write you again, indicating what we would like to have you do. I go forward with this plan boldly, without waiting to hear from you, because of the long time it takes for the exchange of letters. If you would write me, upon the receipt of this letter, that you would be willing to take some active part in these conferences next July, I should be very glad because then an announcement to that effect can be put in our preliminary announcement of Summer School plans. The many Americans who have interested themselves in problems of stellar and solar atmospheres will be exceedingly glad to know that you are coming to America; and many would appreciate an opportunity of hearing you discuss problems in the field where your pioneer work was of great significance.

Princeton University will be closed throughout the summer – from about June 20 to nearly October 1 – but you will get to meet Russell, Theodore Dunham, and several of the Princeton mathematicians and physicists here at Harvard either during the Summer School or during the Tercentenary Conferences in September.

The major astrophysicists of the Mount Wilson Observatory are likely to be East during the summer, but the observatory there is in operation the whole year, of course. It would be quite possible for you to spend July here in Cambridge, go to Mount Wilson for at least a part of August, and return for the meeting of the American Astronomical Society here at Harvard on September 3.

I shall see that you are later supplied with more detailed information concerning these symposia.

Princeton University will be closed throughout the summer – from about June 20 to nearly October 1 – but you will get to meet Russell, Theodore, Dunham and several of the Princeton mathematicians and physicists here at Harvard either during the Summer School or during the Tercentenary Conference in September.

The major astrophysicists of the Mount Wilson Observatory are likely to be East during the summer, but the Observatory there is in operation the whole year, of course. It will be quite possible for you to spend July here in Cambridge, go to Mount Wilson for at least a part of August and return for the meeting of the American Astrological Society here at Harvard on September 5.

The second week of September will have programs that include special symposia on Atomic Structure, on Cosmic Radiation, and on the theoretical physics.

Unfortunately Dr. Menzel will be away at

the eclipse in Siberia next summer, but hopes to return by the 1st of August. At the present time Dr. Chandrasekhar is here for a few months, and Dr. Mulders will be here probably until you come. Dr. Swings of Liége is also here, but returns soon to Belgium.

We should like to hear at your convenience if you plan to bring to America the members of your family.

With assurances of my pleasure to learn that you are coming to visit and study with us, I am

Very sincerely yours,

Harlow Shapley

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Professor M.N. Saha
C/o Professor E.A. Milne
Oxford University
Oxford, England

March 5, 1936

My Dear Professor Saha:

I have your letter of February 12. I am glad to see that the tentative plans I made in my letter of December 23 will be satisfactory for you. I am having your name listed as one of the seminar leaders in one of the courses given during our Summer School. Dr. Rupert Wildt will be the principal lecturer in this course and will be here throughout the summer. Subject to your convenience (we can arrange these details after your arrival in America) I shall ask if you can conduct two of the meetings during one week in July – perhaps two or three weeks after your arrival. We have entitled this series of seminars "Problems in Astrophysics." That gives the seminar leader a wide scope. In fact, I think Dr. Wildt will speak largely on planetary atmospheres. I think it would be appropriate for you to speak twice (or only once if you would prefer) on this general subject you have given me — Stellar Radiation Problems. It is our custom to extend one of these lectures during our summer conferences up to an hour and a half, if the lecturer desires.

Also I shall use the same title –"On Stellar Radiation Problems"—for your contribution at the time of the September symposium on stellar temperatures and constitutions. This is the symposium of which Professor Pannekoek and Professor Russell will be the leaders, and the other contributors will be Dr. Otto Struve and Dr. Theodore Dunham. Your contribution at that time, which should, if feasible, contain whatever new results you have recently attained, should take not to exceed fifteen minutes for presentation, because of the general plan for that program. It might, therefore, well be a summary of the material that you present in more detail during your one or two lectures in our Summer School in July. The audiences at the two times will somewhat overlap,

so far as serious students are concerned, but at the September symposium the whole American Astronomical Society will be in attendance.

I judge from my correspondence with the Carnegie Corporation in New York that they take no active part in the administration of problems or programs of the British committee. I feel quite sure you will have no difficulty in arranging the details with the British committee, since this trip you have planned is obviously one of profit to yourself and to astrophysics. But if you find upon arrival in England that a letter directed from me to the Committee of the Carnegie Corporation in London would be of service, I shall be very glad to write such a letter promptly.

Looking up my files, I find I have already written to the British committee outlining the general plan, and I shall write them again now.

Looking forward with great pleasure to having you with us next summer, I am

Very sincerely yours,

Harlow Shapley

●●●

Professor M.N. Saha
232 Woodstock Road
Oxford, England

Dear Professor Saha:

I have your letter of May 4 and I am glad to know that you are definitely coming to America, possibly arriving here about July first. We shall be able to assign you an office by yourself, and I hope that you will find your visit here profitable. Dr. Bok will be away from Cambridge until about the seventh of July and of course Dr. Menzel does not return until about the tenth of August.

I can hardly believe there will be any objection from the British Committee of the Carnegie Corporation concerning your proposed visit to America. The Secretary of the Committee wrote me on the eleventh of March and also on the seventeenth of March, indicating that it would be appropriate if we suggested that while you were in America, at the Harvard Observatory during July, that you speak two or three times at colloquia for our advanced students and Faculty. There will also of course be many informal conferences, and I think you will find those with Dr. Rupart Wildt, Dr. Theodore Dunham, and Professor Russell of especial interest. Professor Russell is to be here only one week during the summer session, and will at that time talk on the technique of the analysis of spectra. He will return later for the Tercentenary Conference at which time he will speak on the masses of the stars. At that same Tercentenary Conference, Sept. 3, Dr. Pannekoek will speak on the temperature scale of the stars, and both Dr. Otto Struve and Dr. Theodore Durham will also be among those who present brief discussions on those subjects. We are expecting you to appear on

that particular astrophysical –symposium program. I think you are likely to hear from the Tercentenary office to the effect that your name, and the title of your remarks, will appear on the final program of the Conference. Because of the pressure of time on the program your discussion in the September meeting will need to the restricted to not more than fifteen minutes; but during the summer (July) colloquia there is no time limit and we shall be glad to hear from you in detail about your current thoughts and investigations. We shall want to have one of your colloquia during the week that Professor Russell is here; at that time also Dr. Merrill of the Mount Wilson Observatory will be discussing, astrophysically, some of the problems of variable stars. Dr. Wildt's discussions will feel in general with stellar and planetary atmospheres. We would like to know, as the time approaches, on what boat you are arriving and at what port. We shall try to expedite your entrance into the country.

Sincerely yours

Harlow Shapley

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Dr. M.N. Saha
92, Upper Circular Road,
Calcutta India

February 14. 1947

My dear Saha,

I am home and trying to catch up. I found our local astrophysicists very much interested as I was, in the exchange with Professor Plaskett. Knowing of his deep interest in the matter I sent the account on to Russell in Princeton. He has now returned it, and you may be interested in excerpts from his comments:

> Saha's history of his life is really interesting, and as good an example as I know of the importance of providing assistance to a competent man. His history affords a very good argument that the Indian universities and perhaps the Government should be actively interested in securing proper opportunities for men of proven ability. If it would be of any use to you to quote me to that effect you are most welcome to do so. I have always been careful to give Saha the credit for starting the astrophysical application of the ionization theory, I do not think I knew until seeing this letter how much he had done previously in India.

It looks as though we may have two or three first-class graduate students in astrophysics here at the Harvard Observatory as the result of my trip to India. Perhaps we may also be able to make some provision for young Vainu Bappu of

Hyderabad within a year or so.

Please give my regards to your excellent family, and let me assure you again that I profited greatly from my visit to Calcutta under your guidance.

Sincerely yours,

Harlow Shapley

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May 3, 1948

Dear Dr. Saha,

About two weeks ago I mailed to you by ordinary mail the manuscript and figures for my articles entitled "Time and change in the Metagalaxy". I hope you will let me know at once of its arrival. As I have written you before, we are not asking for reprints of this paper because in another version it will be printed in an American journal from which we shall get reprints for the Harvard Observatory reprint series.

On May 11, we are going to have a big party at the Observatory and in my house for the Indian students of the Boston area, and also the Latin-American Students. We shall mix South America with Asia and see what we get out of it. Dr. H.K. Sen will be at the party and will contribute music on the Indian instrument which he is able to borrow from a Swami over in Boston. He plays very interestingly, and I am sure the Latin-American students will be much pleased to see and hear this novelty.

About half of dozen of us will go to the meetings in Zurich; others will go to meetings of the American Astronomical Society in Pasadena at the end of June. In Washington this past week I began the negotiations which will probably end in the enlargement of our scientific exchange bureaus here at the observatory. We shall probably be able to supply certain scientific papers, summaries, and reviews to the scientific journal editors of Asia, if they want them; and perhaps aid the scientists of Asia in various ways.

With best wishes and the hope that I shall be seeing you again soon, I am

Very sincerely yours,

(Harlow Shapley)

Corrections to use: Please enter in ms

[1] page 3, fourth paragraph omit first four words, "in each galactic system".

[2] Page 5, line 6, for ten years read "Fifteen years"

[3] PP 16 & 17- the text is OK, but the letters c and d shall be deleted at beginning of paragraphs

[4] P 10 line 5 second word: for plans read plane

[5] P 9 second paragraph correct coordinates are: $\lambda = 105^{o}$; $\beta = +58^{o}$

●●●

Prof. Harlow Shapley
Director, Harvard College Observatory
International Astronomical Union
Rome

5th September, 1952

My dear Shapley,

I understand from Arthur (Shapley) who had been here for two days with us that you would be coming to Rome for the International Union of Astronomy and Astrophysics. I regret very much that I am losing this opportunity of meeting you this time.

We were very happy that Arthur spent two days with us. We would have been very glad if he could spend a few days more here. He delivered a lecture on his work on the Ionosphere done at Alaska and other places and their correlation with solar phenomena. This excited a good deal of interest and we all appreciated the very good work which he is doing. Our workers here wanted to have more time with him, but he had to catch his plane for Rome, which he very nearly missed due to an accident. I hope he reached safe.

I have told Arthur re: a paper I published in *Nature* in which I predicted the occurrence of the nuclear Hydrogen line $\lambda = 21$ cm. as early in 1946. It was discovered only last year by Ewen and Purcell in Harvard and by others in Holland. I came to know from their preliminary publications that Van der Hulst had predicted it just a year earlier. I was trying to assemble apparatus in order that I might verify the prediction, but certain friends here saw that I would not get the financial assistance I needed and hence the work could not be pushed. I am still particularly interested to know how the work on emission of the H-line $\lambda = 21$ cm. from different parts of space is being carried on, because I found that my line of thought is different from Van der Hulst's.

You must have learnt from Arthur about some of my present activities in which neither you nor other astronomers can be interested. But I wish to assure you and other friends that inspite of other pre-occupations, my interest in astronomy and astrophysics remains unabated.

Hope you will find some time to revisit India.

Yours sincerely,

(M.N. Saha)

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Prof. H. Shapley,
Director, Harvard College Observatory,
Cambridge, Mass., U.S.A.

2nd April, 1953

My dear Shapley,

I learnt from your son, Arthur who spent two days with me at Calcutta, that you are retiring from the Directorship of the Harvard College Observatory. I hope this has not taken place as yet. Nobody can replace you and naturally I am anxious to know who is taking your place. The world of science is indebted to you for the great achievements of the Harvard College Observatory during your regime, due to your inspiration and leadership.

The other day I read an article in the Time Magazine which mentions that a far ultra violet spectroscope was sent up in an Aerobee rocket and it reached a height of 50 km. A good spectrum was secured which showed prominently the Lyman alpha line 1216 Å U in emission. But I am unable to verify the news from any scientific journal. I am naturally anxious whether this is a correct report because you might remember that I published a short report in the Harvard Observatory Bulletin during my stay in your Observatory in 1936 in which I predicted the existence of the Lyman series of hydrogen in the solar ultra violet spectrum and also of the series lines of ___ and suggested photographing the solar spectrum beyond the ozone layer. If this report be correct, it will mean a very important addition to our knowledge on solar physics. It will also throw light on the formation of the ionosphere due to the ultra violet spectrum of the sun and would enable us to verify whether the different layers of ionisation in the ionosphere are due to distinct emission lines in the ultra violet spectrum of the sun or by the continuous spectrum.

I shall be glad if you can kindly give me the detailed information.

Hoping you are alright and with kind regards,

Yours sincerely,

Meghnad Saha

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4.12 LETTERS FROM ARTHUR HOLLY COMPTON: 1936-1937

THE UNIVERSITY OF CHICAGO
Ryerson Physical Laboratory

Professor M.N. Saha, F.R.S.,
C/o Thomas Cook & Sons
Berkeley Street
London, England

May 5, 1936

Dear Saha:

We are much pleased to learn that you are coming to the United States this summer. If your travels should take you through Chicago, I hope you will look us up. Our University is one of the few which is in regular session during the summer period. Our session closes about the end of August, just before you will be going to Harvard where I shall hope to see you again.

Among the other active physics laboratories in the United States you will find Cornell University at Ithaca, Columbia University at New York, University of Michigan at Ann Arbor, and the University of California at Berkeley. Most of the other leading universities have their activities on a relatively much reduced scale from the middle of June until about October 1.

I should be pleased if you can let me know approximately the time when you shall be in this part of the country.

Yours sincerely,

Arthur Compton

Arthur H. Compton

●●●

Professor M.N. Saha, F.R.S.,
232 Woodstock Road
Oxford, England

June 3, 1936

Dear Professor Saha:

I am pleased to know from your letter of May 20 that we may hope to see something of you in Chicago this summer. Unfortunately I shall myself be out of residence during August when you hope to pass through Chicago, though Gale, Mulliken and Eckart are planning to be in residence. Likewise you know our Yerkes Observatory is only about 80 miles from Chicago beside a very delightful lake in Wisconsin, where Otto Struve and his group would be very pleased to welcome you. Though I shall have to miss you here, I shall look forward to seeing you in Cambridge in September.

Under separate cover I have sent you some reprints of our recent papers dealing with cosmic rays.

With kindest regards,

Yours cordially,

Arthur H. Compton

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Professor Megh Nad Saha
Allahabad University
Allahabad, India

February 15, 1937

Dear Professor Saha:

May I thank you for sending me the detailed information which I requested for use in connection with the Nobel nomination[25]. You will be pleased to know that I have received an acknowledgement from them of my suggestion. I want to thank you also for your kindness in sending me copies of your books, "Modern Physics", and "Treatise on Heat".

With hope for the best success,

Yours sincerely,

Arthur H. Compton

25 Also see Saha's letter to Neils Bohr of Sept 30, 1936, about the Nobel nomination

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4.13 LETTER FROM RUDOLF ORTVAY: 1936

Kir. Magy. Pázmány Péter Tudományegyetem Elméleti Fizikai Intezete

Institut für Theoretische Physik der Universitat
University: Department of Theoretical Physics
Direktor: Prof. R. Ortvay

Budapest, VIII, Muzeum-Körút 6-8
(ungarn, Hungary)
16 April 1936

Residential Address: Budapest II
Gábor Áron utca 18

Highly respected Mr. Professor,

I would like to let you know that I have received your postcard of 4.3.1936 from Allahabad and sent an invitation to Istanbul to Cook. Since I didn't receive a response I turned to my respected teacher Geheimrat Sommerfeld, because it was known to me that you are in touch with him and found out that you had to change your travel plans and have not travelled through Istanbul and Budapest.

I only want to assure you, respected Professor, that I and my colleagues had eagerly looked forward to your visit and would have gladly heard from you about your research or about the recently wonderfully blossoming Indian Physics — were it to be in the course of a lecture or in the form of informal discussions.

The interest from my side was particularly great because over many years I have been interested about India and indeed in the old culture as well as in the more recent attempts to come to terms with European spirit. I have been toying for a long time with the idea of visiting India. Therefore it would have been very pleasant for me to discuss with you the possibilities and organization of such a trip.

In the hope of getting to know you personally when the circumstances present themselves, I remain very respectfully

Your highly obliged,

R. Ortvay

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4.14 LETTERS BETWEEN NIELS BOHR AND SAHA: 1936–1946

Universitets Institut for Teoretisk Fysik
Blegdamsvej 15, Kobenhavn 0

den May 19th 1936

Dear Saha,

It was a great pleasure from your kind letter to learn that you may be able to join our informal conference on atomic physics, which is planned for the week from June 14th to June 20th. I need not say how welcome you shall be, and how much we shall all appreciate your taking part in our discussions.

With kindest regards,

Yours sincerely,

Niels Bohr

●●●

From London,
Sept 30 – 1936
Address:
From Oct 3 – Oct 8
C/o Dr. R.K. Das
89 Rue du Lausanne, Geneva

Dear Prof. Bohr,

I have the pleasure of enclosing for you under separate cover a copy of "Science and Culture"- popular scientific journal now being published at Calcutta. I think that you will be interested in the account of the Nuclear Conference held at Copenhagen which I had the pleasure of attending. I hope we did not make many mistakes in our report. Another article in which you may be interested is to be found on page 118, by an ex-pupil of mine, now Professor at Delhi University (see remark, p. 119, footnote).

I wish to write to you on another matter of some delicacy. Recently I have been travelling in America, and met many astronomers and physicists. They were kind enough to tell me that I have not been given proper recognition for my work on Astrophysics which was a pioneering work, and opened up a great field in Astrophysics. There have been much mathematical elaborations to my work, but no new physical thought has yet

been introduced. I may invite your attention to the following passage in Rosseland's Astrophysics (1936), p. xvi

"The impetus given to astrophysics by Saha's work can scarcely be overestimated as nearly all later progress in this field has been influenced by it, and much of the subsequent work has the character of refinements of Saha's ideas".

I have never met or corresponded with Rosseland, and the above remark may be regarded as a spontaneous tribute.

Prof. A.H. Compton has been kind enough to discuss the matter with me, and subsequently he has written to me that he is going to send up my name for the award of the Nobel Prize, and he has secured the assent of Dr. I. Langmuir. Probably he will also get more support. He asked me to secure some support from the Britishers and others. Our relations with the Britishers are a bit delicate and I do not know whether I shall be wise in writing to them. Moreover I am sailing for India on Oct 3, hence I shall have very little opportunity of meeting any of them & I do not wish to commit myself in writing.

I shall be glad if you can see your way in supporting Prof. Compton's proposal. I shall be glad to give any further information, if you are kind enough to take up the matter.

With kind regards,

Yours sincerely,

M.N. Saha

●●●

Dr. M.N. Saha, D.Sc., F.R.S.
Professor of Physics.

Physics Department,
Allahabad

Prof. Niels Bohr,
Institut fur Theoretische Physik,
Blegdamsweh 15, Copenhagen, Denmark.

Dear Prof. Bohr,

I have learnt with great pleasure that you have accepted the invitation of the Indian Science Congress, to be present during the Silver Jubilee session in January, 1938 Indian Science will be honoured by your presence during this unique occasion.

Allahabad is not unfortunately included in the itinerary of the foreign delegates attending the Silver Jubilee, as other cities are more interesting from the tourists' point of view. But it will be a great disappointment to me personally if you do not spend a day at my house at Allahabad. I think that it can be done without much disturbing the general arrangements. The delegates, after

visiting Delhi will take the eastward bound train for Benares. This can be done either via Lucknow or via Allahabad. If you take any evening train via Allahabad, you can reach Allahabad early morning, have your breakfast and lunch at my house, and after some rest, and a visit to my laboratory and to other local places of interest we can drive in private motor cars in the afternoon to Benares, a distance of 80 miles. The road is quite good, and the drive will be quite pleasant.

Though Allahabad is not so famous as Agra, Delhi or Benares, it has got some very interesting old sites. There is an old fort, a good archaeological museum, and it is at the Junction of two very famous rivers of India, the Ganges and the Jumna.

I shall be very glad if you can kindly accept my invitation. I shall send one of my colleagues to guide you from Delhi or Agra.

There would be a good collection of local physicists to welcome you here. Dr. D.S. Kothari (Astrophysics), Dr. R.N. Ghosh (Acoustics), Dr. Toshniwal (Wireless Telegraphy), Dr. P.K. Kichlu (Spectroscopy), Dr. R.C. Majumdar (Astrophysics), and several others. I personally shall feel greatly honoured if you can kindly accept my invitation. If it is found necessary to change the times to suit the programme, it will be attended to.

Looking forward to the happy occasion,

With kind regards,

Yours sincerely,

M.N. Saha

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Netherland Plaza
Cincinnati

Dec 24/1944

Dear Prof. Bohr,

I heard in England that you escaped from Denmark, and you are at present in this country. Dr. Vanevar Bush told me that you are probably in Princeton, and I am writing this letter in the off-chance that it may somehow reach you.

First of all, let me express our sincere joy that you escaped from Nazi domination.

I have been touring this country as member of a Scientific Mission sent by Govt. of India. We expect to be in Princeton sometime between January 29 and February 2. I shall be glad if you can kindly give me 1/2 hour's talk while I am there.

My immediate objective in contacting you is to get some light on some problems on which you worked 32 years ago. I have recently published a paper "On a Physical Theory of the Solar Corona" in which I have tried to provide an explanation of the extraordinary work of Edlen. Unfortunately, I have only one copy of my paper which was

published in India, but I have arranged that a popular version of this paper, prepared at the Geophysical Institute, Washington, be sent to you.

I have adduced arguments to prove that the highly ionised Fe and other atoms in the Corona are due to a nuclear process akin to or identical with Uranium Fission. This brings to the forefront the problem of passage of charged particles through the solar atmosphere, their loss of energy due to ionisation by collision (treated by you 32 years ago, and lately at Copenhagen (1940)), the capture of electrons by these particles (work of Kramers & Brinkmann – *Amst. Proc. 1930*). I am familiar with most of the literature on the subject since published, but I am not satisfied that we have satisfactory solution in all cases, and I am praying for a talk with you to get further illumination on the subject.

With kindest regards,

Yours sincerely,

M.N. Saha

●●●

Mount Royal,
Marble Arch,
London, W.1.
C/o Royal Society,
London

June 14/1946

Dear Prof. Bohr,

I have come here as member of an Indian Scientific delegation to participate in the Commonwealth Scientific Conference, and staying up to July 22nd. We were very glad to learn from the Royal Society that you would be coming here to participate in the Newton Tercentenary. I am really fortunate in having this opportunity of contacting you once more.

During the War, we in India have been deprived of contact with scientific work in other countries, since 1939. I shall be very grateful if you can kindly direct your secretary to send to us reprints of papers from your Institute – by yourself & your colleagues – Moller, Rosenthal, Jakobson, and others, published in the Proceedings of the Royal Danish Academy and elsewhere. They may be sent to the Royal Society Address.

Conditions of travel are not yet very favourable and time at my disposal is short, otherwise I would have liked to come to Copenhagen and spend a few days at your laboratory.

With respectful greetings,

Yours sincerely,

M.N. Saha

●●●

C/o Royal Society
London

July 18/1946

My Dear Prof. Bohr,

I wonder whether it will be possible for you to spare about half an hour for a talk with you. You are so much surrounded by people that it is difficult to get access to you.

The matters on which I want to talk are

a) My paper on the solar corona and some points arising out of it.
b) About my plan of sending my son A. K. Saha to your Institute, next year. He is a good mathematician and experimentalist, and has worked on β-ray spectra, potential barriers etc....He has submitted his Thesis for D. Sc. at Calcutta, and would not try for any degree in Europe, but would go about freely contacting great minds like yourself.

Yours sincerely,

M. N. Saha

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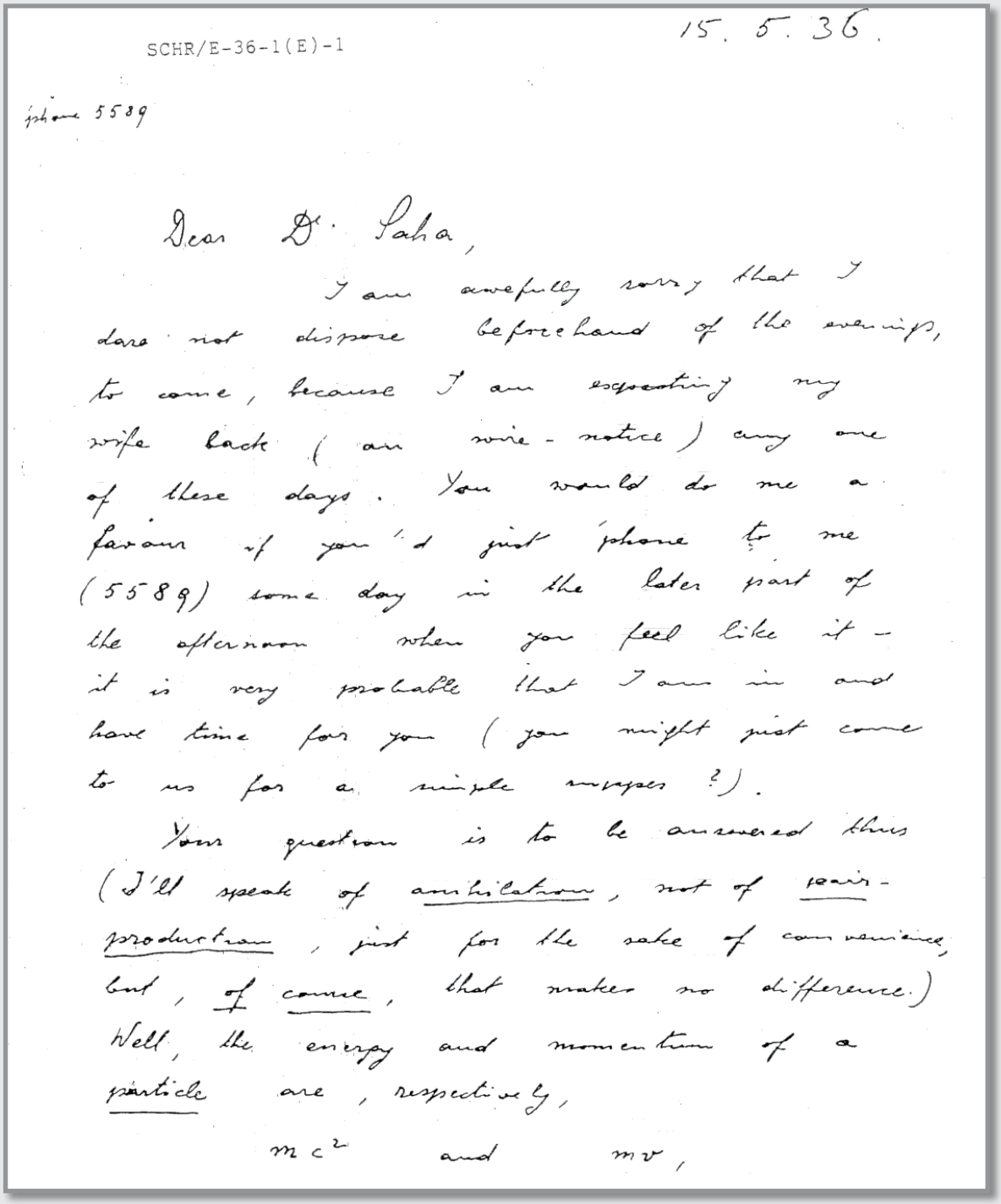
4.15 LETTER FROM ERWIN SCHRÖDINGER: 1936



SCHR/E-36-1(E)-1

15. 5. 36.

phone 5589

Dear Dr. Saha,

I am awefully sorry that I dare not dispose beforehand of the evenings to come, because I am expecting my wife back (an wire-notice) any one of these days. You would do me a favour if you'd just phone to me (5589) some day in the later part of the afternoon when you feel like it - it is very probable that I am in and have time for you (you might just come to us for a simple supper?).

Your question is to be answered thus (I'll speak of annihilation, not of pair-production, just for the sake of convenience, but, of course, that makes no difference.) Well, the energy and momentum of a particle are, respectively,

mc^2 and mv,

where m is the mass in motion $\left(m = \frac{m_0}{\sqrt{1-\beta^2}}\right)$.
The energy and momentum of a light-quant are, respectively

$$h\nu \quad \text{and} \quad \frac{h\nu}{c}$$

Thus, in the case of a particle, the momentum is obtained by multiplying the energy by $\frac{1}{c}\cdot\frac{v}{c}$, in the case of the light-quant just by $\frac{1}{c}$, which is always larger than $\frac{1}{c}\cdot\frac{v}{c}$. Hence, if two particles transform into a light-quant with energy = to the sum of their energies, the latter, i.e. the light quants energy is so large, that its momentum (obtained by multiplying the energy with $\frac{1}{c}$) cannot by furnished entirely by the two particles, even if they moved parallel. —

But what is possible, is the production of two light-quanta, because the vector-sum of their momenta, can, of course, be anything down to zero. To find out

in detail, what they are like, use the system of coordinates in which the resultant momentum of the particles is zero. Then in this system, the two light-quanta must be exactly opposite in direction and equal in frequency; about half a million volts, in the case of electron and positron.

I am not quite sure, but I believe, that this radiation has been observed. I think, that the probability has been estimated, but I could not tell you anything about it offhand.

Yours very sincerely

E. Schrödinger

24, Northmoor Road,
Oxford
15.5.36

phone 5589

Dear Dr. Saha,

I am awfully sorry that I dare not dispose beforehand of the evenings, to come, because I am expecting my wife back (a wire-notice) any one of these days. You would do me a favour if you'd just phone to me (5589) someday in the later part of the afternoon, when you feel like it – it is very probable that I am in and have time for you (you might just come to us for a simple supper?).

Your question is to be answered thus (I'll speak of annihilation, not of pair-production, just for the sake of convenience, but, of course, that makes no difference.).

Well, the energy and momentum of a particle are, respectively,

mc^2 and mv,

where m is the mass "in motion"

$$(m = \frac{m_o}{\sqrt{1-\beta^2}})$$

The energy and momentum of a light-quant are, respectively,

$h\nu$ and $\frac{h\nu}{c}$.

Thus, in the case of a particle, the momentum is obtained by multiplying the energy by $\frac{1}{c} \cdot \frac{v}{c}$, in the case of the light-quant just by 1/c, which is always larger than $\frac{1}{c} \cdot \frac{v}{c}$. Hence, if two particles transform into a light-quant with energy "=" to the sum of their energies, the latter, i.e. the light-quant's energy is so large, that its momentum (obtained by multiplying the energy with 1/c) cannot by furnished entirely by the two particles, even if they moved parallel.

But: what is possible is the production of two light-quanta, because the vector sum of their momenta can, of course, be anything down to zero. To find out in detail, what they are like, use the system of coordinates in which the resultant momentum of the particles is zero. Then, in this system, the two light quanta must be exactly opposite in direction and equal in frequency; about half a million volts, in the case of electron and positron.

I am not quite sure, but I believe, that this radiation has been observed. I think, that the probability has been estimated, but I could not tell you anything about it offhand.

Yours very sincerely,

E. Schrödinger

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4.16 LETTERS FROM ERNEST RUTHERFORD: 1936

Cavendish Laboratory,
Cambridge

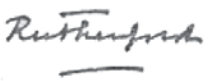
8th May, 1936

Dear Saha,

Many thanks for your good wishes over the Cavendish windfall due to Sir Herbert Austin. It will give us an opportunity of rebuilding part of the Laboratory, and I hope funds for the development of further researches. I must confess I did not expect that the sum would come before we had properly issued the appeal we had in view.

If you are coming to Cambridge at any time, I shall be very pleased to have a chance of a chat with you.

Yours sincerely,



Rutherford

●●●

Chantry Cottage,
Upper Chute,
Andover, Hants

June 21, 1936

Dear Professor Saha,

Since I saw you in Cambridge, I have received formal letters from the Secretary of the Indian congress and also from the Secretary of the B.A. asking me to be the President of the combined meeting in India in 1938. I feel that the holding of such a joint congress is likely to prove of much value both to this country and to India in improving personal relations and for these reasons I am inclined to consider the proposal favourably. Naturally at this early stage, I cannot make a final decision for so much depends on the question whether adequate funds will be available to defray a considerable part of the expenses of a strong group of scientific visitors from this country for it is of first importance that the best of our scientific people should be encouraged to make the journey and to see something of India and its scientific institutions.

In my own case, I hope it will be possible for Lady Rutherford to accompany me to look after me. I shall be interested to hear of the results of your interview with the Marquis of Zetland.

I return to Cambridge on June 29 and my usual address will suffice.

Yours sincerely,

Rutherford

I have not replied to the Secretary of the Indian Congress and will not do so until matters are more advanced.

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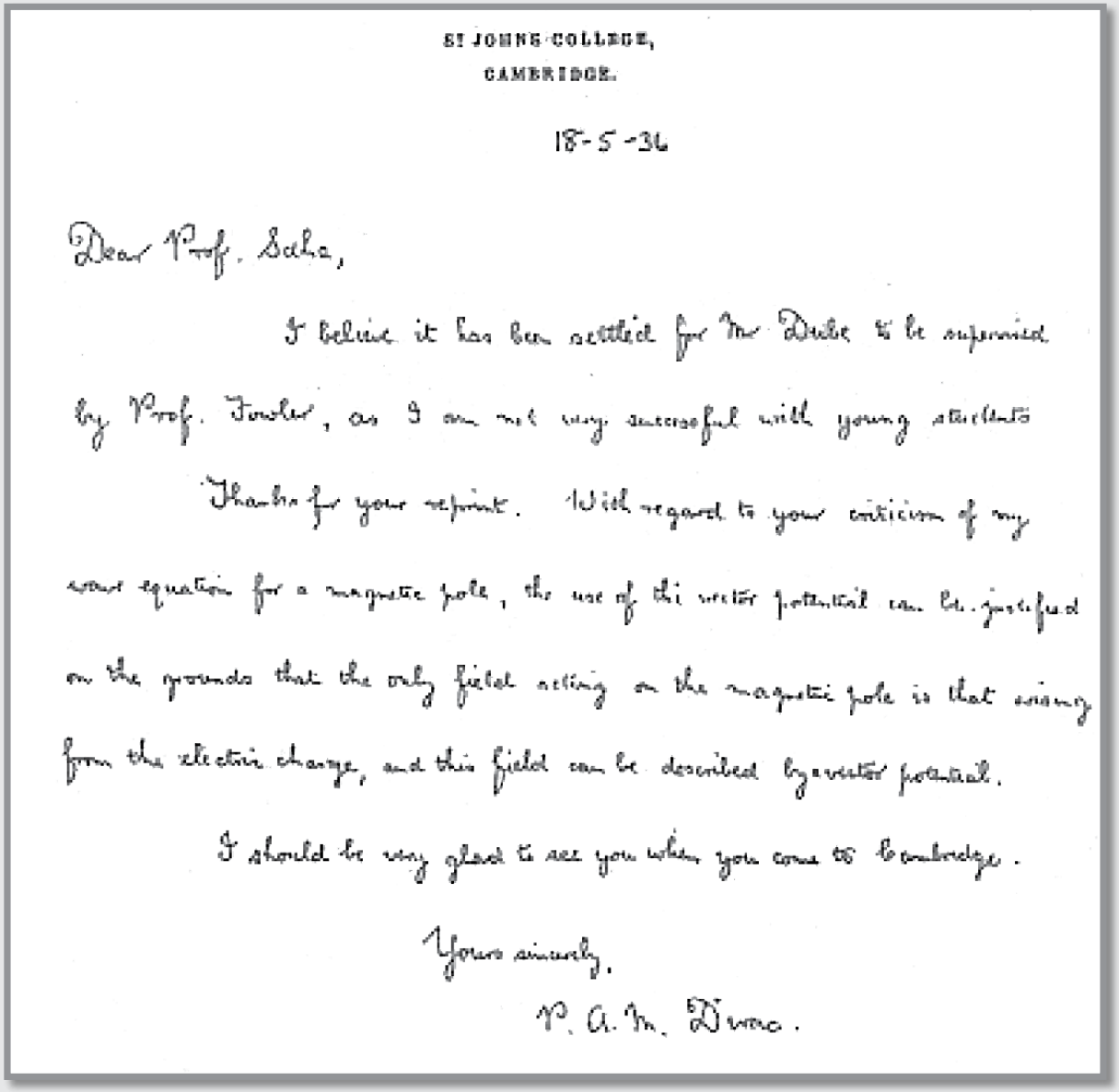
4.17 LETTERS BETWEEN PAUL DIRAC AND SAHA: 1936–1954



ST JOHNS COLLEGE,
CAMBRIDGE.

18-5-36

Dear Prof. Saha,

I believe it has been settled for Mr Dirke to be supervised by Prof. Fowler, as I am not very successful with young students

Thanks for your reprint. With regard to your criticism of my wave equation for a magnetic pole, the use of the vector potential can be justified on the grounds that the only field acting on the magnetic pole is that arising from the electric charge, and this field can be described by a vector potential.

I should be very glad to see you when you come to Cambridge.

Yours sincerely,

P. A. M. Dirac.

St. John's College
Cambridge 18.5.36

Dear Prof. Saha,

I believe it has been settled for Mr. Dube to be supervised by Prof. Fowler[26], as I am not very successful with young students.

Thanks for your reprint. With regard to your criticism of my wave equation for a magnetic pole, the use of the vector potential can be justified on the grounds that the only field acting on the magnetic pole is that arising from the electric charge, and this field can be described by a vector potential.

I should be very glad to see you when you come to Cambridge.

Yours sincerely,

P.A.M. Dirac

26 R.H. Fowler, who was adviser of Dirac himself and Chandrasekhar, among others

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December 16, 1954

My Dear Prof. Dirac,

Thank you for your kind letter of the 14th December. Since you will be able to spend two or three days in Calcutta, I think it would be better if you can deliver a course of 2 to 3 lectures sometimes in January. As our boys are very keen to hear you, I am making you this special request and I hope it will be possible for you to arrange your programme accordingly. In case this arrangement suits you, please let me know so that I can make necessary arrangements at my end.

Yours sincerely,

M.N. Saha
Director

Prof. P.A.M. Dirac,
Tata Institute of Fundamental Research,
Apollo Pier Road, Bombay–1

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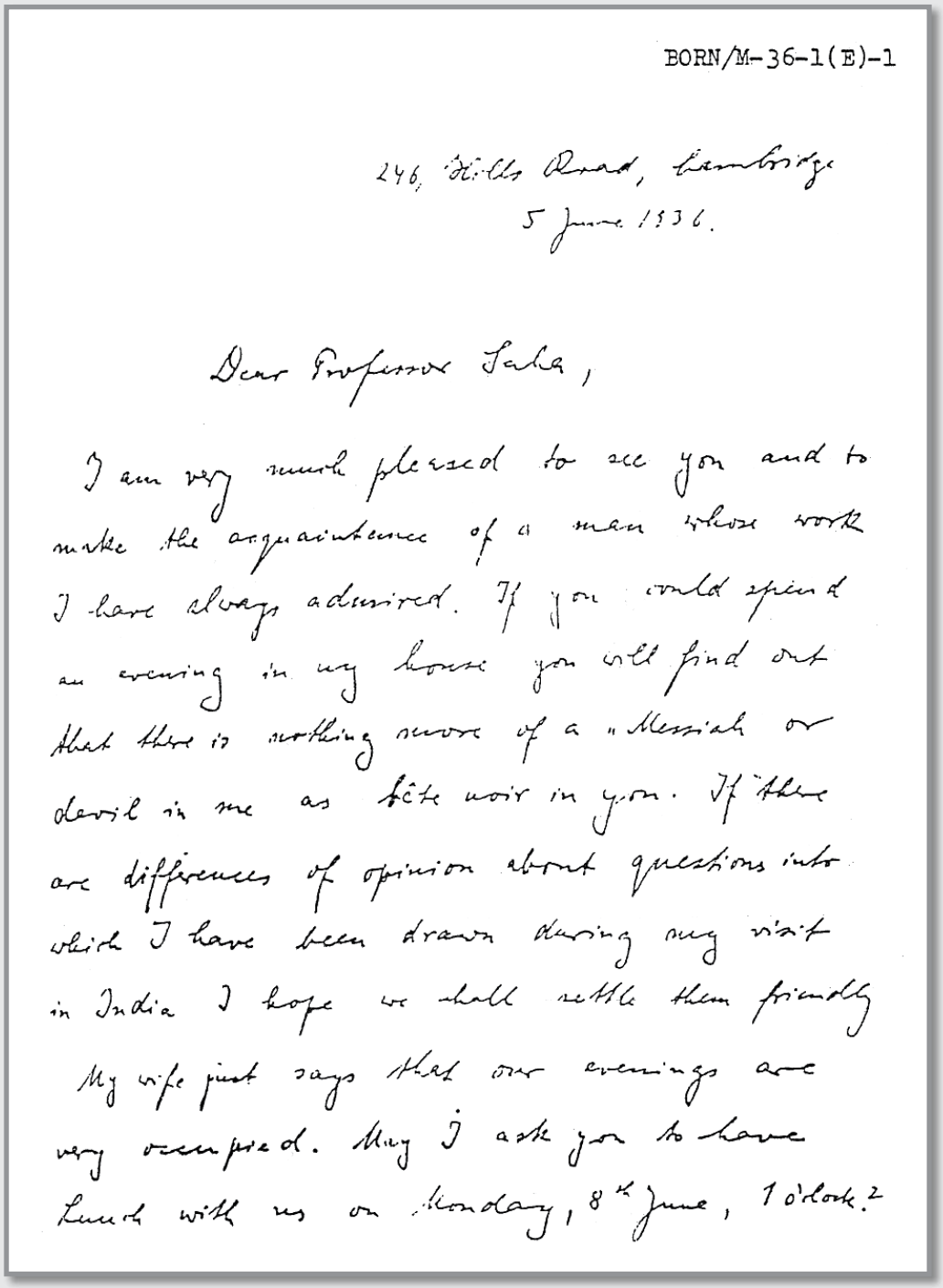
4.18 LETTERS FROM MAX BORN: 1936–1948



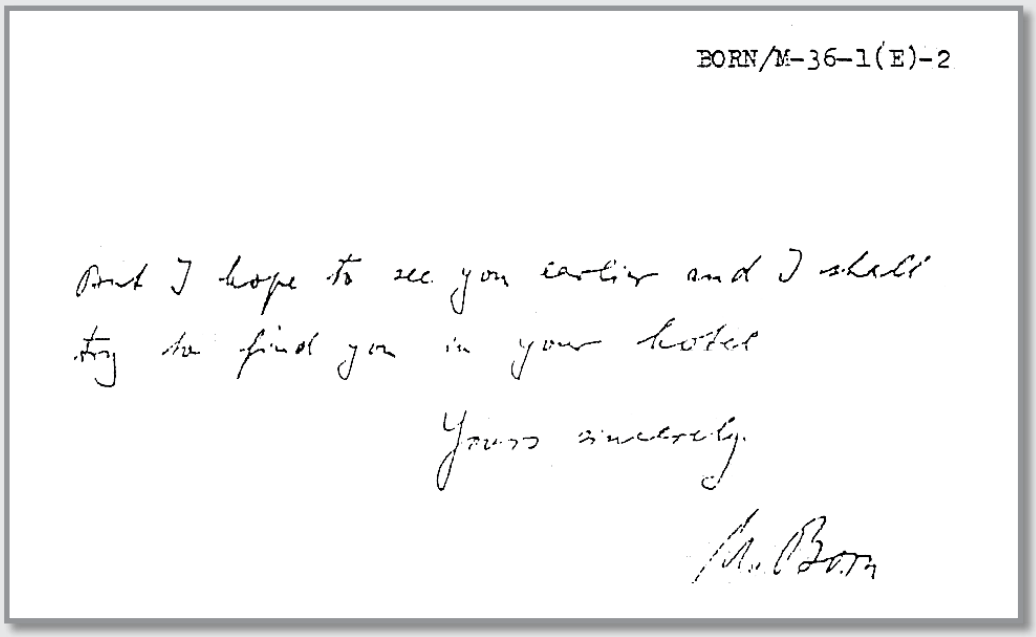
BORN/M-36-1(E)-1

246, Hills Road, Cambridge
5 June 1936.

Dear Professor Saha,

I am very much pleased to see you and to make the acquaintance of a man whose work I have always admired. If you could spend an evening in my house you will find out that there is nothing more of a "Messiah or devil in me as fête noir in you. If there are differences of opinion about questions into which I have been drawn during my visit in India I hope we shall settle them friendly

My wife just says that our evenings are very occupied. May I ask you to have lunch with us on Monday, 8th June, 1 o'clock?



BORN/M-36-1(E)-2

And I hope to see you earlier and I shall
try to find you in your hotel

Yours sincerely,

M. Born

246, Hills Road, Cambridge 5 June 1936

Dear Professor Saha,

I am very much pleased to see you and to make the acquaintance of a man whose work I have always admired. If you could spend an evening in my house you will find out that there is nothing more of a "Messiah" or devil in my as bête noir in you. If there are differences of opinion about questions into which I have been drawn during my visit in India I hope we shall settle them friendly.

My wife just says that our evenings are very occupied. May I ask you to have Lunch with us on Monday, 8th June, 1 O'clock? But I hope to see you earlier and I shall try to find you in your hotel.

Yours sincerely,

M. Born

●●●

Professor M. Born, F.R.S.
Department of Mathematical Physics
(Applied Mathematics)
The University Drummond Street
Edinburgh, 8

Professor M.N. Saha, F.R.S.,
12/1 Keyatala Lane,
P.O. Rashbehary Avenue,
Calcutta 29

30th September, 1948

Dear Saha,

Thank you very much for your letter with the question about that differential equation[27]. I do not know this type and therefore I communicated with Professor Whitaker. He told me that he had never seen this particular equation but that it is a special case of one dealt with in Whitaker & Watson, 4th Edition, Chapter 10, Section 10.6, Page 203. There is created a differential equation with 5 singular points, 4 of them finite and one

27 See Saha's letter to Lennard-Jones about this equation

in infinity, and it is shown that all the usual differential equations of mathematical physics can be obtained by suitable confluences of the 5 singularities. It is clear that your case can also be obtained in this way, reducing the 5 to 2 singularities by confluence, but this will not help too much because no general theory of this equation is given.

I think that if Whitaker does not know about it, nobody will. Therefore it will be necessary to direct some good mathematician to investigate it systematically which seems to me not a particularly difficult job. I have at present nobody here who could do it, but there are plenty of good mathematicians in India whom you could ask.

I was glad to have this opportunity of hearing from you. I have just been in Germany, the first time after a 15 years absence. The situation seems not to be worse than it was a year ago, but I felt estranged, and am glad to live in Britain.

I see your son occasionally and he seems to do very well.

With kind regards, also from my wife,

Yours sincerely,

M. Born

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4.19 LETTERS BETWEEN HARRY PLASKETT AND SAHA: 1946-1947

University College of Science
Department of Physics
92, Upper Circular Road

Calcutta 21 December, 1946

My dear Plaskett,

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 a persistent effort on the part of British scientists to minimize (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 being 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 ionization 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 laboratory. 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 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 some time 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 acquainted with several treatises on Thermodynamics, Plank, Sackur and above all Nernst's Das Neue-Warmesatz. I also got acquainted with Bohr's and Sommerfeld'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 pressure 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 liked 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 printings but never heard anything more about the publication of this paper nor was it returned to me. Years afterwards in 1936, when I visited the 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, 438 (1921)* because in his first paper on the subject in ***Month. Not. R. Ast. Soc., Vol. 64,*** Astrophysical determination of average of an excited Calcium atom, he mentioned by 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 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 be 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 this basis, he used very artificial values of ionisation potential. Later, when I met him he told me that he had not realised the importance of Bohr's theory or of Franck and Hertz's experiments, and bad 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 Clarke's book. His knowledge of the problem of the Stellar interiors was derived from a talk given by Kohlochutter in the Berlin Physicale Colloquium on Eddington's a 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 ionization 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 Eng1and; 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 Kirchhoff'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 the 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 on 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 colleague and a guest, never as a student. I have very great respect for him as a scholar, as a man, as an astrophysicist, 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 the 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.

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 verification 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 a work published in India (No. 5). He read it twice over without asking me a single question, and 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 flame; it could not be more than 10^{-60}, but after all these calculations were of a provisional nature, 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 me to get Nernst's permission for me to work at his laboratory. I received an encouraging reply from Eggert and Nernst himself and went to Berlin in February, 1921.

On arrival in 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 realised 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 out 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 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 cesium 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 observation of ionisation of cesium vapour in contact with heated tungsten filaments. The experiment was carried out in 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, and heard very good reports about my work which he has 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 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.

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6 January 1947.

University Observatory
Oxford

Mr 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 England 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 history of the subject is secure for all time. So much so indeed, that it seemed to me worthwhile 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 Lindmann's pioneer work in this same field. I do not share this doubt. It is true that Lindmann was the first to apply thermal ionization to the problem of the solar 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)

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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 intervened 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 chromospheres, 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 deducted 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 chromospheres. 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 professors 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. Durack, 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 predecessors Mr. Durack 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 practically 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 treadle 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 Society in 1927. This was then regarded in India as a rare distinction and I was congratulated by the Governor of the Province, Sir William S. 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 enabled 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.

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4.20 LETTERS BETWEEN EDWARD APPLETON AND SAHA: 1948

Department of Scientific and Industrial Research
Park House, 24, Rutland Gate, London, S.W.7

13/1/48

My Dear Saha,

This is to thank you for the most interesting reprints you have sent me.

I am glad to see that you and your pupils are tackling magneto-ionic problems in a rigorous manner, and greatly admire your mathematical skill. My original formulation of the theory was very crude and needs the hand of a real mathematician to put things right.

On the question of solar radio noise I expect you will be applying your theory to the thermal radiation from the corona at 104 degrees K. Martyn has written a short article in Nature on the subject and I am reading his fuller Proc. Roy Soc. Paper for him (he is in Australia) on Thursday next at Burlington House.

With kind regards and all good wishes for 1948 to you all.

Yours Ever,

E.V. Appleton

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25th February, 1948

My Dear Appleton,

Thanks for your letter d/- 13.1.48.

I am glad to receive your opinion about our work on the "Propagation of Electro-magnetic waves through the Ionosphere". Coming from a scientist of your eminence, your remark has been highly stimulating to my research scholars and they are now tackling other problems arising out of it, e.g. (i) propagation of e.m. waves over any general station (ii) Virtual and true paths for Chapman Layer.

We shall be very grateful if you can kindly send us reprints by yourself and your scholars from time to time. They will materially help us in our work.

Re: Origin of microwaves from the sun:- My paper deals only with the propagation of the microwaves through the solar atmosphere, but I have not yet tackled seriously the problem of origin

of the microwaves. I am unable to take seriously the suggestion that the corona has a temperature of the order of million degrees. The advocates of the view have to explain how such temperature can be produced in the corona. I have suggested alternative theories of the solar corona a few years ago in a lecture delivered to the Physical Society of London. Though criticisms have been offered against my view, I do not think they are serious enough, but I had not yet time to tackle them seriously.

With kind regards,

Yours sincerely,

M.N. Saha

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4.21 LETTER FROM SAHA TO PATRICK BLACKETT*: 1948

(*Plate 21)

10th September, 1948

My dear Prof. Blackett,

One of my students, Mr. S. Das has set up a cloud chamber for investigation of certain problems in Nuclear Physics. He needs one stereo-camera for photographing the tracks in his chamber. I shall appreciate much if you kindly permit Mr. S. K. Ghosh who is working in your laboratory at Manchester to construct one such for Mr. Das in your workshop there Mr. Ghosh knows all the necessary specifications of the Camera that will be suitable for our purpose. With your permission he can start the work. The cost of making of the camera will be borne by the Calcutta University.

Yours sincerely,

M. N. Saha

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4.22 LETTER FROM SAHA TO JOHN E. LENNARD-JONES: 1949

Sir J.E. Lennard-Jones, K.B.E. F.R.S.,
Plummer Professor of Theoretical Chemistry,
Middlefield, Huntingdon, Road,
Cambridge, England.

20th July, 1949

My dear Lennard-Jones,

I am just asking for a little favour from you. We are in need of a numerical solution of a particular differential equation which has come out as a result of my investigations on the propagation of radio waves through the earth atmosphere. The equation is quite new, like of which is not known in literature. I consulted many eminent mathematicians including Professors Max Born and Hartree and they told me that only way to get solution would be to obtain numerical one. I understand that you have got a machine.

I shall be extremely grateful if you can arrange for its solution. I am willing to pay charges for it. Full details of the equation are enclosed herewith.

With kind regards,

Yours sincerely,

Equations:

1) $\frac{d^2\phi}{dZ^2} + P(Z^2 - a^2)\phi = 0$

2) $\frac{d^2\phi}{dZ^2} + P\frac{(Z^2 - \alpha^2)(Z^2 - \beta^2)}{(Z^2 - \gamma^2)}\phi = 0$

where P, a, α, β, γ are constants.

Case I. P = 13.4, a^2 = 69.4, α^2 = 89.2, β^2 = 49.6, γ^2 = 70.6

under boundary conditions

(i) $\phi(0) = 1$, $\phi'(0) = 0$

(ii) $\phi(0) = 0$, $\phi'(0) = 1$

Solution necessary for the region $-20 \leq Z \leq +20$.

Case II. P = 16.0, a^2 = 763.9, α^2 = 805.6, β^2 = 722.2, γ^2 = 764.9

Under same boundary conditions as above.

Solution necessary for the region $-50 \leq Z \leq +50$.

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4.23 LETTERS FROM SAHA TO IRÈNE JOLIOT-CURIE: 1949

University College of Science & Technology\Institute of Nuclear Physics
92, Upper Circular Road, Calcutta-9

Prof. Madame Irène Joliot-Curie,
Director, Institute of Radium,
11 Rue Pierre Curie,
Paris.

December 17, 1949

Dear Madam,

We are very happy to receive your telegram confirming your visit to Calcutta after the Science congress session at Poona when Dr. Bhatnagar gives the consent on behalf of the Govt. of India. I have written an official letter to him, and I am very sure, he will agree. For this it may be necessary to make some changes in the dates of visit.

The construction of the building of the Institute of Nuclear Physics at Calcutta is just completed for which the funds came from the Central Govt. of India and the Govt. of Bengal. We wish to have a formal inauguration ceremony of the Institute towards the middle of January, 1950. The vice-Chancellor and the Syndicate members of the Calcutta University are very pleased to know that you have agreed to preside over the ceremony of official opening of the Institute. To us it is quite befitting that you preside over the ceremony because it was your parents who 55 years ago provided the key to the nuclear science by their epic discovery of Radium.

You yourself and your husband also have made significant advance by your discovery of artificial radioactivity in 1934.

Here in India we are trying to start some research along the same line, and we are very proud that this premier Institute of Nuclear Physics of our country is being opened by someone who is a celebrity for research in this subject. On this solemn occasion of opening of this Institute there will be a reception in which most of the important people of the city would be present.

In the meantime, we are waiting to receive you and your husband in India and at Calcutta.

With my best wishes,

M.N. Saha

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3rd July 1949

Dear Prof. Joliot,

I have read the interesting article on you in the Bulletin on atomic scientists, April 1949, and I am highly interested in its contents.

You deserve the profound thanks of scientists of all countries for the courageous stand you have taken on the question of secrecy of results on atomic research. Personally I greatly admire your courage and high-mindbender in bravely tackling the problem.

I am myself trying to set up a pile in India and have collected some materials. Like you, I believe that there should be no secrecy, and the blessings of atomic energy research should be extended to all less developed countries of the world, in particular mine, where we have to solve so many problems to raise the standard of living and I shall be glad to receive assistance from you.

This is a specific request. I shall be grateful if you can kindly supply me with details of your pile ZOE, i.e. amount of uranium oxide, and uranium, quantity of graphite, and the degree of its purity, amount of heavy water, drawing etc. which will be needed – in fact all that is needed to duplicate this pile in India. I have already made the request to you through Mr. S.N. Sen, late of the UNESCO.

If compliance with my request places you in any disadvantage, please do not hesitate to decline it.

With kind regards,

Yours sincerely

M.N. Saha

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9th November, 1949

Prof. Madame I. Joliot-Curie

Dear Madam Curie,

I am very glad to learn that yourself and Prof. Joliot Curie have accepted the invitation of the Indian Science Congress Association to visit India during the next session of the Congress at Poona in the 1st week of January, 1950.

I shall be very glad if you can kindly visit Calcutta after the Congress session is over and deliver one or two lectures here. The Indian Association for the Cultivation of Science of which I happen to be the Chairman, had decided to award you the Joykissen Mukherjee Gold Medal in 1947 and you had kindly agreed to accept it but you wrote to us that you will take the medal when you visit Calcutta. Since the Calcutta visit is

now materialising, I hope you will kindly deliver a lecture on any scientific subject of your choice in the halls of the Association. We shall be glad to offer you hospitality during your stay in Calcutta and also bear the expenses of your journey from Poona to Calcutta.

Looking forward to the pleasure of meeting you, in Calcutta.

Yours sincerely,

M.N. Saha

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(Translated from French – courtesy: Mona Saxena)

Prof. Madam Irène Joliot-Curie,
Director, Radium Institute,
11 Rue Pierre Curie, Paris

9th December 17, 1949

Dear Madam,

We are very pleased to receive your telegram announcing your visit to Calcutta after the Science Congress session at Poona provided that Dr. Bhatnagar gives his consent on behalf of the Government of India. I wrote him an official letter, and I am sure he would consent. As for the dates, we will have to make some changes.

The construction of the Institute of Nuclear Physics of the University of Calcutta is just finished, the funds for the construction of the Institute were donated by the Central Government of India and the Provincial Government of Bengal. We intend to have the ceremony of the solemn opening of the Institute in the middle of January, 1950. The Vice Chancellor, and the Unions of Calcutta University are very pleased that you have the kindness to preside at this ceremony of the official opening of the Institute. It seems fair that you preside at this ceremony since it's your parents who provided the key to nuclear science. You and your husband made a very memorable advance together with your discovery of artificial radioactivity in 1934.

Here in India we are about to start a series of similar research, and we will be very proud if the solemn opening of the first Institute of Nuclear Physics of our country is made by someone who has become famous for research in this subject. On the occasion of the solemn cover of the Institute there will be a reception to which most of the important people of the city will be present.

While waiting for the pleasure of receiving you and your husband in India and Calcutta, I send you my best wishes,

M.N. Saha

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4.24 LETTERS BETWEEN LESLIE A. WHITE AND SAHA: 1954

University of Michigan
Ann Arbor, Michigan
College of Literature, Science, and The Arts

Dept. of Anthropology
221 Angell Hall

1 September 1954

Professor M.N. Saha
University of Calcutta,
Senate House, College Street
Calcutta, India

In a little book called POWER, by Dr. Martin Ruhemann, published in London in 1946 (Sigma Books, Ltd.), on page 110 there is the following statement: Professor Saha, a well-known Indian scientist, believes that the standard of life in a country can be measured by the amount of power produced in that country per inhabitant." I am much interested in the energy theory of cultural development, and would like very much to have the bibliographic reference to this statement in your published writings. I contributed an essay on this subject, upon invitation of Professor K.M. Kapadia, University of Bombay, for a memorial volume to Professor G.S. Ghurye which was to have been published last month, although I have not seen it yet.

We had the great pleasure of having the late Professor Benoy Sarkar of Calcutta as visiting Professor in our Department a few summers ago.

Very sincerely yours,

Leslie A. White
Chairman

●●●

the 9 Sept., 1954

Dear Mr. White,

Your letter of 1st September, 1954.

I have given the view mentioned in your letter in a number of publications. You may be referred to one such publication in the well-known scientific weekly *"Nature", Vol. 155, p. 221* (published from London) — "Science in Social and International Planning", (date Feb 24, 1925)

Prof. Goldberg, Director of the Astrophysical Observatory of the University of Michigan is a very good friend of mine.

Please convey my best regards to him.

Yours sincerely,

M.N. Saha

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4.25 LETTER FROM SAHA TO FELIX BLOCH: 1954

Dr. F. Bloch,
Director, Centre European Recherché Neuclaire (C.E.R.N.)
Institute de Physique,
University, Geneva.

12th November, 1954

My dear Dr. Bloch,

I have just returned to India after a pleasant flight.

In continuation of my talk with you, I am sending to you reprints of a paper on the "Mathematical Analysis of the Hahn Spin-Echo Experiment" for your perusal. We are also doing some work on the Beta and Gamma spectroscopy. I shall be glad if you can kindly go through these papers. In addition, we are carrying on some other theoretical and experimental works on "Nuclear Magnetic Resonance Measurements".

I am very much interested in the Electro-magnet which you have brought with you from Stanford and shall be glad to have blue-prints and other details.

Yours sincerely,

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5.

LETTERS: INDIAN SCIENTISTS AND ADMINISTRATORS

5.1 LETTERS BETWEEN ASUTOSH MOOKERJEE AND SAHA: 1921–1922

Senate House
Calcutta

The 9th February, 1921

My Dear Dr. Meghnad,

Yesterday a cable was received at the University from Sir William Meyer, High Commissioner for India, stating that you had applied for a grant for £125 and proposing that the University should finance you. We have sent a cable to the High Commissioner requesting him to pay the money to you at once on our behalf and we are repaying the money to him by a draft which will follow by next mail. I wish you had applied to your Alma Mater and not to the High Commissioner. We are in great financial crisis here on account of the Non-Cooperation movement, but you may be rest assured that so long as it is practicable your Alma Mater will not be slow to help you. When you return to this country I trust you will bring back the instrument to be deposited in our physical laboratory. I have read with much interest your latest papers and I feel proud of the work accomplished by one of my fellow graduates. I trust you will not hesitate to

serve your Alma Mater when you return. I was deeply grieved to hear that Dr. Jnanendra Ghosh had decided to give up his Alma Mater and accept service in the Dacca University. When will the children of our Alma Mater realise that it is absolutely necessary for all of them to stand by Her at the most critical period of the history of Her development.

I hope you are keeping good health.

Your well-wisher,

Asutosh Mookerjee[28]

28 Vice-Chancellor of the Calcutta University and a distinguished mathematician

●●●

The 11th May, 1921

My Dear Dr. Meghnad,

I have read with indescribable interest and pleasure the account of your work. I had already seen the notice in the Nature. I trust that you will utilise to the fullest your great opportunity of your association with Professor Nernst. I have read his published papers and I realise his eminence as a physical chemist. I am now endeavouring to provide for you an independent position and a suitable Laboratory when you return here and I have little doubt that my efforts will be successful. I do not think you will have to regret your decision to stay in your own University rather than accept an appointment elsewhere.

People here understand little about the needs of the University College of Science. Our financial difficulties are consequently great; but I have faith in the cause we have taken up and I trust better days will dawn before long. Can you give a rough estimate of what you require for the purchase of appliances in Germany, so that I may make an effort to raise some money?

I had you nominated as one of the representatives of your Alma mater at the forthcoming Congress of the universities of the Empire. I suppose it will be too expensive for you to go to Oxford from Berlin and again return to your work.

Yours well-wisher,

(Asutosh Mookerjee)

●●●

The 30th June, 1921

My Dear Dr. Meghnad,

It now looks probable that I shall be able to arrange for the creation of a new University Chair of Physics. The initial salary will be, as in the case of Ghosh Professor, Rs. 500 a month; but I am in hopes that it may be possible to make it Rs. 600 before very long. If you are willing to take up work, I shall not look about for anyone else. The Chair will give you an independent position and freedom of work. I hope you will let me know without delay whether this will suit you and for what period you would like the first appointment to be made (three years or five years or seven years). Can you give me a complete list of your papers hitherto published and statement of your career as also of the work you have in hand or intend to undertake? I hope you are keeping good health.

Your well-wisher

(Asutosh Mookerjee)

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To: Sir Asutosh Mookerjee, D.Sc., C.S.I.

20-8-1921

Dear Sir,

Sometime ago, in reply to a letter of yours offering me a newly created chair of physics on Rs.500-600 per month, I had the pleasure of addressing to you a telegraphic message, and a letter practically conveying my acceptance of your terms. I was then so busy with my own experimental work that I had not the time to write a detailed letter which I do now. It gives me great pleasure to be able to inform you that my labours have at last come to a successful end, and I have succeeded in experimentally demonstrating beyond doubt that gaseous atoms can be ionised simply by heat (temperature ionisation of gases)- an effect which under various forms have been looked for in vain by such eminent physicists as Maxwell, Hittorf, Rayleigh and J.J. Thomson. The experiment will shortly be published in the Zeitschrift fur Physik, and either myself or Prof. Nernst will announce it before the forthcoming meeting of the German physicists at Jena, where I also have an invitation.

On my return to India, I wish to continue this line of work, and if you are good enough to procure me a research grant of about £500 (five

hundred pounds), I shall be able to organise a laboratory where, besides works in this line, we can also take up industrial works connected with high temperature (such as ceramics, enamelling, and glass, and metallurgy). I do not mean to say that I am competent to take up works in these lines, but that future (workers) may find the resource and the equipment of laboratory very much useful for their work. For my immediate work, I require about £500.

About the terms, I beg to suggest that Rs. 500 per month is really too small a sum for a city like Calcutta. I shall be perfectly satisfied with Rs. 600 per month, and rising to (by annual increment of Rs. 50) Rs. 800 in five years. I wish to make it clear that considering my contributions to physics, as well as the offers made to other people with similar equipments, I am not making an extravagant demand upon the generosity of the University.

All the educational Institutes are closed up to the 15th Oct. As my work is also finished I have nothing else to do except waiting orders from you. If you kindly send me the passage money, and the appointment letter, by telegraphic message, on the receipt of this letter, I shall be able to pick up a boat about the beginning of Oct., and reach Calcutta in November.

Hoping you are doing well & with Pranam,

Yours sincerely,

M.N. Saha

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MOOK /A-22-6(E)-1

University College of Science,
Department of Physics,
92, Upper Circular Road, Calcutta,

The 6th December, 1922.

From,
Meghnad Saha, Esq,
Khaira Professor of Physics,

To,
The Hon'ble Sir Asutosh Mookerjee, Kt., C.S.I.,
B.A., B.I., D.Sc, Ph.D.,
Vice-Chancellor, Calcutta University.

My dear Sir,

Since my return from Europe last year, I have been trying to build up a laboratory where I can continue my scientific work. But though in my efforts, I have been very kindly and nobly supported by you, the financial condition of the university is so bad that it has not been able to help me with the necessary funds.

The importance of my scientific work has now been recognized all over Europe and America. In a private letter, Prof. Einstein expressed the opinion, that "in the interest of Science, I ought to be helped in carrying out my investigation". Prof. H.N. Russell of the Princeton University, America, writes in the May number of the Astrophysical Journal "The principles of the ionization theory will evidently be of great importance throughout the whole field of astrophysics, and Dr. Saha has made an application of the highest interest to the question of the physical meaning of stellar spectra. The possibilities of the new method appear to be very great. To utilize it fully, years of work will be required * * * * the prospect of increase of our knowledge, as a result of such researches make it urgently desirable that they should be carried out." For details about the recognition my work has received, I beg to enclose a copy of the Modern Review containing an article by Sir P. C. Ray.

While the European and American scientists are eagerly extending their activity into the region opened up by me, here owing to lack of funds, I have been doomed to a state of torpor and inactivity.

I may be allowed to add that during my stay in Germany, I was invited by Prof. A. Sommerfeld of Munich to address the Physicists there on my work. My visit happened to coincide with that

of our Poet, Dr. Rabindranath Tagore who was accompanied by Dr. S. R. Bomanjee, the well-known millionaire of Bombay. Prof. Sommerfeld paid a visit to the Poet, and spoke appreciatively of my contributions to Physics in the presence of Mr. Bomanjee. I have thus been already introduced to that gentleman.

I suggest that Mr. Bomanjee, who is well known for his philanthropic activities, may be approached for a substantial contribution towards the funds of the Science College so that I may be enabled to buy up apparatus and build up my laboratory.

Trusting, that the matter will receive attention from you.

I remain
Sir,
Yours very sincerely,

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5.2 LETTERS BETWEEN PRAFULLA CHANDRA RAY* AND SAHA: 1921–1935

(*Plates 5 & 7)

(Translated from Bengali)

College of Science
Calcutta

21.4.21.
12 A.M.

My Dear Jnan & Meghnad,

I received your letters of 22nd and 25th March just now. I came to know about the state of affairs from letters from Meghnad (direct) and (from Meghnad) via Kanai, and as per request from you (from Meghnad) sent my reply to the 21, Cromwell Road address (in London). Einstein, Laue, Planck, Nernst, each of them has shown so much brilliance that it would be no exaggeration to call them Nobel Laureates. I am delighted by the opinions that they have expressed about Jnan's "bahnbreachende Werke" (path breaking work). I have never enjoyed such undiluted pleasure. I am waiting for the reprint of (the paper) of "younger" Jnan* on "Electrical Conception of Colloids" in "(The Transactions of the) Faraday Society".

What you have written about Royal Society of Britain is in fact literally true. Even an even-tempered man like Nilratan[29] had, writing about the current "election" (of Fellow of the Royal Society), commented that these people are "scounderels". That you three have made your country proud is the result of silent efforts now it is no longer necessary to make empty sounds and stir up matters.

There are some comments on Watson in the Amritabazar that is being sent (to you). Needless to say, you will come to know from the letter to "younger" Jnan[30] (which is also enclosed with this letter) all other matters regarding the person who has inspired these (comments).

It is my opinion that both of you should stay in Berlin for some time. You may never again come in contact with such illustrious persons in your lifetime. In fact, with the exception of one or two, all others in England are mediocre. They cannot appreciate our work with an open mind, perhaps because we are a subject race. Only a genius can appreciate a genius. It is impossible for British scientists to appreciate (our work) without a condescending attitude.

The Registrar will send you a calendar by this mail, in which you will find everything about appointment of professors. I am also sending this week's "Sanjibani".

It would be nice if Jnan can go to Sweden and deliver a lecture; Arrhenius particularly has

29 Nilratan Sircar

30 Jnanendra Nath Mukherjee

not been able to digest this new theory well – I hope this dream (of mine) will come true. I have already written to "younger" Jnan that ("elder") Jnan has every right to become a Nobel Laureate. So much for today. It is time to take a bath and lunch.

My blessings to you,

Shri Prafulla Chandra Ray

P.S. Sir Bose[31] has written an article entitled — in the Baisakh issue of "Prabasi". It starts with "What is your desire, Master! By whose order have you become so inspired?" As if he has been inspired and sent. I heard even persons in Dacca have become irritated after reading it.

31 Sir Jagadis Chandra Bose

●●●

(Translated from Bengali)

R.K.B.K. Harishchandra Institution,
P.O. Raruli,
Dist. Khulna
Date : 24th May, 1921

My Dear Meghnad,

I came to know about everything from your letter of 16th April. I was a little surprised that you have written, after traveling to many places, observing many things and gaining experience, even after mixing with learned people in Germany, that "the kind of people who can serve science and his country with utmost devotion is rare even in Europe". I am now three score years old, but even now if I cannot work in the laboratory for six or seven hours, I feel that the time has been wasted.

Soon, I shall try to eliminate an illusion by making an example of your, Jnans' and others' works – that is, I shall resolve to eliminate the prevailing trend of fooling our countrymen (with the bluff that the lot of the country cannot be improved) without visiting foreign soil or without a "good, bad or indifferent" foreign degree. I am sorry to say that even men like Ashubabu suffer from such illusions.

I have already informed you that for the last one and half months I have travelled extensively to places like Gobardanga, Naogaon, Gaibandha, Santosh, Tangail, and Bagerhat. I was invited by students and teachers. In many places, I have addressed about two to three thousand people at each lecture. The Saha community of Tangail have prospered through trade carried out for a

century or so. Some of them have spent lavishly to try to build schools.

This year we are having some sort of a famine in our area, caused by poor agricultural yield. There is no end to common people's woes. We are busy with relief.

I have received a letter from Jnan. I came to know from his letter that he could not come by the ship that he was supposed to come because of strike in England; he will come by the next steamer. You already know about the nefarious activities of Ashubabu regarding Jnan's appointment. He is trying to cancel it. This would mean that you shall have to stay in College of Science for life because you have signed a lifetime bond to serve in order to draw a salary of Rs. 300!

Time to conclude.

Yours sincerely,

P.C. Ray

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(Translated from Bengali)

University College of Science & Technology
Department of Chemistry
92, Upper Circular Road, Calcutta

4-11-35

My Dear Meghnad,

Received your letter. I hope you will be able to gradually extract some more money from Bengal Chemical.

This month's "Science & Culture" has come out very well. The articles have been written by experts, but I am glad and at the same time sad to see this issue. It is one man's hope, born only out of your encouragement and enterprise. We have many scientists, but there is no driving power in them. You can give battery shock by wireless even from Allahabad. The flaw in the character of us Bengalees as a race is that we wake up and rise for a few days for some special reason, but like opium-eaters become drowsy and go to sleep (afterwards). I am even afraid that during your stay abroad, though it would not become extinct, the quality of the journal will deteriorate.

One more word. The contributions of this (journal) are almost confined to the Bengalees. We should try our best to attract northern Indian scientists, like Birbal Sahni of Lucknow. In the first article you have compared American millionaires to Indian Feudatories and others; in order to suppress the "dancing girl incident" the ex-Holkar

alone had to spend almost one crore of rupees, and was ultimately removed from the throne, and you know how much money each of these so called "great men" waste after going to England and other foreign countries?. But why should we blame only them? What Palit (Tarak Nath Palit) and Ghose (Rashbehari Ghose) contributed to the benefit of this College of Science have been the first and last (of such contributions). We have so many high-ranking officials, like high court, district court & session judges, deputy magistrates, munsiffs and secretariat high officials, vakils (lawyers) & doctors – every one of them is heavily indebted to the Calcutta University. But nobody thought us to be good enough for contribution of even one month's salary. On the other hand, this educated class is pouring enormous sum of money on Gouria Math, Pabna Satsang and the Shibpur Math of Baishnabacharya.

I am at present too busy. I become very much depressed when I think about the Bengalee race. Bengalees went to Behar, U. P., Oudh, Punjab and others places allured by jobs and founded colonies (there). And now their descendants do not get any kind of job. You yourself have said that they are paupers – so many paupers, and the condition of Bengal is also like that. We are becoming strangers in our own province. Even if we exclude Burrabazaar, Chittaranjan Avenue (localities) of Calcutta, in the new (areas like) Manicktala Street, Bibekananda Road big 5-storied buildings are also coming up, all owned by Marwari Bhatias. The Marwaris of Calcutta, like Nagarman Surajmal and others, have individually (i.e. not as joint stock company) constructed three or four large sugar mills by spending 15 or 18 lakhs of rupees. But we could not build anything even after receiving 167 percent protection. Now I find that all zamindaries are also going to Non-Bengalees. Now at the end of my life, when I think of these things I suffer from anxiety and depression. At present I am writing a book entitled "The Problem of Food and the Defeat of the Bengalees" (in Bangali). I have discussed in it why the Bengalees are retreating in every sphere in the struggle of life.

Jnan has returned from Europe. I hope he can now concentrate in "Science & Culture". He should try to induct more life members and from time to time try to obtain contributions from Tropical School of Medicine and others places.

So much for today.

Yours

P. C. Ray

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5.3 LETTER FROM JNAN CHANDRA GHOSH*: 1921

(*Plate 9)

(Translated from Bengali)

The Chummery, Romna P.O.
Dacca 19.7.21

My Dear Meghnad,

I have not received your letter for many days. You must have come to know by now the difficulties that I am facing after coming back (from Germany). Calcutta University has at last released me, but I am still facing problems with Dacca University regarding salary. Hartog[32] is very eager to keep me in Dacca, especially since there is very little chance of Watson coming (here). May be matters will be settled between Dacca University and me by August. How many invitations did you receive as a Representative of Calcutta University, and which places did you go? Very soon, an article will be published in "Prabashi" about your work. But nowadays the public raise their eyebrows when they hear about "research" being carried out in Calcutta University.

I received the proof sheets of my paper in Z. Physikal. Chem. I shall correct (the proof sheets) and send them to you next week. Often, I feel that it might have been better to stay abroad than return home. When are you returning? Are you having any correspondence with the "Chief"?[33]

We are all very well. Satyen[34] is now in Dacca; he has almost made up his mind (to settle here).

Yours affectionately,

Jnan[35]

32 Philip Joseph Hartog

33 Probably refers to Sir Asutosh Mukherjee, Vice-Chancellor of Calcutta University

34 Satyendra Nath Bose

35 Jnan Ghosh, a distinguished chemist and later an academic administrator; also a classmate of M. N. Saha and S. N. Bose

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5.4 LETTERS BETWEEN SATYENDRA NATH BOSE* AND SAHA: 1921

(*Plate 9)

(Translated from Bengali)

19-Aug-21

My Dear Meghnad,

I did not receive your letter for many days, but often, while I was in Calcutta, I received your news from Abinash[36] and others. May be you have boycotted me for writing letters irregularly? Jnan and I are staying at the same place; I heard from him that you have travelled in many places in Germany, and met and talked to many eminent persons. I also learnt that you were supposed to go to Munich. Can we expect a graphic account of all these matters?

I have come to "your country"[37] more than a month ago work here has not started yet. There were many things in "your Dacca College", but perhaps you know something about how they have deteriorated due to negligence. There are many Nicol (Prisms), lens, eyepieces scattered about on the tables of the "sahibs"[38], we shall have to do "research" to find out which part is for what apparatus.

There is a dearth of journals here, but the top bosses of (this) new University have assured us that they would place orders for many journals, together with their back numbers. There are talks of a separate science library. These are the news of this place.

Please write about what you saw and what you did as a delegate of Calcutta University. Did you receive any honorary degree?

With warmest regards,

Yours,

Satyen[39]

36 Dr. Abinash Chandra Saha, he was a colleague of Prof. S.N. Bose & Prof. M.N. Saha when they were teaching physics in the Department of Physics, Calcutta University

37 Means eastern part of Bengal that was the birthplace of Meghnad Saha. It used to be referred to as East Bengal before it became an independent nation that is now known as Bangladesh.

38 White skinned people working in India were often referred to at that time and sometimes even today as "Sahibs"

39 Satyendra Nath Bose

●●●

C/o Royal Society
Burlington House
London

Prof. S.N. Bose
92 Upper Circular Road
Calcutta
India

July 27/1946

My Dear Satyen,

We have today finished the International Conference on Fundamental particles and low temperature physics, which has been in progress for about a week. I have applied for passage and can now fly off any day.

No nuclear physics was discussed, there is still hush-hush about it. It was generally agreed, as Wigner told me in 1944 in the U.S.A., that no definite advance has been made either in the theory of fundamental particles, or theory of quantum electrodynamics since Dirac's work on the electron. There was Pauli, Schrodinger, Bohr, Wentzel, Møller, and all other lesser lights, who have written papers bristling with high mathematics on meson field theories, but it was felt that most of these works, or rather the whole of it, has to be consigned to wastepaper basket and a start on a new basis has to be made. In fact, Proca, giving an outline of his work said apologetically that we have to keep our brains ready for the reception of new discoveries which may be made with the aid of the atomic pile, the cosmic ray, and the betatron and the cyclotron. Darwin told me that nuclear physics is very much in the same position as the atomic physics was before Bohr made his debut in 1912 with his Theory of the H-spectrum. Much excitement was caused by the report given by Møller of a work done by Heisenberg during the War which breaks entirely new grounds, and rejects all former approaches through meson theories. I think you will find it in the ***Zs. f. Physik***, war volumes which are in my room.

I found that all these men – like Pauli, Kemmer etc. – are very modest in their claims, and frank in the candid admission of their failures – in this respect they are quite unlike some of the Indian workers on this field, who shine on their reflected light, and assume proud airs.

There was not much on the low temperature side, except sketch of future work. We had a most interesting lecture by Oliphant who has recently been back from U.S.A. He says that the U.S.A. will have a National Science Laboratory, where they will install a huge cyclotron at a cost of 35 million dollar. The pole piece will have a

diameter of 400 (four hundred inches), and the Yoke of the magnet itself will be the laboratory. The power needed will be 250,00 kw, nearly the whole power used for the city of Calcutta. The construction has been rendered possible by the theoretical work of R.W. Wilson who has shown that the relative increase of mass of the heavy particles at high energies can be countered by the frequency modulation of the emf across the poles. Oliphant himself has been given a grant of 1.5×10^5 pounds for the construction of a Synchotron which will accelerate protons to 500 million electron volts.

We had another conference in London on X-ray physics & Laue was brought for it. I could not go to this meeting, but I have been trying to get reports for your use.

I hope you will pass this letter to Nikhil, to the cyclotron workers, B.D. Nag and my son.

As regards instrumental technique there is a complete revolution. I shall tell more of it to you when we meet.

Yours affly

Meghnad

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5.5 LETTER FROM SNEHAMOY DUTTA: 1922

(Translated from Bengali)

Northbrook Society
21, Cromwell Road
S.w.7
12.4.22

Dear Friend,

Received your letter of 8th March. Many thanks for the good news. But I have not yet received official intimation of the decision regarding P.R.S. (Premchand Roychand Scholarship); what is the reason for this? Kindly enquire about it. I am in dire need of money at present; it would have been nice to receive this money soon. I am not sorry that the money has been shared since it was known that this would happen. But that the money would not have been divided had Dr. Russell's paper been published earlier – I am a little bit surprised in discovering the hand of fate in this remark of yours. Whatever that may be, I am satisfied with what I have received. Because in our country such things are associated with a certain amount of fame. I could not have obtained it had I not received it this time, since this was my last chance. I am especially thankful to you since I agreed to sit for it (the examination for the scholarship) only at your suggestion. Now I am eagerly waiting for the official information. It is necessary to send the money home in instalments, and I want to make arrangements for that as early as possible. Please enquire about the reason for the delay, and please send a reminder whenever you feel necessary.

Dr. Russell's paper has been published in the current issue of the Astronomical Journal. It is full of praise for your theory. In my opinion it is a great triumph for our country. I think it would be a fair guess to conclude that this publication definitely establishes your theory. What I want to emphasize is that nobody else from our country has made a more valuable contribution in this field. I heard from Fowler that Dr. Russell is very keen on your theory – he wants to open a discussion on it during the International Conference meet to be held next May. Most probably Russell has written you about it. I shall let you know about the details when (Alfred) Fowler returns. My earnest request is that please knock at the door of the Royal Society if the theory is accepted at the Conference. It is my belief that the door will open.

That my paper has been quoted for identification of solar lines has led me to believe that it has been accepted as a standard work. But there are a few modifications. The potassium

subordinate series was not confirmed since the Mount Wilson plate did not exhibit those lines of Rowland. According to Rowland those lines are very weak; anyway, if those lines are removed from the solar spectrum table, then my identification also will not be required. You know very well how uncertain is the identification of solar lines, specially the weak lines. I have not received the credit for identification of the P_1 line of potassium, Meggars got it. Fowler is responsible for this – anyway, let bygones be bygones.

I am increasingly getting worried about employment. I cannot remain idle after returning. I prefer I.E.S. (Indian Education Service), but what to do if I do not get it? I shall have to accept the Calcutta chair if it remains vacant till then. You are well aware of my predicament. Do you have any information about any vacancy in I.E.S.? I would very much like to go to Dacca. I am thinking of trying at all places at least once after submitting my thesis. Has any further proposal come from Aligarh?

What are the news of Jnan Mukherji, (Jnan Chandra) Ghosh, and others? I hope you are all well. We are somehow managing our daily routine.

Yours

Snehamoy[40]

40 Snehamoy Dutta, Meghnad Saha's classmate

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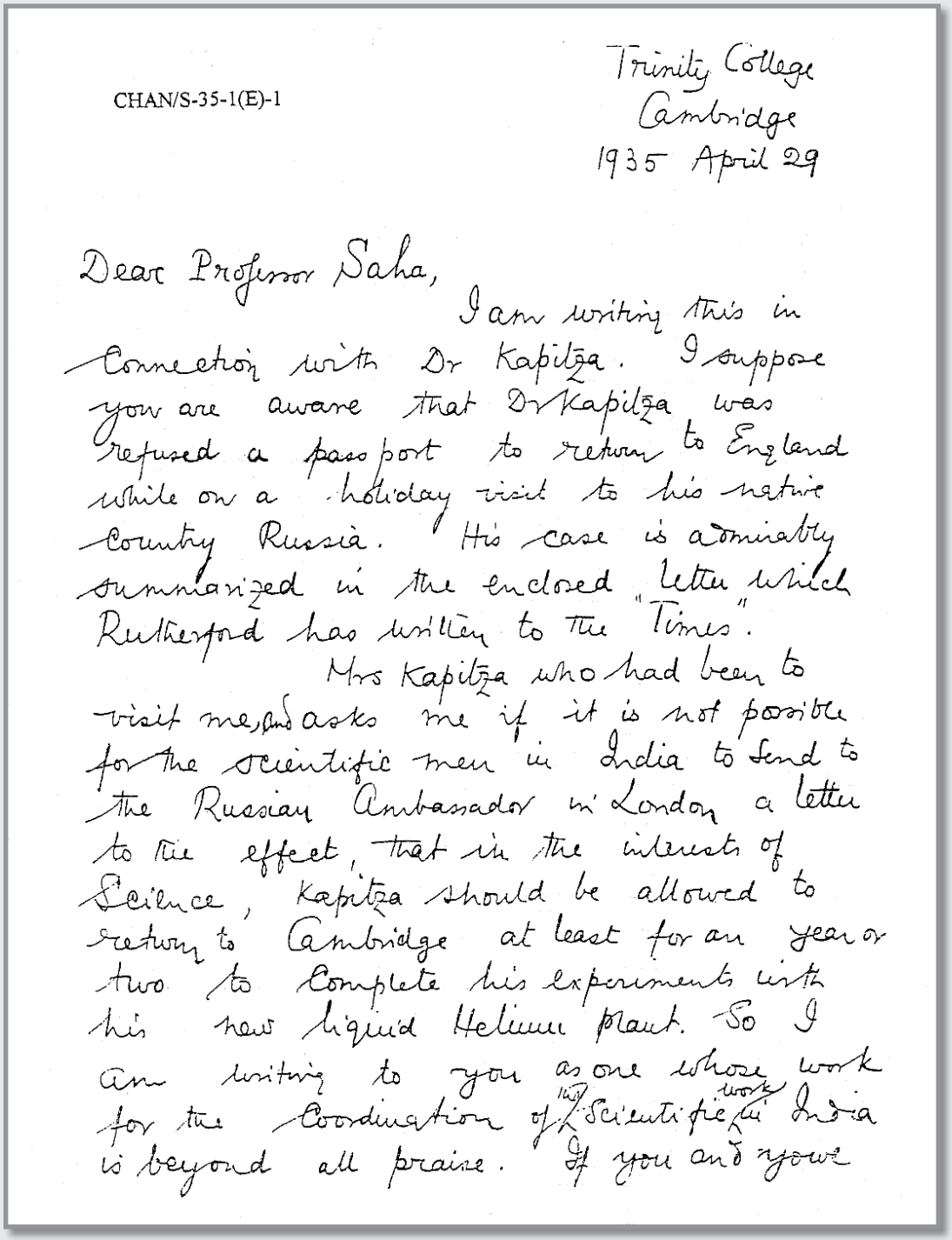
5.6 LETTERS FROM SUBRAHMANYAN CHANDRASEKHAR: 1935



CHAN/S-35-1(E)-1

Trinity College
Cambridge
1935 April 29

Dear Professor Saha,

I am writing this in Connection with Dr Kapitza. I suppose you are aware that Dr Kapitza was refused a passport to return to England while on a holiday visit to his native country Russia. His case is admirably summarized in the enclosed letter which Rutherford has written to the "Times".

Mrs Kapitza who had been to visit me and asks me if it is not possible for the scientific men in India to send to the Russian Ambassador in London a letter to the effect, that in the interests of Science, Kapitza should be allowed to return to Cambridge at least for an year or two to complete his experiments with his new liquid Helium plant. So I am writing to you as one whose work for the Coordination of Scientific work in India is beyond all praise. If you and yours

Collegues would send a Carefully worded letter – Kapitza is a Soviet Citizen and any Sentiment which could possibly offend the Soviet Government would have to be avoided – to the Russian Ambassador, London, it would be gratefully appreciated by Professor and Mrs Kapitza. As I said I am writing this letter at the expressed desire of Mrs. Kapitza.

Perhaps, it would be possible for the newly formed National Institute in India, to send an official letter too. As the Royal Society in London has done it, it would not be amiss for the Society in India which corresponds to the Royal Society here to do the same.

I need hardly suggest anything more – Surely your experiences would suggest to you the proper Course to adopt under these Circumstances. If you do take any steps

it would give Mrs Kapitza the greatest pleasure, if you would inform her of the same.

With many thanks

Yours Sincerely,

S. Chandrasekhar

P.S. The address of Mrs. Kapitza is

173 Huntingdon Road
Cambridge, England

Trinity College
Cambridge

1935 April 29

Dear Professor Saha,

I am writing this in connection with Dr. Kapitsa[41]. I suppose that you are aware that Dr. Kapitsa was refused a passport to return to England while on a holiday visit to his native country Russia. His case is admirably summarized in the enclosed letter which Rutherford has written to the "Times".

Mrs. Kapitsa who had been to visit me, and asks me if it is not possible for the scientific men in India to send to the Russian Ambassador in London a letter to the effect, that in the interests of science, Kapitsa should be allowed to return to Cambridge at least for an year or two to complete his experiments with his new liquid Helium plant. So I am writing to you as one whose work for the coordination of the scientific work in India is beyond all praise. If you and your colleagues would send a carefully worded letter — Kapitsa is a Soviet citizen and any sentiment which could possibly offend the Soviet Government would have to be avoided – to the Russian Ambassador, London, it would be gratefully appreciated by Professor and Mrs. Kapitsa. As I said I am writing this letter at the expressed desire of Mrs. Kapitsa.

Perhaps, it would be possible for the newly formed National Institute in India, to send an official letter too. As the Royal Society in London has done it, it would not be amiss for the Society in India which corresponds to the Royal Society here to do the same.

I need hardly suggest anything more — surely your experiences would suggest to you the proper course to adopt under these circumstances. If you do take any steps it would give Mrs. Kapitsa the greatest pleasure, if you would inform her of the same.

With many thanks

Yours sincerely

S. Chandrasekhar

P.S. The address of Mrs. Kapitsa is 173 Huntingdon Road, Cambridge, England

41 See plate 23

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Trinity College
Cambridge

1935 July 21

Dear Professor Saha,

Thanks very much for your kind letter. Mrs. Kapitsa wishes me to convey to you her best thanks for the interest you have taken in Dr. Kapitsa and especially for the article you have had printed in "Science and Culture".

I have just returned from Paris where I had been to attend the fifth general assembly of the International Astronomical Union. Prof. H.N. Russell and others inquired me of you and I could give only such information as I have seen of your excellent work in print and I am sure that only a very small percentage necessarily appeared in print!

I wonder if you have any students working on astrophysical problems. For my part I am continuing very general astrophysical studies. I am sending by separate cover some two recent reprints.

Again with many thanks and with kindest regards.

Sincerely yours

S. Chandrasekhar

P.S. I am returning Dr. Fermor's letter

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5.7 LETTERS BETWEEN HOMI JEHANGIR BHABHA AND SAHA: 1947

4th February, 1947

My dear Bhabha,

Thanks for your letter dated 25th January.

I am sorry that you could not hold the meeting of the Atomic Energy Committee this time at Delhi. The C.S.I.R. meeting which is being held at Delhi is a budget meeting and unless you formulate your demands now, you lose a year. I think that the only alternative left now is to send a cable to Bhatnagar that they should keep a sum of 8 lakhs for atomic energy research. My own demands are:

Annual Recurring Grant	Rs. 40,000
Non-Recurring	
N.N. Dasgupta's scheme of Wilson Chamber	Rs. 8,000 (first year)
β-ray research	Rs. 1,36,320 (first year)

I am suggesting this step that the C.S.I.R. has now a new member who will probably have a new council. I do not know whether myself or Ghosh will be in the new council. Both of us may go out, and possibly other interests will come. Who they will be, you can easily guess. Our plans may be all thrown to the waste paper basket. You are coming certainly for the Consultative Committee meeting on the 10th, why not come two days earlier and finish the lobby work for the atomic energy finance? This is more important that any meeting, recommendation of which have no effect unless backed up by strong personalities.

I am sorry that Ajit missed you at Delhi. He is still at Delhi, and enjoying some rest after four years of hard work. The amount which he wants is not very small. I am giving you summary of the application as given in the C.S.I.R. proceedings (page 148):-

Dr. A.K. Saha has applied for the following grant:-

Equipment Permanent magnet, electromagnet, Mass Spectrometer, Fission products from Oakridge	Rs. 1,36,320
Contingencies	Rs. 1,000
Staff (Inclusive of Dearness Allowance) (2 Research assistants in the scale of Rs 150-25-200, 1 Mechanic in scale of Rs 100-150)	Rs. 5,694
Total	Rs. 1,43,014

He is going to Switzerland this summer to Zurich and will be a guest in the laboratory of Prof. Scherrer in the Zurich Polytechnique Institute where they have a very fine and efficient, but small compared to American standards, Nuclear Physics Laboratory, consisting of Cyclotrons, Betatrons, B-Ray spectrographs, Mass spectrometers etc. What is more, he can get things made by Messrs. Brown Boveri, Oerliken etc. and Prof. Scherrer has promised to render all the necessary aid. Ajit has found lot of problems in B-Ray spectroscopy, which he can tackle provided he gets apparatus, and this is an opportunity.

Regarding your last para, I am only partly in agreement. It is true that experimental technique has not developed in this country to the same extent as in USA or Europe, but that is not the whole story. The real bottlenecks are:

1. That there is a dearth of good mechanics, laboratory men, in this country.
2. That we have in this country no large engineering or manufacturing concerns producing machinery, electrical goods, scientific instruments or chemicals.
3. That it has been difficult, at least for the last five or six years, to get any machinery or chemicals or scientific instruments from abroad.

You will agree with respect to (1). As regards (2), you are probably aware that all machinery for Cyclotrons and betatrons etc. in England are being made in the workshops of Metropolitan Vickers and other firms, Philips, Mullards, etc. In Switzerland they being made by Brown Boveri and Oerlikon etc. The same is the story about America. I am doubtful whether Lawrence or Kerst, if they were born in India, could have achieved anything, even if they had the ideas.

As regards (3), I have ordered for instruments and machineries from USA and England for two years, but have not been able to get much, though while in USA and England, I visited the firms personally and made personal requests.

The one supreme necessity in this country is to organize first class workshops and train good technicians. Myself and many others have felt this, as you will see from the enclosed leaflet. But nothing moves with the Govt. of India.

I am not so much of a theorist as you seem to think. I have planned and performed with my own hands lot of experiments on spectroscopy and thermal ionization. They have not got the required publicity because most of them were published in Indian Journals.

Yours sincerely,

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Atomic Energy Commission
Government of India

Dr. H.J. Bhabha, *Chairman*
Dr. S.S. Bhatnagar,
Dr. K.S. Krishnan,
Telephone : 35058.59
Telegrams : "ZETESIS"

Apollo Pisa Road
Bombay-1

Ref. : 212/2127

Dear Professor Saha,

I have received your letter No.2.23/1297 of the 30th January proposing to drop the word "Nuclear" from the designation of the Institute of Nuclear Physics.

The change of the name of an Institute like yours is a matter which must be given the most careful consideration and should not be undertaken in a hurry. In any case the matter is of sufficient importance to merit discussion at a full meeting of your Governing Body. If the majority of the members of the Governing Body should be in favour of changing the name at this meeting the matter will then have to be referred to Government as you yourself suggest.

I have sent a copy of your letter to Dr. Bhatnagar to elicit his views on the subject.

Yours sincerely,

H.J. Bhabha

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5 August 1954

My Dear Saha,

This refers to your undated letter of May 1954 to the Prime Minister.

The Prime Minister has decided to call a Conference on "The development of Atomic energy for Peaceful Purposes in India" early in November. The precise date of the Conference will be fixed by him later.

It is intended to keep it a fairly small Conference, so that the discussions may be on a serious level. A list of subjects for discussion is being prepared covering such aspects as geological survey, mining, raw materials processing, reactor design etc. This list will be sent to you in due course. If there are any special subjects which

you wish included for discussion, will you please inform me?

The Prime Minister is of opinion that in accordance with the general considerations mentioned in the preceding paragraph, the number of scientists, engineers and industrial experts to be invited from the universities, research institutions and industrial establishments in India should be restricted to about 30 persons, in addition to the senior members of the staff of the atomic energy commission.

Will you please send a list of persons who in your opinion should be invited to the Conference? The final list will be drawn up by the Prime Minister himself.

It is intended that on each subject to be discussed, as for example, uranium extraction, there should be about 3 to 4 speakers who would each speak on a particular technical aspect of the subject for about 10 minutes, but in such a way as to be understood by a large number of the specialists in other fields also. Thereafter, there may be about 15 minutes for general remarks and discussion. It is planned that the entire Conference should take about two days, and the Prime Minister has decided to preside over it himself.

With kind regards,

Yours sincerely,

H.J. Bhabha

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5.8 LETTER FROM FATHER TO SON (AJIT K. SAHA*)

(*Plate 20)

(Translated from Bengali)

Hotel Sovietkaya
Moscow, July 1

My dear (Khoka),

We left Helsinki on the 29th. I was elected Chairman of the Commission on 'Disarmament and Atomic Weapons' of the World Peace Assembly. Joliot-Curie, Infeld, Bernal, Arnott, etc., were the other members. Soon after I submitted my report, we flew to Moscow via Leningrad. It took about 5 hours.

The Atomic Energy Conference will start from 1st July. People from almost all countries except the U.S.A. are present in this conference. Some common lectures were delivered on the first day – the welcome address of President Nesmjanov, etc. Later, sessions of four sections took place – Mathematics-Physics, Chemistry, Biology, Technology. Lectures on Uranium-Graphite reactors, etc., on the first day; almost similar to the lecture on Reactor Technology that I delivered in Calcutta. But the participants presented all the data they obtained from their own experiments. The subsequent lectures were on Accelerators, Accelerated Particles, reactions produced with them, photoelectric reactions, etc. They have constructed synchrocyclotron under the direction of Veksler, and are getting almost one billion volt particles. About 500 million volt light is being built in their electron-synchrotron. Research is being conducted on this subject in three/four institutes here. They are getting numerous excellent results.

We had our dinner at Hotel Metropole yesterday. I met Kapitsa there. I have decided to go to his house one of these days. I had also a brief conversation with Ponte Cervo one day.

I became acquainted with Bulgarian Professor Najakov here. He is the Vice-President of the Bulgarian Academy; Soviet (Russia) have donated him a cyclotron and a Reactor – it produces 2000 kilowatts of electricity. He has invited me to visit Sophia.

I am sending this letter through my friend – Kundwar Sein. I am in good health. There is no cause for anxiety.

Yours affectionate father,

Meghnad Saha

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5.9 LETTERS FROM SAHA TO J.W. WHITAKER: 1953

Institute of Nuclear Physics
92 Upper Circular Road,
Calcutta

Dr. J.W. Whitaker, MSc., Ph.D.,
Director, Indian Fuel Research Institute
Jamdoba, Dhanbad

My Dear Whitaker,

I am interested in the result of experiments which have been carried out in the National fuel Research Institute on the coking properties of coal in the Central Province, namely Jhilmilli, Korba, Korya and Kanhan under your guidance. I understand that you had also carried out some experiments on blending of non-coking coal from various fields with the coking coal from Jherriah. These are also wanted.

I shall be glad if you would kindly send me the reprints as early as possible.

Yours sincerely,

(M.N. SAHA)
Director

●●●

November 12, 1953

My Dear Whitaker,

Thanks very much for your kindly sending the pamphlets I wanted.

The gentleman who asked me to request you to send these papers requests to have the following information.

Besides Dishergarh Coal, what other high volatile, low ash coal from the Raniganj field such as Poniati, Sanitoria etc., which can be blended with Jharrlah and Raniganj coking coal to yield hard metallurgical coke for use in the Blast Furnace and their permissible maximum percentage in the blended mixture?

Await your early reply.

Yours sincerely,

(M.N. Saha)

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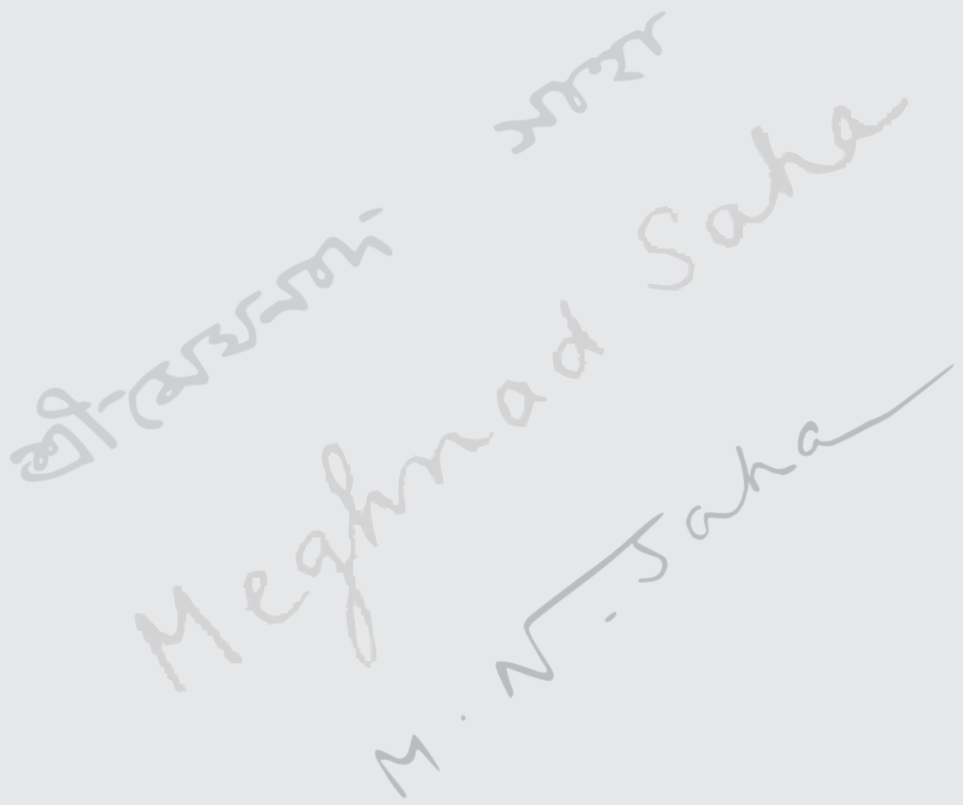
Meghnad Saha
M. N. Saha

6.

MISCELLANEOUS LETTERS (Non-Scientific)

6.1 LETTER FROM ASUTOSH MOOKERJEE: 1920

Council of Post Graduate Teaching,
Senate House,
Calcutta.

The 20th August, 1920

Dr. Meghnad Saha has been personally known to me for many years as a brilliant and devoted student of the University. His attainments have been of a very varied character as will appear from the following statement:

- Passed the Entrance Examination in 1909, stood first in Eastern Bengal; first in Mathematics, first in Literature (English, Bengali and Sanskrit) in Eastern Bengal.
- Passed the I.Sc. Examination in 1919 with German as additional subject; stood third, first in Mathematics and Chemistry.
- B.Sc. in 1913 with First Class Honours in Mathematics; stood second; Hindu College Foundation Scholar.
- M.Sc. in 1915 in Mixed Mathematics (Class 1, stood second).
- D.Sc. in 1919 in Experimental and Theoretical Physics.
- Premchand Roychand Scholar in 1919 in Physics.
- Works – author of "The Principle of Relativity"

containing translations of original papers by Einstein and Minkowski.

- Appointed University Lecturer in Physics and Applied Mathematics in 1916. Lectured to the Post-Graduate students on the following subjects:
  - Advanced Heat including Thermodynamics and Kinetic Theory 1916-20
  - Radiation and Quantum Theory 1916-20
  - Electro and Magnetic-Optics 1919-20
  - Hydrostatics 1916-18
  - Figure of the Earth 1918-20

He has performed his duties as University Teacher to our entire satisfaction and I am not without hope that after his return from Europe he will be able to devote himself to research and to undertake the promotion of research amongst students, with increased zeal and efficiency. I shall watch his work in foreign countries with continued interest, coupled with hope that he may fully maintain the reputation of his Alma Mater.

Asutosh Mookerjee
President

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6.2 LETTER FROM KATHE SCHMIDT: 1921

Berlin, N. 3.11.21

Dear Mr. Saha,

All of us thought of you especially today. By now you must have arrived at home. I can imagine how happy you are to be back again at home with your family and friends after such a long separation. Hopefully you have survived the voyage well without sea sickness. My best thanks for your nice letter from Port Said. Many thanks also for your last greetings from Europe. Gerhard found the postcard with the steamer very interesting. It looks great, the "Paul Lecat", I can imagine such a voyage to be gorgeous.

We are doing well as before. Now Mr. Ganguly and the little Mr. Chowduri are staying with us and we are very happy with them. Temporarily a third person (Paranjki?) is also staying with us. He had met you in Paris. You must have heard that the Indian students have founded an association. I will try my best ever to make your friends feel at home and comfortable with us and I would be very happy if I succeed in that. Mr. Roy comes here from time to time and we like him particularly well. Next week he has invited us to a beautiful concert. Messrs. Students, by the way, have elected me to be an honorary member of the club. What do you have to say about that? I am very proud and happy.

I am enclosing the check herewith, which we found while cleaning up. Today 1 Pound (Sterling) =810 Marks!! Since the first of October the price of everything has increased significantly.

I have saved some of your toilet articles (brushes, toothbrushes, shaving set, etc). I will give the articles to the first man to travel to Calcutta. Mr. Sen writes with very little satisfaction from Munich. He doesn't find an apartment in spite of us giving him many letters of recommendation. The people are not so nice. Everyone stares at him and somebody had asked him why he doesn't wash. The Bavarians are coarse and inconsiderate. Now I shall close. Live well dear Mr. Saha. My regards to your wife. We would be very happy if you would write us now and then. We wish you a lot of happiness and success with your work at home. Hopefully all your apparatuses arrived in good shape. How was it with the luggage? Did you have to pay a lot of duties for your books? Heartiest greetings, also from my husband and the children.

Your

Kathe Schmidt

PS: Since the first of November we have our own telephone. This is very comfortable.

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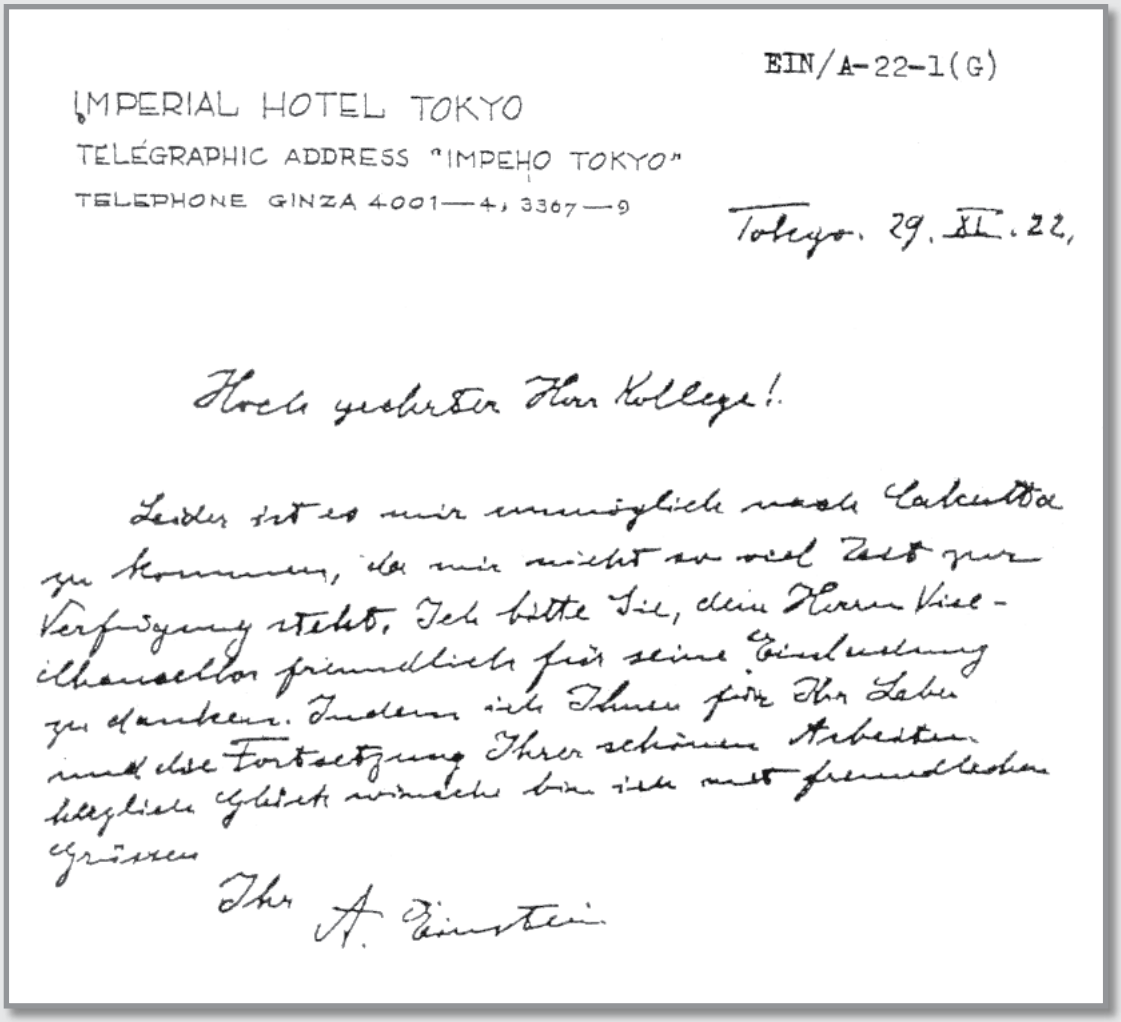
6.3 LETTER FROM ALBERT EINSTEIN: 1922



EIN/A-22-1(G)

IMPERIAL HOTEL TOKYO
TELEGRAPHIC ADDRESS "IMPEHO TOKYO"
TELEPHONE GINZA 4001—4, 3367—9

Tokyo. 29. XI. 22.

Hoch geehrter Herr Kollege!

Leider ist es mir unmöglich nach Calcutta zu kommen, da mir nicht so viel Zeit zur Verfügung steht. Ich bitte Sie, dem Herrn Kiel-ilhansachor freundlich für seine Einladung zu danken. Indem ich Ihnen für Ihr Leben und die Fortsetzung Ihrer schönen Arbeiten herzlich Glück wünsche bin ich mit freundlichen Grüssen

Ihr A. Einstein

(English Translation)

Imperial Hotel Tokyo
Telegraphic Address "Impeho Tokyo"
Telephone Ginza 4001-4, 3367-9
Tokyo, 29.11.22

Highly respected colleague!

Unfortunately it will be impossible for me to come to Calcutta since I do not have so much time at my disposal. I request you to thank the Vice-Chancellor for his kind invitation. While wishing you best of luck for your life and for continuation of your excellent work, with kind regards I am,

Yours

A. Einstein

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6.4 LETTER FROM MARGOT MILNE TO MRS. SAHA: 1935

Dornoch
July 25. 1935

Dear Mrs. Saha,

My husband has suggested that I should write to you and let you know something about houses in Oxford. I am very glad to tell you what I can. Professor Saha writes that you & the children may come over with him in the autumn & that you think of looking for a furnished house. Sometimes people leave Oxford for the winter & there are I believe always some such houses to be had. The cost depends partly on the part of the town the house is in & of course also on the size. An unfurnished house costs from £100 a year; probably a small furnished house would cost from £3 a week upwards, £4 or £5 being a more likely sum. This would not include heating of any kind. I do not know whether you would bring your own servants but I might mention that the wages of a good English maid are round about £1 a week.

We know of a good boarding house conveniently placed & thought we should send you the address in case you would care to go there for a little while till you can look around & see what will suit you best. The address is

The Cedars, 2, Park Town, Oxford

I believe the charge for one adult is £3-3-0 a week. This includes all food, heating & everything. I know it is quite comfortable as I have known several people who have lived permanently.

I hope this may be of some use to you. If I can do anything for you do write & let me know.

With kind regards,

Yours sincerely

Margot Milne

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6.5 LETTER FROM R. ORTVAY: 1936

(English translation)

Kir. Magy. Pázmány Péter Tudományegyetem Elméleti Fizikai Intézete

Institut für Theoretisehe Physik der Universität
University: Department of Theoretical Physics
Direktor: Prof. R. Ortvay

Budapest, VIII, Muzeutn-Körút 6-8,
(Ungarn, Hungary)
The 15 April 1936

Prof Dr. A. Sommerfeld, Munich

Highly Respected Mr. Geheimrath,

Many, many thanks for your charming letter. It pleased me very much to know that on the whole you find my plan to be executable and would be so kind as to support it.

If I could board free in some of the cities where I would lecture and perhaps receive some honoraria for one or the other plan appears viable.

I received with pleasure the suggestion to talk about some of the problems at the boundaries of modern physics and philosophy and have noted some topics for myself, where the relevant literature has not yet exhausted and trivialized the field.
Unfortunately, the thought that I should speak off the cuff in English is causing me serious misgivings. Many foreigners visit here to lecture, some of whom speak off the cuff while others read it out. Fejer read out in America. In the Hungarian Academy of Sciences and in the Mathematical-Physical Society I speak off the cuff on minor occasions whereas on more formal occasions I read out. Others do that too, who are more gifted as orators. Naturally it should not be a monotonous recitation and it should be practiced a couple of times, but I believe that a well read out lecture is always a thousand times better than a somewhat uncertain off the cuff talk. Of course, I would explain the formulae on the chalk board.

As for my tentative travel plans are concerned, I would only like to mention that it depends somewhat on whether living in India turns out to be too expensive for me or not or whether I receive more or less in honoraria. If it is less, then I would shorten the trip and also deliver fewer lectures because some of the places I would like to visit in any case. My plan would be approximately: Colombo — Candy — Anaradhapura (Anuradhapuram) — Madura (Madurai) — Bombay./ Depending on circumstances, throw in: Madras, Bangalore, or Hyderabad./ In Bombay I would like to visit Elephanta as well as make a three to four day excursion to Ellora. From Bombay on: Udaipur —

Jaipur — Delhi — Agra —/Gwalior/-- Allahabad — Benares and then perhaps back to Bombay and back home. If I have the possibility to remain longer, I would include Calcutta and Darjeeling and perhaps also visit Puri in Orissa because of the temples. From Calcutta by train to Bombay or by ship perhaps via Rangoon to Colombo and back. Duration of stay in India from January to middle or end of February.

Important for me would be if 1 could obtain an invitation at the earliest possible, but not later than early fall in any case For the invitation it is not necessary that all details be already clarified. Because it is a long and arduous procedure with the authorities until I can undertake the required preparations and for that I need to be able to show something.

I am enclosing the letter to Prof Saha directly herewith, and would be very thankful if you would arrange to get it to him.

I remain thankfully yours very obediently:

R Ortvay

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6.6 LETTER FROM ARNOLD SOMMERFELD: 1936

(Translated from German)

Instiut Für Theoretische Physik
Munchen 2 NW
Universität Amalienstrasse

Munich, The 21, April 1936

Dear Saha,

I am so indiscreet as to send you the letter from my dear ex-student Ortvay[42] on top of my own letter to you. He wants to pay for his India trip from his own pocket but would like some invitations to lectures in order to present it to the bureaucrats in addition to the possibility of free boarding in some of the places, perhaps also (e.g. in Bombay, Calcutta?) a small honorarium. Would you be so kind to invite him to Allahabad to a couple of lectures and write him at the same time that you would organize invitations to Bombay, Benares, Calcutta,....? I believe there is an organization in India through whom you can get the organization done. Ortvay is a very learned man, in physics and philosophy. His lectures could deal with (an hour each):

1) Hungarian physics in general
2) The work of his institute
3) Natural philosophical issues.

Ortvay would be very thankful to you for any help and advice related to his trip.

Yesterday I spoke in the colloquium about the form of the Compton Band and B.B. Ray. Your mail came at a very appropriate time for this. Many thanks! Please excuse the trouble! Your visit has left behind an all-round positive impression.

With heartiest greetings

Your

A. Sommerfeld

42 See Letter from Ortvay in this Section

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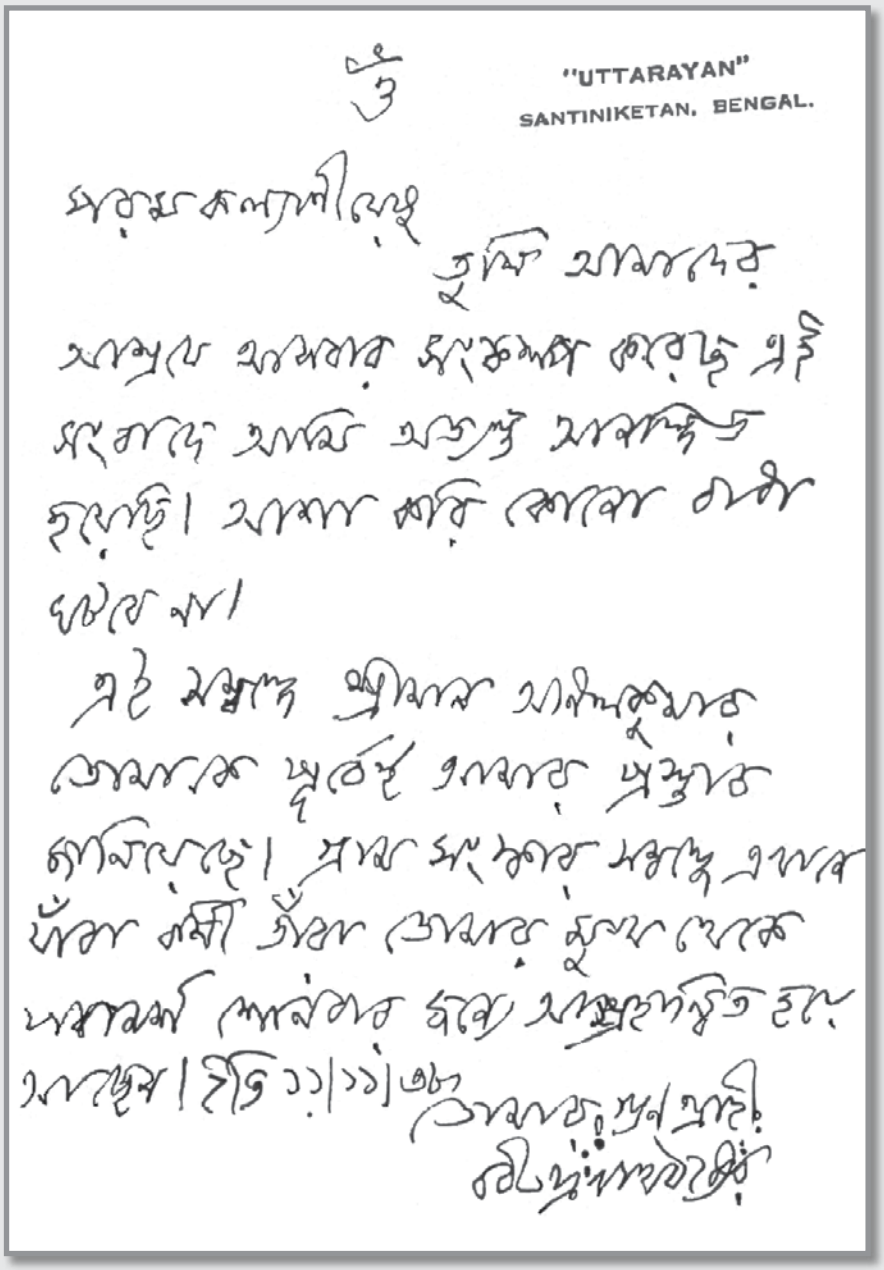
6.7 LETTER FROM RABINDRANATH THAKUR (TAGORE): 1938



"UTTARAYAN"
SANTINIKETAN. BENGAL.

Prama Kalyanieshu (most blessed):

I have been very delighted to know that you have decided to visit our Ashram (Abode of peace). I hope that there will be no obstacle.

In this connection Sriman Anil Kumar has already informed you that the people who are engaged in the village reform activities are eagerly waiting to listen to your advice.

11/11/38

Your admirer

Rabindranath Thakur

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6.8 LETTER FROM SAHA TO NEHRU*: 1954

(*Plate 19)

December 27, 1954

My dear Prime Minister,

Before leaving Delhi, I was on a visit to Maulana Abul Kalam Azad, and he told me a most amazing story.

He said that some important person had told you that I was supposed to have sat in some office chair somewhere in the Secretariat and boasted to some persons that now that Dr. D. S. Kothari was becoming Secretary of the Ministry of N.R. & S.R., and Director of Scientific & Industrial Research, I was going to control all the national laboratories and scientific institutions of the country. I was further told that you have been persuaded to accept this tale as a fact.

May I assure you that this story is a pure and simple fabrication and that you should give no credence to it? I know that for some time Kothari's name has been in the air in this connection, and for that very reason I have been extremely careful to avoid all talks on this matter with all persons. I have been asked by Dr. S.S. Bhatnagar to talk to you and the Maulana Saheb in his favour, but I had told him bluntly that the P.M. and Maulana Saheb know their own minds and it will be impertinence on my part to advise them. I have seen Maulana Saheb dozens of times, and you may ask him whether I had ever mentioned anything about this matter to him or he had ever asked my advice on this matter.

I shall be grateful if you would kindly confront me with the teller of this tale, and I would tell him in his face what I think of him. Whether you believe me or not, it is a fact that I have never told a lie or an exaggerated story in my life in any form and that is one of the reasons why I have retained the esteem and affection of hundreds of students who have passed through my hand, and are now occupying very important positions in life.

My request to you is that you do not smother your Desdemonas on the report of men like this particular YAGO (vide Shakespeare's Othello). I sometimes believe that there are too many YAGOs about you, as there have been in history about every person of power, and prestige.

With kind regards,

Yours sincerely,

M.N. Saha.

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7.

SAHA'S WRITINGS ON SCIENTISTS AND OTHER ISSUES

Pyotr Kapitsa: Confinement in the Soviet Union
(Excerpt from an unpublished article by Chitra Roy)

A CHAPTER OF HISTORY: THE CASE OF THE MISSING SCIENTIST

Letters written to and by Meghnad Saha, the eminent scientist, if published, would run into volumes. Saha's was a many-faceted personality. The letters allow us a glimpse at the man and the times he lived in. Perhaps, it will not be an exaggeration to say that during pre-independence years, in spite of his brush with colonial administration, Saha was recognized as the main organizer of and inspiration behind scientific activities in the country. This was recognized even by the colonial masters. Certainly, his was the most audible voice in scientific matters of the day, both within and outside the country. Two letters written by Subrahmanyan Chandrasekhar to Meghnad Saha in 1935 will illustrate this, and the letters would be an eye-opener to the student of the history of science in modern India[43].

43 The letters by Chadrasekhar are included in the Section 5

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The second letter is a grateful acknowledgement of Saha's response (of which no copy is available to us) where evidently Saha recounted the steps he had taken to publicise Kapitsa's case. For this purpose,

Saha had written an article entitled "Professor Kapitsa and Soviet Government" in the June 1935 issue of "Science and Culture" of which he was the editor then, and which happened to be the leading popular scientific journal at the time. To quote from "Science and Culture" article:

> Indian scientific men, who are accustomed to contemptuous neglect by the state in their efforts, have watched with admiration the liberal policy followed by the soviet authorities in matters of scientific research. This has led to an extraordinary growth of scientific contribution by Russian scientists which we can only admire and hold up to our own Government as an example. But it will indeed be a sad day for science if Prof. Kapitsa's activities are cut short in his most productive period. We of course feel that the Soviet Government, following its own enlightened policy, will build for Kapitsa a fine laboratory, but even with the best of intentions, it will take years before such a laboratory can come up to the standard of his laboratory at Cambridge. On behalf of the Indian Scientists, we have therefore much pleasure in supporting the appeal issued by Lord Rutherford and sincerely hope that in the interests of science and humanity, the Soviet Government will allow Prof. Kapitsa to come to Cambridge and continue his researches there. (Saha, Meghnad "Prof. Kapitsa and Soviet Government", ***Science and Culture,*** no. 1, June 1935, p.43)

What is interesting about the article is that apart from presenting the Kapitsa case, Saha expresses his admiration of the way the Soviet Government organized scientific research in that country. Saha enclosed alongside Lord Rutherford's appeal on the subject already published in the "Times" on April 29, 1935. Lord Rutherford had reported in the "Times":

> Last summer Prof. Kapitsa visited Russia as in previous years, gave some lectures there, and was invited to attend the conference in honour of the great Russian chemist, Mendeleev. A few days before his return to Cambridge he was officially informed that he must stay and work in Russia. Professor Kapitsa although he had long resided in England, remained a Soviet citizen (and a loyal one who regularly visited Russia). By the distinction of his work here, which added materially to the already high reputation of Russian science, he was influential in promoting happy scientific relations between the two countries and in securing for his scientific compatriots a cordial welcome in English laboratories. (Lord Rutherford, republished in Saha, Meghnad "Prof. Kapitsa and Soviet Government", ***Science and Culture,*** no. 1, June 1935, p.44)

A sequel to all this comes much later. Kapitsa remained incognito for several years including the war years. After the war the American photo-journal "Life" published a full-scale picture of the scientist with the caption "P. Kapitsa in the company of an unknown scientist". The unknown scientist happened to be Meghnad Saha[44]. During

44 The picture is included in this volume (plate 23)

the summer of 1945, at the invitation of President Komarov of the USSR Academy of Sciences, Saha had visited Russia and met Professor Kapitsa personally. The photo is a record of that meeting. Saha's impressions of Soviet Russia have been given in the book entitled "My Experiences in Soviet Russia" (The Bookman, Calcutta, 1947) and reproduced in The Collected Works of Meghnad Saha, volume 4.

For the student of history, two conclusions are to be drawn from the letters. One, as we have already mentioned, is the influence that Saha exerted on the contemporary academic scene in the 20s and 30s, where scientific matters and scientific personalities were concerned. The second point that emerges is Saha's admiration of the Soviet organization of science, expressed both in the "Science and Culture" article and in ***My Experiences of Soviet Russia*** where he studied extensively the Soviet scientific organization. Evidently he found Kapitsa quite happy with his research environment in Soviet Russia, notwithstanding the anxiety of Kapitsa's erstwhile colleagues in the western world. Whether this centralization would ultimately work well with scientists anywhere in the world is a different question altogether.

●●●

ALBERT EINSTEIN (1879–1955)[45]

45 Meghnad Saha and Satyen Bose produced the first translation of the work of Einstein and Minkowski on Relativity in 1919, that is only three years after Einstein's publication of General Relativity in German. It is noteworthy that Saha wrote this scholarly article when he was an elected member of the Indian Parliament and just one year before he died.

(Reproduced from the Indian Journal of Meteorology and Geophysics, v. 6, 1955)

1. Introduction

On 17th July 1955, a large number of distinguished physicists of the world assembled at Berne, the capital city of Switzerland, to celebrate the fiftieth anniversary of the publication in 1905 of 'Zur Elektrodynamik der bewegten Korper"* — by Albert Einstein: then a name quite unknown to science. This paper ushered in a new age in science: the Age of Relativity.

Einstein lived long enough to see the fruition of this great work and of other great scientific works which flowed from his brain and compelled rightful homage from the whole world. To the great regret of the scientific world, he had passed away a few months before the celebrations.

It will appear paradoxical to the modern generation to be told that the merits of the paper were not immediately recognized.

* On Electrodynamics of moving bodies

It is said that this paper had been previously submitted for the doctorate degree of the Zurich University, but was not approved by the Faculty, because it contained phrases like "Die Einführung des Lichtäthers wird sich als überflüssing sein"— the introduction of a light ether will be shown to be superfluous — which were quite unpalatable to orthodox physical thought of those times. "Young man, write something more sensible", with these words, young Einstein, then 25 years of age, was dismissed by the sages of the Faculty.

Somewhat discouraged, Einstein sent the paper to then leading German journal on physics "Annalen der Physik", where it was published in Ju1y, 1905. In the meantime he worked on Brownian movement, and sent for publication several papers to learned journals. On the strength of these works, he was admitted to the doctorate degree.

By this time his paper on "Relativity" had seen the light of the day, and immediately attracted the attention of the foremost leaders of thought in physical science like Max Planck in Berlin, and H. Poincare and Madame Curie in Paris, who wrote to him congratulatory letters which were very flattering to a young man still unknown to fame. From 1906, he suddenly burst into scientific fame, which later became world-wide after confirmation of the predictions from his Generalised Theory of Relativity published in 1916. The halo round him had not diminished in lustre till his death a few months ago.

2. Why is the theory of Special Relativity considered Revolutionary and how had the ideas contained in this paper shocked the older orthodox physicists?

Since the time of Galileo and Newton, the science of physics had been built on three fundamental conceptions, viz., those of mass, time and space. Each one of these conceptions is fundamental and defies definition. We sometimes say: mass is the quantity of matter, time is universal flux, and space is what we measure by measuring rods. These are not definitions but attempts at clearing our confusions.

The laws of dynamics make use of these concepts and, in the form

Mass × acceleration = force

enunciated first by Galileo (1570-1642) or its various refinements, have been shown to account for the laws of planetary motion (Newton, 1642-1727) and innumerable phenomena in physics. But doubts began to be expressed about the correctness of these ideas with the rise of electrodynamics (1880) and discovery of the electron In 1897.

It was found impossible to define the aether the all-pervading medium which was supposed to be the seat of all electromagnetic phenomena, including light, vide for example the failure of Michelson-Morley experiments and a number of other experiments. The mass of the electron was

experimentally found to vary with velocity, thus knocking down one of the fundamental concepts of classical dynamics, in which "mass" is regarded as invariable.

Voigt and Lorentz tried methods of finding out transformation in space and time coordinates which would account for these anomalies. In fact what are known as "Lorentz-Einstein transformations" were first given by Voigt of Gottingen in 1887, on purely mathematical grounds.

But these contributions, ingenuous as they were, remained unnoticed on account of their highly artificial nature. It was only in 1905, when working as a clerk in the Swiss Patent Office at Berne, that Einstein was able to give simple physical deductions of the 'Lorentz-Einstein transformations" by making use of the concept of simultaneity, and the constancy of the velocity, of light when observed from any frame of reference. His "Elektro-dynamik der bewegten Korper" remains a classic in the history of science*.

This paper and others which followed brought out a lot of revolutionary ideas, which could be put to experimental test, the ultimate court of appeal of all theories. Some results may be mentioned.

The mass of a particle is not invariant, but varies with its velocity v according to the law $m = m_o/\sqrt{1 - v^2/c^2}$ where m_0 = rest-mass. This deduction was neatly verified in the laboratory up to velocities approaching that of light.

* It is said that at a war bond rally in Kansas, a manuscript copy of this paper was sold for six million dollars, see Einstein, Philosopher and Scientist, p. 695

Mass and energy are equivalent, $E = mc^2$, so that if we have a measuring system in which the velocity of light is unity, m should equal E.

This law was verified by Lord Rutherford and his co-workers in the nuclear experiments earned out about 1919. It forms the corner-stone of nuclear physics. The theory of special relativity thus knocks down the classical ideas on mass, time and space, and demands a new formulation of dynamics.

3. Einstein's Early Years

Albert Einstein was born in a small Swabian Jewish community at UIm, Wurttemberg, on 14 March, 1879. He spent his boyhood at Munich where his father had temporarily settled to pursue his business interests in some electrochemical works. Here he attended the Humanistische Gymnasium.

In 1894, his family migrated to Italy, but young Albert was sent to a cantonal school at Aarau in Switzerland. We learn from his autobiographical notes that he was attracted to mathematics at quite an early age and had mastered, between the ages 12-16, most of the elements of mathematics, including differential anti integral calculus, basic ideas of analytical geometry, infinite series, etc. His interest in natural sciences was roused by his reading of Bernstein's People's Books on Natural Science, a work of 5 or 6 volumes, which he read over and over again "with breathless attention".

From Aarau, Einstein went to Zurich and studied at the Federal Institute of Technology,

where he attended the mathematical lectures of Hermann Minkowski, who later in 1908 applied the ideas of four dimensional geometry to the special theory of relativity and put the results in a very elegant mathematical form. Sommerfeld tells us that, strangely enough, no personal contacts developed between Minkowski and Einstein; and when Minkowski in 1908 developed his "World Geometry", now a classic in the special theory of relativity, Einstein is reported to have once remarked, "Since the mathematicians have invaded the theory of relativity, I do not understand it myself any more." Later on, however, when Einstein took upon himself the problem of working out the general theory of relativity, he readily acknowledged the indispensability of Minkowski's four-dimensional scheme.

At the Federal institute of Technology, Einstein found greater interest in physics and spent more time in the physical laboratories. He also got interested in the works of Kirchhoff, Helmholtz and Hertz and studied their papers at leisure hours.

Einstein left the Federal Institute of Technology in 1900 and spent a year as a tutor at Schaffhausen. He used to spend his leisure hours at the library of the Patent Office, poring over papers and books on mathematics and physics. Here he attracted the attention of the Director, on whose recommendation, the impecunious youth, as he was then, was appointed examiner of patents at the Federal Patent Office in Berne, a position which he held till 1909. At this time he also registered himself as a Swiss citizen. These seven or eight years that he spent at Berne as an examiner of patents were the most fruitful and productive in his scientific career. A spate of papers, all of the highest order from the point of view of originality and some of them of profound fundamental importance, appeared in quick succession, mostly in Annalen der Physik, on a wide range of subjects— statistical mechanics and thermodynamics, kinetic theory, quantum mechanics and relativity. In one single fruitful year, 1905, he published papers on:

(i) ***The photo-electric equation,***
(ii) ***Brownian movement,***
(iii) ***The special theory of relativity,*** and
(iv) ***A new determination of molecular dimensions.***

The following year appeared his famous theory of specific heat in which he successfully applied Planck's quantum ideas to explain the anomalies in the value of specific heat discovered by Nernst and his co-workers.

The photo-electric effect, that is, the emission of electrons from illuminated metal plates, was discovered by Hertz in 1887. The fact that electrons are emitted only when the frequency of the incident light exceeds a certain threshold frequency resisted all attempts at explanation on the basis of the classical electromagnetic theory of light. Einstein, who had been following Planck's theory of black body radiation with great interest, found the answer by adopting far bolder view that emission and absorption both take place in quanta and that

radiation itself has a quantum or corpuscular-like structure. On the basis of this revolutionary concept of light- quanta, he was immediately able to deduce his famous photoelectric equation. In the citation on the Nobel Prize award (1921) to him, only the photo-electric equation is mentioned, so that it appears that even by 1921, the dispensers of this great prize were not able to make up their mind regarding the merits of his contributions to Relativity.

In 1905, Einstein worked out the complete theory of Brownian motion and expressed the displacement of the particles In the following equation:

$$\overline{x^2} = 2KUtT$$

where $\overline{x^2}$ is the average of the squares of displacements of particles, K the Boltzmann constant, t the time, T the temperature and U a constant containing the viscosity co-efficient of the liquid medium. The Avogadro number, determined by using this equation and measuring the displacements of particles under a microscope, agreed with the values obtained by Perrin. The equation gave a complete theoretical basis of this puzzling phenomenon.

4. Einstein at Berlin (1913-1933)

The scientific world was quick to recognize the great originality and even the revolutionary character of Einstein's contributions. In 1909 he was appointed an associate professor of theoretical physics at the Zurich University. Two years later he was offered the Chair of Physics at Prague which he accepted only to return to the Federal Institute of Technology at Zurich as a full professor of theoretical physics the same year. Offers also came from Germany. In 1913, the famous physical chemist Nernst, who recognized Einstein's merit from his contributions to specific heat theory, a subject which was Nernst's own child, succeeded in getting him to accept a professorship of the Berlin University, specially created for him and the Directorship of the Kaiser-Wilhelm Physical Institute in Berlin.

His friendship with the great German scientists — Planck, Nernst, Sommerfeld, von Laue, and others remained unshaken through the dramatic vicissitudes of his own career till the rise of Hitler.

He was also elected a member of the Royal Prussian Academy of Sciences and given a stipend sufficient to enable him to devote all his time to research without any routine duties. This was an ideal situation for Einstein, for he never liked delivering lectures. At Zurich he was obliged to deliver lectures to students much to his dislike. He never had any orderly lecture-manuscripts; whatever he managed to prepare generally got 1ost by the time he had to deliver them. At Berlin he was free from these obligations, although he could freely choose to deliver lectures at the University. Sommerfeld tells us that the completion of his general theory of relativity was in no small way due to the leisure he enjoyed in Berlin.

Einstein was soon elected member of many scientific academies and societies in and

outside Germany, for example, of the Bavarian, Amsterdam and Copenhagen Academies. He was elected a foreign member of the Royal Society of London in 1921. A number of Universities-Geneva, Manchester, Rostock and Princeton, had also conferred honorary degrees upon him. He was awarded Nobel Prize in physics for his work on the photo-electric phenomena in 1921. The Copley Medal of the Royal Society and the Gold Medal of the Royal Astronomical Society of London were awarded to him in 1925 and 1926, respectively, in recognition of his theory of relativity.

The political troubles in which Einstein got involved after World War I, which reached a climax during Hitler's regime, are well known. He was already marked for his leftist views. When the repression of the Jews began, he found conditions of living in Germany almost intolerable for him. During the "relativity-rumpus" which occasionally sank to the level of anti-semitic mass meetings and demonstrations, he seriously considered leaving Berlin and it was only the persuasive powers of Max Planck, for whom he had great respect, the he repeatedly revised his decision. Matters came to a head in 1933 shortly after Hitler came to power. For a number of his statements which appeared in the French and the American press, he was charged of participation in atrocity-mongering in foreign countries and of giving convenient handle to the enemies of Germany.

He was obliged to withdraw his membership of the Prussian Academy of Science, The same year (1933) while he was on a lecture tour in England and America, he was deprived of his Berlin professorship and his post as Director of the Kaiser-Wilhelm Physical Institute. Even his personal possessions were confiscated.

5. Scientific Life at Berlin: The Generalized Theory of Relativity

The Berlin period (1913-33), saw the most fruitful years of Einstein's life – the period when he gave out to the world ***'The Generalized Theory of Relativity'***. He had, during these years, the congenial company of a great many giants in physics in the German capital city. Planck, Nernst, Haber, von Laue, and a host of younger workers who attained great reputation later and who had a genuine appreciation and understanding of his greatness. To this group may also be added Sommerfeld, though he lived in Munich, and Born though he lived at Göttingen. He was a regular visitor to the physical colloquium held once weekly at the Physikalische Institut of the University, and listened very carefully to the discussions, but seldom spoke. The World War I, which intervened, does not appear to have much effect on his scientific work.

Here in 1915, he presented before the Royal Prussian Academy of Sciences, a report of his work on the Generalized Theory of Relativity published later as ***Die Grundlage der Allgemeinen Relativitäts-theorie in the Annalen der Physik,*** **49**, 1916. But the ideas appear to have been present in his mind since 1911, when he contributed a paper to the *Annalen* on interpretation of Eötvös's experiments

on the equivalence of inertial and gravitational masses.

In this and other series of papers which were to follow, Einstein set to himself the following fundamental question: Physics recognises the principle of inertia as the keystone of physics, and the experiments by von Eötvös had shown that the inertial mass was equal to the gravitational mass. For enunciating the principles of inertia, we have to conceive a system of reference (the Galilean system) which is supposed to have an absolute space connotation; and after the rise of the special theory of relativity, the space connotation should be changed to the absolute space-time framework connotation, But it is contrary to the mode of thinking in science to conceive of a thing which acts itself but cannot be acted upon. The space-time continuum itself, therefore, should be modified by the gravitational field. The special theory of relativity concerns itself with the invariance of the physical laws in two systems of reference, one of which is moving with uniform velocity relative to the other. This uniform motion is a restriction, that is why it is often called as the restricted theory of relativity. Einstein's *general* theory of relativity removes this restriction, the relative motion of the two systems need not be uniform but is *accelerated*. Let us consider an observer stationed inside an elevator which falls freely from the top of a sky-scraper. If he places his watch in the mid-air of the elevator, it would not fall to the bottom but remain stationary there. Newton's law of inertia which states that every material body continues its state of rest or uniform motion until disturbed by external force is valid within the elevator that falls freely; whereas an observer stationed on the earth finds that all bodies when left in the air fall to the ground because of the gravitation. We have here two systems of reference in relative accelerated motion, the system in the elevator may be called the inertial system because in it the force of gravity has been wiped out and as such the law of inertia is valid; whereas in the other system, to the observer stationed on the earth the forces of gravity exists. So when we consider the two systems of reference moving relative to each other with accelerated motion, a transition from one system to another involves the presence or absence of the force of gravitation. So, the force of gravitation becomes linked up with the geometrical description of the events in the system of reference. The next logical step is to formulate the laws of nature in such a way that they are valid in any of the systems. Einstein solved the problem by incorporating the force of gravitation within the geometry of the system and by an invariant formulation of physical laws valid for any arbitrary system. The space-time continuum of the inertial system is Euclidean, whereas for any other system in which the force of gravitation exists he takes the four-dimensional continuum to be non-Euclidean.* So the geometry of the world

* The mathematical apparatus in which Einstein expressed his ideas were given about a century earlier by Riemann and put into an elegant mathematical form by Schwarz and Christofell, which Einstein followed.

and the force of gravitation become synonymous; the presence of mass deforms the surrounding space-time continuum. The square of the distance between two adjacent world-points is given by

$$ds^2 = g_{\mu v}\, dx^{\mu}\, dx^{v}$$

when $g_{\mu v}$ is the metric tensor (symmetric) and depends on the distribution of the masses and their motions in the four dimensional continuum. In the absence of the masses the metric tensor reduces to

$$g_{11} = g_{22} = g_{33} = 1,\ g_{44} = -1,\ g_{\mu v} = 0,\ \mu \neq v$$

We then have the flat Euclidean space. Further, Einstein discarded the Newtonian concept of action-at-a-distance which implies that, if a new star is born out of nothing, it exerts force instantaneously all around the universe. Einstein's formulation of the gravitational action is a field theory in line with the electromagnetic field theory of Maxwell. So the gravitational influence, according to the field concept, spreads with finite velocity from the source to other bodies, the field quantities that describe the gravitational action vary with position and time in accordance with the field equations of Einstein which in an empty space take the simple form $R = 0$ where $R_{\mu v}$ is the contracted Riemann-Christoffel tensor tensor and is a function of the $g_{\mu v}$'s.

The salient points of the general theory of relativity are: (1) The formulation of the field equations that describe how the gravitational field varies in space-time continuum and the equations of motion of a particle in such a field; (2) The above equations are valid in any arbitrary system of reference; (3) The geometry of space-time deformation replaces the concept of the gravitational force.

6. Consequences of the General Relatively Theory

Now we shall state some of the consequences of the General Relativity Theory.

In the first approximation the theory gives the same result as the Newtonian theory of gravitation, but when higher approximations are taken we have the following results which were worked out by Einstein and given as predictions to be verified.* They are:

(a) *Light passing past a field of gravitation will suffer deflection which can be calculated*

This can be verified only during a total solar eclipse by photographing the field of stars about the sun. The deflection according to GR theory, would be 1.74" for a star just beyond the solar disc, while according to the Newtonian theory, i.e., taking a light particle to possess a mass of hv/c^2, it would be just half. viz. 0.87." These predictions were made in the midst of the First World War; but they happened to leak through

* The considered view of the scientists who assembled at the Jubilee of Relatively Theory at Berne is that the deduction of the General Theory of Relativity needs careful re-analysis in the light of recent interpretations of the experimental results.

the blockade to the outer world, and received attention from the Royal Astronomical Society of Great Britain. On the initiative of the society, eclipse expedition were organized by the observatories of Greenwich and Cambridge, who obtained for the deflection the values $1.98'' \pm 0.12''$ and $1.61'' \pm 0.30''$. This confirmed Einstein's prediction that the gravitational deflection of light on the surface of the sun was ~1.74″, i.e, twice that of the Newtonian deflection of 0.87″. (***N. B.*** – Since 1919, many eclipse expeditions were organized to obtain the correct value of the gravitational shift. It will be out of place to discuss them here).

(b) ***The wavelength of monochromatic radiation emitted by a stellar body will be increased in direct proportion to the value of the gravitation potential on the surface of the stellar body***

On The sun, this amounts to a velocity effect to the red of 0.65 km per cm giving for a wavelength of 3883 Å the shift of 0.008 Å.

It is extraordinarily difficult to measure such a small deviation correctly, as wavelength of lines on the sun are subject to may influences, e.g. pressure shift, Doppler effect etc. Many celebrated workers like St John and Evershed have claimed to have shown the shift on the solar surface to be nearly 0.008 Å. but the results have not appeared convincing to most readers.

But a confirmation of this prediction came from a very unexpected quarter. The bright star Sirius has a faint Companion which had puzzled astronomers for a longtime. While Sirius has a mass of 4 times the solar mass, the Companion has about a mass nearly equaling the solar mass, though it is 6000 times fainter than Sirius. This might be due two facts –

(1) that the temperature of the companion is much smaller than that of Sirius. This probability is excluded by an examination of the spectrum of the Companion which shows it to be of the C-class ($\theta = 8500^0$ K) while that of Sirius is of A-class ($\theta = 10{,}000^0$ K). So we turn to the second possibility.

(2) the surface of the Companion is extremely small, compared to that of Sirius.

Thus the GR Theory is responsible for the discovery of Astronomy of the density of the Companion which reaches the extraordinary value of ~50,000, i.e., a rupee on the companion will weigh a ton, as Eddington graphically put it. This conclusion was arrived at by Böttlinger and Eddington, and the latter pointed out that it will render the gravitational potential on the surface of the Companion extraordinarily high and the relativity shift would be 0.30 Å. instead of the puny amount of 0.008 Å. on the sun.

On putting the matter to test, Adams at Mt. Wilson found the shift of H-lines on the Companion to be 0.32 Å.

Thus the GR Theory is responsible for the discovery to Astronomy of a remarkable class of stars, the white dwarfs, which probably would have otherwise taken an indefinite period to be discovered.

(c) *Motion of the perihelion of planets*

Since about fifty years from the time of the astronomer Le Verrier, it has been known that the perihelion of the planet mercury has a motion of 43″ per century. This could not be accounted for by Newton's theory of gravitation and was an outstanding problem in theoretical astronomy. Einstein explained it from the GR Theory, and gave the expression

$$\delta\,(in\ radians) = \frac{24\pi^3 a^2}{(1-e^2)\,c^2 T^2}$$

a = semi-major axis
T = period of revolution in seconds
e = eccentricity of the orbit.

This completely accounted for the perihelion motion of mercury.

Verification of these predictions greatly enhanced the prestige of Einstein, and he became the recipient of honours from many scientific societies and academics. At the same time he became deservedly a world figure — the only scientist whose name became familiar to the man in the street all over the world.

But his socialistic views, which were well-known, made him a bête noir amongst the extreme sections of German nationalists though the rank and file of German scientists claimed his achievement as a great triumph for German Science. In non-German countries, however, his Germanic origin was scarcely referred to. His achievements were regarded as triumphs for a cool thinking Jewish savant, who could achieve such a great feat because he stood aloof from the saber-rattling of extreme German nationalist. Speaking of these polemics, Einstein remarked with a dry humour, "Had my prediction been not verified, the opinions would have been just reversed. The non-German world would have denounced me as a metaphysical German, and the Germans as a pedantic Jew".

7. Cosmology

We have seen that presence of masses make the space-time continuum non-Euclidean or curved—to put it crudely. When the space-time is curved, the question arises whether it is closed or not, the former will make space-time expanse finite and the latter infinite. What do we mean by saying that the space-time expanse is finite? When it is finite, a light signal sent from any point would return back to that point after a lapse of time. It is difficult to conceive things in a four-dimensional space, so for simplicity we consider the two dimensional surface of a three dimensional space, say an ant crawling straight forward on the surface of a tennis ball will always come back to the starting point after some time. The size of the universe would depend on the degree of curvature of the space which, in Its turn, depends on the distribution of masses in the universe. We shall see what data the giant telescopes reveal about the distribution of masses. Einstein in a paper to the ***Proceedings of the Prussian Academy of Sciences (1917)*** set us

thinking on the cosmological consequences of the General Theory of Relativity. The conclusion of the paper was wrong because of an error in the calculation but the idea and the method of approach was taken up by de Sitter, Friedman, Lemaitre and Robertson.

The entire universe consists of islands of matter in an otherwise sea of almost* complete emptiness. We call these islands of matter as nebulae; the solar system together with the nearest stars constitute one such nebula which is our galaxy. Our scale of distance in the universe is the light-year — a distance that light travels in one year. The average diameter of a nebula is about 20,000 light-years and the average distance of separation between them is 3,000,000 light-years. The nebula farthest from us is at a distance of half-a-billion light-years. Within the range of two billion light years, as a 200-inch telescope can reach, there are something like a billion nebulae according to Hubble. That gives an idea how sparsely populated the universe is with nebulae.

There is another important feature about the nebula, their spectra show a red shift (Döppler effect) which means the nebulae are running away from us. Moreover, it was found by the Mount Wilson astronomers, Hubble and others that the degree of red-shift increases with the distance, the more distant the nebula from us, the greater is the speed of the running nebula.

The cosmologists assume that the universe is uniform with respect to the distribution of the nebulae and further that the nebulae are not only running away from us but also running away from each other. The proposed cosmological models are based on the principle of uniformity and the consequence of the redshifts.

We maintain that the whole matter of the universe was concentrated in a very small volume— as electrons, protons and neutrons, then something happens and the fragments of matter start flying apart from each other, the distances these fragments have travelled depend on their speed. That explains why the speed of a nebula is in proportion to its distance from us. So far it is all right. But the velocity of recession appears to be too large, making the age of the universe to be too small. It makes the universe expand from a highly concentrated mass about five billions of years ago, while the age of archean and prearchean rocks on the earth, from well documented experiments, is found to reach the value of at least three and according to some authorities six billion years. Thus we arrive at the paradox: *the child is older than the parent.* There have not been enthusiasts wanting who would sacrifice the evidence of ages of rocks in favour of the deduction from the relativity theory but Einstein himself, as expected, does not favour this view. He expresses the difficulty in the following words:

"The situation becomes complicated by the fact that the entire duration of the expansion of space to the present, based on the equation

* Almost because the internebular space contains matter, to a density of about to 10^{-30} gm per cm^3.

in their simplest form, turns out smaller than appears credible in view of the reliably known age of terrestrial mineral. But the introduction of the *'cosmological constant'* offers absolutely no natural escape from the difficulty. This latter difficulty given by way of the numerical value of Hubble's expansion constant and the age measurement of minerals, completely independent of any cosmological theory provided that one interprets the Hubble effect as Döppler-effect."

8. Contributions to Quantum Physics

The Berlin period also saw the phenomenal rise of the quantum theory, and of wave-mechanics which, taken together, have thrown a flood of light on radiation and atomic phenomena. Though in the midst of these great discoveries, Einstein appears to have been little attracted to them, except on two occasions.

The first occasion was in 1917 when, poring over the implications of the Bohr atom, he wrote a paper on Einstein A and B-coefficients, the A-coefficients denoting the spontaneous transition probability of excited states to lower ones, the B-coefficients denoting the transitions to higher and lower states under the effect of radiation. These contributions are rightly regarded as fundamental to atom physics.

It was these considerations which led to a collaboration between Einstein and Ehrenfest under the title "Quantum theorie des Strahlungsgleichgewichts" in the ***Zeitschrift für Physik, Vol. 19, pp. 301-306.*** While the author of this note was studying this paper together with S.N. Bose in 1924, the latter was led to a very ingenuous deduction of the Planck law of radiation. He showed that the number of cells A in a phase space can be determined by the expression

$$A = \frac{1}{h^3} \int \int \ldots. \, dp_r dq_r$$

the integration extending over the whole phase-space covered by the particles.

One can then proceed to fill up the phase-space by the number of quanta available by using combination with repetition, (a device already taken by Debye), and thus get the Planck law in a very simple manner. This is in fact what was done by S.N. Bose.

Bose's paper, published in the ***Zeitschrift f. Physik***, Vol. 26, 1924 attracted the attention of Einstein (in fact, he translated it into German) and he made a very ingenious application of it to explain to 'gas-degeneration', a phenomenon with which he had become acquainted in course of his visits to the laboratory of his friend Prof. W. Nernst, in the ***Physicalische Chemische Institut of the Berlin University*** (Sitzungsber d. Preuss, Akademie, 1924 and 1925). Einstein showed that Bose's method of calculating the number of phase-cells could be extended to a statistical system representing a gas, and the most probable state of the gas can be worked out by using permutation with repetition. He thus worked out a theory of gaseous matter at low temperature, and tried to explain the deviations

from the perfect gas law discovered by Nernst and his co-workers. Fermi, on the other hand, used combination *without* repetition and worked out what is known as Fermi-Dirac Statistics. It is how recognised that while the Fermi-Dirac theory correctly represents the statistics of particles with spin '½,' the Einstein-Bose theory correctly represents statistics of particles with spin zero, or an integral number. It was later shown, from wave-mechanics, that the two modes represent the two different ways of combining wave functions representing particles viz., the symmetrical and the antisymmetical ways.

9. Einstein in the USA

After his expulsion from Germany, Einstein received requests from a number of countries to become their citizen. He finally decided in favour of USA and settled at Princeton, whose famous Institute for Advanced Studies offered him the kind of quiet atmosphere for study and thinking he liked. For the remaining 22 years of his life, Princeton remained Einstein's home.

The important part played by Einstein in persuading the government of President Roosevelt to take interest in the development of military application of atomic energy is now well-known. Many scientists from Europe, Jewish as well as non-Jewish, had taken refuge in the USA to escape from the anti-Semitic orgies of Hitler and Mussolini. They included, *e.g.*, men like Fermi, Szilard, Wigner and a host of others who were notable nuclear scientists.

World War II broke, out in September 1939, and the years 1939, 1940 and 1941 were years of great anxiety for the whole world. It appeared that the Nazi hosts were going to overrun the whole world, and impose their rule over the major parts of the world. Public opinion in the USA was thoroughly alarmed, and everybody began to think how the defeat of the Nazis could be achieved.

Nuclear fission had been discovered, early in 1939, in Germany by Otto Hahn and many scientists of different countries had foreseen its potentialities. In fact, Joliot-Curie and Halban in France, Fermi, Amaldi and their group in Italy, Flügge and others in Germany had already thought of turning nuclear energy to useful work by making use of the fission process, and they had independently thought of the reactor. The foreign scientists who had escaped from Nazi oppression in Europe, notably Fermi, Szilard and Wigner had much clearer ideas about the possibilities of developing military use of nuclear energy than most American scientists, but they recognized that it required effort on a huge scale which only the State could afford. In the year 1939-40, Szilard and Wigner persuaded Einstein to write to President Roosevelt about the military possibilities of atomic energy and the desirability of embarking upon an intensive programmed of nuclear research and development to that end. A suggestion from Einstein could not be ignored and Roosevelt immediately appointed an Advisory Committee under the Chairmanship of Lyman Briggs, the Director of the National

Bureau of Standards. This Committee reported unfavourably on the subject, but the matter was again referred to Prof. A.H. Compton who, along with his students, had done some work on nuclear fission. Compton reported favourably and work (the Manhattan project) went on with mounting enthusiasm and suspense, culminating in the first atomic explosion at Alamogordo in the desert of New Mexico on 16 July, 1945, that ushered in human history a new age, *the Atomic Age*. To be true, the Atomic Age bad really begun with Einstein in 1905 when from his special theory of relativity he deduced his famous mass-energy equivalence law $E = m_0c^2$, the basic principle of any atomic energy development.

Einstein had later to regret his decision for having advised President Roosevelt to start 'Atomic Energy Work'. For the Atom-Bomb was used on two Japanese cities without warning, killing nearly 3,00,000 innocent people within the twinkling of the eye, and unleashing the cold atomic war. Einstein foresaw the horrors of the atomic war, returned to its denunciation with all the authority associated with his great name, but without the slightest effect on protagonists of war. He realized that a Frankenstein monster has been created, which was about to devour its own creators! He died a deeply disappointed man, full of gloomy foreboding to future! His last legacy was a touching letter to the philosopher, Lord Bertrand Russell, advocating disarmament, banning of atomic weapons, and universal peace.

10. Epilogue

Einstein was easily the greatest scientific figure of his age, which was one of the greatest in science, and he was one of the giants of all time. He lived in the midst of science, yet apart from its routine. Picking up 'pearls' from an amorphous mass of matter was his specialty and in this, he was eminently successful.

His was essentially an 'One-track mind". He was always working his mind would be full of the work he had in hand and any attempt to deflect him away from the problem which occupied his mind turned out be rarely successful. It is said that one of the professors of the Princeton University had some difficulty with theories of specific heat, and fixed up an appointment with Einstein, who asked him, when the latter presented himself, what was the specific purpose for which he had chosen to call on him. He said that be wanted to discuss with him some difficulties he had in the quantum theory of specific heat. 'But I have never worked on that subject". "Why", said the astonished Professor "yours was the pioneering contribution to this subject?" "Really, but cannot recall when I worked on it", said Einstein, and It required some effort on the part of the bewildered professor to convince Einstein that he had really made the first contribution to the modern theory of specific heat!

Unlike many other great scientific men he was entirely free from jealousy of his contemporaries.

He was singularly free from personal jealousy from which many eminent workers suffered, e.g., Newton and Leibnitz, Newton and Hooke, and many others.

Unlike many other scientific men, he was ready to admit his mistakes and shortcomings. He conceived the idea of proving the existence of "molecular magnets" and devised the experiment now known as the 'Gyromagnetic Effect' which enables one to show that electrons are responsible for the magnetism of metals. The method enables one to find out the ratio between magnetic moment and angular momentum of electrons, which comes out to be e/2mc when magnetism is wholly ascribed to the orbital motion of electrons. In 1918, Einstein and de Bass performed this experiment, the only experiment Einstein performed in his life using the tenuous method of resonance, and found the experimental value to be almost exactly (e/2mc). Later this experiment was repeated by E. Beck in the Zurich hs1tute of Technology who found the ratio to the more nearer e/mc. When Einstein returned to Zurich, Beck showed him his experiments and explained his hesitation at publishing a result which contradicted Einstein's. Einstein carefully went through the experiments and remarked *"Perhaps you are right, for you have performed the experiment much more carefully than we had done! You should not hesitate to publish the result."* It is now known that Beck was correct, for the magnetism in metals investigated by Einstein and de Haas, and Beck, is mostly due to the spin of electron, for which the ratio is e/mc. But at this time, there was no idea of rotating electrons, and Einstein and de Haas's result appeared to be iron-clad. But still Einstein had the magnanimity to acknowledge the superior experimental technique of Beck*.

We can thus divide Einstein's scientific life into three periods:

(1) The Swiss period, 1900-1913
(2) The German period, 1913-1933
(3) The American period, 1933-1955

Of these the first two periods were the most fruitful, creative years of his life. The Swiss period saw the blossoming of his genius, the German period its full ripening and maturity. Probably he could achieve so much because he stood on the shoulders of the great workers who passed before him – Voigt and Lorentz, Michelson and Morley, Eötvös and Riemann. The American period has not been so productive, not because his powers were declining, but more probably because the stimulus given by his genius had not fully worked out its course on the sciences of astronomy and physics.

Strangely enough, though taking interest in wave mechanics which has enriched the physical sciences beyond recognition since 1926, he did not believe in many of its philosophical Implications. "I cannot believe", he remarked to the present writer in course of a conversation in 1927, that "the dear God has made the α-particle a wave".

* Heard the story from Beck himself.

Later he said "I am, in fact, firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that the theory operates with an incomplete description of physical systems."

It is this belief which kept him from falling in line with the opinion of all contemporary theoretical physicists.

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SIR JAGADIS CHANDRA BOSE (1858-1937)

Jagadis Chandra Bose (plate 9) was born on 30 November 1858, in the town of Mymemsingh in Bengal, where his father, Bhagwan Chandra Bose was then posted as a Deputy Magistrate. He spent the early years of his life in the town of Faridpur. Later he came to Calcutta and joined the St. Xavier's College, an institution maintained by the Society of Jesuits. Here he studied Physics under Father Lafont, an inspiring teacher, and, like the latter, he developed in later life a flair for lecture demonstrations. After graduating in Calcutta he went to London to study medicine, but owing to repeated attacks of malaria, which he had contracted prior to his departure for London, he gave up the study of Medicine and took up Natural Science. He went to Cambridge and joined Christ's College. There he came under the influence of such teachers as the late Lord Rayleigh, Sir James Dewar, Sir Michael Foster and Francis Darwin. In the early eighties he graduated in science from London and Cambridge universities. On returning to India he was appointed a professor of Physics in the Presidency College, Calcutta, in 1884. His connection with this College continued unbroken till his retirement from Government service in 1915, when he was made an Emeritus Professor.

From 1896 to 1931, he went out on successive lecture tours to Europe and America. He was knighted in 1917 and elected a Fellow of the Royal Society in 1920. He was also a member of the Academy of Sciences, Vienna, and of the Committee for Intellectual Co-operation of the League of Nations. With increasing age Bose gradually withdrew from all public activities and for the last few years lived in retirement. He passed away peacefully on the 23 November 1937, at his summer house at Giridih, a small town in the Manbhum district of Bengal.

Besides his scientific work, of which an account is given later on, Bose's great achievement was the building up of a research institute known as the Bose Institute of which he made a gift to his nation. Bose realized in early life the difficulties which Indians had to face in obtaining adequate facilities for research work in their own country. Since 1896 he cherished the idea of founding an institute on the model of the Royal Institution, London, which should provide facilities for original work and also for the dissemination of the results of scientific investigations by means of popular

lectures. For this purpose he began to save all his surplus income, which he invested under the most favourable conditions. The receipt of some private benefactions and of public donations enabled him to start the Institute in. 1917. At present the income of the Institute from endowments is nearly £4000 a year, and an equal amount is received from the Government of India as an annual grant. In the foundation of the Institute, he received great help from the then Secretary of State for India, the late Edwin Montague.

His other benefactions were not inconsiderable. After his death Lady Bose has, according to the directions left by her husband, given away in donation approximately £30,000 for the endowment of Fellowships and Scholarships for biophysical research and for primary education and temperance work.

Bose was a man of many-sided activities and interests. He was a friend and supporter of many of the foremost artists and writers of Bengal, and he himself was a writer of some distinction in Bengali. His friendship with Rabindranath Tagore during the most creative periods of their lives is an interesting chapter in the cultural history of Bengal. The Institute buildings erected under Bose's direction are a beautiful example of Hindu architecture adapted to the requirements of modern life. He was an artist in his sensibilities and romantic by temperament. His intense nationalism was based upon a sympathetic understanding of the ideals and traditions of epic and Buddhistic India.

Bose married in 1887 Abala, second daughter of Mr. Durga Mohan Das, a leading advocate of the Calcutta High Court, a reformist and a political leader. Their fifty years of married life was very happy, full of varied and interesting experiences. Lady Bose was a constant companion and helpmate for her husband, accompanying him in his early tours to places of religious and historical interest in India and in many excursions to Himalayan heights and glaciers. Later in life, she accompanied her husband in all his lecture tours to foreign countries.

Besides completely identifying herself with her husband's work, Lady Bose has done a large amount of pioneering work in the sphere of women's education in Bengal, in the training of widows as teachers, and in the organization of home industries.

Scientific Researches

Bose's scientific researches fall under three periods. In the first period he was engaged in the construction of compact apparatus for the generation of short electromagnetic waves and in the study of their optical properties. From a detailed study of the materials used as coherers for the detection of these short waves, he discovered that a large class of inorganic substances under the influence of different kinds of stimulation, mechanical, electrical and optical, responded in a way which showed unexpected similarity with the behaviour of living tissues—this similarity was shown under the influence of

stimulation of different degrees of intensity and persistence leading to the phenomena of fatigue and recovery from fatigue after rest. It was during this period that he formulated the generalization usually associated with his name 'On the similarity of response in the living and the non-living', which was first stated in a paper communicated to the International Science Congress held in Paris in 1900.

The second period of his investigation was concerned with the intensive study of the behaviour of plant tissues under different modes of stimulation, with the idea of showing the exact parallelism in the behaviour of animal and plant tissues. During the course of these investigations be devised many delicate and accurate instruments for the automatic recording of plant responses.

Bose's first paper, published in the Proceedings of the Asiatic Society of Bengal in May 1895, deals with the polarization of electric waves by double refraction. His first communication to the Royal Society was published in the Proceedings in October 1895. About this period he, simultaneously with Lebedew and Righi, constructed apparatus for the generation of very short electromagnetic waves, of about 1 cm. wavelength. His investigations were concerned mainly with the demonstration of the similarity in the optical properties of these short electromagnetic waves with those of light waves. Considering the very primitive workshop facilities available in Calcutta at this time, the very compact nature of his apparatus excited a great deal of appreciation in England, and was described in many textbooks of this period by J.J. Thomson, Poincare and others.

Like other investigators in this field, Bose had to devote a great deal of attention to the capricious behaviour of his *coherer*, which consisted of a number of contacts between metallic filings whose resistance altered under the impact of electric radiation. A detailed investigation led him to the view that this so-called "coherer effect" was characteristic of a large class of contact sensitive substances including semiconductors like selenium, iron oxides, etc. Bose and Shilford Bidwell may be mentioned as pioneers in the field of investigation of the properties of photo-conductivity and contact rectification shown by this class of semi-conductors, All these effects, including an interesting theory of the origin of latent image in photographic plates, Bose tried to explain by a molecular stress and strain theory. The subsequent study of the fatigue phenomena shown by these substances led Bose to postulate his theory of the similarity of response in the living and the non-living.

Bose's Contribution to Plant Physiology

Bose was unusual as a physicist in talking up biological materials for his investigations, and the major portion of Bose's research work was concerned with comparative physiology and with plant physiology in particular. The main thesis of Bose's biological investigations was to establish the generalization that all the characteristics of response exhibited by animal tissues are equally

exhibited by plant tissues.

At the Paris International Congress of Physicists (1900), Bose read a paper in which be showed parallelism between the conductivity changes of ferromagnetic magnetite and the curves of muscle contraction. His conclusion was that both these phenomena represent the same fundamental molecular reaction common to all matter. In 1901 Bose submitted to the Royal Society a preliminary note "On the electric response of inorganic substances", the main conclusion of which was that plant tissues offer an interesting link between inorganic substances and animal tissues. In this note he showed how he had obtained from plants strong electric response to mechanical stimulus, and this not only from sensitive plants like *Mimosa*, but from roots, stems and leaves of horse-chestnut vine, white lily, rhubarb and horse-radish. This paper was communicated to the Society by Sir Michael Foster and was read on 6 June, but was not published on account of the opposition of Sir John Burdon Sanderson, the leading electro-physiologist of the time, who maintained that only sensitive plants exhibit electric response to mechanical stimulus. A detailed paper on the subject, however, was published later in the Linnean Society's Journal (1902).

The general scientific outlook of the period was naturally quite different from what it is now, for the existence of electrons had only just been discovered and atoms still maintained their indivisible and immutable identity, without rival isotopes. Bio-electric investigations, biochemistry and physical chemistry were in their infancy, and biology was much more concerned with classification than with synthesis. This explains in part the first reaction in the orthodox scientific world to Bose's biological work. If one takes into consideration Bose's outlook and philosophy as expressed in his characteristic style in a discourse at the Royal Institution (10 May 1901), one can easily understand the subsequent criticism, and also appreciation, with which his findings were received in England. After showing the autographic records of the history of stress and strain in inorganic substances and living tissues, he concluded his lecture thus:

"How similar are the writings! So similar indeed that you cannot tell one from the other part. We have watched the responsive pulses wax and wane in the one as in the other. We have seen response sinking under fatigue, becoming exalted under stimulants, and being killed by poisons, in the non-living as in the living.

"Amongst such phenomena how can we draw a line of demarcation, and say, 'Here the physical process ends, and there the physiological begins'? No such barrier exists.

"Do not the two sets of records tell us of some property of matter common and persistent? Do they not show us that responsive process, seen in life, have been foreshadowed in non-life?— that the physiological is, after all, but an expression of physical, that there is no abrupt break, but one uniform and continuous march of law?

"It was when I came upon the mute witness of these self-made records, and perceived in them

one phase of a pervading unity that bears within it all things—the mote that quivers in ripples of light, the teeming life upon our earth, and the radiant suns that shine above us-it was then that I understood for the first time a little of that message proclaimed by my ancestors on the banks of the Ganges thirty centuries ago: 'They who see but one, in all the changing manifoldness of this universe, unto them belongs Eternal Truth—unto none else, unto none else!"

It is easy for a physicist with his laws of energetics to accept one general ground plan of the universe which includes both living and non-living substances. But a physiologist finds it extremely difficult to lump together the living, including both plants and animals, and the non-living, and pursue his investigations. In fact one of Burdon Sanderson's objections to Bose's paper was based on his contention that the word 'response' should not be used for a mere physical reaction. But Bose the physicist, in describing parallel reactions of inorganic substances and living tissues, felt no compunction in freely using terms of the physiologists which are associated not merely with particular functions but with very specific structures as well, The result was that it was not until Bose could make the plants automatically record their responsive movements, like animal muscles, that his generalization of the fundamental similarities between the responsive reactions of plant and animal was accepted by the physiologists.

In 1904 Bose submitted a series of papers to the Royal Society showing the similarities of both the electric and mechanical responses of plants and animals. For obtaining records of mechanical response of plant tissues he first introduced the optical lever in plant physiology to magnify and photographically record the minute movements of plants, both sensitive and ordinary. This series of papers met the same fate as the previous one and now reposes in the archives of the Society. Bose published his results, however, in great experimental detail in his two books: ***Plant Response*** (1936), which shows the ingenuity of his experimental technique, and ***Comparative Electro-physiology (1907)***, which contains the most accurate and exhaustive bio-electric observations on plant tissues of the time.

By 1911 Bose perfected his resonant recorder (***Philosophical Transactions, 1913***) which enabled him to determine with remarkable accuracy, within a thousandth part of a second, the latent period of response of ***Mimosa pulvinus***, the velocity of transmission of excitation through the petiole to pulvinus of Mimosa, and die amplitude of response. These automatic records established the fact that the process involved in the transmission of excitation through the petiole to pulvinus is not one of transference of matter, as was then held, but is similar to what happens when an excitatory impulse is transmitted through a nerve to muscle, i. e. a process of propagation of disturbance. He showed that variation of temperature and passage of homodromous and heterodromous electric current (***Proceedings of the Royal Society, 1915***) produced modifications of transmission of

excitation in the petiole of Mimosa exactly similar to the modification induced in the nerve of a frog. In fact Bose was able to show that a "petiole-pulvinus preparation of Mimosa" (***Proceedings of the Royal Society, 1916***) as an analogue of a nerve-muscle preparation of a frog. He devised his oscillating recorder (***Irritability of Plants, 1913***). for making minute lateral leaflets of ***Desmodium gyrans*** automatically record their pulsating movements With these records he showed the parallelism which exists between the pulsations of a ***Desmodium*** leaflet and the beats of an animal heart, in respect of the comparative rates of contractile and expansive movements of a complete pulse, the effort of variation of temperature on the amplitude and frequency of pulsations and the changes induced by chemicals.

He took up the problem of automatically recording the growth movements of plants. To record these microscopic movements he devised his high magnification crescograph (***Proceedings of the Royal Society, 1919***); with this instrument he obtained a magnification of 10,000 times, and was able to record automatically the growth elongation of plant tissues and their modifications by various external stimuli. Not content with the highly respectable magnification of 10,000, he perfected his magnetic crescograph, obtaining an extreme magnification from one to ten million times. Regarding the performances of this crescograph, the following note appeared in ***Nature*** (6 May 1920):

"Sir Jagadis Bose's crescograph is so remarkably sensitive that doubt was recently expressed as to the reality of its indication as regards plant growth, and the suggestion was made that the effects shown by it were due to physical changes. A demonstration at the University College, London, on 23 April, has however led Lord Rayleigh, Professors Bayliss, V.H. Blackman, A.J. Clark, W.C. Clinton and F.G. Donnan to state in. The Times of 4 May: 'We are satisfied that the growth of plant tissues is correctly recorded by this instrument, and at a magnification from one million to ten million times'. Sir William Bragg and Professor F.W. Oliver, who have seen similar demonstration elsewhere, give like testimony that the crescograph shows actual response of living plant tissues to stimulus."

The magnetic crescograph did not exhaust Bose's inventive genius. To explore the electric changes in different layers of plant tissues, he devised his electric probe (***Transactions of the Bose Institute, 1919***), the first microelectrode. His photosynthetic recorder (***The physiology of photosyntheisis,*** 1924) automatically records the rate of assimilation of gaseous food by plants.

Bose's explanation of the mechanism involved in the transport of sap, as also the generalizations he derived from his analysis of plant movements, have remained debatable. From his investigations of the problem of ascent of sap, he came to the conclusion that mere physical forces are not adequate to explain the phenomenon. He held the view that the co-ordinated pulsating activity of the inner critical layer of cells was the primary factor involved in the transport of sap. He explained the different reactions of plant tissues to all forms of

stimuli, including light and gravity in terms of the following generalizations, which he derived from his numerous automatic records: (i) under normal conditions all forms of stimuli of moderate intensity give rise to negative response, i.e. contraction, diminution of turgor, electromotive change of galvanometric negativity and diminution of electric resistance; (ii) feeble stimuli give rise to responses which are positive, i.e. the reverse of the above; (iii) in the subtonic condition of a tissue the response wider moderate stimulus is positive; (iv) strong stimuli give rise to *multiple* responses; and (v) all forms of direct stimulus induce *contraction*, indirect stimulus, *expansion*. Bose was a physicist and a physicist he remained in his outlook to the very end. In his generalizations of the observed effects of stimuli on plant movements, it was the intensity of the stimulus and the point of its application which seemed more significant to him than the induced physio-chemical changes in the complex structure of the living plant tissue.

Justifiable criticism has been made of the absence of references in some of Bose's writings to previous and contemporary workers. But the priority of many of Bose's observations, *e.g.* positive and multiple responses alike, electrical and mechanical, and transmission of death excitation, is seldom given the acknowledgment due, in current literature of plant physiology. To sum up Bose's contribution to physiology: he tried to show that all the characteristics of response exhibited by animal tissues are equally exhibited by plant tissues; he has invented unique series of automatic recorders of great precision and extreme sensitivity; he has secured the automatic records of all forms of plant movements, e.g. microscopic growth elongations and their modifications by external stimuli, the response of a single leaflet of Mimosa, the pulsations of leaflets of ***Desmodium gyrans***, the multiple responses of ***Biophytum sensitivum*** leaflets, the tropic movements of leaves and stems.

He has left behind nineteen volumes which form a record of the work carried out and directed by him over a period of nearly thirty-seven years.

In the compilation of this obituary the writer has received invaluable help from Professor D.M. Bose, successor of Sir Jagadis C. Bose as director of the Bose Research Institute, and of Mr. Boshi Sen, one of the former pupils of Sir Jagadis C. Bose, and. at present Director of the Vivekananda Research Laboratory, Almora.

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SIR PRAFULLA CHANDRA RAY

(First Published in The Calcutta Municipal Gazette–Sir P.C. Roy Memorial Supplement, 26, 24 June, 1944)

After the last Great War (World War I) there was a great rush of people going to England. The Scindia Steam Navigation Co., one of the pioneer Indian shipping concerns, had engaged a vessel-

Loyalty to take passengers to Europe. It was in one of her trips that Sir P. C. Ray (plates 5 & 7) made, I think, his seventh voyage to England. The passengers were mostly Indians: amongst them many have become subsequently famous, e.g., Dr. Jivraj Mehta of Bombay, Prof. N. K Siddhanta of Lucknow and Dr. B. S. Guha, the anthropologist. I was also amongst the passengers. But most of the passengers were raw boys from Bengal, the Punjab and other provinces of India, going England for study. A few ICS officers were also there, because they could not get passages in bigger and more comfortable boats. After we had neared Aden, there was a complaint that this Swadeshi venture was a failure – the food was bad, the cabins were ill-kept and the passengers were not well cared for. This was being freely talked about, and one day when Sir P. C. Ray was on the deck talking to some of us, a petition to some shipping authorities, purported to be given by the passengers, was brought to him. It stated that the Scindia Steam Navigation Company should no longer be allowed to run the service, as they had made no arrangements for the comfort of the passengers. Those who brought the representation to him were mostly young boys from the Punjab, with some Bengalis as well. They requested Sir P. C. Ray to put his signature to it He read the petition once, twice, thrice and then asked the boys whether any of them had ever travelled to Europe before. They said "No". Then he asked them how then did they know that the arrangements on this particular boat were worse than those on purely British or any other European liner. They said that many of the English passengers had told them so, and they particularly mentioned the name of an English member of the Indian Civil service who was a fellow passenger. Sir P. C. Ray said: "My young friends, this is the seventh trip I am making to Europe, and before this I had travelled by the P & O and ships of other European companies; I can assure you that the food and other arrangements here are as good as on any British or other foreign boat." Then the boys had a long argument with him but ultimately they confessed that they were actuated to this course of action on the advice of one of the European passengers. Sir P. C. Ray then asked, "What am I to do with this petition: Can I tear it and throw it into the sea?" They all agreed to this and he tore the petition and threw it overboard.

In certain quarters in India, Sir P. C. Ray has been known as a parochial patriot loving only Bengal. This incident shows that his patriotism was not confined within such narrow limits. A Swadeshi venture, whether it was from Bengal, or Bombay, was equally dear to his heart.

Sir P. C. Ray was once invited to Lahore to deliver a course of lectures on Hindu Chemistry, a subject which he had made specially his own. By his researches in ancient Sanskrit literature, he published, in two large volumes, the ***History of Hindu Chemistry***. These two volumes show his deep erudition and his perseverance in dealing with a very unknown phase of the activities of ancient India in the domain of science. While

Sir Prafulla Chandra was addressing the Lahore University, there was, among his audience, a young Englishman who was a professor in one of the local colleges. He had just arrived in India and was apparently not very much impressed with conditions here. Sir P.C. Ray Was talking of the chemical processes practised by the ancient Hindus. The apparatus which they used were not very striking. They used to carry out processes like distillation in very crude earthen pots heated by wood-fire. While he was exhibiting those processes with the aid of diagrams, the young Englishman could hardly suppress his sneers. The old man had noticed it and was apparently annoyed. After the apparatus had been described, he took in his hand a lump of Makaradhwaja, which is resublimed mercuric sulphide, still prepared by Kavirajas according to old Hindu methods and used as a medicine, sometimes even by European physicians. Sir Purdy Lukis, who was Surgeon-General with the Government of Bengal, would often prescribe it to his patients as a stimulant. Sir P.C. Ray took the lump in his hand and said: "Look here, my friends, with such crude apparatus, the Indians, two thousand years ago used to prepare such a fine chemical and used it to alleviate human sufferings and this at a time when the ancestors of our friend over there were eating raw berries and wearing raw hides". The young Englishman's face became red and he darted out of the hall. Afterwards he became a great admirer of Sir P.C. Ray and his other Indian friends.

Many people have wondered how Sir P.C. Ray could live such a great life. He was a permanent invalid and had begun to suffer from dysentery even in his early boyhood. I have seen very few men with such regular habits as Sir P.C Ray had. As he had never married, he had learnt to depend on himself alone. He would rise very early in the morning, much earlier than any of the numerous students and others who lived with him, and take a stroll round about the corridors of the Science College. His study hours were from the early hours of the morning up to about 10 o'clock. Woe to anybody who dared to disturb him during his study hours! He would then work in the laboratory up to about 12 noon and then come to his room for his midday meal, take a short rest and in the afternoon he would again come to the laboratory and finish his correspondence. By about 4 p.m. he would be ready for his public engagements. In the evening he would relax himself in the maidan in the company of a few select friends whose ages ranged from 80 to 15 and who came from all classes and ranks in life I think this was the secret of his long life and the vast output of work in the various fields. While taking a long journey say to Bangalore or Bombay, he used to calculate beforehand the number of meals which he had to take and get everything ready, or write to his pupils, who might be on the way, to come and see him in the station with a gentle hint that they should bring something for him, which they very readily did.

He used to tell us that it was at the request of Prof. Berthelot that he undertook the compilation of the ***History of Hindu Chemistry***. This took him about 8 to 9 years, and he had to engage

a Pundit to pore over Sanskrit manuscripts to find out their meaning, compare their chemical processes with modern processes and also read into historical literature to find out the chronology of the authorities on Hindu chemistry. As a result of these 9 years of arduous work his health had completely broken down, and he was warned by his friend and physician, Sir Nilratan Sircar, to regulate his mode of life. I think these instances will help many of my young friends who take too much liberty with their health and generally, nowadays, do not appreciate early rising. He always used to quote Benjamin Franklin's famous saying- "Early to bed and early to rise makes a man healthy, wealthy and wise" — a saying which he followed to the letter in his own life.

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SCIENTIFIC REMINISCENCES – C. V. RAMAN

(First published in "India and the World (Ed. Kalidas Nag) v. 2, 1933")

In the year 1913, writer of this article, who was then a student in the Presidency College, Calcutta, went to attend a lecture at the Indian Association for the Cultivation of Science at 210, Bowbazar Street, Calcutta. He does not remember the name of the lecturer or the subject of the lecturer or the subject of the lecture. But he remembers that he noticed there for the first time a tall, stalwart turbaned figure apparently a South Indian, moving with the vivacity of Nataraja Siva, so familiar in South Indian Iconography, and making the lecturer rather uneasy by his penetrating questions. This figure was no other than Sir C.V. Raman, then an officer in the finance department, who was destined to be the first Indian to win the much-coveted Nobel Prize in science, seventeen years later.

The ancestors of Sir C.V. Raman came from the Tanjore district, and as he once informed the writer of this article, his grandfather took a trip on foot to Nuddeah in Bengal to learn the Nyayn Philosophy. His father was Professor of Vizianagaram in the Andradesa where Sir C.V. Raman saw the light of day nearly forty five years ago. So he is claimed both by Tamils and Telugus to be belonging to them. But in reality he belongs to none of these people, but properly to Bengal which was the first to discover his genius, and provide him with a laboratory where he could give free play to his talents.

He distinguished himself in his early School and College days, carrying away prizes and scholarships. While a student of the M.Sc. class at the Madras Presidency College, he carried out several improvements on Mele's experiments on the vibration strings, and his originality of mind attracted the notice of Mr. Jones, the Professor of Physics, for whom Sir C.V. Raman cherishes the highest regard. Mr. Jones offered him a job in the educational department, but as during these times all the higher posts in the educational service

were exclusively reserved for Europeans, young Raman did not find the job sufficiently tempting, and instead of accepting it, went for the Finance Competitive Examination. He was successful and when barely 19, found himself in the service of the Government of India with a starting salary of Rs. 500, and with prospects of ring up to Rs. 3,000 per month, and adding rupees, annas and pies for the benefit of the bureaucracy.

But neither the depressing atmosphere of the finance service, nor the security of an easy life could dry up Raman's super abundance of energy. He astonished the department, particularly his official chief by retaining his interest in physics, and continuing to publish original papers in European journals. In 1912, when he was transferred to Calcutta, he found a place for work in the Indian Association for the Cultivation of Science, a private organisation for spread of scientific knowledge founded by Dr. Mahendra Lal Sircar, a leading medical practitioner of Calcutta. Like Count Rumford who founded the Royal Institution of Great Britain with the object of devising a method for the cheap feeding of the poor by scientific methods, Dr. Sircar had founded the Science Association in the hope that Science would solve the problem of poverty, disease and superstition in his country. The Royal Institution was the nursing ground of the geniuses of Davy and Faraday; the Science Association became the nursing ground of the genius of Raman, and will probably continue to produce more scientific worthies in future times.

In 1912, the Calcutta University was fortunate to get two princely gifts from Sri T.N. Palit and Sir R.B. Ghosh, two eminent Calcutta laywers, for the foundation of a number of chairs for the Calcutta University. But they found great difficulty in the choice of suitable Indians who could fill up worthily these chairs. At last Sir Asutosh Mookerjee, then Vice-Chancellor of the Calcutta University and a keen 'fisher of men' pitched his choice on Raman, and was able to induce him to forsake the easy pastures of the finance department for perilous exploration in the unknown land of Science, under the flag of a disguised Swaraj regime. To his colleagues in the finance service, the adventure, appeared to be not only imprudent, but destined to failure, and while granting him leave for joining his new post, his official chief wished him success in his new venture as 'Experimental' Professor of Physics (The official designation was Professor of Experimental Physics).

Raman joined his post in 1917, and from that time, he had devoted all his time and energy to original research in physics in the Indian Association for the Cultivation of Science. He attracted students from all parts of India, and many of his students achieved great distinction in original work. His laboratory is permeated with his dynamic personality and work is going on incessantly, from morning till night. He seems to have taken as his adage the saying of the great Frenchman, Pasteur "Travaillez, mes enfants, toujoura travaillez" (Work, my boys, always work)

The crowning achievement of his life came in 1928 when he discovered "Combination Scatering", now called fittingly after him 'Raman Effect'. This was not due to mere stroke of good fortune but vas

the culmination of years of hard and continuous work. The blue colour of the sky and the sea has always impressed poets and authors, but a Scientific explanation of it was first given by Lord Rayleigh and was supported by the experiments of Tyndall. Lord Rayleigh showed that the phenomena were due to the breaking up of light waves by the molecules which compose our atmosphere. The sky appears blue because more blue light is thrown side ways than red light.

Raman's interest in the subject seems to have been infused by experiments which were carried out about 13 years ago by the Hon'ble T. Strutt, the present Lord Rayleigh (and son of the first Lord Rayleigh) for experimentally demonstrating the validity of his father's theories. He devised very delicate apparatus for experimentally investigating the scattering by molecules of different types and in cooperation with his numerous students carried out hundreds of experiments on these lines. In course of these investigations he was troubled by experiences of a new kind. He found that light seems to be modified in its colour when scattered by matter. What happens is this: When light falls on matter, which is itself excited by heat or any other agency, the electromagnetic vibrations which constitute light become locked up with the vibrations of matter, and is profoundly modified when it comes out. But this modification cannot be detected if white light be used, because white light is a jumble of many lights and the modified lights get lost in the medley crowd. So light of one single colour has to be used, and the modified light has to be analysed by a spectroscope. This crucial experiment was carried out by Raman and Krishnan in 1928, and was immediately successful.

The effect of this discovery on the Scientific world was immense, for it not only brought to light new phenomena, but it opened a new way for investigating the properties of matter. The importance of the result was first recognised by P. Pringsheim, Professor at Berlin who in an article in the German Naturwisscenchaft, gave an account of the discovery, and called attention to its great importance in molecular physics. From this time onwards, the interest in the work has remained unabated, and investigations on Raman Spectrum (Spectrum of combination scattering due to molecules) have become common features in journals of Physics and Chemistry. A great amount of these contributions has come from Prof Raman's own Laboratory. His great services to Science were recognised by the award of a Nobel Prize in 1930 and strange to say, even the governing body of the Government of India, seemed to signify its appreciation by conferring on him a knighthood in 1929, a distinction usually reserved for bureaucrats and politicians of a class whose activities are of an approved type.

As is usual in these days, controversies regarding precedence immediately arose after the announcement of the discovery, for two Russian workers, Landsberg and Mendelstamm had been working on similar lines and had been forestalled by Raman by only two months. An Austrian Professor Dr. Smekal, had predicted the effect from thermodynamical reasoning, and Kramers and Heisenberg, two pupils of the famous Niels

Bohr had worked out a theory of refraction on these bases. Experiments to verify the theory were carried out at Bohr's laboratory but without success. It was reserved for in Indian to achieve the first success in these lines.

This is rather strange, because European writers are never tired of describing the Indians as given over completely to metaphysical speculations, and possessed of little practical abilities, Here were the rules reversed – an Indian giving the first practical effect to the theoretical speculation of European savants, which they themselves have been unable to verify.

The prediction of this effect by other savants does not take away from Raman the great credit which is rightly due to him. An analogy will do, the fact that the Earth is round was first said to be clearly enunciated by the Pythagoreans particularly by Anaxogoras who flourished in the 6th Century BC. The radius of the earth was actually measured by Eratosthenes of Alexandria in the 3rd Century BC by using the same methods which are how used in trigonometric surveys all over the world, But these predictions do not certainly take away from the great credit which is due to Columbus and other great navigators who undertook perilous voyages in unknown seas, in moral support of these theories. Credit is always due to the man who can actually turn a speculation into a practical reality.

After twenty-five years stay at Calcutta, Professor Raman is called away to his native South for shouldering the responsibility of the Indian Institute of Science at Bangalore. He leaves with the best wishes from the people of the land of his adoption, who sincerely hope that his new mission will be as much crowned with success as his labours at Calcutta had been. Though he is gradually passing into the afternoon of life, his store of dynamic energy shows no sign of deterioration, and it is hoped that they will be more fruitful in the new land of venture!

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THE PROPOSALS FOR AN INDIAN ACADEMY OF SCIENCE

(Presidential address : Proc. Ind. Sci. Cong. 42, 1934)

At the present time, the hopes of World-Peace based on a scientific handling of economic and political problems appear to be, as you are well aware, very distant, for the world forces are moving in such a direction that it is more likely that instead of establishing an era of world-peace and coordinated scientific exploration for the benefit of the mankind, the present generation will fulfill Spengler's pessimistic prophecy of the end of a cycle resulting in the crash of modern civilization. In any case whether we move towards peace or war, it is clear that the prospects of a better future lie entirely with science. Every country must, for the sake of self-preservation, as well as for the benefit

of mankind husband all its resources scientifically and must foster and stimulate scientific thought. The great industrial ventures, whether private or State, mast make increasing use of science, for otherwise they will not answer the world needs and achieve the desired results.

So in every country, India not excepted, the State must awaken to the need for a proper organization of her scientific brains. Confining to our country, it is well known that a good amount of scientific work, which often led to great industrial enterprises, has been done in the past by the various scientific Services. Within the last few years, the Agricultural Research Council has been founded at which a happy departure is noticeable. Unlike the older services, whose organization probably did not enable them to make use of non-official scientific brain available in the Universities or private institutions, the constitution of the Agricultural Council has been such as to enable it to make full use of all available talent which is a happy departure in the policy of the Government of India.

In this connection the non-official scientific men have a just cause of grievance against the Government. I remember to have read a few years ago, a short article by our energetic secretary Dr. Agharkar in the Modern Review where he invited attention to the close co-operation existing between Workers in the scientific services of the Government, and Professors in Universities in other countries, and complained of its total absence in this country. In other Countries, the co-operation is so complete that it is not unusual to find that Universities and Services very often exchange their workers to the mutual advantage of both. I do not see why this co-operation should not be secured in our country as well, and the existing services should not be reorganized on the lines of the Agricultural Research Council.

Need for a River Physics Laboratory

There are many lines in which the Government of India as well as the Provincial Governments can profit immensely if they take the trouble of obtaining proper scientific guidance before launching on large-scale enterprises. As many such cases are not known to you all, I would refer to only one. You are aware that year after year, the government and other public bodies spend an enormous amount in constructing bridges and water reservoirs, in opening canals, in development schemes, in hydroelectric schemes, and in city drainage schemes. These schemes are certainly highly beneficial and undertaken with the best of intentions, but you are certainly also aware that from time to time, very unpleasant facts leak through the columns of the news agency, which show that these schemes are mishandled at some stage or other. Every scientific man knows that before the actual working commences the plans should be scientifically studied in Hydraulic Research Laboratories, with the aid of models and the engineers in charge of constructions should have a clear-cut idea of the work before they

are put in charge of it. You may be knowing that inspite of the fact that next to the United States of America, India is the country which has undertaken such works on the most gigantic scale and has spent hundreds of crores of rupees on these works, the Government has not yet thought it fit to establish a single Hydraulic Research or River Physics Laboratory in this county while in other civilized countries no such enterprise is allowed to be undertaken unless the plans are examined in suitable laboratories attached either to the Universities, Technical High Schools, or State departments with the aid of suitable models. I should merely add that I am not against the launching of the schemes, in fact many of them, like the Punjab Canals, have done immense good to the country. But others, like the Orissa and Midnapur Canals, were constructed on faulty lines, and involved the state in huge financial losses while other schemes like the laying of railway lines through major parts of Bengal without a proper examination of topography and of the river system have plunged the country into perpetual outbreaks of malarial epidemics and led to the sapping of the vitality of the population. I do not bold that either the engineers or the officials are responsible for these failures and disasters. In fact I think that most of them like Sir F. Spring tried to make the best use of a bad situation. But the fault is due to lack of imagination on the part of those who have taken upon themselves the task of Government and to their failure in devising a proper system of co-ordinated work in which preliminary scientific study in suitable laboratories should form an essential part of the organizations.

Need for a National Academy of Sciences

It is clear that for these and other reasons which are too numerous to be discussed here, it is necessary that scientific men in India should be better organized, and should try to impress upon the Government, the States, and other public bodies the need for encouragement of scientific research, and for a scientific handling of economic and industrial problems of the country. The best way for them to do so is to organize themselves into a corporate body, and it appears to me that the movement for establishing an Indian Academy of Sciences has started at the right moment. Our energetic secretaries have in the pamphlet now in your hands acquainted you with the full history of the movement, as well as at the various proposals for carrying the idea into action. To this I have little to add except that the United Provinces Academy of Sciences, which is now three years old, has been moving in a similar direction, and has passed the following resolutions which I have the honour of submitting to you for your opinion and action thereon.

Resolution No. 2 at the Academy of Sciences UP dated August 10, 1983:

'That the academy welcomes the proposals of the council that the Academy of Sciences, UP, should become the All-India Academy of Sciences.'

Resolution No. 2 of the Academy of Sciences, UP dated December 19, 1933:

'That Dr. Saha be requested to bring to the notice of the Scientists attending the ensuring Science Congress at Bombay the resolution of the Academy of Sciences, UP, dated the 10th of August, 1933, and to discuss the whole question with them, and to report to the Academy at its next meeting.'

On going through the various proposals, one finds, as is naturally expected, considerable divergence of opinion, regarding the manner and details of organization and it has appeared to me advisable that it will probably help the general body here to arrive at a practicable course of action, if I were to summarize these views and give my own ideas on them. You are not expected to adhere to my views, but these may be regarded as commentaries on the proposal which the secretaries have brought forward for discussion. These are:

[i] Whether it is desirable to form an Indian Academy on lines similar to those in Europe or America.

[ii] If answer to [i] is in the affirmative, whether (a) there should be one Academy for all branches of learning, or (b) there should be separate Academies for Arts and Letters.

I have no reason to doubt that answer to (i) would be in the affirmative.

Regarding (ii) there are two separate issues involved:

First of all, it is clear that the Academy cannot assume the functions of the sectional societies dealing with different branches of science, consisting of scientist engaged in search in these branches like the Indian Chemical Society, or the Indian Botanical Society, or the various Mathematical societies having an unlimited membership. This is neither necessary, desirable, nor feasible. It is true that many branches of science have not yet organized themselves into societies. it is hoped that they will do so in the near future, and it will be one of the duties of the Academy to help in the organization of these societies. But as long as they are not in existence, the Indian Academy may continue to take care of these subjects.

The Indian Academy will therefore be a Central Society, on which all branches of science will be represented, or to borrow a simile, it will form like the Royal Society in England, or the Prussian Academy in Germany, the apex of pyramid of societies devoted to particular subjects. It should have therefore a limited membership. Its membership should be regarded as a mark of distinction and honour and the Academy should be associated with the State in a number of responsible duties involving scientific work.

If the Academy is started on the above lines, I think it may undertake the following works:

(i) It will publish '*Comptes Rendus*' or Proceedings like those published by the National Academy of Sciences, United States of America, which

will contain only the results in long or short abstracts. It should not in general undertake the publication of a Journal devoted to a particular subject. These will be left to the societies and services. It may in addition publish memoirs and transactions.

(ii) It should take over the organization of the Indian Science Congress.

(iii) It should try to induce the State to form National Research Committees, on which the Academies should have their representation

(iv) It should secure and manage funds for Scientific research.

(v) It may act as a liaison body between societies for various branches of science, provincial academies, universities, services, and private organizations for scientific research.

(vi) It may undertake and promote enquiries regarding problems of national welfare.

(vii) It should represent India in National Bodies for International Co-operation.

Our Prototype – the Royal Society

In drawing the above scheme, I have taken as our prototype the Royal Society of London whose function and organization seems best suited to our conditions. The Royal Society was founded in 1663 in response to a widespread movement initiated by Frances Bacon for the cultivation of scientific subjects. The other famous Academics of Europe like the French Academy in Paris or the Academic dei Lincei in Rome were founded about the same time, some earlier, others later and they were all the result of a widespread movement which may now be called romantic and which believed that the cultivation of science would usher in a new era in human civilization. The history of the past three centuries shows that the movements for scientific study of Nature have led to an enormous increase of power of the European people, which has enabled them to dominate the whole world. When these societies were started, science was one, but in process of time, it has branched out in innumerable lines, and to meet the special needs of the growing sciences, societies were started at different dates out of the first mother society. We can refer to the chemical, physical, geological, botanical, astronomical and other societies which have grown in this way in England. But his has not rendered the Royal Society useless. On the other hand, its importance has increased as the meeting ground of all schools of scientists, and at present, the Royal Society is regarded as the apex of the pyramid of societies, and every British man of science regards it as the highest scientific honour to be elected to its Fellowships which are limited in number. It will probably interest you to known the representation of different sciences in the present body of fellows of the Royal Society. It is as follows:

Physics and Meteorology	77	Botany	34
Mathematics & Astronomy	53	Geology	35
Engineering	24	Psychology	5
Chemistry (all branches)	70	Miscellaneous	12
Medicine (all branches)	90	National Work	8
Zoology	36	Archaeology	3
			447

The next point for us to consider is whether the India Academy of Science should be organized as an individual unit, or should form part of a larger organization including an Academy of Letters, and possibly an Academy of Social Science. You may see from the Pamphlet issued by our secretaries that a good deal of interest in this subject is being taken by a number of distinguished British public men including the Rt. Hon'ble the Marquis of Zetland (better known in this country as the Earl of Ronaldshay who was some time Governor of Bengal) and Prof F.W. Thomas, the distinguished Oxford Orientalist. I would ask you to join with me thanking these gentlemen for taking so much interest in Indian affairs. The first letter from this committee was addressed to a number of Indians distinguished in the field of letters and science, and in public life, asking their opinion on the advisability of starting an Indian Academy comprising Science, Letters and Arts to which replies must have been sent. We are not in the possession of these opinions, but the observations of the British Committee on these opinions are before you in the form of a second letter by the Marquis of Zetland and Prof. Thomas which, with their permission, has been printed by our secretaries. It is rather regrettable that it was not possible for us to act on the suggestion contained in these letters that representatives of literary opinion should be invited to discuss the matter with us, as the programmes of all conferences which may be assigned to 'letters' are fixed a year ahead, but I have suggested to our secretaries that next year an effort may be made to hold all conferences, e.g. the Oriental, the Philosophical, the Economic and the Indian Science Congress at one central place to which also other distinguished men of letters not belonging to any specified group may be invited. If this plan materializes, the representatives of different bodies can meet and discuss the points raised by our British friends.

But the Indian Science Congress need not wait for this conference. It can forthwith proceed to the task of organizing an Indian Academy of Science. In fact there is an advantage in taking the lead now, for as you are all aware the men of letters and those representing the Social Sciences in our country are not as well-organized as scientific men, and if we succeed in evolving an organization, they may take it as their model. I find, however, that our British friends are desirous of ascertaining whether the proposed Indian Science Academy is likely to adhere to the greater organization of an Indian Academy, containing sections on Letters and possibly also on social sciences modeled on the lines of the British Academy of Great Britain. Unfortunately I am myself ignorant of

the constitution of the British Academy, but personally I see no harm in a loose federation, and it is a fact that in France and Germany, federations of the academies of sciences, Letters and social sciences do exist. I would only refer to the famous institute of Paris which is divided only refer to the famous Institute of Paris which is divided into sections, each retaining its individual office organization, funds, and its own rules for management and election of fellows, but joining hands on certain momentous occasions, and for sending representatives to National Councils and International organizations. The Prussian Academy of Science has similarly sections of Philosophy and Letters, and on Physical and Biological sciences. I would suggest that the members of the general body here with whom the final decision rests will carefully go through the letters of the Marquis of Zetland and Prof. Thomas, and arrive at some conclusion which is likely to be beneficial for the future of India. Personally I think that the offer of help so kindly promised particularly in securing for us a Royal Charter, should be thankfully accepted.

To consider the standard of attainment considered necessary for election as a member of the academy, which will determine the size, I think that the general body here will only fix up the maximum and assign the quanta to various sciences, and other interests and leave the rest to be filled up later by a committee which will be elected to frame the constitution, rules for elections etc.

The abstract of the composition of the R.S. which I have prepared may give some guidance. The final recommendations of the committee which may be authorised to correspond with the Government of India, the various provincial Governments, Societies in India and outside and with the group represented by the Marquis of Zetland and Prof. Thomas, may be placed before the next Science Congress. I would only suggest that in the case of each person who is finally selected to be a member of the Academy, a full list of qualifications and other attainments should be prepared and circulated.

The relation of the Academy to the existing learned societies may be left to the committees.

The location of the headquarters and the method of work is the point which is likely to produce the greatest amount of divergence of opinion. It is natural that everyone will place the claim of the particular city to which he is attached for one reason or another, but if we wish to achieve anything substantial, it is clear that we must stand above our petty interests and view the question from the higher platform of the interest of progress of science in this country. I would personally like that this question be not considered in the present meeting, but we may wait till the labours of the committee are completed; and the point may be thought before the next meeting of the Science Congress for final decision.

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8.

SCIENTISTS IN THE LETTERS: BRIEF NOTES

WESTERN SCIENTISTS

Adams, Walter Sydney (1876-1956)

An American Astronomer born of missionary parents in Turkey, Walter Sydney Adams was the director of Mt. Wilson observatory in Pasadena and made important contributions to his field using spectroscopic studies for which he received the Gold Medal of the Royal Astronomical Society.

Appleton, Edward Victor (1892-1965)

An English physicist who studied under Sir J.J. Thompson and Lord Ernest Rutherford in the Cambridge University, Sir Edward Appleton proved the existence of ionosphere in the Earth's atmosphere. His works led to the development of radar that played a decisive role during the World War II. Appleton received many honors and awards, including the **Nobel Prize for Physics in 1947**. He was a professor at the University of London, Cambridge University and Vice Chancellor of the University of Edinburgh.

Birge, Raymond Thayer (1887-1980)

An American Physicist, Raymond Thayer Birge was Chairman of the Physics Department, University of California, Berkeley and President of the American Physical Society. He made important contributions to the standardization of the values of Physical Constants and played important role in the emergence of the Berkeley Physics

department as one of the premier departments in the world.

Blackett, Patrick (1897-1974)

Educated in the Cambridge University, Lord Patrick Blackett did his doctoral research in Physics with Rutherford and received the Royal Medal in 1940 and the **Nobel Prize in Physics in 1948.** He made seminal contributions to the study of cosmic rays using cloud chambers, confirmed the existence of positrons and discovered pair production of electrons and positrons from gamma-rays. Later his research moved in the direction of paleomagnetism and verification of the concept of Continental Drift. Blackett was a Professor of Physics in the Manchester University and later Head of the Physics department in the Imperial College, London.

Bloch, Felix (1905-1983)

A Swiss and later American citizen, Felix Bloch was the first graduate student of the physics legend Werner Heisenberg who was just four years his senior. He was among a small group of physicists who made seminal contributions in both theory and experiments. He initially decided to become an engineer but ended up becoming a physicist and winning the **Nobel Prize in Physics in 1952** for the discovery of Nuclear Magnetic Resonance (NMR) that, besides its many applications, laid the foundation for MRI (Magnetic Resonance Imaging). Being a person of Jewish faith, Bloch felt compelled to leave Germany, where he held a teaching position in his alma mater in Leipzig, when Hitler came to power and finally became a Professor of Physics at the Stanford University, and later the first Director-General of CERN.

Bohr, Neils (1885-1962)

One of the great physicists of the 20th century and widely known even among non-scientists, Neils Bohr grew up in a family of academics in Denmark and founded the Institute of Theoretical Physics in Copenhagen that became a centre of attraction for physicists, especially the younger generation working in the field of quantum mechanics during his time. Bohr made revolutionary contributions to the field of quantum mechanics, received numerous awards and honors including the **Nobel Prize in Physics in 1922** for what is usually referred to as the Bohr model of atom. He managed to escape from Nazi occupation of Denmark, an event noted in one of Meghnad Saha's letters to Bohr.

Born, Max (1882-1970)

One of the founding fathers of the field of quantum mechanics and lattice dynamics, Max Born was a German physicist and mathematician who received the **Nobel Prize for Physics in 1954** for his fundamental contributions to quantum mechanics. Because of his Jewish ancestry, Born felt compelled to emigrate from Germany after Hitler came to

power. He taught at the Cambridge University and the University of Edinburgh and spent six months in 1935-36 at the Indian Institute of Science in Bangalore at the invitation of Raman who unsuccessfully tried to create for him a position of Mathematical Physics at the Institute.

Compton, Arthur Holly (1892-1962)

An American physicist who received the **Nobel Prize in Physics in 1927** for the discovery of the Compton Effect that demonstrated increase of wave length (or loss of energy) of X-rays upon scattering by electrons, a phenomenon that is best explained by treating electromagnetic radiation as particles, as suggested by Einstein. Compton began his professional academic career as a physics professor at the University of Washington, St. Louis, followed by appointments as a physics professor at the University of Chicago (1923-45) and as the Chancellor of the University of Washington. He also played a major role in the Manhattan project.

Curie, Irène (1897-1956) and Joliot, Jean Frédéric (- 1948)

Daughter of the famous Nobel Prize winning couple, Pierre and Marie Curie, Irène Curie was married to Jean Frédéric Joliot, a chemical engineer turned a radiochemist under her tutelage. Both decided to use the hyphenated last names Joliot-Curie, and went on to equally share the **Nobel Prize in Chemistry** in 1935 (once again in the same family!) for their pioneering work on the synthesis of new radioactive elements that, among others, found major applications in the Medical Sciences. However, they were afraid of the potential military use of their discovery and tried to take actions to contain it. Both died of medical complications arising from over exposure to radiation, as did Iréne Curie's mother, Marie Curie.

Dirac, Paul (1902-1984)

A legendary physicist, Paul Dirac earned a bachelor's degree in electrical engineering from the Bristol University and then drifted to mathematical physics, earning his Ph.D. from the Cambridge University, where he was later appointed in 1932 to the famous Lucasian Chair of mathematics. Dirac was awarded the **Nobel Prize in Physics** in the following year for his work on relativistic quantum mechanics. After his retirement from the Cambridge University, Dirac accepted a position at the University of Miami, Tallahassee, USA, where he remained for the rest of his life.

Dunham, Theodore, Jr. (1897-1894)

With degrees in Medicine (M. D. from Cornell) and Physics (Ph. D. from Princeton), Theodore Dunham Jr. made important contributions in Astronomy, in the study of Venusian and Martian atmospheres, and application of Physics to Medical research. He worked at the Mt. Wilson Observatory, Pasadena and later at the Australian National University and Harvard College Observatory.

Eddington, Arthur Stanley (1882-1944)

A British Astronomer and Astrophysicist of great distinction, and a Professor in the Cambridge University, Sir Arthur Eddington has been widely credited for the first observational verification of Einstein's General Theory of Relativity. In contrast, he was also responsible for the delayed acceptance of Chandrasekhar's Nobel Prize winning work on Chandrasekhar limit by his stubborn objection to the theory, especially being as influential a figure as he was at that time. He received the gold medal of the Royal Astronomical Society and several other awards.

Einstein, Albert (1879-1955)

Please read the essay on Einstein by Meghnad Saha in this volume. Einstein wrote the letter included in this volume from a hotel in Japan during a planned trip to Asian countries; he refused to make any change in his schedule in order to be able to attend the Nobel ceremony in Stockholm and receive the **1921 Physics Prize** that was awarded to him in 1922 for his work on the Photoelectric effect. No Nobel Prize in Physics was awarded in 1921 since the Nobel committee could not come to grips with the Theory of Relativity that had the overwhelming support among the physicists.

Fowler, Alfred (1868-1940)

An English Astronomer, Alfred Fowler was a Professor of Astrophysics at the Imperial College, London, and was known primarily for his spectroscopic studies that played important roles in the interpretation of stellar spectra. Meghnad Saha and his classmate from school, Snehamoy Dutta, worked in Fowler's group about the same time.

Fowler, Ralph Howard (1889-1944)

A British Physicist and Astrophysicist, Sir Ralph Fowler held the Chair of Theoretical Physics in the Cambridge University and was known for his contributions in the fields of thermodynamics and statistical mechanics. At Cambridge, he supervised the doctoral research of a very large number of students that included Paul Dirac, Chandrasekhar, Nevill Mott, Lennard-Jones and Homi Bhabha, of which the first three won Nobel Prize. Ralph Fowler received many awards including the Royal Medal, and was also a good cricket player.

Gale, Henry Gordon (1874-1942)

An American Astrophysicist, Henry Gordon Gale was educated at the University of Chicago, where he later served as the Chairman of the Physics

Department and Dean of the Graduate College. He also served as an editor of the Astrophysical Journal.

King, Arthur Scott (1876-1957)

An American Physicist and Astrophysicist, Arthur Scott King had the unique distinction of being the first Ph.D. (1903) in Physics from the University of California, Berkeley. His specialty was spectroscopic studies and he was the co-discoverer of an isotope of carbon, the ^{13}C, with Raymond Birge.

Lennard-Jones, John Edward (1894-1954)

One of the pioneers of the field of theoretical chemistry, Sir Lennard-Jones held a Chair Professorship in the Cambridge University and is best known for what is known as the Lennard-Jones potential that describes the potential energy of interaction between two neutral atoms or molecules as a function of their distance of separation.

Millikan, Robert Andrews (1868-1953)

One of the great experimental physicists from USA, Robert Andrews Millikan was Professor of Physics at the University of Chicago and later at CALTECH. While at Chicago, he carried out his famous "oil-drop experiments" in which he determined the charge of an electron and also verified the photoelectric effect predicted by Einstein's Nobel Prize winning work, something that he really did not quite believe in, along with the determination of the value of the Planck's constant. Millikan received the **Nobel Prize in Physics in 1923** for these studies.

Milne, Edward Arthur (1896-1950)

A British Astrophysicist and Mathematician of great distinction, Milne held the position of a Professor of Applied Mathematics at the University of Manchester and Professor of Mathematics in the Oxford University. Milne worked extensively on Relativity, Cosmology and Stellar Atmospheres and Structures where Meghnad Saha's work on thermal ionization equilibrium played very important role. He received the gold medal of the Royal Astronomical Society and became a president of the society.

Minnaert, Marcel (1893-1970)

A Dutch astronomer, Marcel Minnaert was a professor of astronomy at the University of Utrecht and director of the Sonnenburgh Observatory. He received the Gold Medal of the Royal Astronomical Society for his contributions to the field of solar physics and spectrophotometry.

Ortvay, Rudolf (1885-1945)

A Hungarian physicist, who was a Professor of Theoretical Physics in the Pázmány Péter University in Budapest, Hungary in the nineteen thirties, Rudolf Ortvay was a former student of Arnold Sommerfeld, and also a friend and supporter of John Von Neumann. Some correspondences between him and Von Neumann on political and scientific issues can be found in the "John Von Neumann: Selected Letters".

Plaskett, Harry Hemley (1893-1980)

A Canadian Astronomer, Harry Plaskett held positions of Professor of Astronomy-Astrophysics at the Harvard University and the University of Oxford. He applied the thermal ionization theory of Meghnad Saha to interpret the spectra of the massive stars in our galaxy, was awarded the gold medal of the Royal Astronomical Society and became its president.

Regener, Erich (1881-1955)

A German experimental physicist of distinction, who was known for his design of instruments to measure cosmic ray intensities at different altitudes, invention of scintillation counter and calculation of electronic charge. Among his best known students is the Indian scientist D.M. Bose, a well-known cosmic ray physicist and the longest serving director of the Bose Institute, Kolkata (named after his uncle Sir J.C. Bose).

Russell, Henry Norris (1877-1957)

An American astronomer, Russell was a Professor of Astronomy and the Director of the Astronomical observatory at the Princeton University and became a President of the American Astronomical Society. He received a number of prestigious awards including the gold medal of the Royal Astronomical Society. Russell's research since 1920 was greatly influenced by Meghnad Saha's work on thermal ionization equilibrium.

Rutherford, Ernest (1871-1937)

One of the greatest experimental physicists of all time and widely regarded as the father of Nuclear Physics, Lord Ernest Rutherford was born and raised in a farming family in New Zealand, and went on to become the director of the Cavendish Laboratory of the Cambridge University, succeeding his mentor, Sir J.J. Thompson. A recipient of numerous awards, Rutherford received the **Nobel Prize in Chemistry in 1908** for "investigation into disintegration of elements and chemistry of radioactive substances", but his best known work, the Rutherford model of atom, proving the existence of atomic nucleus, came later. Rutherford mentored a large number of outstanding scientists

and many of his students and associates received Nobel Prizes for works initiated at his advice.

Shapley, Harlow (1885-1972)

An American Astronomer who became a Professor of Astronomy at the Harvard University and also a director of the Harvard College Observatory, Harlow Shapely received many awards and honors and is best known for estimating the size of the Milky Way Galaxy and showing that the Sun lies on the central plane but far off the centre that was in contrast to the prevailing notion of its central position.

Schrödinger, Erwin (1887-1961)

An Austrian physicist and one of the founders of the field of quantum mechanics, Erwin Schrödinger shared the **Nobel Prize in Physics in 1933** with one of the Letter writers, Paul Dirac, for his fundamental contribution to quantum mechanics and specifically for the development of the electronic wave equation. A fascinating story behind the development of this monumental contribution within a few weeks can be found in an article by one of the Letter writers, Felix Bloch, in the December issue of Physics Today, 1976. Schrödinger held professorial positions in a number of institutions, including ETH Zurich and the Oxford University, and wrote extensively on different subjects. One of his books, "What is Life?" was greatly influential in the development of the field of biophysics; therein he also discussed his great admiration for the philosophical writing, known as Upanishad, of the ancient India.

Sommerfeld, Arnold (1868-1951)

Sommerfeld played major roles in the developments of atomic physics, quantum mechanics and hydrodynamics. He was a legendary teacher and mentor, with seven of his students receiving the Nobel Prize! Sommerfeld received many prestigious awards in Physics and was nominated 84 times for the Nobel Prize (the highest number of nominations received by an individual), but did not receive the prize itself – arguably a stubborn omission. Sommerfeld's many books under the title of Lectures on Theoretical Physics and others have been greatly influential in the field.

Westpahl, Wilhelm (1882-1978)

A German physicist from a family chain of senators in Hamburg, Westpahl was a Professor of Physics at the University of Berlin and in the Technische Hochschule, also in Berlin. He authored several books in physics on a wide range of topics from the theory of relativity to everyday physics for fun; one of these books, Physik, had 24 editions.

White, Leslie A (1900-1975)

An American Anthropologist, who was the chairman of the Department of Anthropology of the University of Michigan.

INDIAN SCIENTISTS

Bhabha, Homi Jehangir (1909-1966)

One of the most distinguished nuclear physicists from India, Bhabha received a degree in mechanical engineering from the Cambridge University, a direction towards which he was pushed by his father and his uncle, Sir Dorab Tata (of the famed Tata Iron and Steel group), but later followed his nose to complete a Mathematics Tripos and Theoretical Physics Ph.D in Cambridge. Bhabha's name is associated with Bhabha scattering and Bhabha-Heitler theory. He was the founding director of the Atomic Energy Establishment (renamed later as Bhabha Atomic Research Centre) and the Tata Institute of Fundamental Research. Bhabha was the first president of the international conference on the peaceful use of atomic energy. He died of a plane crash in Alps.

Bose, Satyendra Nath (1894-1974)

A theoretical physicist regarded by many, including Lev Landau, as one of the greatest in his generation, Satyendra Nath Bose was a classmate of Meghnad Saha while studying for his B.Sc. and M.Sc. degrees in the Presidency College, Calcutta (now Kolkata). Along with Saha, Bose was appointed as a lecturer in the Calcutta University by the Vice-chancellor Sir Asutosh Mookherjee. Later he moved to the Dacca University, now in Bangladesh, where he did his famous work on Bose Statistics to explain black body radiation, and that led, thanks to Einstein's involvement, to the formulation of Bose-Einstein statistics and prediction of Bose-Einstein condensation (BEC).

Chandrasekhar, Subrahmanyan (1901-1995)

An American Astrophysicist of Indian origin and widely regarded to be among the greatest of all time in his field, Chandrasekhar received his doctorate from the Cambridge University and spent his entire professional career at the University of Chicago. He worked on a wide range of theoretical topics and received the **Nobel Prize in Physics in 1983** for his contribution to the understanding of the structure and evolution of stars in which the discovery of "Chandrasekhar limit" at the age of nineteen played the central role. This work is of major importance in the understanding of the survival of white dwarfs and formation of black holes.

Dutta, Snehamoy (1894-1955)

A classmate of Meghnad Saha in the school in Dacca, now in Bangladesh, Snehamoy Dutta was an experimental physicist who was educated in the Presidency College, Calcutta, and the Imperial College of London, where he overlapped with Saha and worked with Alfred Fowler for his D.Sc.

Mookherjee, Asutosh (1864-1924)

An accomplished mathematician with a D.Sc. degree, Sir Asutosh Mookherjee founded the Indian Mathematical Society, became a judge of the Calcutta High Court and then the Vice-Chancellor of the Calcutta University in 1906. He had an uncanny ability to spot brilliance and appointed, without "due process", Meghnad Saha, Satyen Bose and C.V. Raman to faculty positions in the Physics Department of the Calcutta University before they made their ground breaking discoveries.

Ghosh, Jnan Chandra (1894-1959)

An Indian Chemist of distinction, Sir Jnan Chandra Ghosh was in the same graduating group in the Intermediate Science (I.Sc.) as Meghnad Saha and Satyendra Nath Bose and got his Ph.D. from the Imperial College, London. Ghosh held a number of important academic and administrative positions that include the position of Departmental Head of Chemistry, Dacca University (now in Bangladesh), Director of the Indian Institute of Science, Bangalore, Director of the Indian Institute of Technology, Kharagpur and Vice-Chancellor of the Calcutta University.

Roy, Prafulla Chandra (1861-1944)

Please read Meghnad Saha's article about Sir Prafulla Chandra Ray in Section 7.

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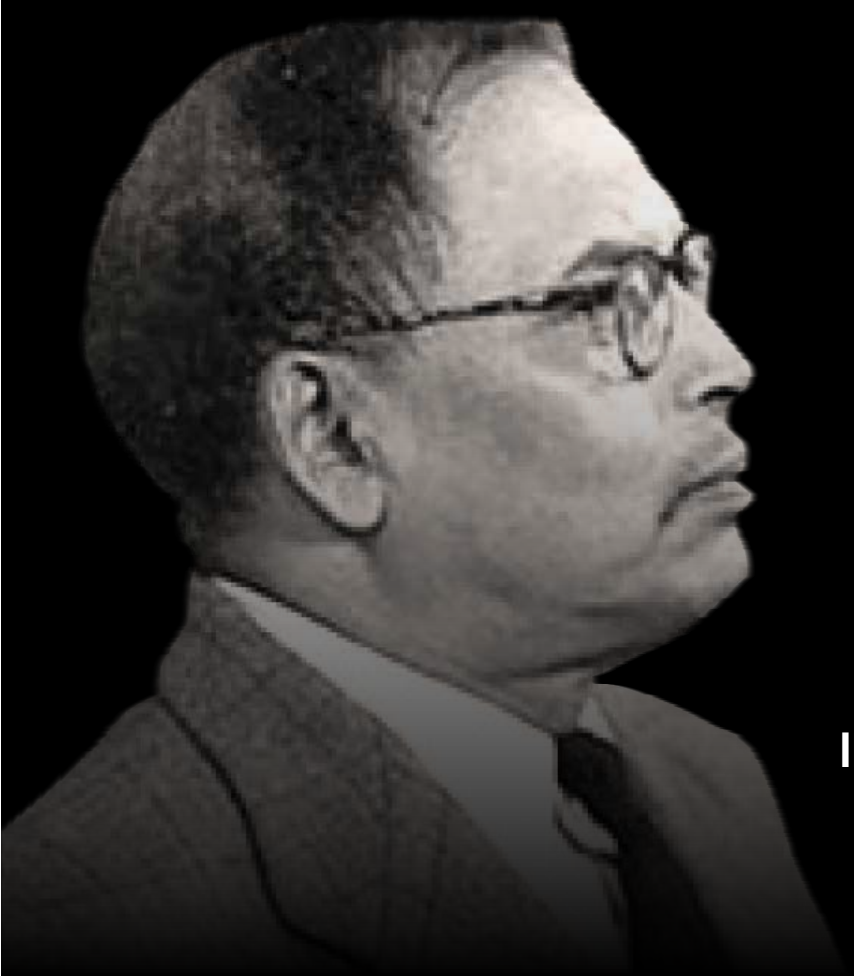
ABOUT THE AUTHOR

The author, Jibamitra Ganguly, was educated in India (Calcutta and Jadavpur Universities) and the University of Chicago, USA, where he received his Ph.D. degree in Geophysical Sciences. This was followed by post-doctoral research at the Yale University and the University of California, Los Angeles, and appointment to a faculty position at the University of Arizona, USA, where he is currently a Professor Emeritus of Geosciences. He visited India under the joint CSIR-UNESCO program of TOKTEN (Transfer of Know-how through Expatriate Nationals), China at the invitation of the Chinese Academy of Sciences, and worked briefly in several Indian and European Universities and organizations as a scientist or a visiting professor. Prof. Ganguly has published extensively and made major research contributions in a wide range of areas in the Earth and Planetary sciences. He is the author of the books Mixtures and Mineral Reactions (co-author: S.K. Saxena) and Thermodynamics in Earth and Planetary Sciences (also translated to Chinese), and editor of the volume Diffusion, Atomic Ordering and Mass Transport. Prof. Ganguly is a fellow of the Mineralogical Society of America and the American Geophysical Union, and a recipient of the Alexander von Humboldt research prize from Germany.



"If we desire to fight successfully the scourge of poverty and want from which 90 percent of our countrymen are suffering, if we wish to remodel our society and renew the spring of our civilization, and lay the foundation of a strong and progressive national life we must make the fullest use of the power which a knowledge of Nature has given us. We must rebuild our economic system by utilizing the resources of our land, harnessing the energy of our rivers, prospecting for the riches hidden under the bowels of the earth, reclaiming deserts and swamps, conquering the barriers of distance and above all, we must mould anew the nature of man in both individual and social aspects so that a richer, more harmonious and happier race may live in this great and ancient land of ours. Towards the realization of this ideal, we people must adopt ourselves to the new philosophy of life and train the coming generations for the services of the community in scientific studies and research".

– M.N. Saha

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